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PHILOSOPHICAL TRANSACTIONS,

OF THE

ROYAL SOCIETY

OF

LONDON.

VOL. LXXV. For the Year 1785.

PART I.

LONDON,

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MDCCLXXXV.

ADVERTISEMENT.

THE Committee appointed by the *Royal Society* to direct the publication of the *Philosophical Transactions*, take this opportunity to acquaint the Public, that it fully appears, as well from the council-books and journals of the Society, as from repeated declarations which have been made in several former *Transactions*, that the printing of them was always, from time to time, the single act of the respective Secretaries, till the Forty-seventh Volume: the Society, as a Body, never interesting themselves any further in their publication, than by occasionally recommending the revival of them to some of their Secretaries, when, from the particular circumstances of their affairs, the *Transactions* had happened for any length of time to be intermitted. And this seems principally to have been done with a view to satisfy the Public, that their usual meetings were then continued for the improvement of knowledge, and benefit of mankind, the great ends of their first institution by the Royal Charters, and which they have ever since steadily pursued.

But the Society being of late years greatly inlarged, and their communications more numerous, it was thought advisable, that a Committee of their members should be appointed to reconsider the papers read before them, and select out of them such, as they should judge most proper for publication in the future *Transactions*; which was accordingly done upon the 26th of March 1752. And the grounds of their choice are, and will continue to be, the importance and singularity of the subjects, or the advantageous manner of treating them; without pretending to answer for the certainty of the facts, or propriety of the reasonings, contained in the several papers so published, which must still rest on the credit or judgment of their respective authors.

It is likewiſe neceſſary on this occaſion to remark, that it is an eſtabliſhed rule of the Society, to which they will always adhere, never to give their opinion, as a Body, upon any ſubject, either of Nature or Art, that comes before them. And therefore the thanks, which are frequently propoſed from the chair, to be given to the authors of ſuch papers as are read at their accuſtomed meetings, or to the perſons through whoſe hands they receive them, are to be conſidered in no other light than as a matter of civility, in return for the reſpect ſhewn to the Society by thoſe communications. The like alſo is to be ſaid with regard to the ſeveral projects, inventions, and curioſities of various kinds, which are often exhibited to the Society; the authors whereof, or thoſe who exhibit them, frequently take the liberty to report, and even to certify in the public news-papers, that they have met with the higheſt applauſe and approbation. And therefore it is hoped, that no regard will hereafter be paid to ſuch reports, and public notices; which in ſome inſtances have been too lightly credited, to the diſhonour of the Society.

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THE President and Council of the Royal Society adjudged, for the Year 1784, the Medal on Sir GODFREY COPLEY's Donation, to EDWARD WARING, M. D. Lucasian Professor of the Mathematics at Cambridge, for his Mathematical Communications to the Society.

PHILOSOPHICAL TRANSACTIONS.

I. *An Account of an artificial Spring of Water.* By Erasmus Darwin, *M. D. F. R. S.*

Read November 4, 1784.

To the President and Fellows of the Royal Society.

GENTLEMEN, Derby, July 16, 1784.

CONFIDENT that every atom which may contribute to increase the treasury of useful knowledge, which you are so successfully endeavouring to accumulate, will be agreeable and interesting to the Society, I send you an account of an artificial spring of water, which I produced last summer, near the side of the river Darwent in Derby.

Near my houſe was an old well, about one hundred yards from the river, and about four yards deep, which had been many years diſuſed on account of the badneſs of the water, which I found to contain much vitriolic acid, with, at the ſame time, a ſlight ſulphureous ſmell and taſte; but did not carefully analyſe it. The mouth of this well was about four feet above the ſurface of the river; and the ground, through which it was ſunk, conſiſted of a black, looſe, moiſt earth, which appeared to have been very lately a moraſs, and is now covered with houſes built upon piles. At the bottom was found a bed of red marl, and the ſpring, which was ſo ſtrong as to give up many hogſheads in a day, oozed from between the moraſs and the marl: it lay about eight feet beneath the ſurface of the river, and the water roſe within two feet of the top of the well.

Having obſerved that a very copious ſpring, called Saint Alkmund's well, roſe out of the ground about half a mile higher on the ſame ſide of the Darwent, the level of which I knew by the height of the intervening wier to be about four or five feet above the ground about my well; and having obſerved, that the higher lands, at the diſtance of a mile or two behind theſe wells, conſiſted of red marl like that in the well; I concluded, that, if I ſhould bore through this ſtratum of marl, I might probably gain a water ſimilar to that of St. Alkmund's well, and hoped that at the ſame time it might riſe above the ſurface of my old well to the level of St. Alkmund's.

With this intent a pump was firſt put down for the purpoſe of more eaſily keeping dry the bottom of the old well, and a hole about two and an half inches diameter was then bored about thirteen yards below the bottom of the well, till ſome ſand was brought by the auger. A wooden pipe,

which was previouſly cut in a conical form at one end, and armed with an iron ring at the other, was driven into the top of this hole, and ſtood up about two yards from the bottom of the well, and being ſurrounded with well-rammed clay, the new water aſcended in a ſmall ſtream through the wooden pipe.

Our next operation was to build a wall of clay againſt the moraſſy ſides of the well, with a wall of well-bricks internally, up to the top of it. This completely ſtopped out every drop of the old water; and, on taking out the plug which had been put in the wooden pipe, the new water in two or three days roſe up to the top, and flowed over the edges of the well.

Afterwards, to gratify my curioſity in ſeeing how high the new ſpring would riſe, and for the agreeable purpoſe of procuring the water at all times quite cold and freſh, I directed a pipe of lead, about eight yards long, and three-quarters of an inch diameter, to be introduced through the wooden pipe deſcribed above, into the ſtratum of marl at the bottom of the well, ſo as to ſtand about three feet above the ſurface of the ground. Near the bottom of this leaden pipe was ſewed, between two leaden rings or flanches, an inverted cone of ſtiff leather, into which ſome wool was ſtuffed to ſtretch it out, ſo that, after having paſſed through the wooden pipe, it might completely fill up the perforation of the clay. Another leaden ring or flanch was ſoldered round the leaden pipe, about two yards below the ſurface of the ground, which, with ſome doubles of flannel placed under it, was nailed on the top of the wooden pipe, by which means the water was perfectly precluded from riſing between the wooden and the leaden pipes.

This being accomplished, the bottom of the well remained quite dry, and the new water quickly rose about a foot above the top of the well in the leaden pipe; and, on bending the mouth of this pipe to the level of the surface of the ground, about two hogsheads of water flowed from it in twenty-four hours, which had similar properties with the water of St. Alkmund's well, as on comparison both these waters curdled a solution of soap in spirit of wine, and abounded with calcareous earth, which was copiously precipitated by a solution of fixed alkali; but the new water was found to possess a greater abundance of it, together with numerous small bubbles of aërial acid or calcareous gas.

The new water has now flowed about twelve months, and, as far as I can judge, is already increased to almost double the quantity in a given time; and from the rude experiments I made, I think it is now less replete with calcareous earth, approaching gradually to an exact correspondence with St. Alkmund's well, as it probably has its origin between the same strata of earth.

As many mountains bear incontestible marks of their having been forcibly raised up by some power beneath them; and other mountains, and even islands, have been lifted up by subterraneous fires in our own times, we may safely reason on the same supposition in respect to all other great elevations of ground. Proofs of these circumstances are to be seen on both sides of this part of the country; whoever will inspect, with the eye of a philosopher, the lime-mountain at Breedon, on the edge of Leicestershire, will not hesitate a moment in pronouncing, that it has been forcibly elevated by some power beneath it; for it is of a conical form, with the apex cut off, and

and the ſtrata, which compoſe the central parts of it, and which are found nearly horizontal in the plain, are raiſed almoſt perpendicularly, and placed upon their edges, while thoſe on each ſide decline like the ſurface of the hill; ſo that this mountain may well be repreſented by a bur made by forcing a bodkin through ſeveral parallel ſheets of paper. At Router, or Eagle-ſtone, in the Peak, ſeveral large maſſes of grit-ſtone are ſeen on the ſides and bottom of the mountain, which by their form evince from what parts of the ſummit they were broken off at the time it was elevated; and the numerous looſe ſtones ſcattered about the plains in its vicinity, and half buried in the earth, muſt have been thrown out by exploſions, and prove the volcanic origin of the mountain. Add to this the vaſt beds of toad-ſtone or lava in many parts of this county, ſo accurately deſcribed, and ſo well explained, by Mr. WHITEHURST, in his Theory of the Formation of the Earth.

Now as all great elevations of ground have been thus raiſed by ſubterraneous fires, and in a long courſe of time their ſummits have been worn away, it happens, that ſome of the more interior ſtrata of the earth are expoſed naked on the tops of mountains; and that, in general, thoſe ſtrata, which lie uppermoſt, or neareſt to the ſummit of the mountain, are the loweſt in the contiguous plains. This will be readily conceived if the bur, made by thruſting a bodkin through ſeveral parallel ſheets of paper, had a part of its apex cut off by a penknife, and is ſo well explained by Mr. MICHELL, in an ingenious paper on the Phænomena of Earthquakes, publiſhed a few years ago in the Philoſophical Tranſactions.

And as the more elevated parts of a country are ſo much colder than the vallies, owing, perhaps, to a concurrence of

two

two or three causes, but particularly to the less condensed state of the air upon hills, which thence becomes a better conductor of heat, as well as of electricity, and permits it to escape the faster; it is from the water condensed on these cold surfaces of mountains, that our common cold springs have their origin; and which, sliding between two of the strata above described, descend till they find or make themselves an outlet, and will in consequence rise to a level with the part of the mountain where they originated. And hence, if by piercing the earth you gain a spring between the second and third, or third and fourth stratum, it must generally happen, that the water from the lowest stratum will rise the highest, if confined in pipes, because it comes originally from a higher part of the country in its vicinity.

The increasing quantity of this new spring, and its increasing purity, I suppose to be owing to its continually dissolving a part of the earth it passes through, and hence making itself a wider channel, and that through materials of less solubility. Hence it is probable, that the older and stronger springs are generally the purer; and that all springs were originally loaded with the soluble impurities of the strata, through which they transuded.

Since the above-related experiment was made, I have read with pleasure the ingenious account of the King's wells at Sheerness, in the last volume of the Transactions, by Sir Thomas Hyde Page, in which the water rose three hundred feet above its source in the well; and have also been informed, that in the town of Richmond, in Surrey, and at Inship near Preston in Lancashire, it is usual to bore for water through a lower stratum of earth to a certain depth; and that when it

is

is found at both thoſe places, it riſes ſo high as to overflow the ſurface of the well: all theſe facts contribute to eſtabliſh the theory above-mentioned. And there is reaſon to conclude, that if ſimilar experiments were made, artificial ſprings, riſing above ground, might in many places be thus produced at ſmall expence, both for the common purpoſes of life, and for the great improvement of lands by occaſionally watering them.

II. *An Account of an* English *Bird of the Genus* Motacilla; *ſuppoſed to be hitherto unnoticed by* Britiſh *Ornithologiſts; obſerved by the Rev.* John Lightfoot, *M. A. F. R. S. In a Letter to Sir* Joſeph Banks, *Bart. P. R. S.*

Read November 18, 1784.

SIR, Uxbridge, Nov. 20, 1783.

AS every diſcovery in natural hiſtory is eſteemed worthy the notice of that Society which was inſtituted on purpoſe to improve natural knowledge, I have taken the liberty to ſend you a deſcription and drawing of a bird which haunts the reeds of the river Coln, in the neighbourhood of Uxbridge, and which ſeems to have hitherto eſcaped the notice of writers on Britiſh Ornithology; and therefore ſome account and deſcription of it will not, I truſt, be unacceptable to the Society over which you ſo laudably preſide.

The neſt and eggs of the bird I am about to deſcribe firſt attracted my attention, and led to the diſcovery of the bird itſelf. They were repeatedly brought by a fiſherman on the Uxbridge river, in the pariſh of Denham, to her grace the Ducheſs Dowager of Portland, who firſt communicated them to me. They were ſuppoſed by the fiſherman to belong to the *Sedge-bird* of PENNANT, or *Motacilla Salicaria* of LINNÆUS; but being well acquainted with the neſt and eggs of this, I was very ſure he was miſtaken, though he actually produced this bird as the true proprietor of the ſubjects in queſtion. The ſtructure and poſition of the neſt having a ſingular appearance, and both that

that and the eggs belonging to a bird unknown to me, I became desirous of finding out the secret architect, and to that end made use of such means as I thought most likely to promote the discovery.

In a short time my expectations were gratified; for on the 26th day of July, 1783, intelligence was brought me, that such a nest as I wanted was found. I had given previous direction, that it should not be disturbed before I had seen it. Upon examination, I instantly perceived it to be of the same kind and structure with that under enquiry, containing two eggs, and two young ones just excluded from the shell. One of the old birds was sitting at this time upon the nest, which a person in company attempting to seize, it flew at him with so much resentment and acrimony, as to draw blood from the hand that dared to molest its instinctive operations. Both the parent birds continued hovering about their nest with much watchful care and anxiety, while I made several attempts to take them alive; but, finding all endeavours in vain, lest I should lose the opportunity of examining them with accuracy, I at length, with reluctance, caused them to be shot. From these specimens the following descriptions were made, which, with an accurate drawing of one of them, together with its nest and egg, are humbly submitted to your notice.

From the generic characters delivered by LINNÆUS, our bird must evidently be reduced to the family of his *Motacilla*, for it has a weak, slender, subulate bill, almost straight; the mandibles nearly equal; the nostrils oval and naked, or not covered with bristles; the tongue lacerated at the extremity; the legs slender; the toes divided to the origin, except that the exterior one is joined, at the under part of the last joint, to the middle toe; the claws of nearly equal length.

The male and female have the same coloured plumage, so that one description will serve for both. They differ a little in size, but their external appearance is the same. They are both larger than the *Pettychaps* described by WILLOUGHBY; smaller than the *White-throat*, and nearly of the same size with the *Willow-wren*; but to be more particular.

The cock-bird weighed, when just killed, exactly seven pennyweights and nine grains; the hen six pennyweights and nine grains, or one pennyweight less.

The males measured, from tip to tip of the extended wings, seven inches and a half; the female six and three-quarters.

From the end of the bill to the extremity of the tail, the cock measured five inches and a half; the hen only five inches.

The bill in both measured half an inch, which is longer in proportion than in most of this genus. The *upper* mandible is of a dark horn colour, slightly incurved near the extremity, with a minute indenture on either side near the point; the *lower* is pale red or flesh-coloured, with a shade of yellow; the inside of the mouth deep orange-coloured; the tip of the tongue cloven and ciliated; the nostrils oval, and destitute of a bristly covering; but at the base of the upper mandible, on either side, near the angle of the mouth, arise three short *vibrissæ* pointing downwards, black at their summits, white at their bases; a circumstance common to many others of this genus. The *iris* of the eye is olive-brown; the pupil black. The short feathers of the orbits or eye-lashes are of a dirty white colour. From the corner of each eye to the nostril is a broad stroke or band of tawny-white feathers, lying over each other, and running narrowest towards the bill; this affords an excellent mark to distinguish the species.

The

The feathers of the head, neck, back, coverts of the wings and rump, are of an olive-brown, with a slight tinge of green. The quill and tail feathers are all of a darker hue, or simply brown; their outward edges of a paler shade. The tail is two inches long, slightly cuneated, the middle feathers being a little longer than the rest, the others gradually shorter; all of one uniform dun-brown colour edged with paler brown, and a little wedge-shaped at their ends.

The chin is white; the throat, breast, belly, and parts about the vent, are white with a slight shade of buff or tawny; but all these feathers (as in several others of this genus) when blown asunder, or closely examined, are found to have their base or lower half black, except the shafts, which are white throughout.

The ridge and under coverts of the exterior angle of the wing are of a yellowish-tawny colour, as are also the feathers of the thighs; but those of the knees are a shade darker, or a pale yellowish brown.

The legs are a light olive; the soles of the feet bright yellow, with a tinge of green, which soon fades after the bird is dead. The instep is covered with seven large imbricated scales, and five smaller on the toes, as in others of the genus. The toes stand three before, and one behind: the claws are nearly of equal length and curvature; but the hindmost is thickest and strongest.

From the foregoing remarks it is evident, that the bird mentioned is a species of *Motacilla*, which, as I can find no such described by any systematic writer, I shall venture to name, after the LINNÆAN manner,

Motacilla (*arundinacea*) supra olivaceo-fusca, subtus albida, loris et orbitis fusco-albescentibus, angulo carpi subtus luteo-

luteo-fulvo, cauda subcuneata fusca, plantis luteo-virescentibus.

In regard to synonyms, the only author I can find who can be suspected of having noticed this bird is SEPP, who, in a late splendid work, in the Dutch language, intituled, Nederlandsche Vogelen *(fol. chart. max.)* p. 101. has described and figured a bird, under the name of *Turdus arundinaceus minimus*, called in Holland *Karrakietje*, which in many respects agrees with our bird; but as the colour of the wings in that figure is made a reddish brown, instead of an olive-brown, and the tawny-white *Lora* (a most essential character to distinguish the species) are not at all expressed; and the eggs are made to be of a pale-bluish colour with dark spots, instead of a dirty-white with olive spots; I cannot pronounce for certain, that the bird there intended by that writer is the same which we have now described; though, if some allowance be made for ill-colouring and other omissions, it may possibly have been designed for the same species.

As we have already a bird, called in English the *Willow-wren*; ours, being nearly of the same size and shape, as well as the same genus, may, from its haunts, not improperly be denominated the *Reed-wren*.

It frequents the banks of the river Coln near Uxbridge, as far as from Harefield-Moor down to Iver, about the space of five miles, and very probably most other parts of the same river, though not as yet observed.

It is also certainly found in the neighbourhood of Dartford in Kent, from whence a nest and eggs were communicated by the ingenious Mr. LATHAM of that place, but without knowledge of the bird to which they belonged; so that there is little doubt but that it may be found in many parts of the kingdom.

Its

Its food is insects, at least in part, for I observed it catching flies. It hops continually from spray to spray, or from one reed to another, putting itself into a stooping posture before it moves. I heard it make no other than a single note, not unlike the sound of the word *peep*, uttered in a low plaintive tone; but this might probably be only a note of distress, and it may have, perhaps, more pleasing and melodious ones at other times, with which I am unacquainted.

The nest of this bird is a most curious structure, unlike that of any other I am acquainted with, enough to point out the difference of the species, if every other character was wanting.

It may not be amiss here to observe, that there is such a manifest diversity in the materials, locality, and formation of nests, and such variety of colours in the eggs of many birds (in other respects hard to be distinguished), that it is pity this part of Ornithology has not been more attended to. I am well convinced, that as many species of *insects*, nearly allied to each other in colours and shape, and reputed to be only *varieties*, are frequently, from a due attention to their *larvæ* (which are often extremely different), discovered to be species *totally distinct*; so, amongst *birds* of similar genus and feather, their true differences may be often found by carefully observing their nests and eggs, when other characters are so minute, in the birds themselves, as to be distinguished with difficulty. By experience I have found this to be remarkably verified in some of the Lark kind.

But to return to the nest I was going to describe. It is composed externally of dry stalks of grass, lined, for the most part, with the flowery tufts of the common reed, or *Arundo vallisneria*, but sometimes with small dead grasses, and a few black horse-hairs to cover them. This nest is usually found

suspended

suspended or fastened on, like a hammock, between three or four stalks of reeds, below the panicles of flowers, in such a manner that the stalks run through the sides of the nests at nearly equal distances; or, to speak more properly, the nest is tied on to the reeds with *dead grass*, and sometimes (as being more eligible when it can be had) even with *thread* and *pack-thread*, emulating the work of a sempstress; as was the case of the nest exhibited in the drawing. The bird, however, though generally, does not always confine her building to the support of reeds; sometimes she fixes it on to the branches of the *Water-dock*; and, in one instance only (that here delineated), it was found fastened to the trifurcated branch of a *Syringa* bush, or *Philadelphus*, growing in a garden hedge by the river side.

She lays commonly four eggs; the ground colour a dirty white, stained all over with dull olive-coloured spots, but chiefly at the greater end, where are generally seen two or three small irregular black scratches; but these are sometimes scarcely visible.

I must not omit, that both the nest and eggs which I have now described, whether designed for the same or not, are well expressed by SEPP, in the work above cited, under the article *Turdus Calamoxenus*, or *Rietvinck*, p. 97.; but as the bird there represented is evidently the *Motacilla Sylvia*, LIN. or common *White-throat* (which is known to make a very different nest), I am inclined to believe, that the author, by mistake, placed a bird and nest in the same plate which do not belong to each other.

I have reason to think, that the bird I have been characterizing is a bird of migration; for the inhabitants on the sides

of the Coln do not recollect ever to have ſeen it in the winter months; and its food being inſects, it is probable, it muſt be obliged to ſhift its quarters for a warmer climate at the approach of a ſevere ſeaſon; but this at preſent is only matter of conjecture, and not certainty.

I am, &c.

JOHN LIGHTFOOT.

III. *An Account of* Morne Garou, *a Mountain in the Island of* St. Vincent, *with a Description of the Volcano on its Summit. In a Letter from Mr.* James Anderson, *Surgeon, to Mr.* Forsyth, *His Majesty's Gardener at* Kensington; *communicated by the Right Honourable Sir* George Yonge, *Bart. F. R. S.*

Read November 18, 1784.

THE many ridges of mountains which intersect this island in all directions, and rise in gradations, one above the other, to a very great height, with the rivers tumbling from their sides over very high precipices, render it exceeding difficult to explore its interior parts.

The most remarkable of these mountains is one that terminates the N.W. end of the island, and the highest in it, and has always been mentioned to have had volcanic eruptions from it. The traditions of the oldest inhabitants in the island, and the ravins at its bottom, seem to me to vindicate the assertion As I was determined, during my stay in the island, to see as much of it as I could; and as I knew, from the altitude of this mountain, there was a probability of meeting with plants on it I could find in no other part of the island; I should have attempted going up if I had heard nothing of a volcano being on it. But viewing the mountain at a distance, the structure of it was different from any in the island, or any I had seen in the West Indies. I could perceive it divided into many

 different

different ridges, separated by very deep chasms, and its summit appeared quite destitute of any vegetable production. On examining several ravins, that run from the bottom a great way up the mountain, I perceived they were quite destitute of water, and found pieces of pumice-stone, charcoal, several earths and minerals, that plainly indicated there must be some very singular place or other on some part of the mountain. I also recollected a story told by some very old men in the island, that they had heard the captain of a ship say, that between this island and St. Lucia he saw, towards night, flames and smoke issuing from the top of this mountain, and next morning his decks were covered with ashes and small stones. This, you may readily imagine, was excitement enough to examine it, if I possibly could; but I was much discouraged upon being told, it was impossible to gain the summit of it; nor could I get either white men, Carribbee, or Negro, that would undertake to conduct me up for any reward I could offer; nor could I get any information relative to it. But as difficulty to attain inhances the value of the object, so the more I was told of the impossibility of going up, the more was I determined to attempt it.

After I had examined the basis of it, as far as I could for the sea and other mountains, to find the most probable place to commence my journey, I observed an opening of several large and dry ravins, that seemingly ran a great way up; but I was not sure if they were not intersected by some rocks or precipices I could not get over. I came to Mr. MALOUNE'S, about a mile distant from the mountain, but the nighest house to it I could stay at all night. Here I met with a friendly reception and great hospitality. After communicating my intentions to him, he told me, he would give me every assistance

he could, by ſending ſome truſty negroes with me, and wiſhed he was able to go with me himſelf. This was a kind offer to me, in my then ſituation, as negroes were what I only wanted, having only one boy belonging to Dr. YOUNG with me. I knew, if I had great difficulties in the woods, he and I both ſhould be inadequate to the taſk, as in a ſhort time we ſhould be ſo wearied as to be unable to proceed: from what I had ſeen of the mountain, I knew I muſt be under the neceſſity of carrying water with me; and from the great diſtance to the top, and obſtructions we might naturally expect, I ſhould at leaſt require two days to accompliſh it.

By examining the ſide of the mountain towards me with a good glaſs, I imagined I ſaw two ridges I might get up. I perceived they were covered great part of the way with thick wood; yet I hoped, with a little cutting, I ſhould be able to ſcramble through them. I appointed next morning to begin my route by one of theſe ridges.

February 26, 1784, I left Mr. MALOUNE's about ſun-riſe, with two ſtout negroes and Dr. YOUNG's boy; each of us having a good cutlaſs, as well to clear our way through the woods, as to defend us in caſe we ſhould be attacked by Caribbees or run-away negroes. We arrived at the bottom of the mountain a little before ſeven in the morning. To get to either of the ridges, we found we had a rock to climb above forty feet high: it was with great difficulty we ſcrambled up, aſſiſting one another in the beſt manner we could; here we found it neceſſary to contract our baggage. After getting up this rock, I found myſelf in the bottom of a narrow and deep ravin. Having aſcended this ravin a little way, I ſaw ſome cleared ground on its ſides, with tobacco growing. This I conjectured was the habitation of ſome Caribbees; but I was much

ſurpriſed

ſurpriſed when one of the negroes I had with me told me, it was the habitation of a Mr. GASCO, a Frenchman. What could induce a ſtout healthy man in the prime of life, and a good mechanick, with ſeveral negroes, to take up his reſidence among rocks and precipices, excluded from the whole world, is a myſtery to me. Beſides, by every torrent of rain that happens, he may expect himſelf and all his habitation to be waſhed over the rocks into the ocean. Notwithſtanding his ſingular ſituation, I found him an intelligent man, and I experienced every hoſpitality his poor cottage could afford.

The difficulty of going through woods in the Weſt Indies, where there are no roads or paths, is far beyond any thing an European can conceive. Beſides tall trees and thick underwood, there are hundreds of different climbing plants twiſted together like ropes, and running in all directions to a great extent, and even to the tops of the higheſt trees; by puſhing on they cannot be broke, and many of them with difficulty cut; beſides a ſpecies of graſs, the *Schoenus Lithoſpermos*, with ſerrated leaves, that cuts and tears the hands and face terribly. With ſuch obſtructions as theſe it was above two hours before we got on the ridge, where I was in hopes our paſſage would have been eaſier; but I ſoon found my miſtake, for I was ſurrounded with a thick foreſt, much more difficult to get through than before, on account of the large piles of trees broken down by the hurricanes, to paſs which in many parts we were obliged to creep on our hands and feet to get below them, and in other places to climb a great height above the ſurface of the ground, to get over large trunks lying on one another, and theſe being frequently rotten, occaſioned us to tumble headlong down to a great depth, among rotten wood and graſs, ſo that it was with great difficulty I and the negroes could extricate ourſelves. By

constantly cutting to clear our way, I, as well as my companions, grew much fatigued, and they wished much to return back. About four in the afternoon I could not prevail upon them to proceed farther; if they did, they could not return before dark, and they would not sleep all night in the woods; but said, if I stayed they would return to me next morning. I saw it was impossible to gain the summit of the mountain with the boy only by that route: I likewise saw the woods growing more difficult, my water also totally expended: from these considerations I intended to go down to the Frenchman's, and remain there all night, and try another route with my boy next morning, hoping I might be fortunate enough to find an easier passage. I arrived at Mr. GASCO's a little after sun-set, being much fatigued and thirsty, and never experienced more hospitality and kindness than from this man in his miserable cot; for we ought not to judge of the value of the things received, but of the disposition of the heart with which they are given. He parted with his hammock to me, and slept on a board himself. This I at first refused; but he insisted on it, telling me, from my hardships of the day I was much more tired than he. I took the hammock, but I found it was impossible to close my eyes during the night with cold. His hut was built of *roseaux* or large reeds, between each of which a dog might creep through, and the top was covered with dry grass. It is situated in the bottom of a deep gully, where the sun does not shine till nine in the morning, nor after four in the afternoon. It is surrounded by thick wood, and during the night the whole of the mountain is covered with thick clouds, from which it frequently rains; this makes the night air exceedingly cold. I got ready to renew my journey next morning, having only Dr. YOUNG's boy with me, who continued

very faithful to me during this excursion, being very active and hardy: I do not know if I could have gone through this fatigue had it not been for his assistance. I now determined to commence this day's route up the ravin, as it seemed to widen and apparently run a considerable way up in the direction I wished for; and if I could get out of it upon the other ridge, it would at least be two miles nearer than the way I had attempted yesterday, and probably, after getting out of it, I might find wood easier of access. In this ravin I got up about a mile and a half, without meeting with any considerable obstruction. Encouraged by getting so far, although the ravin was narrowing fast, with numbers of rocks and precipices to climb over, with vines and bushes difficult to get through, I was resolved to persist in this route, and determined by every possible means to get to the object of my wishes, well knowing if I could not perform it this way, I might abandon it entirely. After climbing over a number of difficult passes, the ravin terminated at the bottom of a very high precipice; how far it was to the summit I did not know, being covered toward the top with thick wood; but from the bottom upwards it was loose sand as far as I could see, with ferns and tufts of grass, which, as soon as I took hold of them, came out at the roots. The precipice being so very steep, with no trees or bushes on it to assist me in getting up, I plainly saw the attempting to climb it was at the risk of my life: however, I was resolved to try it, and telling the boy to keep some distance behind me, in case I should tumble and drive him down along with me, I began to ascend, holding the tufts of grass as lightly as possible, and digging holes with my cutlass to put my feet in; but I often lost my hold, and frequently slipped down a considerable distance: however, as it was nothing but loose

loose sand, I could easily push my cutlass into it to the handle, and by grasping it could recover myself again. Had I not taken the resolution before I began to ascend to divest myself of fear, I could not possibly have gone, for the terror of falling would have been the means of it every instant. I got up to some wild plantains, which I saw continued all the way to the place where the bushes and trees began to grow. I here rested myself, and waited for the boy's getting to me, which he did much easier than I, although he had the provisions and water, owing to the track I had made, and because, being much lighter, he could better trust himself to the grass and ferns. After some labour we arrived at the top of the precipice. I found myself on a very narrow ridge, thickly covered with wood, and bounded by two ravins, the bottoms of which I could not see; the descent to them seemed to be nearly perpendicular, yet all the way covered with thick wood. After refreshing ourselves, we began our fatigue, the boy and I cutting, and carrying our water and provisions, alternately. When we had got some way, I found I was on an exceeding narrow ridge, in many parts not six feet broad; on each side a tremendous gulf, into one or other of which I was often in danger of falling, so that with great caution I was obliged to lie down on my belly, to see through the bushes how the ridge tended. Here I began to smell sulphur, or rather a smell like gunpowder. As I knew this smell must come from the top of the mountain, being in the direction of the wind, I was in hopes we could not be far from it, as the smell grew stronger and stronger as I ascended. I saw a rising before me, and thought if I was once on it, if the top of the mountain was near I could have a view of it; but having got on this rising I could only see a high peak on the N.W. end of the mountain, and by appearance I thought

myſelf very little nearer than when I was at the bottom. The woods now became very difficult to get through; great quantities of fallen trees lying buried under long graſs and being rotten, when I thought myſelf walking on the ground, I was frequently buried a great depth among them. Being now about noon, and my turn to carry the baggage, and conſequently my turn of reſt, I was ſurpriſed to hear a ruſtling among the buſhes, and ſomething like a human voice behind me. As we were now in a place where I had little reaſon to ſuppoſe there had been a human foot before, and could not imagine there could be habitations of Caribbees or run-away negroes, ſince from the barrenneſs of the mountain they could not poſſibly find any proviſions to ſubſiſt on, I told the boy to ſtand ſtill, and let us wait their coming up; for if they were Caribbees advancing with an intention to hurt us, there was no alternative but to defend ourſelves. You may imagine my ſurpriſe when I ſaw one of the negroes who had been with me the day before, with three others, which Mr. MALOUNE had ſent to my aſſiſtance, with plenty of proviſions. After refreſhment, with this aſſiſtance, I renewed my labours with freſh ſpirits, and thought I was ſure of reaching the top before night. Having proceeded a little, I had a fair view of the ravin on my left, which was of prodigious depth, and ran from near the top of the mountain to the ſea; its bottom ſeemed to be a rock of a colour nearly reſembling lava, and appeared as if there had been vaſt torrents of ſulphureous matter running in it ſome time. I regretted much I knew not of this ravin before I commenced my excurſion, as by paſſing a head-land in a canoe, and getting into the ravin, I might have gained the ſummit of the mountain, without experiencing the delays and difficulties I here encountered. It was now about

4 P.M.

4 P.M. and I had no prospect of the mountain's top; but from the ascent of the ravin below, I knew it was a great way off. I thought if I could get into the ravin before night, I could get easily up next morning. After cutting a great way through wild plantains, the sun near setting, I found myself almost over the verge of a precipice; by catching hold of some shrubs I prevented myself from falling. We were now about half-way down; but all the way below us, as far as we could see, was a perpendicular precipice of rock, several hundred feet high, to pass which was impossible. I had a view of some part of the top of the mountain, which I saw was yet far from me; nor could I attempt any other way than the ridge I had left. Being now sun-set, and the negroes very discontented, because they could not return that night, I found we must take up our night's residence in the place where we were. It was a very unfavourable one, there being nothing but plantains growing, which retaining the rain long in their leaves, and being frequently agitated by the wind, were constantly dropping, and kept the ground always moist. Being almost dark, we had time to make us no other habitation, than placing two or three sticks against an old stump of a tree, and slightly covering them with plantain leaves. After getting together some little wood to make a fire to keep us comfortable, it began to blow and rain violently, which continued all night. We soon found our building afforded us no shelter, and the wood would not burn, so that we could not get any fire; and the ground on which we were situated would not allow the least exercise to keep us warm. From such a miserable night I experienced no mitigation for the fatigues of the day. I wished for the rising sun, to renew my labours; which I at last beheld with inexpressible joy.

As

As ſoon as we could ſee, we returned to the ridge we left the night before, and began to work with alacrity, as we were almoſt chilled with cold. I puſhed on as faſt as poſſible, and about ten o'clock found the woods began to grow thin. I could not ſee the top of the mountain, but had a view of ſeveral ridges that joined it. From the wind falling, and the heat growing intenſe, I thought we muſt then be under the cover of the ſummit: I here found many new plants. About eleven A.M. I was overjoyed to have a full view of the ſummit of the mountain, nearly a mile diſtant from us, and that we were nearly out of the woody region. The top ſeemed to be compoſed of ſix or ſeven different ridges, very much broken in the ſides, as if they had ſuffered great convulſions of nature; they were divided by amazing deep ravins. without any water in them. I obſerved where the ridges meet the edge of a large excavation, as it ſeemed to be, on the higheſt part. I imagined this might be the mouth of the crater, and directed my courſe to a high peak which overlooked it. I found here a moſt beautiful tree which compoſed the laſt wood. After that I entered into a thick long graſs, intermixed with fern, which branched and ran in every direction. To break it was impoſſible, and with great difficulty I could cut it; ſo that in clearing our way through this graſs, eight or ten feet high, there was equal difficulty as in the woods, and it ſeemed to continue very near to the top of the mountain. Being now about noon, I and the negroes were ſo fatigued as hardly to be able to ſtand; our thirſt very great, to allay which, as much as poſſible, we chewed the leaves of the Begonia obliqua. Two of the negroes returned, and the others ſaid they would go no farther with me, as they muſt periſh for want of water, and it would be impoſſible to get to the bottom before night, and they muſt all

die in the woods. The propriety of their reasoning was evident to me; yet I thought it hard, after the fatigues of three days and two nights, to be within half a mile of the top, and not be able to get up, and to know little more about it than I did at the bottom. As the negroes had not the same motive for going up as I, all my reasoning was to them ineffectual; I found I was obliged to return myself, as I could not persist alone. At half past twelve we began to descend the same way we came. As there was now a clear path all the way to the bottom, we got down to Mr. GASCO's by sun-set. After sitting some time here, I was hardly able to rise again, I was so tired; and my feet were so sore I could hardly stand on them, for, my shoes being torn to pieces, I came down the whole way bare-footed. I continued my journey, however, to Mr. MALOUNE's, where I arrived between six and seven at night.

March 4th, being the day I had fixed to finish my excursion, about four in the morning, I left the house of Mr. FRASER, who out of curiosity agreed to accompany me, of which I was very glad, as he was a sensible young man; and with the assistance of two negroes we pursued our journey. We found very little obstruction in our way up, until we got to the place where I returned; and there, for about a quarter of a mile, we had considerable difficulty to clear our way through grass and ferns. After we came within a quarter of a mile from the top, we found ourselves in another climate all at once, the air very cold, and the vegetable productions changed; here was nothing but barrenness over the whole summit of the mountain. On the confines of the grassy region and the barren I found some beautiful plants. Moss grows here in such plenty, that I frequently sunk up to my knees in it. This is the only place in the West Indies that produced any moss that I have seen. About noon

we gained the top of the peak I had directed my course to before; when, in an instant, we were surprised with one of the grandest and most awful scenes I had ever beheld. I was struck with it amazingly, as I could not have conceived such a very large and so singularly formed an excavation. It is situated on the center of the mountain, and where the various ridges unite. Its diameter is something more than a mile, and its circumference to appearance a perfect circle. Its depth from the surrounding margin is above a quarter of a mile, and it narrows a little, but very regularly, to the bottom. Its sides are very smooth, and for the most part covered with short moss, except towards the south, where there are a number of small holes and rents. This is the only place where it is possible to go down to the bottom: it is exceedingly dangerous, owing to the numberless small chasms. On the west side is a section of red rock like granite, cut very smooth, and of the same declivity with the other parts. All the rest of the surrounding sides seems to be composed of sand, that looks to have undergone the action of intense fire. It has a crust quite smooth, of about an inch thick, and hard almost as rock; after breaking through which, you find nothing but loose sand. In the center of the bottom is a burning mountain of about a mile in circumference, of a conic form, but quite level. On the summit, out of the center of the top, arises another mount, eight or ten feet high, a perfect cone; from its apex issues a column of smoke. It is composed of large masses of red *granite-like* rock of various sizes and shapes, which appear to have been split into their present magnitudes by some terrible convulsion of nature, and are piled up very regular. From most parts of the mountain issue great quantities of smoke, especially on the north side, which appears to be burning from top to bottom,

and the heat is ſo intenſe, that it is impoſſible to go upon it. Going round the baſe is very dangerous, as large maſſes of rock are conſtantly ſplitting with the heat, and tumbling to the bottom. At the bottom, on the north ſide, is a very large rock ſplit in two; each of theſe halves, which are ſeparated to a conſiderable diſtance from each other, is rent in all directions, and from the crevices iſſue efflorescences of a gloſſy appearance, which taſte like vitriol, and alſo beautiful cryſtallizations of ſulphur. On all parts of the mountain are great quantities of ſulphur in all ſtates; alſo alum, vitriol, and other minerals. From the external appearance of this mountain, I imagine it has only begun to burn lately, as on ſeveral parts of it I ſaw ſmall ſhrubs and graſs, which looked as if they had been lately ſcorched and burnt. There are ſeveral holes on the ſouth, from which iſſues ſmoke, ſeemingly broken out lately, as the buſhes round are but lately burnt. On two oppoſite ſides of the burning mountain, eaſt and weſt, reaching from its baſe to that of the ſide of the crater, are two lakes of water, about a ſtone's throw in breadth; they appear to be deep in the middle; their bottom to be covered with a clay-like ſubſtance. The water ſeems pleaſant to the taſte, and is of a chalybeate nature. I ſuppoſe theſe lakes receive great increaſe, if they are not entirely ſupported, by the rain that tumbles down the ſide of the crater. I obſerved on the north ſide of the bottom traces of beds of rivers, that to appearance run great quantities of water at times to both theſe lakes. By the ſtones at their edges, I could perceive that either abſorption or evaporation, or perhaps both, go on faſt. The greater part of the bottom of the crater, except the mountain and two lakes, is very level. On the ſouth part are ſeveral ſhrubs and ſmall trees. There are many ſtones in it that ſeem to be impregnated with minerals: I ſaw ſeveral pieces of pumice-

mice-ſtone. I alſo found many ſtones about the ſize of a man's fiſt, rough, on one ſide blue, which appearance, I imagine, they have got from heat, and being in contact with ſome mineral. Theſe ſtones are ſcattered over the whole mountain, one or two of which I have ſent you, with ſome others.

After I had got up from the bottom of the crater, I could not help viewing it with admiration, from its wonderful ſtructure and regularity. Here I found an excavation cut through the mountain and rocks to an amazing depth, and with as much regularity and proportion of its conſtituent parts, as if it had been planned by the hand of the moſt ſkilful mathematician. I wiſhed much to remain on the mountain all night, to examine its ſeveral ridges with more attention next day; but I could not prevail on my companion to ſtay, and therefore thought it adviſable to accompany him.

I obſerved the motion of the clouds on this mountain to be very ſingular. Although there are ſeveral parts on it higher than the mouth of the crater, yet I ſaw their attraction was always to it. After entering on its eaſt or windward ſide, they ſunk a conſiderable way into it; then, mounting the oppoſite ſide, and whirling round the north-weſt ſide, they ran along a ridge, which tended nearly north-eaſt, and afterwards ſunk into a deep ravin, which divided this ridge from another on the north-weſt corner of the mountain, and the higheſt on it, lying in a direction nearly ſouth and north. They keep the courſe of this ridge to the ſouth end, and then whirl off weſt in their natural courſe.

I took my departure from the mountain with great reluctance. Although I encountered many difficulties to get up, yet it amply rewarded me for all my toil; but I had not time to examine it with that attention I wiſhed. When I got on the

peak

peak from which I had my first view of it, and from which I could see its different parts, I could not help reviewing it several times. After imprinting its structure on my mind, I took my final adieu of it, and returned down, and got to Mr. FRASER's house about seven at night, much fatigued.

I am sorry I had no instruments, to take the state of the air, nor the exact dimensions of the different parts of the mountain; but, I believe, on measurement, they will be more than I have mentioned.

From the situation of these islands to one another, and to the continent of South America, I imagine there are sub-marine communications between the burning mountains or volcanoes in each of them, and from them to the volcanoes on the high mountains of America. The islands, which are situated next the continent, seem to tend in the direction of those mountains; and I have observed, that the crater in this island lies nearly in a line with Soufriere in St. Lucia and Morne Pelée in Martinique, and I dare say from Morne Pelée to a place of the same kind in Dominique, and from it to the others; as it is certain there is something of this kind in each of these islands, Barbadoes and Tobago excepted, which are quite out of the range of the rest.

There is no doubt but eruptions or different changes in some of them, although at a great distance, may be communicated to and affect the others in various manners. It is observed by the inhabitants round these burning mountains, that shocks of earthquakes are frequent near them, and more sensibly felt than in other parts of the island, and the shocks always go in the direction of them.

I cannot omit mentioning the great assistance I received in the above excursion from Dr. YOUNG, Mr. MALOUNE, and Mr. FRASER;

FRASER; for, without the aid of their negroes, I could not have possibly gone through with it.

References to the figure, tab. II.

A 1. The summit that overlooks the crater, from which the drawing is taken.

AAAA. The circumference of the crater.

BBBB. The circumference of the bottom.

C. The burning mountain.

D. The small one on its summit.

EE. The two lakes of water.

F. The section of the rock on the west side of the crater.

G. The large ravin.

HHHH. Ravins of great depth.

I. Efflorescence on the north end of the rock, which at a distance looks like alum or nitre.

1.2.3.4.5.6. The different ridges on the summit of the mountain, as they join the crater.

7. Woods destroyed by the hurricane.

8.8. The clouds going to the southward of the west ridge, after passing north on the west side of the crater.

9.9.9. Where I descended into the bottom of the crater.

11 and 10. The summit and base of the ridge on which I ascended the mountain.

IV. *A Supplement to the Third Part of the Paper on the Summation of infinite Series, in the* Philosophical Transactions *for the Year* 1782. *By the Rev.* S. Vince, *M. A.; communicated by* Nevil Maskelyne, *D. D. F. R. S. and Astronomer Royal.*

Read November 25, 1784.

THE reasoning in the third part of my paper on the Summation of infinite Series having been misunderstood, I have thought it proper to offer to the Royal Society the following explanation. When I proposed, for example, to sum the series $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} -$ &c. *fine fine*, I wanted to find some quantity which, by its expansion, would produce that series, and that quantity I called its sum; not (as I conceived must have been evident to every one) in the common acceptation of that word, that the more terms we take, the more nearly we should approach to that quantity, and at last arrive nearer to it than by any assignable difference, for there manifestly can be no such quantity; but as being a quantity from which the series must have been deduced by expansion, which quantity I found to be $-\frac{1}{2} + \text{H. L. } 2$. If therefore in the solution of any problem, the conclusion, whose value I want, is expressed by the above series, and which arose from the necessity of expanding some quantity in the preceding part of the operation, surely no one can deny but that I may substitute for it $-\frac{1}{2} + \text{H. L. } 2$. For whatever quantity it was, which by its expansion produced at first

first a series, the same reduction which, from that series, produced the series $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} -$ &c. must also have produced $-\frac{1}{2} +$ H. L. 2. from the quantity which was expanded. This value of the series I obtained in the following manner. I supposed the series $\frac{1}{2} - \frac{2}{3} + \frac{3}{4} -$ &c. to be divided into two parts; the first part to contain all the terms till we come to those where the numerators and denominators become both infinitely great, in which case every term afterwards may be supposed to be equal to unity: the second part, therefore, would necessarily be (supposing the first part to terminate at an even number of terms) $1 - 1 + 1 - 1 +$ &c. *sine fine*. The first part, by collecting two terms into one, becomes $-\frac{1}{2 \cdot 3} - \frac{1}{4 \cdot 5} - \frac{1}{6 \cdot 7} -$ &c. which series, as it is continued till the terms become infinitely small, is equal to $-1 +$ H. L. 2. The second part $1 - 1 + 1 -$ &c. has not, taken abstractedly of its origin, any determinate value (as will be afterwards observed), but considered as part of the original series it has, for that series must have been deduced from the expansion of the binomial $\overline{1+x}|^{-1}$, or $\frac{1}{1+x}$; and hence, when $x = 1$, $1 - 1 + 1 -$ &c. can in this case have come only from $\frac{1}{1+1}$, which, therefore, must be substituted for it; consequently the two parts together give $-\frac{1}{2} +$ H. L. 2.

Having thus explained the nature of the series which I proposed to sum, and the principle upon which the correction depends, I must beg leave to acknowledge my obligations to my very worthy and ingenious friend GEORGE ATWOOD, Esq. F.R.S. who first observed that the series $1 - 1 + 1 - 1 +$ &c. has no determinate value in the abstract, as it may be produced by $\frac{1}{1+1+1+\&c.}$ whatever be the number of units in the denominator;

nator *; and it may alſo be added, that the ſame ſeries ariſes from $\frac{1+1+1+\&c.}{1+1+1+1+\&c.}$, provided the number of units be greater in the denominator than in the numerator. The correction will therefore be different in different circumſtances, and will depend on the nature of the quantity which was at firſt expanded. In the third part of my paper, I applied the correction to thoſe caſes where the original ſeries aroſe from the expanſion of a binomial, where the correction is in general as I there gave it; but as I did not apply my method to any other ſeries, I confeſs that it did not appear to me, that the correction would then be different, which it neceſſarily would had I extended my reaſoning to other caſes. I ſhall therefore add one example to ſhew the method of correction in other inſtances, where the value of the correction will be found to be different, according as we begin to collect at the firſt or ſecond term. Let the ſeries be $\frac{2}{1}-\frac{3}{2}+\frac{5}{4}-\frac{6}{5}+\frac{8}{7}-\&c.$ *fine fine*, which came originally from $\frac{1}{1+x+x^2}$; now if we begin to collect at the firſt term, the ſeries becomes $\frac{1}{1\cdot 2}+\frac{1}{4\cdot 5}+\&c.$ and for the ſame reaſon as before, the correction, to be added, is $\frac{1}{3}$; but $\frac{1}{1\cdot 2}+\frac{1}{4\cdot 5}+\&c.=\frac{4}{3}$ of a circular arc (A) of 30° to the radius $\frac{\sqrt{3}}{2}$; hence the ſum required $=\frac{4}{3}A+\frac{1}{3}$. If we begin to collect at the ſecond term the ſeries becomes $2-\frac{2}{2\cdot 4}-\frac{2}{5\cdot 7}-\&c.$; and the correction to be ſubtracted is $\frac{2}{3}$; for the ſecond part of the original ſeries is now $-1+1-1+1-\&c.$ which was produced by $\frac{1+1}{1+1+1}$; but

* I have been ſince informed by Mr. WALES, F. R. S. that a pupil of his, Mr. POND, made the ſame obſervation.

$2 - \frac{2}{2 \cdot 4} - \frac{2}{5 \cdot 7} - \&c. = 1 + \frac{4}{3} A$; therefore the ſum required $= \frac{1}{3} + \frac{4}{3} A$ as before. In the ſame manner we may apply the correction in all other caſes. Although, therefore, the ſeries $1 - 1 + 1 - 1 + \&c.$ or $-1 + 1 - 1 + 1 - \&c.$ have no determinate value in the abſtract, yet the given ſeries will fix its value by pointing out the quantity from which the ſeries muſt have been originally produced.

V. *Description of a Plant yielding* Asa fœtida. *In a Letter from* John Hope, *M. D. F. R. S. to Sir* Joseph Banks, *Bart. P. R. S.*

Read December 9, 1784.

TO SIR JOSEPH BANKS, BART. P. R. S.

SIR, Edinburgh, August 18, 1784.

I BEG you will do me the honour of presenting the inclosed account of the Asa fœtida, and the botanical description of the plant, with the drawings, to the Royal Society.

I have the honour of being, with much respect and esteem, &c.

JOHN HOPE.

ASA FOETIDA.

PLANTA umbellifera, tripedalis, erecta, ramosa, glauca, flore luteo.

Radix perennis.

Folia radicalia sex, procumbentia, trilobo-ovata, multoties pinnatim divisa; foliolis incisis, subacutis, subdecurrentibus; petiolo communi superne plano, linea elevata longitudinaliter per medium decurrente.

Caulis bipedalis, erectus, teretiusculus, annuus, leviter striatus, glaber, nudus præter unam circa medium foliorum imperfectorum conjugationem; petiolo membranaceo, concavo.

Rami nudi, patuli; quorum tres inferi, alterni, sustinentur singuli folii imperfecti petiolo membranaceo concavo.

Quatuor intermedii verticillati sunt. Supremi ex apice caulis octo, quorum interni erecti.

Omnes hi rami summitate sustinent umbellam compositam sessilem terminalem, et præterea 3—6 ramulos externe positos, umbellas compositas ferentes.

Hoc modo, rami inferiores sustinent 5, raro 6 ramulos; intermedii 3 vel 4; superiores 1 et 2.

Cal. *Umbella universalis* radiis 20—30 constat.

——— *partialis* flosculis subsessilibus 10—20.

Umbella composita sessilis convexo-plana.

——— ——— pedunculata hæmispherica.

Involucrum universale nullum.

——— *partiale* nullum.

Perianthium proprium vix notabile.

Cor. *universalis* uniformis.

Flosculi umbellæ sessilis fertiles.

——— ——— pedunculatæ plerumque abortiunt.

propria petalis quinque æqualibus, planis, ovatis: primo patulis, dein reflexis, apice ascendente.

Stam. *Filamenta* 5, subulata, corolla longiora, incurvata. *Antheræ* subrotundæ.

Pist. *Germen* turbinatum, inferum.

Styli duo, reflexi.

Stigmata apice incrassata.

Per.

PER. nullum: fructus oblongus, plano-compressus, utrinque 3 lineis elevatis notatus est.

SEM. duo, oblonga, magna, utrinque plana, 3 lineis elevatis notata.

Planta odorem alliaceum diffundit. Folia, rami, pedunculi, radix, truncus, secti succum fundunt lacteum, sapore et odore Asæ fœtidæ.

THOUGH Asa fœtida has been used in medicine for many ages, having been introduced by the Arabian physicians near a thousand years ago; yet there was no satisfactory account of the plant which yielded it, till KÆMPFER published his Amœnitates Exoticæ about seventy years ago.

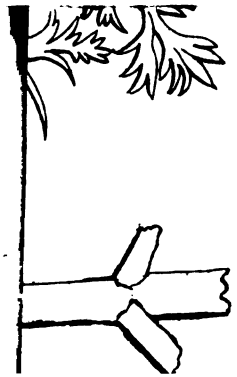
KÆMPFER, towards the end of the last century, travelled over a great part of Asia, and was in Persia, and upon the spot where the Asa fœtida is collected. He gives a full account of the manner of collecting it. He describes the plant; and also gives a figure of it, differing in many respects from those which I now present to the Society *.

Six years ago, I received from Dr. GUTHRIE, of St. Petersburg, F. R. S. two roots of the Asa fœtida, with the following card from Dr. PALLAS, addressed to Dr. GUTHRIE:

"Dr. PALLAS's compliments to Dr. GUTHRIE; he sends "him two roots of the Ferula Asa fœtida, a plant which he

* Probably KÆMPFER's Asa fœtida Plant is a different species from that described by Dr. HOPE in this paper. KÆMPFER was himself upon the mountains where the drug is collected, and his fidelity in describing, as well as delineating, has not hitherto been impeached. Sanguis Draconis, and some other gums, are indifferently the produce of various species of plants; and why may not Asa fœtida be similarly circumstanced? JOS. BANKS.

"thinks



"thinks never was cultivated in any European garden, and "which nobody has been so fortunate as to raise from seed but "himself, though the seeds sent to the Academy from the "mountains of Ghilan in Persia had been distributed among "several curious persons."

Both these roots were planted in the open ground, in the Botanic Garden at Edinburgh; one died; the other after some time did well, and last summer flowered and produced seed. I had an accurate drawing of the plant made by Mr. FIFE, which I now have the pleasure of laying before the Society. It expresses very well the general habit of the plant, which was of a pale sea-green colour, and grew to the height of three feet. The stem is deciduous, but the root is perennial. Every part of the plant, when wounded, poured out a rich milky juice, resembling in smell and taste Asa fœtida; and at times a smell resembling garlick, such as a faint impregnation of Asa fœtida yields, was perceivable at the distance of several feet.

In Persia, at the proper season, the root is cut over once and again; from the incisions there flows a thick juice like cream, which, thickened, is the Asa fœtida.

I have only further to observe, that as the plant grows in the open air, without protection, and even in an unfavourable season produced a good deal of seed, and as the juice seems to be of the same nature with the officinal Asa fœtida, there is some reason to hope, that it may become an article of cultivation in this country of no inconsiderable importance.

Edinburgh, Jan. 1783.

VI. *Catalogue of Double Stars.*

By William Herſchel, *Eſq. F. R. S.*

Read December 9, 1784.

INTRODUCTORY REMARKS.

THE great uſe of Double Stars having been already pointed out in a former paper, on the Parallax of the Fixed Stars, and in a latter one, on the Motion of the Solar Syſtem, I have now drawn up a ſecond collection of 434 more, which I have found out ſince the firſt was delivered.

The happy opportunity of giving all my time to the purſuit of aſtronomy, which it has pleaſed the Royal Patron of this Society to furniſh me with, has put it in my power to make the preſent collection much more perfect than the former; almoſt every double ſtar in it having the diſtance and poſition of its two ſtars meaſured by proper micrometers; and the obſervations have been much oftener repeated.

The method of claſſing them is in every reſpect the ſame as that which has been uſed in the firſt collection; for which reaſon I refer to the introductory remarks that have been given with that collection * for an explanation of ſeveral particulars neceſſary to be previouſly known. The numbers of the ſtars are here alſo continued, ſo that the firſt claſs ending there at

* See Philoſophical Tranſactions, vol. LXXII. p. 112.

24 begins here at 25, and the ſame is done with the other claſſes.

Moſt of the double ſtars in my firſt collection are among the number of thoſe ſtars which have their places determined in Mr. FLAMSTEED's extenſive catalogue; but of this collection many are not contained in that author's work, I have therefore adopted a method of pointing them out, which it will be proper to deſcribe.

The finder of my reflector is limited, by a proper diaphragm, to a natural field of two degrees of a great circle in diameter. The interſection of the croſs wires, in the center of it, points out one degree; and by the eye this degree, or the diſtance from the center to the circumference, may be divided into $\frac{1}{2}$, $\frac{1}{3}$, $\frac{3}{4}$, $\frac{1}{7}$, and $\frac{2}{3}$. Thus we are furniſhed with a meaſure which, though coarſe, is however ſufficiently accurate for the purpoſe here intended; and which, if more than two degrees are wanted, may be repeated at pleaſure.

In ſuch meaſures as theſe I have given the diſtance of a double ſtar, whoſe place I wanted to point out, from the neareſt ſtar in FLAMSTEED's Catalogue. And ſince, beſides the diſtance, it is alſo required to have its poſition with regard to the ſtar thus referred to, I have uſed the neighbouring ſtars for the purpoſe of pointing it out.

The uſefulneſs of this method is ſo extenſive, that I ſhall be a little more particular in deſcribing its application. When a ſtar is thus pointed out, as for inſtance the 32d in the firſt claſs, where it is ſaid, "About $\frac{3}{4}$ degree ſ. preceding the 44th Lyncis, "in a line parallel to θ Urſæ majoris and the 39th Lyncis;" we are to apply one eye to the finder, and placing the 44th Lyncis into the center of the field, we are to look at θ Urſæ majoris and the 39th Lyncis in the heavens with the other eye by the

ſide of the finder. The naked eye then will immediately direct us, by means of the two ſtars juſt mentioned, towards the place where, in the finder, the armed eye will perceive the double ſtar in queſtion about ¾ degree from the 44th Lyncis. I need hardly obſerve, that we muſt recollect the inverſion of the finder, as thoſe who are in the habit of uſing teleſcopes with high powers, always furniſhed with inverting finders, will of courſe look for the ſmall ſtar in the upper part of the field, as in fig. 1.

At the 45th ſtar, in the firſt claſs, the deſcription ſays, "About 1¾ degree ſ. preceding μ, towards ι Aurigæ." This double ſtar will accordingly be found by placing μ Aurigæ firſt into the center of the finder; then, drawing the teleſcope towards ι, which the naked eye points out, the ſtar we look for will begin to appear in the circumference as ſoon as μ is about ¾ degree removed from the center, as in fig. 2.

It will ſometimes happen, that other ſtars are very near thoſe which are thus pointed out, that might be miſtaken for them. In ſuch caſes an additional precaution has been uſed by mentioning ſome circumſtance either of magnitude or ſituation, to diſtinguiſh the intended ſtar from the reſt. After all, if any obſerver ſhould be ſtill at a loſs to find theſe ſtars without having their right aſcenſion and declination, he may furniſh himſelf with them by means of FLAMSTEED's Atlas Cœleſtis; for my deſcription will be ſufficiently exact for him to make a point in the maps to denote the ſtar's place; then, by means of the graduated margin, he will have its AR and declination to the time of the Atlas, which he may reduce to any other period by the uſual computations.

Before I quit this ſubject I muſt remark, that it will be found on trial, that this method of pointing out a double ſtar is not only

only equal, but indeed ſuperior, to having its right aſcenſion and declination given: for, ſince it is to be viewed with very high powers, not ſuch as fixed inſtruments are generally furniſhed with, the given right aſcenſion and declination would be of no ſervice. We might, indeed, find the ſtar by a fixed or equatorial inſtrument; and, taking notice of its ſituation with regard to other neighbouring ſtars, find, and view it afterwards, by a more powerful teleſcope; but this will nearly amount to the very ſame way which here is purſued, with more deliberate accuracy than we are apt to uſe, while we are employed in ſeeking out an object to look at.

It will be required, that the obſerver ſhould be furniſhed with FLAMSTEED's Atlas Cœleſtis, which muſt have the ſtars marked from the author's catalogue, by a number eaſily added to every ſtar with pen and ink, as I have done to mine. The catalogue ſhould alſo be numbered by an additional column, after that which contains the magnitudes. I hope in ſome future editions of the Atlas to ſee this method adopted in print, as the advantage of it is very conſiderable, both in referring to the catalogue for the place of a ſtar laid down in the Atlas, and in finding a ſtar in the latter whoſe place is given in the former.

I would recommend a precaution to thoſe who wiſh to examine the cloſeſt of my double ſtars. It relates to the adjuſtment of the focus. Suppoſing the teleſcope and the obſerver long enough out in the open air to have acquired a ſettled temperature, and the night ſufficiently clear for the purpoſe; let the focus of the inſtrument be re-adjuſted with the utmoſt delicacy upon a ſtar known to be ſingle, of nearly the ſame altitude, magnitude, and colour, as the ſtar which is to be examined, or upon one ſtar above and another below the

ſame. Let the phænomena of the adjuſting ſtar be well attended to; as, whether it be perfectly round and well defined, or affected with little appendages that frequently keep playing about the image of the ſtar, undergoing ſmall alterations while it paſſes through the field, at other times remaining fixed to it during the whole paſſage. Such deceptions may be detected by turning or unſcrewing the object-glaſs or ſpeculum a little in its cell, when thoſe appendages will be obſerved to revolve the ſame way. Being thus acquainted with the imperfections as well as perfections of the inſtrument, and going immediately from the adjuſting ſtar, which for that reaſon alſo ſhould be as near as may be, to the double ſtar which is to be examined, we may hope to be ſucceſsful. The aſtronomical Mr. AUBERT, who did me the honour to follow this method with γ Leonis, which he did not find to be double when the teleſcope was adjuſted by γ itſelf, ſoon perceived the ſmall ſtar after he had adjuſted it upon Regulus. The inſtrument, being one of Mr. DOLLOND's beſt 3½ feet achromatics, ſhewed Mr. AUBERT the two ſtars of γ Leonis in very cloſe conjunction, or rather one partly hid behind the other. On comparing theſe appearances with my obſervations of that double ſtar, we muſt not be ſurpriſed to find that I place them at a viſible diſtance from each other: for the Newtonian reflectors, on the plan of my 7-feet one, as I have found, will give a much ſmaller image of the ſtars than the 3½ feet achromatic refractors; wherefore the two ſtars, which in refractors as it were run into each other, will in the reflector remain ſeparate. For this reaſon alſo, thoſe who only uſe ſuch refractors muſt not be diſappointed if they cannot perceive the 26th, 30, 31, 36, 41, 44, 46, 47, 60, 75, 82, 86, and 87th ſtars of my firſt claſs to be double,

A

All the obſervations in the following catalogue on the relative magnitude, colour, and poſition of the ſtars, are to be underſtood as having been made with a power of 460, unleſs they are marked otherwiſe. This will account for the difference which obſervers may find in the relative magnitude; for ſhould they uſe only a power of about 200, many of the ſmall ſtars that are ſaid to be very unequal and extremely unequal, muſt appear to them perhaps a degree lower in the ſcale, and become extremely and exceſſively unequal: and this will happen, though the quantity of light ſhould be the very ſame which the reflector has that ſerved me to ſettle theſe particulars. I need not ſay, that on other accounts, ſuch as a real difference in the light of the teleſcope, the preſence of the moon, twilights, auroræ boreales, or other cauſes, many of the ſmall ſtars may be found to be of a different comparative luſtre from what is aſſigned to them in the catalogue. The ſmall ſtar near Rigel, for inſtance, appears of a beautiful pale red colour, full, round, and well defined, with my 20-feet reflector; the 10-feet inſtrument ſhews it alſo very well in fine evenings; the 7-feet requires more attention, nor is the ſmall ſtar defined, but of a duſky pale red colour. A good 3½ feet achromatic, of a large aperture, when Rigel is on the meridian, may, perhaps, alſo ſhew the ſmall ſtar, although I have not been able to ſee it with a very good inſtrument of that ſort, which ſhews the ſmall ſtar that accompanies the pole-ſtar; but the evening was not very favourable.

The meaſures of the diſtances were all taken with a parallel ſilk-worm's-thread micrometer, and a power of 227 only. They are not, as in the former catalogue, with the diameters included, but from the center of one ſtar to the center of the other.

other. I have adopted these measures on finding that I could procure threads fine enough to subtend only an angle of about 1″ 13‴, and that by this means there was no longer any great difficulty of judging when the stars were centrally covered by the threads. However, I do not know whether these measures, with stars at a considerable distance, may not be liable to an additional error of perhaps one second, owing to the remaining uncertainty in judging of their exact central position while the measure is taking.

The positions have all been measured (unless marked otherwise) with a power of 460, adapted to an excellent micrometer, executed by Mess. NAIRNE and BLUNT, according to the model given in the Philosophical Transactions, vol. LXXI. page 500. fig. IV.; but with a great and necessary improvement of making the wheel *d*, *d*, of that figure perform its whole revolution; by which means the two silk-worms-threads may be adjusted to a greater degree of exactness; for if they are not placed so as perfectly to bisect the circle, the two threads will not coincide exactly after having performed one semi-revolution, which they must be made to do with the utmost rigour. I found the absolute necessity of this precaution when I came critically to examine the positions of the Georgium Sidus, as they are given in table III. Phil. Trans. vol. LXXI. p. 497. The measures were affected with a small and pretty regular error, which I was at a loss to account for; and the distance of this star being then totally unknown, I looked for the cause of the deviation at first in a diurnal parallax of that heavenly body; but soon found it owing to the inconvenience before-mentioned, of not being able experimentally to adjust the moveable thread to that critical nicety which I have

have now introduced and used in all the angles of the following catalogue *.

Datchet near Windsor, Nov. 1, 1784. **W. HERSCHEL.**

CATALOGUE OF DOUBLE STARS.

FIRST CLASS.

L. 25. A Orionis. Fl. 32. Sub humero in consequentia.
Jan. 20. Double. Considerably unequal. L. fine w.; S. w.
1782. inclining to pale rose colour. The distance or black division between the two stars with 278 is about ¼ diameter of L.; with 460, near ½ diameter of L. Position with 278, 52° 10′ s. preceding.

26. ω Leonis. Fl. 2. Anteriorem pedem dextrum præcedens.
Feb. 8. A very minute double star. Considerably unequal.
1782. Both r. With 227 there is not the least suspicion of its being double; with 460 it appears oblong, and, when perfectly distinct, we see ¼ of the apparent diameter of a small star as it were emerged from behind a larger star; with 932 they are more clear of each other, but not separated; the focus of every power adjusted upon the 3d and 6th Leonis. November 6th, 1782, I

* The divisions on the moveable circular index (*a*) of this micrometer should be read off by means of a line drawn on a small plate fastened to the side *t*, and projecting with a proper curvature against the plane of the divisions towards *r*, so as to be nearly in contact; a coincidence of lines being by far the best method of ascertaining the situation of the index. A nonius of four sub-divisions may also be used, whereby the 60 divisions, already divided into halves upon the index-plate, will be had in eighths, each of which, on the construction of my present one, will be equal to three minutes of a degree of the circle.

I. first suspected a separation; and November 13th, fairly saw a division between them. April 4, 1783, with an improved reflector of 20 feet 3 inches focal length and 12 inches aperture, I saw them evidently divided. Position 20° 54′ f. following*.

27. FL. 90 Leonis. Infra eductionem caudæ.

Feb. 9. 1782. Treble. The two nearest—very unequal. L. w.; S. rw. With 278, 1¼ diameter of L; with 460, 1½ diameter of L. Position with 278, 61° 9′ f. preceding. The two farthest—very unequal. S. dusky r. Distance from L. 53″ 43‴. Position 35° 12′ f. preceding.

28. γ Leonis. FL 41. In collo lucida.

Feb. 11. 1782. A beautiful double star. Pretty unequal. L. w.; S. w. inclining a little to pale red. With 227 and 278 distinctly separated; with 460, ⅓ diameter of S.; with 625, ¼ diameter; with 932, full ¼ diameter, or when

* I suspect these stars to recede from each other. It is, however, very possible, that the opening which I observed between them, at the latter end of the year 1782 and beginning of 1783, may be owing to very favourable weather, or to my being better acquainted with the object. Could we increase our power and distinctness at pleasure, we might undoubtedly separate any two stars that are not absolutely in a direct line passing through the eye of the observer, and the centers of both the stars. This will appear when we consider that perhaps 59 thirds out of one second, which the diameter of the star may subtend, are spurious; so that a double star seemingly in contact, or even partly hiding each other in appearance, may still be far enough asunder to admit of a fair and considerable separation by applying an adequate magnifying power. It would have been curious, if a considerable difference in the colours could have led us to discover which of the two stars is before the other! But the far greatest part of their apparent diameters being, as we have observed, spurious, it is probable, that a different coloured light of two stars would join together, where the rays of one extend into those of the other; and so, producing a third colour by the mixture of it, still leave the question undecided.

best

I. best ½ diameter of S.; with 1504, ¾ diameter, well-defined, and the difference of colours still visible; with 2176, not quite a diameter of S, pretty well defined, but exceedingly tremulous; with 2589, less than 1 diameter; with 3168, still pretty distinct, and about ¾ diameter of S: with 4294, more than a diameter of S, but attended with the utmost difficulty of managing the motions; with 5489, the interval still somewhat larger, and if the object could be kept in the center of the field, the eye might adapt itself to the focus, and get the better of the violent aberration; but the edges of the glass being of a different focus, the eye is constantly disappointed in its endeavours to define the object; with 6652, I had but a single glimpse of the star quite disfigured; however, I ascribe it chiefly to the foulness of the glass, which, on account of its smallness, is extremely difficult to be cleaned; with a 10-feet reflector, 9 inches aperture, power 626, above ½ diameter of S. very distinct; with a 20-feet reflector, power 350, too bright an object to be quite distinct, though I see it very well. Position 5° 24′ n. following. A third star preceding. Dist. 1′51″23‴, pretty accurate for so great a distance. Position 31° 0′ n. preceding. A fourth star preceding the third, and somewhat smaller.

29. Parvula juxta Fl. 44am Leonis,

Feb. 17. 1782. Double. About 4′ following the 44th Leonis, which being double in the finder, this is the least of the two. Extremely unequal. L. w. S. d. With 227, 1½ diameter of L.; with 460, 2 diameters of L. Position 26° 32′ n. following.

I. 30. Secunda ad ι Cancri. FL. 57.

March 5. 1782. Double. Pretty unequal. Both pr. With 227, about $\frac{1}{4}$ diameter; with 278, $\frac{1}{4}$ diameter; with 460, about $\frac{1}{2}$ diameter or less. Position 68° 12′ n. preceding. A beautiful minute object.

31. Inter FL. 41^{am} et 39^{am} Lyncis.

March 5. 1782. Double. Near 1$\frac{1}{4}$ degree n. preceding the 41st Lyncis; towards *n* Ursæ majoris. A little unequal. Both w. With 460, $\frac{1}{4}$ or at most $\frac{1}{3}$ diameter, Position 51° 21′ s. preceding.

32. FL. 44^{a} Lyncis australior et præcedens.

April 3. 1782. Double. About $\frac{3}{4}$ degree s. preceding the 44th Lyncis; in a line parallel to θ Ursæ majoris and the 39th Lyncis. Very unequal. L. r.; S. bluish r. With 227, 1 diameter of L. or 1$\frac{1}{4}$ when best; with 460, 1$\frac{1}{4}$ diameter, or when best, near 2 diameters of L. The diameters are so small that the length of the time, and attention of looking, makes a considerable difference in the estimation of the distance. Position 8° 27′ s. preceding.

33. ξ Libræ. FL. 51. Primam chelam Scorpii attingens.

May 12. 1782. Treble. Without great attention, and a considerable power, it may be mistaken for a double star; but the largest of them consists of two. Very little unequal. Both w. With 460, $\frac{1}{4}$ or at most $\frac{1}{3}$ diameter asunder; with 932, full $\frac{1}{3}$ diameter of L. or near $\frac{1}{2}$ diameter of S. Position, with 278, 82° 2′ n. following. For measures of the third star see the 20th of the second class.

34. FL. 55. Cassiopeiæ. ι Ptolemæi. In pedis extremitate.

Treble

I. Treble. The two nearest very unequal. L. w.; S. colour of pale red blotting paper. With 278, ½ diameter of S. Position with 227, 20° 30′ n. preceding. For measures of the third star see the fourth in the third class.

June 11, 1782.

35. Fl. 38. Serpentarii. Dextrum infra pedem.

June 11, 1782. Double. Very unequal. L. w.; S. d. With 460, 1¼ diameter of L. As the situation is too low for 460, I tried 227, but it only shewed the star wedge-formed. Position 60° 48′ n. preceding.

36. ζ Herculis. Fl. 40. In dextro latere.

July 18, 1782. A fine double star. Very unequal. L. w.; S. ash-colour. With 460, less than ½ diameter of S.; with 932, 1 full diameter of S.*. Position with 811, 20° 42′ n. following.

37. φ (Fl. 11ª.) Herculis borealior et sequens.

July 22, 1782. Double. About ⅓ degree n. following φ; in a line parallel to the 35th and 42d Herculis; the most south of two very small telescopic stars. Considerably unequal. Both reddish. With 227, they can but just be seen as two stars; with 460, near 1 diameter; with 932, not less than 1½ diameter of L. Position 59° 48′ s. following.

* The interval between very unequal stars, estimated in diameters, generally gains more by an increase of magnifying power than the apparent distance of those which are nearer of a size. Instances of the former may be found in the first class, the 1st, 7, 29, 35, 37, 39, 53, 59, 63, 64, 72d stars; of the latter, the 16th, 28, 33, 45, 46, 73, 81st stars. However, this only seems to take place when there is a difficulty of seeing the object well with a low power, which being removed by magnifying more, the distance is, as it were, laid open to the view.

I. 38. FL. 18am Persei præcedens ad boream. In capite.

Aug. 20, 1782. Double. About ½ degree n. preceding the 18th; in a line parallel to σ and τ Persei; of two stars that next to the 18th. A little unequal. Both pr. With 278, a most minute and beautiful object; with 460, ½ diameter of either. Position with 278, 9° 42′ n. preceding.

39. β (FL. 11am) Cassiopeiæ præcedens ad austrum.

Aug. 25, 1782. Double. About ¾ degree s. preceding β; in a line parallel to η and α Cassiopeiæ; the following and largest of two very considerable stars. Very unequal. L. pr.; S. r. With 278, ¼ diameter of S.; with 460, ½, or when best, ¾ diameter of S. Position 50° 42′ n. preceding.

40. FL. 25am Cassiopeiæ præcedens ad boream.

Aug. 28, 1782. Double. About ½ degree n. preceding the 25th; towards α Cassiopeiæ; the first telescopic star in that direction. Very unequal. Both r. With 460, ¾ diameter of S.; difficult to be seen. Position 50° 30′ s. following.

41. FL. 31a Draconis borealior.

Aug. 29, 1782. A very minute double star. About ¾ degree n. of the 31st; in a line parallel to γ and ξ Draconis; the most south and preceding of two. Considerably unequal. Both pr. or r. With 227, they appear only as a lengthened or distorted star; with 460, ¼ diameter of S.; or in very fine nights ⅓ diameter of S.; with a new speculum and 500, near ½ diameter when best; with 932, ½ diameter. Position 84° 21′ n. preceding. Requires every favourable circumstance to be seen double.

I. 42. δ Serpentis. Fl. 13. In primo flexu colli.

Sept. 3, 1782. A beautiful double ſtar. Conſiderably unequal. L. w.; S. greyiſh. With 227, ⅓ diameter of S.; with 278, not quite ½ diameter of S.; with 460, near ⅔ diameter of S.; with 932, near 1 diameter of S.; with 1504, above 1 diameter of S. Poſition 42° 48′ ſ. preceding.

43. Ad Fl. 48am Draconis.

Sept. 3, 1782. A very minute double ſtar. The moſt north of three, forming an arch; or that which is towards o Draconis. Conſiderably unequal. Both pale pink. In fine nights, with 460, it has the ſhape of a wedge; with 932, a fine black diviſion juſt viſible; in a very clear dark night a diviſion may be ſeen with 500, and with 932, it will be about ⅓ diameter. Poſition with 500, 88° 24′ n. preceding.

44. Fl. 4. Aquarii. Supra veſtimentum manus ſiniſtræ.

Sept. 3, 1782. A minute double ſtar. Very unequal. Both pr. With 460, almoſt in contact, or at moſt ⅓ diameter of S. Poſition 81° 30′ n. preceding. A third ſtar of the ſixth claſs in view, n. preceding.

45. μ Aurigæ (Fl. 11am) præcedens ad auſtrum.

Sept. 5, 1782. Double. About 1¼ degree ſ. preceding μ, towards ι Aurigæ; a pretty conſiderable ſtar in a minute teleſcopic conſtellation. A little unequal. Both pr. or r. With 227, ⅓ diameter of S.; with 278, near ½ diameter of S.; with 460, about ½ diameter, or near ⅔ diameter of S. Poſition 47° 33′ ſ. preceding.

46. ν (Fl. 13am) Aquarii ſequens ad boream.

Sept. 7, 1782. Treble. About 1¼ degree n. following ν, in a line parallel to β and α Aquarii; the middle of three that are

I. are in the ſame direction. The two nearest very unequal. L. rw.; S. pr., With 460, about 1 diameter of L. or more. Poſition 62° 27′ n. preceding. The two farthest very unequal. S. pr. Diſtance with 227, 1′ 22″ 42‴. Poſition 35° 51′ n. following.

47. FL. 29am Capricorni præcedens ad boream.

Sept. 27, 1782. A minute double ſtar. About ¾ degree n. preceding the 29th, in a line parallel to γ and α Capricorni. A little unequal. Appears diſtorted with 227 and 278; nor will 460 ſhew it ſeparated; with 657, two ſtars viſible; 932 confirms it. Difficult to be ſeen diſtinctly on account of its low ſituation. Poſition 84° 48′ n. preceding. 20-feet reflector, 200. Both w.

48. FL. 6am Cephei præcedens. In dextro brachio.

Sept. 27, 1782. A very minute and beautiful double ſtar. Near ¾ degree preceding the 6th towards η Cephei; a pretty conſiderable teleſcopic ſtar. A little unequal. Both pr. Almoſt in contact with 460; with 625, better divided; with 657 ſtill better. Poſition 14° 9′ ſ. preceding.

49. λ Cephei (FL. 22am) ſequens ad boream.

Sept. 27, 1782. Double. About 1¼ degree n. following λ, in a line from ζ through λ Cephei continued. Extremely unequal. Both dw. Cannot be ſeen with 278, except with long attention; with 460, 1½ diameter of L. Poſition 85° 48′ n. following; perhaps a little inaccurate.

50. λ Aquarii (FL. 73am) præcedens.

Sept. 30, 1782. Double. About 2½ degrees preceding, and a little ſouth of λ Aquarii; a conſiderable ſtar. Very unequal. L. w.; S. dw. With 278, leſs than 1 diameter of L; with 460, 1¼ diameter of L. Poſition with 227,

I. 41° 12′ n. preceding. The measure inaccurate on account of the low power, and probably 3° or 4° too small.

51. Quæ sequitur ι (Fl. 32am) Cephei.

Sept. 30, 1782. Double. About 2¼ degrees n. following ι, towards γ Cephei; a considerable star. A little unequal. Both pr. A pretty object with 227; with 460, 1½ diameter nearly. Position 3° 36′ s. preceding.

52. Parvula Fl. 25tæ Orionis adjecta.

Oct. 2, 1782. Double. A few minutes n. following the 25th Orionis, in a line parallel to b Eridani and ε Orionis. Very unequal. L. ash w.; S. dw. With 460, 1 diameter of L. Position 52° 48′ n. preceding.

53. Parvula Fl. 30mæ Orionis adjecta.

Oct. 2, 1782. Double. About 10′ preceding the 30th, in a line parallel to λ and γ Orionis. Very unequal. L. w.; S. d.; with 460, 1 diameter of L. Position 43° 24′ n. following.

54. τ (Fl. 20am) Orionis præcedens. In malleolo sinistri cruris.

Oct. 4, 1782. Double. Near ¾ degree preceding τ, in a line from θ through τ Orionis continued. Very unequal. L. r.; S. dr. With 227, about 1 diameter of L.; with 460, about 2 diameters of L. Position 35° 42′ n. preceding; a little inaccurate.

55. Fl. 8am Tauri præcedens ad boream.

Oct. 9, 1782. Double. About 1⅓ degree n. preceding the 8th Tauri, or near 2 degrees s. following the 65th Arietis, in a line parallel to the Pleiades and ε Tauri; a small telescopic star not easily found. A little unequal. L. r.; S. d. With 227, less than 1 diameter of S.; with

I. with 460, near two diameters. Position 82° 48′ f. following.

56. FL. 54am Ceti ſequens ad auſtrum.

Oct. 12, 1782. Double. About ½ degree f. following the 54th, towards δ Ceti. Nearly equal. Both r. With 227, about 1 diameter; with 460, about 1½ diameter. Poſition 87° 39′ n. following.

57. FL. 70am et 67am Orionis præiens.

Oct. 12, 1782. Multiple. In a ſpot which appears nebulous in the finder, and is about 50′ from the 67th, and 45′ from the 70th Orionis. More than 12 ſtars in view with 460; among them is a double ſtar. The largeſt of the baſe of an iſoſceles triangle, n. preceded by four ſtars in a line. Conſiderably unequal. With 460, 1 full diameter of L. Poſition 19° 48′ f. following.

58. δ Lyræ (FL. 12am) ſequens. Inter eductionem cornuum.

Oct. 24, 1782. Double. About ½ degree following the 12th, in a line continued from the 11 through the 12th Lyræ; the laſt of a ſmall teleſcopic triangle. Extremely unequal. L. r.; S. d. Not eaſily ſeen with 227; with 460, near 2 diameters of L. Poſition 13° 0′ n. preceding.

59. Ab ι (FL. 18a) Lyræ β verſus.

Oct. 24, 1782. Double. The moſt ſouth of two very ſmall teleſcopic ſtars, which are the ſecond pair ſituated in a line from ι towards β Lyræ. A little unequal. Both d.; the fainteſt object that can be imagined. With 460, about 1 diameter. Poſition 75° 0′ f. preceding; the meaſure is liable to ſome error from the obſcurity.

60. E teleſcopicis γ et λ Lyræ auſtralioribus et ſequentibus.

Double

I.

Oct. 24, 1782. Double. About ⅜ degree f. following λ, in a line parallel to α and γ Lyræ; a very ſmall teleſcopic ſtar. Extremely unequal. Both dr. With 227, 1 full diameter of L; with 460, near 2 diameters of L. Poſition 16° 48′ n. preceding.

61. Præiens FL. 1am Equulei.

Oct. 26, 1782. A minute double ſtar. About ¾ degree n. preceding the 1ſt Equulei, in a line parallel to α Equulei and γ Aquilæ; a large ſtar. Very unequal. Both pr. With 460, ½ diameter of S. Poſition 18° 24′ n. preceding. A pretty object, but requires fine weather.

62. Sequitur FL. 2am Equulei.

Oct. 29, 1782. Double. About ¾ degree f. following the 2d Equulei, in a line parallel to δ Delphini and δ Equulei. Conſiderably unequal. Both r. With 460, 1¼ or 1½ diameter of S. Poſition 35° 9′ f. preceding.

63. γ Equulei (FL. 5a) auſtralior.

Oct. 29, 1782. Double. Full ½ degree f. of γ, in a line from the 5th through the 6th Equulei continued. Equal. Both dr. With 227, about ¼ diameter ſcarce viſible; with 460, about ½ diameter. Poſition 5° 57′ f. preceding.

64. π Arietis. FL. 42. In poplite.

Oct. 29, 1782. Treble. Exceſſively unequal. L. w; S. both mere points. With 227, neither of the ſmall ſtars can be ſeen, except with conſiderable and long continued attention, when they alſo appear; the neareſt with this power is ¾ or ⅘ diameter of L.; with 460, 1½ or 1¾ diameter of L. The third is about 25″ or 26″ diſtant from L, by exact eſtimation. Poſition of both, being all three in a line 19° 19′ f. following; as exact as the obſcurity will permit.

I. 65. In Nubecula β Sagittæ adjecta et ſequenti.

Nov. 4, 1782. Double. ⅓ degree n. following β Sagittæ, towards 29th Vulpeculæ; the largeſt and moſt ſouth of a cluſter of ſmall ſtars that appear cloudy in the finder. Very unequal. L. rw.; S. pr. With 227, full 1 diameter of L.; with 460, about 1¾ or 2 diameters of L. Poſition 14° 0′ n. preceding. A third ſtar in view, of the 5th or 6th claſs.

66. β (FL. 23ª) Draconis auſtralior et præcedens.

Nov. 4, 1782. Double. About 1¼ degree ſ. preceding β, in a line from ν continued through β Draconis. Pretty unequal. Both pr. With 460, 1½ or 1¾ diameter of L. Poſition 2° 24′ ſ. preceding.

67. Nebulam Aurigæ pedem dextrum ſequentem, præcedens.

Nov. 4, 1782. Double. About 55′ from the 37th Nebula of M. MESSIER; the largeſt and moſt preceding of two ſtars. Very unequal. Both pr. With 460, near 2 diameters of L. Poſition 23° 57′ n. following.

68. Parvula FL. 10ª Orionis quam proximè adjecta.

Nov. 5, 1782. Double. The ſmall ſtar not many minutes from the 10th Orionis. A little unequal. Both whitiſh. With 460, near 1 diameter. Poſition 84° 54′ ſ. following; a little inaccurate on account of the difficulty of ſeeing the ſtars well.

69. In Lyncis pectore.

Nov. 13, 1782. Double. About 3 degrees ſ. preceding the 19th Lyncis, in a line drawn from the 19th Lyncis to τ Aurigæ; the 24th and 19th Lyncis alſo point to it nearly: in a very clear evening it may juſt be ſeen with the naked eye. A little unequal. Both rw. With 227, ¾ dia-

I. $\frac{3}{4}$ diameter; with 460, $1\frac{1}{4}$ or near $1\frac{1}{2}$ diameter. Position 77° 0′ f. following.

70. ζ (Fl. 123i) Tauri borealior et præcedens.

Nov. 13, 1782. A very pretty double ſtar. Near 1 degree n. preceding ζ Tauri towards Capella; the corner of a rhomboid made up of ζ, this, and two more, and oppoſite to ζ. Conſiderably unequal. L. pr.; S. a little deeper r. With 227, almoſt 1 diameter of L.; with 460, $1\frac{3}{4}$ diameter of L. Poſition 36° 24′ f. preceding.

71. Fl. 44am Urſæ majoris præcedens ad auſtrum.

Nov. 19, 1782. Double. Nearly in the interſection of a line from β Urſæ majoris to the 39th Lyncis, croſſed by one from ψ to υ Urſæ majoris; the laſt line ſhould bend a little towards ψ Urſæ majoris. A little unequal. Both whitiſh. With 460, near 2 diameters of S. Poſition 2° 6′ n. following.

72. Fl. 65. Urſæ majoris.

Nov. 20, 1782. Double. Exceſſively unequal. L. pr.; S. a point. Not viſible with 227, nor hardly to be ſuſpected unleſs it has been firſt ſeen with a higher power; with 460, $1\frac{3}{4}$ diameter of L. or, when long viewed, full 2 diameters of L. Poſition 53° 45′ n. following. A third ſtar in view. Equal to L. Colour rw. Diſtance 1′ 0″ 4‴. Poſition 22° 21′ f. following.

73. β (Fl. 6i) Arietis borealior et præcedens.

Nov. 22, 1782. Double. About $1\frac{3}{4}$ degree n. preceding β Arietis, towards β Andromedæ; a conſiderable ſtar. Very unequal. L. r.; S. deeper r. With 227, about $\frac{3}{4}$ diameter of L.; with 460, full $1\frac{1}{4}$ or almoſt $1\frac{1}{2}$ diameter of L. when beſt. Poſition 77° 24′ f. following.

I. 74. FL. 39ª Arietis borealior et præcedens.

Dec. 22, 1782. Double. About ⅔ degree n. preceding 39 Arietis, towards γ Trianguli; a pretty large telescopic star. A little unequal. Both pr. With 227, near 1 diameter of L.; with 460, about 1½ diameter of L. Position 20° 36′ n. preceding.

75. FL. 26ᵗᵐ Orionis præcedens ad austrum.

Jan. 9, 1783. Double. About ¼ degree s. preceding the 26th, in a line parallel to δ and β Orionis; the farthest of two; or ¾ degree s. preceding the 30th in the same direction. Nearly equal. Both w. or rw. With 460, perhaps a diameter. Position 89° 36′ n. preceding; but not very accurate.

76. In pectore Lyncis.

Jan. 23, 1783. Double. Not easy to be found. A line from the 19th Lyncis to υ Geminorum crossed by one from θ Ursæ majoris to ε Aurigæ, points out a star but just visible in a fine evening; it is perhaps about three degrees from the 19th Lyncis; when that star is found, we have the double star about 1 degree n. following the same, in a line parallel to τ Geminorum and the 19th Lyncis. Considerably unequal. Both ash w. With 460, ¼ diameter of S. Position 0° 0′ preceding. A third large star in view. Distance 1′ 7″ 46‴. Position 3° 42′ s. preceding.

77. α (FL. 7ª) Crateris borealior.

Jan. 31, 1783. Double. Near 2¾ degrees north of α Crateris; a small telescopic star, about ¼ degree following the most north of two large ones. Pretty unequal. Both whitish. With 227, less than half diameter of S.; with

L. with 460, near 1 diameter; with 625, a little more than 1 diameter. Position 82° 24′ n. following.

78. Fl. 11ª Libræ borealior.

Jan. 31, 1783. Double. Near 2½ degrees north of the 11th Libræ, in a line parallel to μ Virginis and the 109th of the same constellation. Equal. Both inclining to r. With 460, full 1 diameter. Position 58° 24′ n. preceding, or s. following.

79. Fl. 46 Herculis. In dextro latere.

Feb. 5, 1783. Double. Extremely or almost excessively unequal. L. w.; S. d. With 227, it is hardly visible; with 460, near 1 diameter of L. Position 66° 36′ s. following.

80. Fl. 81 Virginis.

Feb. 7, 1783. Double. Equal. Both pr. With 227, near ½ diameter; with 460, ⅔ diameter. Position 41° 12′ n. following or s. preceding.

81. π Serpentis (Fl. 44ᵃᵐ) præcedens ad austrum.

Mar. 7, 1783. Double. About 1¼ degree s. preceding π, towards κ; the most north of two. A little unequal. Both r. With 460, 1½ diameter of L. Position 49° 48′ s. preceding. A third large star in view; paler than the other two. Distance from the two taken as one star 56″ 28‴. Position, with L. of the two, 31° 48′ s. preceding.

82. Fl. 49 Serpentis.

Mar. 7, 1783. Double. The most north and following of two stars. A little unequal. Both pr. With 227, ¼ or ⅓ diameter, and a very minute and beautiful object; with 460, ¾ diameter. Position 21° 33′ n. preceding.

I. 83. λ Ophiuchi. FL. 10. In ancone siniſtri brachii.

Mar. 9, 1783. A very beautiful and close double ſtar. L. w.; S. blue; both fine colours. Conſiderably or almoſt very unequal. With 460, ¼ or ⅓ diameter of S.; with 932, full ⅓ diameter of S. Poſition 14° 30′ n. following.

84. FL. 50ª Aurigæ auſtralior.

Mar. 18, 1783. Double. Near 1 degree ſ. of the 50th Aurigæ, in a line parallel to β and θ. Very unequal. L. r.; S. dr. With 227, about ¾ diameter of L.; with 460, almoſt 1¼ diameter of L. Poſition 14° 0′ n. following.

85. FL. 36ᵃᵐ Lyncis ſequens ad auſtrum.

Mar. 24, 1783. Double. Near ½ degree ſ. following the 36th Lyncis, in a line parallel to the 31ſt Lyncis and *n* Urſæ majoris; of two the neareſt to the 31ſt Lyncis. Conſiderably unequal. Both w. With 227, 1 diameter of L.; or when long kept in view, 1¼ diameter of L.; with 460, and after long looking, 2 diameters of L; otherwiſe not near ſo much. Poſition 88° 57′ n. following.

86. FL. 105ª Herculis borealior.

Mar. 27, 1783. Double. One full degree n. of the 105th Herculis, in a line from the 72d Serpentarii continued through the 105th Herculis; a ſmall teleſcopic ſtar. Conſiderably unequal. Both dr. With 460, a little more than 1 diameter of L. Poſition 79° 24′ n. preceding.

87. *q* Ophiuchi. FL. 73.

April 27, 1783. A very minute double ſtar. Conſiderably unequal. L. r. S. r. With 227, not to be ſuſpected unleſs known to be double, but may be ſeen wedge-formed, and with

I. long attention I have alſo perceived a moſt minute diviſion; with 460, about $\frac{1}{4}$ or $\frac{1}{3}$ diameter of S. Poſition 2° 48′ ſ. preceding.

88. τ Ophiuchi. Fl. 69. In dextra manu ſequens.

April 28, 1783. The cloſeſt of all my double ſtars; can only be ſuſpected with 460; but 932 confirms it to be a double ſtar. Pretty unequal. Both pr. or wr. It is wedge-formed with 460; with 932, one-half of the ſmall ſtar, if not three-quarters ſeem to be behind the large ſtar. Poſition of the wedge 61° 36′ n. preceding. ν Ophiuchi, juſt by, is perfectly free from this wedge-formed appearance.

89. Illas ad Fl. 56am Andromedæ præcedens ad boream.

July 28, 1783. Double. About $\frac{2}{3}$ degree preceding, and a little north of the two ſtars that are about the place of the 56th Andromedæ, in a line towards μ; a conſiderable ſtar; and of two in a line parallel to β and γ Trianguli that which is neareſt to the 56th Andromedæ. Pretty unequal. L. drw.; S. dpr. With 227, near 1 diameter of L.; with 460, about $1\frac{1}{2}$ diameter of L. Poſition 75° 30′ ſ. following.

90. β Aquarii (Fl. 22am) præcedens ad auſtrum.

July 31, 1783. Double. About $4\frac{1}{2}$ degrees from β towards μ Aquarii. A little unequal. Both dw. or pr. With 460, $1\frac{1}{2}$ diameter or near 2. Poſition 77° 36′ ſ. following.

91. γ Aquilæ (Fl. 50am) præcedens ad boream.

Aug. 7, 1783. Double. About $\frac{1}{3}$ degree n. preceding γ, in a line parallel to γ and ζ Aquilæ; of two that neareſt to γ. Very unequal. L. dpr.; S. d. With 227, hardly viſible, and like a ſtar not in focus; with 460, appears nebulous

I. nebulous on one ſide, but is a double ſtar; with 932, about 1½ diameter of L. Poſition 8° 18′ n. preceding.

92. π Aquilæ. FL. 52. Duarum in ſiniſtro humero ſequens.

Aug. 27, 1783. A minute pretty double ſtar. A little unequal. Both pr. With 460, ½ diameter of L. or near ¾ diameter of S. Poſition 34° 24′ ſ. following.

93. FL. 62am Aquilæ præcedens ad boream.

Sept. 12, 1783. A minute double ſtar. About ¾ degree n. preceding the 62d, in a line parallel to θ and ζ Aquilæ; a pretty conſiderable ſtar. Very unequal. Both inclining to pr. With 278, almoſt in contact; with 460, near ¾ diameter of S.; when in the meridian, and the air fine, near 1 diameter of L. Poſition 19° 9′ n. preceding.

94. δ Cygni. FL. 18. In ancone alæ dextræ.

Sept. 20, 1783. Double. Very unequal. L. fine w.; S. aſh colour inclining to r. With 278, about ½ diameter of L.; with 460, ¾ diameter of L.; with 932, full 1½ diameter of L. in hazy weather, which has taken off the rays of L. and and thereby increaſed the interval. Poſition 18° 21′ n. following; perhaps a little inaccurate.

95. FL. 33am Cygni ſequens ad auſtrum.

Sept. 22, 1783. Double. Full 1½ degree ſ. following the 33d, towards ζ Cygni; a pretty conſiderable ſtar. Very unequal. L. w.; S. inclining to r. With 460, at firſt about ¾ diameter of L.; but, after looking a conſiderable time, and in a fine air, near 1½ diameter. Poſition 72° 15′ n. preceding.

96. η (FL. 21am) Cygni ſequens ad auſtrum.

Sept. 23, 1783. Treble. Full 1¾ degree n. following η, in a line parallel to β and λ Cygni. The two neareſt conſiderably unequal.

I. unequal. Both pr. With 460, 1 diameter of S. or ⅓ diameter of L. Position 89° 18′ f. following. The two farthest considerably unequal; the colour r. Dist. . . . . Position 56° 3′ n. preceding.

97. Fl. 51am Cygni sequens.

Sept. 24, 1783. A minute double star. About 2½ degrees following the 51st, in a line parallel to δ and α Cygni; the largest and most south of an obtuse-angled triangle; a very considerable star. Pretty unequal. Both rw.; but S. a little darker r. With 278, ½ diameter of S. and beautiful; with 460, ¾ diameter of S. Position 46° 24′ n. following.

SECOND CLASS OF DOUBLE STARS.

H. 39. Procyonem juxta.

Feb. 2, 1782. Double. About 2 degrees f. following Procyon, in a line from λ Geminorum continued through Procyon. Excessively unequal. L. pr.; S. not visible with 278; with 460, more than 3 diameters of L. Position, by the assistance of a wall * and micrometer 54° 28′ f. following.

40.

* When the small star is so faint as not to bear the least illumination of the wires, its position may still be measured by the assistance of some wall or other object; for an eye which has been some time in the dark, can see a wall in a star-light evening sufficiently well to note the projection of the stars upon it, in the manner which

II. 40. * Secunda ad φ Cancri. FL. 23.

Feb. 2, 1782. Double. A little unequal. Both rw. With 227, near 2 diameters; with 460, 2½ diameters of L. Position 56° 42′ n. following.

41. * Prima ad υ Cancri. FL. 24.

Feb. 2, 1782. Double. Considerably unequal. Both pr. With 227, 1½ diameter of L.; with 460, 4 diameters of L. Position 32° 9′ n. following.

42. E telescopicis *k* Virginis precedentibus †.

Feb. 6, 1782. Double. About 1¼ degree s. preceding *k* Virginis, in a line parallel to ζ and θ; the most south of three forming an arch. Extremely unequal. L. w.; S. hardly visible with 227 (but with a ten-feet reflector S. b.); with 460, above 2 diameters of L. Position 52° 24′ s. following.

43. FL. 43am Leonis præcedens ad austrum. In dextro genu.

Feb. 17, 1782. Double. Near ⅔ degree s. preceding the 43d, in a line parallel to α and the 14th Leonis. Very unequal. L. w.; S. d. With 227, near 2¼ diameters of L. when best. Position 85° 2′ n. following.

44. *o* Virginis. FL. 84. Versus finem alæ dextræ.

Feb. 17, 1782. Double. Extremely unequal. L. w. inclining to r.; S. d. Requires attention to be seen with 227; with 460, 2½ diameters of L. Position, with 278, 29° 5′ s. preceding.

which has been described with the lamp-micrometer, Phil. Trans. vol. LXXII. p. 169 and 170. Then, introducing some light, and adapting the fixed wire to the observed direction of the stars on the wall, the moveable wire may be set to the parallel of the large star, which will give the angle of position pretty accurately.

† See note to IV. 51.

45.

II. 45. Fl. 54 Virginis.

April 3, 1782. Double. A little unequal. Both w. With 227, 1½ or near 1¾ diameter. Position 57° 0′ n. following.

46. Fl. 42am Comæ Berenices sequens ad austrum.

April 15, 1782. Double. About 1¾ degree from the 42d Comæ towards *υ* Bootis; the most south of a telescopic equilateral triangle. Excessively unequal. L. pr.; S. d. With 278, 2½ diameters of L.; not so well to be seen with higher powers. Position 6° 42′ s. following. A third star preceding, above 1′.

47. Fl. 2 Comæ Berenices.

April 18, 1782. Double. Considerably unequal. L. rw.; S. pr. With 278, 2 diameters of L; with 460, above 2 diameters of L. Position 27° 42′ s. preceding.

48. Prope Fl. 16am Aurigæ.

Aug. 28, 1782. A minute double star. Less than ¼ degree s. preceding the 16th, in a line parallel to the 10 and 8 Aurigæ; the preceding star of a small triangle of which the 16th is the largest and following. A little unequal. Both pr. With 227, 1½ or, when best, 1¾ diameter of L. Position 15° 48′ n. following.

49. *o* (Fl. 110i) Piscium borealior. In lino boreo.

Sept. 3, 1782. Double. About ½ degree n. of, and a little preceding 110th, towards *η* Piscium. A little unequal. Both wr. With 460, about 3 diameters of L. Position 59° 6′ n. preceding. A third star in view, about 1¾ min.

50. Fl. 38. Piscium. In austrino lino.

Sept. 4, 1782. Double. Pretty unequal. Both pr. With 227, full 2 diameters of L.; with 460, about 4 diameters of L. Position 25° 3′ s. preceding.

II. 51. ρ Capricorni. FL. 11. Trium in rostro sequens.

Sept. 5, 1782. Double. Very unequal. Both rw. With 460, 1½ diameter of L. Position 84° 0′ s. following. A third star in view.

52. ο (FL. 40^{am}) Persei præcedens ad boream.

Sept. 7, 1782. Double. Almost ½ degree preceding the 40th, in a line parallel to ζ and the 38th Persei. Equal. Both w. With 227, nearly 2 diameters. Position 8° 24′ n. preceding.

53. FL. 12^{am} Camelopardali præcedens.

Sept. 7, 1782. Double. Less than ¼ degree preceding the 11th and 12th, in a line from the 1st Lyncis continued through the 12th Camelopardali. Extremely unequal. Both dr. With 227, it appears like a star with a tail; but 932 shews it plainly to be only a double star; with 227, not much above 1 diameter of L.; with 932, about 3½ diameter of L. Position 18° 33′ s. following; a little inaccurate.

54. Quæ præcedit ε (FL 74^{am}, oculum boreum) Tauri.

Sept. 7, 1782. Double. Near ½ degree s. preceding ε, in a line parallel to α and γ Tauri; a small star. Extremely unequal. L. rw.; S. d. With 460, above 3 diameters of L. Position 68° 42′ s. preceding.

55. FL. 4^{a} Ceti australior et sequens.

Sept. 9, 1782. Double. About 1 degree s. following the 4th and 5th in a line parallel to η and τ Ceti; in the shorter leg of a rectangular triangle. Very unequal. L. r.; S. d. With 278, rather more than 2 diameters. Position 21° 42′ n. preceding.

56. β (FL. 6^{am}) Arietis præcedens ad boream.

II. Double. Almost 1 degree n. preceding β Arietis,
Sept. 10, towards ζ Andromedæ; a small star. A little unequal.
1782. Both reddish. With 227, full 2 diameters of L. Position 23° 12′ n. preceding. A third star 2′ or 3′ preceding, in the same direction with the two stars of the double star.

57. Ad Fl. 72am Aquarii.

Sept. 27, Treble. About 2½ degrees following κ, in a line parallel
1782. to α and η Aquarii. The nearest a little unequal. Both r. With 460, 2½ diameters of L. Position 25° 51′ s. preceding. The two farthest a little unequal. Of the 5th class. About 50° or 55° s. following.

58. Fl. 56i Ceti australior et sequens.

Sept. 27, Double. About ¾ degree s. following the 56th, in a
1782. line parallel to η and τ Ceti. Considerably unequal. Both dw. With 278, 1½ diameter of L. Position 25° 12′ n. preceding; too low for accuracy.

59. ρ (Fl. 46am) Aquarii sequens ad austrum.

Sept. 30, Double. About 2 degrees s. following ρ, in a line parallel to β and δ Aquarii; there is a very considerable star
1782. between this and ρ, not much out of the line. Pretty unequal. Both dr. With 227, 2½ or 2¾ diameter of L. Position 61° 12′ n. preceding.

60. ξ (Fl. 5am) Canis majoris sequens ad boream.

Sept. 30, Double. About ½ degree n. following the 2d ad ξ,
1782. in a line from the 4th continued through the 5th Canis majoris nearly. Very unequal. L. rw.; s. d. With 227, 1¼ diameter. Position 67° 36′ n. preceding.

61. π (Fl. 47am) Orionis sequens ad austrum.

Oct. 2, Treble. About 1½ degree s. following π in a line
1782. parallel to φ and α Orionis; the smallest and most south of three forming an arch. The two nearest extremely unequal.

H. unequal. L. dw.; S. a mere point. With 227, 1¼ or 1¾ diameter of L. Position 4° 54′ n. following; too obscure for accuracy. The two farthest extremely unequal. S. a mere point. Of the fourth class. Position about 50° s. following.

62. FL. 3ᵉ Pegasi adjecta.

Oct. 4, 1782. Double. In a line with, and north of, the two stars that are about the place of the third Pegasi. A little unequal. Both dusky r. With 227, about 3 diameters of S. Position 88° 24′ n. preceding; perhaps a little inaccurate.

63. FL. 2ᵃᵐ et 4ᵃᵐ Navis præcedens.

Oct. 12, 1782. Multiple. Near 2 degrees preceding the 2d and 4th Navis; the middle one of three. One of the multiple is double. Nearly equal. Both w. or ash colour. With 227, about 2½ diameter, and not less than 20 stars more in view; with 460, about 3 diameters. Position 30° 12′ n. preceding.

64. *g* (FL. 81ᵃᵐ) Geminorum ad austrum sequitur.

Oct. 13, 1782. Double. About ½ degree s. following *g*, in a line from ζ continued through *g* Geminorum nearly; the nearest and largest of two. Very unequal. L. r.; S. bluish r. With 227, above 3 diameters of L. Position 4° 9′ n. preceding.

65. Pollucem sequens ad boream.

Oct. 13, 1782. Double. Full ¾ degree n. following β, in a line from δ continued through β Geminorum; the star next to the middle one of three, nearly in a line. Excessively unequal. L. rw.; S. d. With 227, above 2½ or near 3 diameters of L. and 5 other stars in view; with 460, above 3 diameters of L. Position 89° 12′ n. following.

II. 66. Juxta γ Delphini.

Oct. 19, 1782. Double. Full ¼ degree f. preceding γ, towards δ Delphini. Considerably unequal. L. pr.; S. r. With 227, 1½ diameter of L. Position 78° 42′ n. preceding.

67. β (Fl. 10am) Lyræ præcedens ad boream.

Oct. 19, 1782. Double. The 4th telescopic star about 1½ degree n. preceding β, in a line parallel to γ and α Lyræ. Extremely unequal. L. r.; S. dr. With 227, 1¼ or almost 1½ diameter of L. With 460, above 2 diameters of L. Position 68° 6′ f. following.

68. Proximè ρ Lyræ.

Oct. 24, 1782. Treble. About 2¼ minutes f. following ρ Lyræ. The two nearest, a little unequal. Both dr. With 460, 3 full diameters. Position 8° 24′ n. following. The farthest as large as L. of the two nearest at least. Colour dr. Position with L. 25° 57′ f. preceding. Distance of ρ Lyræ, which is in view, from the two nearest 2′ 17″ 30‴. Position 65° 12′, ρ being n. preceding, or the double star f. following.

69. Fl. 4am Cygni sequens ad boream.

Oct. 24, 1782. Double. Near ½ degree n. following the 4th Cygni, in a line from γ Lyræ continued through the 4th Cygni. A little unequal. Both w. With 227, about 2 diameters of L. or 2½ when best. Position 29° 12′ n. following.

70. τῶν 8 telescopicarum ϰ (Fl. 15.) Sagittæ sequentium ultima.

Nov. 6, 1782. Double. About 1¼ degree f. following ϰ Sagittæ, in a line parallel to γ Sagittæ and γ Delphini. Extremely unequal. Both r.; S. deeper r. With 227, 1½ diameter

II. meter of L.; with 460, above 2 diameters of L. Position 72° 57′ n. following.

71. FL. 58ª Aurigæ auſtralior.

Nov. 6, 1782. Multiple. About ¾ degree ſ. of the 58th Aurigæ, in a line parallel to β and θ. A cluſter of ſtars containing a double ſtar of the ſecond, and one of the third claſs. That of the ſecond very unequal. Both r. With 460, about 2½ diameter of L. Poſition 44° 36′ n. following; that of the third equal. Both r. With 227, above 20 ſtars in view. Diſtance 17″ 41‴. The two double ſtars are in the following ſide of a ſmall teleſcopic trapezium.

72. FL. 13ª Lyncis auſtralior.

Nov. 13, 1782. A pretty double ſtar. About 1½ degree ſ. of the 13th Lyncis, towards θ Geminorum; a conſiderable ſtar. Nearly equal. Both pr. With 227, full 2½ diameters; with 460, almoſt 4 diameters. Poſition 11° 0′ ſ. preceding.

73. FL. 21ª Urſæ majoris.

Nov. 17, 1782. Double. Very unequal. Both rw. With 227, 2½ diameter of L.; with 460, above 3. Poſition 36° 45′ n. preceding.

74. ν (FL. 4ª) Crateris borealior.

Nov. 20, 1782. Treble. Near ½ degree n. preceding α Crateris, towards α Leonis. The two neareſt equal. Both dw. With 227, 2½ or 3 diameters. Poſition 71° 33′ n. following. The fartheſt larger than either of the two other ſtars. Of the ſixth claſs. Poſition about 68 or 69° ſ. preceding the double ſtar.

75.

II. 75. Fl. 118 Tauri.

Dec. 7, 1782. Double. A little unequal. L. w.; S. w. inclining to r. With 278, 2½ diameter of L.; with the ſame power by the micrometer 4″ 41‴; more exactly with 625, 5″ 2‴. Poſition 77° 15′. I could juſt ſee it with an 18-inch achromatic, made by Mr. Nairne; it was as cloſe as poſſible, and a pretty object.

76. τ (Fl. 63ᵃ) Arietis auſtralior et præcedens.

Dec. 23, 1782. Double. About 1 degree ſ. preceding τ Arietis, towards μ Ceti; the moſt ſouth of two ſmall teleſcopic ſtars. Nearly equal. Both w. With 227, above 3 diameters; by the micrometer 5″ 47‴. Poſition 15° 24′ ſ. preceding.

77. * Fl. 17 Hydræ.

Dec. 28, 1782. Double. The largeſt of two. A little unequal. Both w. With 227, 2¼ diameter of L.; with 460, 1¾ diameter. Poſition 90° 0′ north.

78. χ (Fl. 63ᵐ) Leonis ſequens ad auſtrum.

Jan. 1, 1783. Double. About ⅓ degree ſ. following χ, towards τ Leonis; the ſmalleſt of two. Very or extremely unequal. L. r.; S. d. With 227, 3 full diameters of L. Poſition 75° 21′ ſ. following.

79. Fl. 39 Bootis.

Jan. 8, 1783. A pretty double ſtar. A little unequal. Both pr. With 227, near 1½ diameter of L.; with 460, near 2 diameters of L. Poſition 38° 21′ n. following.

80. *d* (Fl. 40ᵉ) Eridani adjecta.

Jan. 31, 1783. Double. About 1½ min. ſ. following *d* Eridani. Very unequal. Both dr. With 227, hardly viſible; with 460, very obſcure. Poſition 56° 42′ n. preceding.

II. Distance of L. from *d* Eridani, with 227, 1′ 21″ 47‴. Position of L. 17° 53′ s. following *d* Eridani.

81. FL. 49am Eridani sequens.

Jan. 31, 1783. Double. Near 1 degree following the 49th Eridani, towards δ Orionis. Very unequal. Both dw. With 227, full 1 diameter of L.; with 278, 1½ or 1¾ diameter of L.; with 460, 2½ or 3 diameters of L. Position 51° 36′ n. preceding.

82. FL. 31am Bootis sequens ad austrum.

Feb. 3, 1783. Double. Near 1 degree s. following the 31st, in a line from *v* continued through the 31st Bootis; the most south of two. A little unequal. L. w.; S. dw. With 227, about 1¾ diameter of L.; with 460, about 3 diameters of L. Position 1° 0′ s. following. A third star in view, 20° or 30° n. preceding.

83. FL. 22a Andromedæ borealior.

Feb. 26, 1783. Double. Within ½ degree north of the 22d, in a line parallel to the 19th and 16th Andromedæ; the following and smallest of two. Considerably unequal. L.w.; S. d. With 227, 1¼ or 1½ diameter of L.; with 460, more than 2 diameters of L. Position 5° 48′ n. following.

84. FL. 65 Piscium.

Feb. 27, 1783. Double. Nearly equal. Both pr. With 227, near 1½ diameter of L.; with 460, full 2 diameters. Position 30° 57′ n. preceding.

85. *b* (FL. 36a) Serpentis borealior et sequens.

Mar. 4, 1783. Double. About 1½ degree n. following *b*, nearly in a line from the 32d continued through the 36th Serpentis. Extremely unequal. L. w.; S. dw. With 227, 1 full diameter of L.; S. hardly to be seen; with 460, full 2 diameters of L. Position 46° 9′ n. preceding.

86.

II. 86. Fl. 49am Serpentis præcedens ad auſtrum.

Mar. 7, 1783. Double. About 1½ degree ſ. preceding the 49th, in a line with the 49th and another between this and the 49th Serpentis, each nearly at ¾ degree diſtance. Very unequal. L. dw.; S. d. With 227, 2 diameters, or 2¼ when beſt. Poſition 53° 9′ ſ. following.

87. Fl. 29â et 30â Monocerotis auſtralior.

Mar. 8, 1783. Multiple. It makes nearly an equilateral triangle with the 29th and 30th Monocerotis towards the ſouth. Among many, the fourth from the ſouth end of an irregular long row is double. A little unequal. Both pr. With 227, 1 diameter of L. and 16 more in view. Poſition 86° 12′ ſ. following.

88. ω (Fl. 51am) Serpentis præcedens ad auſtrum.

Mar. 8, 1783. Double. About ½ degree ſ. preceding the 51ſt, towards the 13th Serpentis. Very or extremely unequal. Both r. With 227, 2¼ diameter of L. when beſt; with 460, near 3 diameters of L. Poſition 44° 45′ n. preceding.

89. Ad Genam Monocerotis.

Mar. 26, 1783. Double. About 1 degree n. preceding the 12th Monocerotis, in a line parallel to α and λ Orionis; the ſmalleſt and moſt north of two. Conſiderably unequal. L. r.; S. bluiſh r. With 227, near 4 diameters of L. when beſt. Poſition 50° 51′ n. following.

90. Fl. 100am Herculis præcedens ad boream.

Mar. 27, 1783. Double. About 1¾ degree n. preceding the 100th, towards μ Herculis; a very ſmall teleſcopic ſtar; the moſt towards μ and ſmalleſt of three forming an arch. Conſiderably unequal. Both dw. With 227, about 2 diameters of L. Poſition 75° 9′ ſ. following.

II. 91. z (FL. 15[a]) Sagittæ auſtralior.

Apr. 5, 1783. Treble. About twice as far ſouth of z Sagittæ, as z and the ſtar near it are from each other; a ſmall ſtar. The two neareſt very unequal. L. pr.; S. r. With 227, 1½ diameter of L. Poſition 74° 54′ ſ. preceding. The third with L. extremely unequal. S. d. With 227, about 3 diameters of L. or more. Poſition about 40° or 50° n. preceding. With more light this would be a fine object.

92. In Camelopardali clune.

Apr. 30, 1783. Double. About four times the diſtance of the 10th and 12th Camelopardali, north of the 10th, and almoſt in the ſame direction with the 10th and 12th, is a ſtar of between the 5th and 6th magnitude not marked in FLAMSTEED; naming that ſtar A, we have the following direction. About ½ degree preceding A Camelopardali, in a line from the 2d Lyncis continued through A; the ſecond from A. Very unequal. L. w.; S. d. With 227, 1½ or 2 diameters of L. Poſition 22° 42′ ſ. following. Very inaccurate.

93. ε (FL. 13[a]) Aquilæ auſtralior.

May 25, 1783. Double. Near ¼ degree ſouth of, and a little following ε, towards λ Aquilæ, a very ſmall ſtar. Very unequal. L. dw.; S. dr. With 460, above 2 diameters of L. Poſition 16° 0′ n. preceding.

94. ι (FL. 17[am]) Andromedæ præcedens ad boream.

Aug. 19, 1783. Double. About 1½ degree n. preceding ι Andromedæ in a line parallel to α and β Caſſiopeiæ; in the ſide of a trapezium of four ſmall ſtars. Pretty unequal. Both r. With 460, 2½ diameters of L. Poſition 34° 24′ n. preceding.

95.

II. 95. η (Fl. 55ᵢ) Aquilæ auſtralior.

Sept. 12, 1783. Double. About ⅓ degree ſouth of η, in a line from α continued through η Aquilæ; a ſmall ſtar. A little unequal. Both duſky aſh-coloured. With 460, near 3 diameters of L.; with 278, near 2 diameters of L. Poſition 29° 3′ n. preceding.

96. θ (Fl. 65ᵢ) Aquilæ borealior et ſequens.

Sept. 12, 1783. Double. About 1¼ degree n. following θ Aquilæ, towards ε Delphini; more accurate towards 29 Vulpeculæ; a very conſiderable ſtar. Nearly equal. Both rw. With 278, about 1¼ diameter of L.; with 460, full 2 diameters. Poſition 56° 12′ ſ. preceding.

97. ζ (Fl. 64ᵃᵐ) Cygni præcedens.

Sept. 15, 1783. Treble. About 1 degree preceding ζ, towards the 41ſt Cygni; a large ſtar. The two neareſt extremely unequal. L. w.; S. pr. With 460, 2½ diameters of L. Poſition 45° 15′ n. preceding. The third with L. extremely unequal. Of the 5th or 6th claſs; about 50° ſ. preceding.

98. Fl. 49 Cygni.

Sept. 15, 1783. Double. Very unequal. L. r.; S. bluiſh r. With 278, 1½ diameter of L.; with 460, 2½ diameters of L. Poſition 31° 48′ n. following.

99. β (Fl. 6ᵃᵐ) Cygni ſequens ad boream.

Sept. 15, 1783. Double. Near ½ degree n. following β, towards ξ Cygni. Very unequal. Both dw. With 278, 1½ diameter of L.; with 460, about 2 diameters of L. Poſition 87° 48′ n. following.

100. Fl. 51ᵃ Cygni borealior et ſequens.

Sept. 24, 1783. Double. Near two degrees n. following the 51ſt Cygni, in a line parallel to ο Cygni and α Cephei; a

II. pretty considerable star. Very unequal. L. w.; S. inclining to blue. With 278, extremely unequal. and 1½ diameters of L. when best; requires attention to be seen well with this power; with 460, full 2 diameters of L. or 2¼ when best, otherwise much less. Position 15° 51′ n. following.

101. FL. 57am :: Camelopardali præcedens ad boream.

Sept. 26, 1783. Double. About 2 degrees n. preceding the 57 ::, towards the 42d Camelopardali; a considerable star near three smaller, forming an arch. About 1 degree from the double star V. 135. Considerably unequal. Both pr. With 278, 1⅔ diameter of L.; with 460, 2½ diameters of L. Position 67° 15′ n. preceding.

102. *e* (FL. 29a) Orionis australior et præcedens.

Sept. 27, 1783. Double. About ½ degree s. preceding *e*, in a line parallel to ρ and β Orionis; the largest of several. Very unequal. L. pr.; S. inclining to garnet. With 278, near 2 diameters of L. With 460, 2½ diameters of L. Position 52° 24′ s. following.

THIRD CLASS OF DOUBLE STARS.

III. 47. *e* Pollucis: FL. 38 Geminorum. In calce.

Dec. 27, 1781. Double. Extremely unequal. L. rw.; S. r. Distance, with 460, 7″ 48‴. Position 89° 54′ s. following. Two more in view, the nearest of them perhaps 40″; they form a rectangle nearly.

48.

III. 48. *r* (Fl. 61am) Geminorum præcedens ad boream.

Dec. 27, 1781. Double. About ½ degree n. preceding *r*, in a line parallel to *κ* and the 60th Geminorum; near two degrees from *δ*. A little unequal. Both pr. Distance 6″ 15‴. Position 43° 54′ n. following.

49. *δ* (Fl. 4am) Hydræ præcedens ad boream.

Jan. 20, 1782. Double. About 1¼ degree n. preceding *δ*, in a line from *η* continued through *δ* Hydræ. Pretty unequal. L. r.; S. garnet. Distance 12″ 30‴. Position 62° 48′ n. following.

50. *θ* Virginis. Fl. 51. De quatuor ultima et sequens.

Feb. 6, 1782. Treble. The two nearest extremely unequal. L. w.; S. d. Distance 7″ 8‴; but inaccurate on account of the obscurity of S. Position 69° 18′ n. preceding. For measures of the two farthest see VI. 43.

51. Fl. 88 Leonis. In dextro clune.

Feb. 9, 1782. Double. Extremely unequal. L. rw.; S. r. Distance 14″ 38‴; a little inaccurate. Position 47° 33′ n. preceding.

52. Fl. 10am Orionis sequens.

Feb. 17, 1782. Double. Above ¾ deg. n. following the 10th, towards *ω* Orionis. Considerably unequal. Both pr. Distance with 278, 13″ 40‴. Position 37° 3′ n. following.

53. *γ* Virginis borealior et sequens.

Feb. 17, 1782. Double. Near 2½ degrees n. following *γ*, in a line parallel to *ε* and *α* Virginis; a considerable star; a line from *γ* to this passes between two of nearly the same magnitude with this star. A little unequal. Both d. Distance 12″ 58‴. Position 79° 0′ n. preceding.

III. 54. Secunda ad σ Urſæ majoris. FL. 13. In fronte.

June 2, 1782. Double. Extremely unequal. L. w.; S. r. Diſtance 7″ 56‴. Poſition 13° 0′ n. preceding.

55. υ (FL. 18im) Coronæ borealis ſequens ad boream.

June 14, 1782. Double. Conſiderably unequal. L. dr.; S. d. Diſtance with 227, about 3 or 4 diameters of L. being too obſcure for the micrometer. Poſition 53° 48′ ſ. preceding. Diſtance of the largeſt of the two from υ Coronæ 1′ 18″ 8‴. Poſition of the ſame with υ, 64° 24′ n. following.

56. S (FL. 72d) Serpentarii borealior.

June 16, 1782. Double. About 2½ degrees n. of the 72d Serpentarii; a conſiderable ſtar. A little unequal. Both r. Diſtance 7″ 37‴. Poſition 9° 42′ ſ. preceding. A third ſtar about 1′ preceding.

57. In Anſeris corpore.

Aug. 11, 1782. A pretty double ſtar. About ¾ degree n. of a cluſter of ſtars formed by the 4th, 5th, 7th, 9th Anſeris; in a line parallel to the 6th Vulpeculæ and β Cygni; that of two which is fartheſt from the cluſter. A little unequal. Both r. Diſtance 7″ 1‴. Poſition 58° 36′ ſ. following.

58. θ Perſei. FL. 13. In ſiniſtro humero.

Aug. 20, 1782. Double. Extremely unequal. L. w. inclining to r.; S. d. Diſtance with 932, 13″ 31‴. Poſition 20° 0′ n. preceding. A third ſtar, very unequal, within 1′; towards the ſouth.

59. Ad FL. 19im Perſei. In capite.

Aug. 20, 1782. Double. It is perhaps the 19th Perſei removed, or more likely a ſtar not marked in FLAMSTEED's Catalogue; the 19th being either vaniſhed, or miſplaced by FLAMSTEED.

III. FLAMSTEED. Pretty unequal. L. bw.; S. br. Distance 12″ 2‴. Position 0° 0′ following.

60. Secunda ad *p* Persei. FL. 20. Illas in larva præcedit.

Aug. 20, 1782. Double. Extremely unequal. L. rw.; S. d. Distance 14″ 2‴. Position 30° 30′ f. following.

61. Sub finem caudæ Draconis.

Aug. 29, 1782. Double. Of two considerable stars, about half-way between *α* and *ι* Draconis, that which is towards *ι*. The two stars are parallel to *ζ* and *ε* Ursæ majoris. Very unequal. L. pr.; S. db. Distance 12″ 30‴; perhaps a little inaccurate. Position 87° 42′ n. preceding.

62. FL. 35 Piscium. In lino austrino.

Sept. 4, 1782. Double. Considerably unequal. L. rw.; S. pr. Distance 12″ 30‴. Position 58° 54′ f. following.

63. Prope FL. 65am Sagittarii. Ad extremum paludamentum.

Sept. 5, 1782. Double. Near ½ degree f. following the 65th Sagittarii towards *ζ* Capricorni. Very unequal. Too low for colours; perhaps dw. Distance 14″ 20‴. Position 73° 48′ n. following.

64. FL. 26 Aurigæ. In dextri cruris involucro.

Sept. 5, 1782. Double. Very unequal. L. rw.; S. r. Distance 13″ 25‴. Position 2° 36′ n. preceding.

65. *e* (FL. 58a) Persei australior. In dextri pedis talo.

Sept. 7, 1782. Double. About 10′ south of the 58th Persei, in a line parallel to *ζ* and *ι* Aurigæ; a small telescopic star. Very unequal. L. r.; S. d. Distance with 625, 11″ 22‴. Position 48° 54′ n. following. Very inaccurate: windy.

66. *e* Tauri. FL. 30. In dextri humeri scapula.

Sept. 7, 1782. Double. Extremely unequal. L. w.; S. r. Distance 11″ 16‴; inaccurate on account of obscurity. Position 17° 15′ n. following.

III. 67. ι Leporis. FL. 3. Borea præcedentis lateris quadrilateri ad aures.

Sept. 7, 1782. Double. Excessively unequal. L. w.; S. d. With 227, there was not a possibility of measuring the distance, though the glass was carefully cleaned; on trying 625, I found the star so strong that it bore a very tolerable good light *. Distance with this power 12″ 20‴. Position 89° 21′ n. preceding.

68. η (FL. 17ª) Arietis australior et præcedens.

Sept. 10, 1782. Double. Full 1 degree south preceding η, in a line parallel to α and γ Arietis. Very unequal. L. pr.; S. d. Distance 8″ 5‴. Position 55° 42′ s. following.

69. Prope FL. 64ᵃᵐ Aquarii. In dextro femore.

Sept. 27, 1782. Double. Full 1½ degree n. following the 64th ::, in a line parallel to λ and φ Aquarii; the largest of two that follow a very obscure triangle in the finder. Extremely unequal. L. rw.; S. db. Distance 12″ 46‴. Position 20° 3′ s. following.

70. κ Cephei. FL. 1. In dextro crure.

Sept. 27, 1782. A beautiful double star. Extremely unequal. L. fine w.; S. r. Distance 5″ 47‴. Position 32° 30′ s. following.

* With regard to small stars, that become visible by an increase of magnifying power, we may surmise, that it is partly owing to the greater darkness of the field of view, arising from the increased power, and partly to the real effect of the power; for, though the real diameter of a star, notwithstanding it be magnified a thousand times, should still remain smaller than the minimum visibile, yet since a star of the seventh magnitude may be seen by the naked eye, we may conclude, that the light of a star subtends incomparably a larger angle than its luminous body; and this may be in such a proportion, with very small stars, that the power of the telescope shall be just sufficient to magnify the real diameter so as to bring it within the limits of this proportion, whereby the star will become visible.

71.

III. 71. Tiaram Cephei præcedens.

Sept. 27, 1782. Treble. About 1½ degree preceding the *garnet ſtar* *, in a line parallel to ι and ζ Cephei. The two neareſt very unequal. L. w.; S. db. Diſtance 11″ 35‴. Poſition 35° 24′ ſ. following. The two fartheſt conſiderably unequal. S. db. Diſtance 18″ 37‴. Poſition 73° 57′ n. preceding. The place of the *garnet ſtar*, reduced to the time of FLAMSTEED's Catalogue, is about ☌ 21 h. 45′. P.D. 32°½.

72. Tiaram Cephei præcedens.

Sept. 27, 1782. Double. Within ¼ degree of the foregoing treble ſtar. Conſiderably unequal. L. rw.; S. pr. Diſtance 13″ 7‴. Poſition 32° 0′ n. following.

73. FL. 25ª Ceti auſtralior et ſequens.

Oct. 2, 1782. Double. About ¾ degree ſ. following the 25th, in a line parallel to θ and τ Ceti. Pretty unequal. Diſtance with 278, 14″ 50‴. Poſition 89° 12′ ſ. preceding; perhaps a little inaccurate.

74. FL. 18ª Pegaſi auſtralior. Ad oculum ſiniſtrum.

Oct. 4, 1782. Double. About ¾ degree ſ. preceding the 18, in a line parallel to η and ε Pegaſi; the moſt north and largeſt of two. A little unequal. Both rw. Diſtance 14″ 29‴ full meaſure. Poſition 31° 33′ n. following.

75. Ad Genam Monocerotis.

Oct. 4, 1782. Double. About 1 degree n. of, and a little preceding the ſix teleſcopics in the place of the 12th, in a line parallel to the 12th Monocerotis and μ Geminorum.

76. τῶν quatuor teleſcopicarum, δ Orionis ſequentium, penultima.

Oct. 4, 1782. Double. About ¾ degree n. following δ, in a line parallel to τ and ι Orionis. Extremely unequal.

* Phil. Tranſ. vol. LXXIII. p. 257.

III. L. r.; S. d. Distance with 278, 9″ 12‴. Position 13° 6′ n. preceding.

77. FL. 65am Arietis sequens ad austrum.

Oct. 9, 1782. Double. About ¾ degree s. following the 65th Arietis, in a line parallel to the Pleiades and ε Tauri; the preceding of two. Very unequal. L. r.; S. bluish. Distance 8″ 32‴. Position 73° 18′ s. following.

78. FL. 13am Tauri præcedens ad austrum.

Oct. 9, 1782. Double. About 1¾ degree s. preceding the 13th Tauri, in a line parallel to ε Tauri and δ Ceti. Nearly equal. Both pr. Distance 7″ 10‴. Position 87° 57′ n. preceding.

79. ε (FL. 83a) Ceti borealior.

Oct. 13, 1782. Double. About ¾ degree n. of ε Ceti; the nearest of three forming an arch. Extremely unequal. L. rw.; S. darkish red. Distance with 278, 10″ 48‴. Position 45° 12′ s. preceding.

80. σ (FL. 76am) Ceti præcedens. In sinistro crure.

Oct. 13, 1782. Double. Full 1½ degree preceding σ, towards τ Ceti. Extremely unequal. L. rw.; S. br. Distance 11″ 16‴. Position 22° 24′ n. preceding.

81. Parvula à ζ Lyræ ε versus.

Oct. 19, 1782. Double. Above ½ degree from ζ towards ε Lyræ. Extremely unequal. L. r.; S. dr. Distance 9″ 27‴ full measure. Position 66° 18′ n. following.

82. FL. 41 Aurigæ.

Nov. 6, 1782. A pretty double star. Considerably unequal. L. w.; S. grey inclining to r. Distance 8″ 32‴. Position 80° 0′ n. preceding.

83.

III. 83. Fl. 19 Lyncis.

Nov. 13, 1782. Double. A little unequal. L. rw.; S. bw. Distance 14″ 11‴. Position 46° 54′ f. preceding.

84. Fl. 40 Lyncis. In Urſæ majoris pede.

Nov. 13, 1782. Double. Very or extremely unequal. L. wr.; S. r. Diſtance 7″ 11‴. Poſition 48° 12′ n. preceding.

85. Fl. 2 Canum Venaticorum.

Nov. 13, 1782. Double. Very unequal. L. r.; S. bluiſh. Diſtance 12″ 12‴. Poſition 11° 0′ f. preceding.

86. Fl. 57 Urſæ majoris.

Nov. 20, 1782. Double. The largeſt of two ſtars. Exceſſively unequal. L. w.; S. a red point without ſenſible magnitude. With 227, S. is but juſt viſible. Poſition 75° 36′ n. following.

87. Fl. 59ª Urſæ majoris borealior.

Nov. 20, 1782. A pretty treble ſtar. Near 1½ degree n. of the 59th, in a line parallel to ψ and β Urſæ majoris nearly. The two neareſt conſiderably unequal. L. pr.; S. r. Diſtance 12″ 30‴. Poſition 0° 0′ preceding. The two farthest very unequal. S. dr. Diſtance 32″ 21‴, Poſition 4° 0′ n. following.

88. Fl. 11ª Tauri borealior et ſequens.

Nov. 25, 1782. Double. About ½ degree n. following the 11th Tauri, towards ι Aurigæ. Very unequal. L. w.; S. pr. Diſtance with 278, 13″ 37‴. Poſition 89° 51′ n. following.

89. Ad 63ᵗᵐ Herculis. In linea per δ et ε ducta.

Nov. 26, 1782. Double. About 4 degrees from δ towards ε Herculis, near the 63d. Very unequal. L. r.; S. r. Diſtance 11″ 53‴. Poſition 47° 48′ n. following.

90.

III. 90. FL. 103^{d} Tauri borealior.

Nov. 29, 1782. Double. About three degrees directly n. of the 103 Tauri; the largeſt of three, forming an obtuſe angle. Conſiderably unequal. L. rw.; S. pr. Diſtance with 278, 13″ 6‴. Poſition 64° 0′ n. following.

91. FL. 62^{d} Arietis borealior et ſequens,

Dec. 23, 1782. Double. Near 1 degree n. following the 62d Arietis, towards ε Perſei. Nearly equal. Both dw. Diſtance 11″ 17‴; not very accurate. Poſition 12° 24′ n. preceding or ſ. following.

92. ξ (FL. 77^{am}) Cancri præcedens ad boream.

Dec. 28, 1782. Double. About 1 degree n. preceding ξ Cancri, in a line parallel to ε Leonis and the 41ſt Lyncis; a conſiderable ſtar. A little unequal. Both rw. Diſtance 8″ 50‴. Poſition 65° 12′ ſ. preceding.

93. FL. 117 Tauri.

Dec. 31, 1782. Double. Almoſt equal. Both rw. Diſtance 12″ 12‴. Poſition 52° 27′ ſ. following.

94. ν (FL. 7^{am}) Leporis præcedens ad boream.

Dec. 31, 1782. Double. About 1⅓ degree n. preceding ν Leporis, in a line parallel to κ and ε Orionis; the ſecond in that line. Equal. Both rw. Diſtance 11″ 44‴. Poſition 4° 0′ ſ. following or n. preceding.

95. ν (FL. 48^{am}) Eridani præcedens ad auſtrum.

Jan. 2, 1783. Double. Near ⅓ degree ſ. preceding ν, in a line from the 51ſt continued through the 48th Eridani. Extremely unequal. L. rw.; S. d. and hardly to be ſeen with 227. Diſtance with 278, 15″ 21‴; very inaccurate on account of obſcurity. Poſition 9° 18′ ſ. preceding.

III. 96. Fl. 17 Crateris.

Jan. 10, 1783. Double. Nearly equal. Both rw. Distance 9″ 46‴. Position 64° 27′ f. preceding.

97. Fl. 54 Hydræ.

Jan. 10, 1783. Double. Very unequal. L. w.; S. bluish r. Distance 11″ 17‴; too low for great accuracy. Position 38° 15′ f. following.

98. Ad Genam Monocerotis.

Jan. 13, 1783. Double. About ⅔ degree f. preceding the most f. of a cluster of six telescopics in the place of the 12th, in a line parallel to the 15th and 12th Monocerotis. Excessively unequal. Position 61° 57′ f. preceding.

99. Fl. 55 Eridani.

Jan. 31, 1783. Double. A very little unequal. L. pr.; S. rw. Distance 9″ 9‴. Position 44° 9′ n. preceding.

100. Fl. 55am Eridani præcedens ad austrum.

Jan. 31, 1783. Double. About 2¼ degrees f. preceding the 55th Eridani, in a line parallel to Rigel and γ Eridani. Considerably unequal. L. pr.; S. db. Distance 11″ 53‴. Position 16° 24′ f. preceding.

101. *k* Centauri. Fl. 3.

Jan. 31, 1783. Double. Considerably unequal. L. dw.; S. dpr. Distance 11″ 35‴. Position 22° 0′ f. following.

102. *h* (Fl. 29am) Herculis præcedens ad austrum.

Feb. 3, 1783. Double. About 1¼ degree f. preceding *h* Herculis towards ε Serpentis; a small star. Very unequal. Both r. Distance 14″ 2‴. Position 67° 12′ n. following.

103. ε (Fl. 37a) Serpentis borealior et sequens.

March 4, 1783. Double. Near two degrees f. following ε, in a line parallel to the 13th Serpentis and 10th Serpentarii. Very unequal. L. pr.; S. r.; but a *dry fog*, if I may

so

III. ſo call it, probably tinges them too deeply. Diſtance with 278, 12″ 34‴; with 625, 12″ 23‴. Poſition 50° 12′ n. preceding.

104. FL. 83am Herculis præcedens.

Mar. 26, 1783. Double. About ⅓ degree preceding the 83; the ſecond ſtar towards the 79th Herculis. Very unequal. L. r.; S. darker r. Diſtance 14″ 20‴. Poſition 83° 48′ n. preceding.

105. γ (FL. 12a) Sagittæ borealior et præcedens.

April 7, 1783. Double. About 2′ preceding the double ſtar V. 106. Pretty unequal. L. r.; S. d. Diſtance 14″ 29‴; very inaccurate, on account of obſcurity. Poſition 50° 24′ ſ. preceding.

106. FL. 5 Serpentis.

May 21, 1783. Double. Exceſſively unequal. L. rw.; S. db. Too obſcure for meaſures. Of the third claſs, far. Poſition about 30° or 40° n. following.

107. Congerie Stellularum Sagittarii borealior.

June 6, 1783. Double. Above 1¼ degree n. of the 20th cluſter of ſtars of the *Connoiſſance des Temps*, in a line parallel to γ Sagittarii and the cluſter: the moſt ſouth of many. Conſiderably unequal. Diſtance with 278, 15″ 10‴. As accurate as the priſmatic power of the atmoſphere, which lengthens the ſtars, will permit. Poſition 54° 48′ ſ. preceding *.

108.

* What I call the priſmatic power of the atmoſphere, of which little notice has been taken by aſtronomers, is that part of its refractive quality whereby it diſperſes the rays of light, and gives a lengthened and coloured image of a lucid point. It is very viſible in low ſtars; FOMALHAND, for inſtance, affords a beautiful priſmatic ſpectrum. That this power ought not to be overlooked in delicate

III. 108. Fl. 19im Aquilæ præcedens ad boream.

July 7, 1783. Double. Above ¾ n. preceding the 19th, in a line parallel to β and ζ Aquilæ. Very unequal. L. r.; S. dr. Diſtance 12″ 58‴. Poſition 58° 27′ ſ. following.

109. Fl. 19am Aquilæ præcedens ad Boream.

July 7, 1783. Double. About 1⅓ degree n. preceding the 19th, in a line parallel to ε and δ Aquilæ. Pretty unequal. Both rw. Diſtance 10″ 13‴. Poſition 22° 6′ n. preceding.

110. Fl. 77a Cygni borealior et præcedens.

Sept. 17, 1783. Quadruple. Full ¾ degree n. preceding the 17th, in a line parallel to σ and α Cygni; a ſmall ſtar. The two neareſt extremely unequal. L. r.; S. d. Diſtance with 625, 13″ 54‴. Poſition 67° 36′ ſ. following. The two largeſt a very little unequal. Both r. Diſtance with 278, 25″ 58‴. Poſition 40° 33′ n. following.

and low obſervations, is evident from ſome meaſures I have taken to aſcertain its quantity. Thus I found, May 4, 1783, that the perpendicular diameter of ε, Flamsteed's 20th Sagittarii, meaſured 16″ 9‴, while the horizontal was 8″ 35‴; which gives 7″ 34‴ for the priſmatic effect: the meaſures were taken with 460, near the meridian, and the air remarkably clear. And though this power, which depends on the obliquity of the incident ray, diminiſhes very faſt in greater altitudes, yet I have found its effects perceivable as high, not only as α or γ Corvi in the meridian, but up to Spica Virginis, and even to Regulus. Experiments on theſe two latter ſtars I made November 20, 1782; when Regulus, at the altitude of 49°, ſhewed the purple rather fuller at the bottom of the field of view than when it was at the upper edge; which ſhews that the priſmatic powers of the edges of the eye lens were aſſiſted in one ſituation by the power of the atmoſphere, but counteracted by it in the other. I turned the eye lens in all ſituations, to convince myſelf that it was not in fault. This experiment explains alſo, why a ſtar is not always beſt in the center of the field of view; a fact I have often noticed before I knew the cauſe.

III. lowing. The farthest very unequal. S. d. Position almost in a line with the two largest.

111. ε (FL. 46ª) Orionis borealior et sequens.

Sept. 20, 1783. Treble. About 1¼ degree n. following ε, towards α Orionis. The two nearest of the third class.

112. δ (FL. 18ᵃᵐ) Cygni sequens ad austrum.

Sept. 22, 1783. Double. About 1 degree s. following δ, towards the 47th Cygni; a pretty considerable star. Equal, or perhaps the southern star the smallest. Both pr. Distance with 278, 10″ 8‴. Position 71° 0′. s. following.

113. FL. 27ᵃᵐ Cygni præcedens ad austrum.

Sept. 23, 1783. Quadruple and Sextuple. About ½ degree s. preceding the treble star I. 96.; the middle of three, the most north whereof is the 27th Cygni. In the quadruple or n. preceding set, the two nearest very unequal. Distance with 278, 11″ 16″. Position 26° 0′ n. preceding; the two largest almost equal. Both r. Distance with 278, 29″ 27‴. Position 57° 12′ n. following. In the sextuple or s. following set, the two largest pretty unequal. Both r. Distance with 278, 19″ 20‴. Position 27° 36′ s. preceding. All the other stars are as small as the smallest of the quadruple set, and some of them much smaller.

114. FL. 16ᵃᵐ Monocerotis præcedens ad boream.

Jan. 23, 1784. Double. About 1¼ degree n. preceding the 16th.

FOURTH

FOURTH CLASS OF DOUBLE STARS.

IV. 45. In pectoris crate Orionis.

Dec. 27, 1781. Double. About $\frac{2}{3}$ degree following ψ, towards n Orionis. Extremely unequal. L. pr.; S. dr. Distance with 278, 20″ 3″. Position 62° 24′ f. following.

46. Fl. 21 :: Geminorum *.

Dec. 27, 1781. Double. A little unequal. Both pr. Distance about 25″. Position

47. Fl. 3 Leonis.

Feb. 2, 1782. Double. Excessively unequal. L. r.; S. d.; not visible with 227. Distance estimated with 460, about 24″. Position a little n. following. A third star in view. Distance perhaps 2′. Position about 15° f. following.

48. H (Fl. 1am) Geminorum præcedens ad boream.

Feb. 6, 1782. Quintuple. In the form of a cross. About $\frac{2}{3}$ degree n. preceding H Geminorum, in a line parallel to the 65th Orionis and ζ Tauri; the middle of three. The two nearest or preceding of the five extremely unequal. Distance 20″ 57‴. Position 7° 27′ f. preceding. The last of the three, in the short bar of the cross, has an excessively obscure star near it of the third class. Five more in view, differently dispersed about the quintuple.

* The 21st and 20th Geminorum are not in the heavens as they are marked in Flamsteed's Atlas, so that it becomes doubtful whether the N° 21. is right.

IV. 49. ξ (FL. 4am) Virginis ſequens ad boream.

Feb. 6, 1782. Double. 1 full degree n. following ξ Virginis, in a line parallel to ι and β Leonis. A little unequal. L. pr.; S. dr. Diſtance 27″ 28‴. Poſition 56° 30′ ſ. preceding.

50. FL. 17 Virginis. In pectore.

Feb. 6, 1782. Double. Conſiderably unequal. L. w.; S. bluiſh. Diſtance 20″ 9‴. Poſition 58° 21′ n. preceding.

51. *k* Virginis :: FL. 44 :: †. In ala auſtrina.

Feb. 6, 1782. Double. A ſtar ſouth of three forming an arch, and of the ſame magnitude with the middle one of the arch. Extremely unequal. L. w.; S. db. Diſtance 22″ 17‴; inaccurate. Poſition 32° 30′ n. following.

52. * ι Cancri. FL. 48. In boreali forfice.

Feb. 8, 1782. Double. Conſiderably unequal. L. rw.; S. d. garnet. Diſtance 29″ 54‴. Poſition 39° 54′ n. preceding; a little inaccurate.

53. π Geminorum. FL. 80. Supra capita.

Feb. 9, 1782. Double. Exceſſively unequal. L. garnet; S. d. Diſtance with 460, 21″ 30‴. Poſition Other very ſmall ſtars in view.

54. δ (FL. 4am) Hydræ ſequens.

Feb. 11, 1782. Double. About ½ degree following δ, towards ζ Hydræ. Pretty unequal. Both pr. S. deeper. Diſtance 25″ 43‴. Poſition 59° 24′ n. following.

55. FL. 41am Lyncis ſequens. In caudæ fine.

Mar. 5, 1782. Double. About 3½ minutes n. following the 41ſt Lyncis. Extremely unequal. L. r.; S. dr. Diſtance 15″ 52‴; a little inaccurate. Poſition 50° 48′ n. preceding; inaccurate.

† Perhaps the 45th; requires fixed inſtruments to determine.

56.

IV. 56. Fl. 18 Libræ.

April 3, 1782. Double. The following of two. Extremely unequal. L. r.; S. b. Distance 17″ 59‴. Position 44° 45′ n. following.

57. Fl. 42am Comæ Berenices sequens ad austrum.

April 15, 1782. Double. About 3 degrees s. following the 42d Comæ Berenices towards *υ* Bootis; the vertex of an isosceles triangle. Extremely unequal. Distance with 625, 16″ 42‴. Position 46° 31′ s. preceding.

58. Fl. 36am Comæ Berenices præcedens ad boream.

April 18, 1782. A pretty double star. About 2½ degrees n. preceding the 36th, in a line parallel to the 42d and 15th Comæ Berenices; the following of two unequal stars. A little unequal. Both rw. Distance 15″ 52‴. Position 67° 57′ s. preceding.

59. Prope *α* Lyræ.

May 12, 1782. Double. About 2 or 3 minutes s. preceding *α* Lyræ. Very unequal. Both d. Distance with 278, 22″ 20‴. Position 33° 57′ n. preceding. Position of the largest with regard to *α* Lyræ 59° 12′ s. preceding.

60. Fl. 4am Ursæ majoris sequens ad boream.

June 6, 1782. Double. Near 1 degree n. following the 4th, in a line parallel to *o* and *h* Ursæ majoris; a pretty large star. Extremely unequal. L. r.; S. d. Distance near 30″; but too obscure for measures.

61. *ζ* (Fl. 7i) Coronæ australior et præcedens.

July 18, 1782. Double. Near ½ degree s. preceding *ζ*, towards *η* Coronæ bor. Nearly equal. Both pr. Distance 16″ 46‴. Position 4° 57′ n. following.

62.

IV. 62. τ (FL. 22[d]) Herculis auſtralior et ſequens.

Aug. 11, 1782. Double. About 2¼ degrees ſ. following τ Herculis, in a line parallel to ι and γ Draconis; a conſiderable ſtar. Very or extremely unequal. L. w.; S. br. Diſtance 16″ 51‴. Poſition 72° 15′ ſ. preceding.

63. FL. 42 Herculis. Dextrum ſupra genu.

Aug. 11, 1782. Double. Very unequal. L. r.; S. rw. Diſtance 21″ 31‴. Poſition 3° 42′ ſ. following.

64. Prope *q* (FL. 12[am]) Perſei.

Aug. 20, 1782. Double. Within a few minutes of *q* Perſei. Pretty unequal. Both pr.; but S. a little darker. Diſtance 21″ 59‴. Poſition 57° 57′ ſ. preceding.

65. Prope FL. 3[am] Caſſiopeiæ.

Aug. 25, 1782. Double. Within 10 minutes of the 3d Caſſiopeiæ. Very unequal. L. pr.; S. r. Diſtance 20″ 46‴; very inaccurate. Poſition 41° 12′ ſ. following.

66. θ (FL. 33[am]) Caſſiopeiæ præcedens.

Aug. 28, 1782. Double. About 1¼ degree ſ. of, and a little preceding θ, in a line from δ continued through θ Caſſiopeiæ. Extremely unequal. L. r.; S. db. Diſtance 24″ 2‴; very inaccurate. Poſition 13° 12. n. following; inaccurate.

67. + FL. 40 et 41 Draconis.

Aug. 29, 1782. Double. A little unequal. L. rw.; S. pr. Diſtance 20′ 39‴ mean meaſure; very accurate. Poſition 35° 15′ ſ. preceding *. There is a third, much ſmaller ſtar. Diſtance 3′ 16″ 33‴. Poſition about 30° ſ. following.

* The proper motion of one of theſe ſtars at leaſt ſince the time of FLAMSTEED is evident, as he gives us their difference in AR 2′, and in PD 3′ 5″. Poſition ſ. preceding. Hence we have the hypotenuſe or diſtance above 3′ 40″, inſtead of 20″ 39‴; and the angle 86° 17′ inſtead of 35° 15′.

IV. 68. Fl. 77 Piſcium. In lini flexu.

Sept. 3, 1782. Double. A little unequal. L. wr.; S. pr. Diſtance 29″ 36‴. Poſition 4° 48′ n. following. In both meaſures the weather too windy for accuracy.

69. Fl. 23am Andromedæ præcedens.

Sept. 4, 1782. Double. Full 1½ degree preceding the 23d, in a line parallel to ν and ι Andromedæ. Of two double ſtars in the finder the largeſt of the preceding ſet. Very unequal. L. r.; S. d. Diſtance with 278, 21″ 58‴. Poſition 70° 36′ n. preceding.

70. Fl. 51 Piſcium. In auſtrino lino.

Sept. 4, 1782. Double. Very unequal. L. rw.; S. d. Diſtance with 278, 22″ 29‴. Poſition 0° 36′ n. following.

71. * o Capricorni. Fl. 12. Trium in roſtro auſtrina.

Sept. 5, 1782. Double. Pretty unequal. Both rw. Diſtance 23″ 30‴. Poſition 30° 45′ ſ. preceding.

72. Fl. 55â Perſei borealior.

Sept. 7, 1782. Double. About ¼ degree n. of the 55th Perſei; of three in a line the moſt north. Pretty unequal. L. rw.; S. pr. Diſtance with 278, 16″ 51‴. Poſition 27° 24′ n. following.

73. In Conſtellatione Camelopardali.

Sept. 7, 1782. Double. Between Fl. 2 and 8 Cam.; the ſmalleſt of two that are within ¼ degree of each other. Conſiderably unequal. Diſtance 19″ 32‴. Poſition 85° 0′ ſ. preceding.

74. δ (Fl. 68am) Tauri ſequens ad boream.

Sept. 7, 1782. Double. Near ½ degree n. following δ, towards ι Tauri. Very unequal. L. pr.; S. r. Diſtance 16″ 31‴. Poſition 25° 45′ n. following.

75.

IV. 75. *r* (FL. 66am) Tauri ſequens.

Sept. 7, 1782. Double. About 1¼ degree n. following *r*, in a line parallel to *μ* Tauri and the 9th Orionis. Very unequal. L. r.; S. dr. Diſtance 22″. 35‴. Poſition 61° 36′ ſ. following.

76. FL. 13am Ceti præcedens ad auſtrum.

Sept. 9, 1782. Double. About 1 degree ſ. preceding the 13th, towards the 8th Ceti. Conſiderably unequal. L. rw.; S. br. Diſtance with 278, 18″ 35‴. Poſition 40° 24′ n. following.

77. FL. 37a Ceti borealior. In dorſo.

Sept. 22, 1782. Double. About ¼ degree n. preceding the 37th, towards the 36th Ceti. Very unequal. L. r.; S. dr. Diſtance 19″ 6‴. Poſition 63° 24′ n. preceding.

78. *η* (FL. 3im) Cephei præcedens.

Sept. 27, 1782. Double. About 1¼ degree preceding *η*, in a line from *ε* continued through *η* Cephei. Very unequal. L. r.; S. d. Diſtance 19″ 32‴. Poſition 40° 36′ n. following.

79. *μ* Cephei. FL. 13. Ad coronam.

Sept. 27, 1782. Double. A little unequal. L. w.; S. rw. Diſtance 21″ 3‴. Poſition 77° 48′ ſ. preceding.

80. *β* (FL. 2b) Canis majoris borealior.

Sept. 30, 1782. Double. About 1¾ degree n. of *β* Canis majoris towards the 11th Monocerotis; the moſt n. of two. Conſiderably unequal. Diſtance 17″ 59‴; difficult to take, and perhaps a little inaccurate. Poſition 2° 24′ n. following.

81. *ν* Canis majoris. FL. 6. In dextro genu.

Sept. 30, 1782. Double. Conſiderably unequal. L. rw.; S. pr. Diſt. 18″ 19‴. Poſition very near directly preceding.

IV. 82. Prope Fl. 16am Cephei. In cingulo.

Sept. 30, Double. Above ¾ degree following the 16th Cephei, 1782. in a line parallel to β and α Cassiopeiæ. Considerably unequal. L. orange. S. r. Distance 28″ 5‴. Position 79° 18′ n. preceding.

83. Fl. 26 Ceti. Supra dorsum.

Oct. 2, Double. Very unequal. L. rw. S. db. Distance 1782. 17″ 2‴ mean measure. Position 14° 36′ f. preceding.

84. m Orionis. Fl. 23 In crate pectoris

Oct. 2, Double. Considerably unequal. L. w; S. pr. 1782. Distance with 278, 26″ 9‴. Position 59° 33′ n. following.

85. Fl. ultima Lacertæ.

Oct. 4, Treble. The two nearest extremely unequal. L. 1782. rw.; S. d. Distance 20″ 27‴. Position 79° 23′ n. preceding. The next very unequal; S. r. Distance 54″ 57‴; inaccurate. Position 44° 24′ n. following. A fourth and fifth star in view.

86. Fl. 8 Lacertæ. In media cauda.

Oct. 4, Quadruple. The two largest and nearest a little une- 1782. qual. Both rw. Distance 17″ 14‴. Position 84° 30′ f. preceding. The two next very unequal, of the fourth class. The two remaining considerably unequal, of the fifth class. They form an arch.

87. e (Fl. 29am) Orionis præcedens. In sinistro calcaneo.

Oct. 4, Double. About 1 degree preceding e, in a line pa- 1782. rallel to σ Orionis and b Eridani nearly. Considerably unequal. Both pr. Distance 29″ 18‴. Position 82° 18′ n. following.

IV. 88. FL. 7 Tauri. In dorso.

Oct. 9, Double. Very unequal. L. pr.; S. dr. Distance
1782. 19″ 50‴. Position 23° 15′ n. following.

89. E telescopicis caudam Arietis sequentibus.

Oct 9, Double. The vertex of an isosceles triangle follow-
1782. ing τ Arietis; a very small star. Very unequal. L. r.; S. d. Distance with 278, 20″ 3‴. Position 62° 0′ s. following.

90. Ad FL. 18^{am} Ursæ minoris. Prope eductionem caudæ.

Oct. 12, Double. The largest of six or seven stars, and most
1782. south of a triangle formed by three of them. A little unequal. L. pr.; S. deeper pr. Distance 26″ 24‴. Position 3° 12′ n. following.

91. FL. 2 Navis.

Oct. 12, A pretty double star. A little unequal. L. w.; S.
1782. w. inclining to r. Distance 17″ 23‴. Position 69° 12′ n. preceding.

92. β inter et ζ Delphini.

Oct. 17, Treble. Between β and ζ, but nearer to β Delphini.
1782. All three nearly equal. All w.r. Distance of the two nearest with 278, 21″ 33‴. Position 18° 27′ n. preceding.

93. ε (FL. 4ta) Lyræ sequens.

Oct. 19, Double. About 3 degrees following ε, in a line pa-
1782. rallel to α and β Lyræ; the largest of two. Extremely unequal. L. w.; S. r. Distance 19″ 50‴. Position 24° 0′ f. preceding.

94. E borealibus telescopicis β Lyræ præcedentibus.

Oct. 19, Double. Full 2 degrees n. preceding β Lyræ, in a
1782. line parallel to the 18th and ε; the sixth telescopic star.

Considerably

IV. Considerably unequal. L. rw.; S. pr. Distance 22″ 53‴. Position 5° 24′ n. following.

95. Fl. 25am Monocerotis præcedens.

Oct. 19, 1782. Quadruple. About 2½ degrees preceding, and a little n. of the 25th Monocerotis. Two large stars always to be seen, and two more only visible in dark nights. The nearest which is that to the smallest of the two large ones, extremely unequal. Distance 20″ 27‴. Position following.

96. Fl. 25am Monocerotis sequens. In latere.

Oct. 19, 1782. Double. About 1¼ n. following the 25th, in a line parallel to the 21st Monocerotis and Procyon. A little unequal. Both dr. Distance 18″ 19‴. Position 24° 0′ s. preceding.

97. Fl. 29 Monocerotis. In femore.

Oct. 19, 1782. Double. Extremely unequal. L. wr.; S. d. Distance 29″ 54‴. Position 15° 12′ s. following. Six more in view.

98. α (Fl. 58am) Orionis ad austrum præsens.

Oct. 29, 1782. Double. About ½ degree preceding α, towards ζ Orionis. Equal. Both r. Distance 17″ 59‴; a little inaccurate.

99. Duarum telescopicarum δ Sagittæ ad austrum sequentium borea.

Nov. 6, 1782. Treble. Of a trapezium, consisting of this treble star, δ, ζ, and the 9th Sagittæ, it is the corner opposite to ζ; the nearest to ζ of two. The two nearest very unequal. L. pr.; S. db. Distance 21″ 22‴; inaccurate. Position 0° 0′ following. The two largest a little unequal; of the fifth class. Position 10° 36′ s. preceding.

IV. 100. χ Sagittæ. FL. 13. Infra mediam arundinem.

Nov. 6, 1782. Treble. The largest of three. The two nearest equal. Both r. Distance 23″ 2‴. Position 10° 12′ s. preceding. The third is a large star. Distance above 1 minute. Position about 10° or 15° n. preceding the other two.

101. φ (FL. 24ª) Aurigæ borealior et præcedens.

Nov. 6, 1782. Double. Near ¾ degree n. preceding φ, in a line parallel to the 21st and 8th Aurigæ. Pretty unequal. L. rw. S. bluish. Distance 25″ 29‴. Position 76° 0′ n. preceding.

102. FL. 59 Aurigæ.

Nov. 6, 1782. Double. The apex of an isosceles triangle. Very or extremely unequal. L. rw.; S. Distance 23″ 30‴. Position 50° 3′ s. preceding.

103. FL. 77am Draconis sequitur.

Nov. 13, 1782. Double. Near ¾ degree following the 77th Draconis, in a line parallel to κ Cephei and the 76th Draconis nearly; of a rectangular triangle the leg nearest the 77th. Very unequal. L. r.; S. bluish r. Distance 22″ 35‴. Position 45° 48′ n. following.

104. Inter γ et 55am Andromedæ.

Nov. 13, 1782. Double. A little more than 1 degree n. following the 55th Andromedæ, in a line parallel to β Trianguli and Algol. Considerably unequal. L. r.; S. d. Distance with 278, 18″ 57‴. Position 22° 33′ n. following.

105. δ Corvi. FL. 7. Duarum in ala sequente præcedens.

Nov. 13, 1782. Double. Extremely unequal. L. w.; S. r. Distance 23″ 30‴. Position 54° 0′ s. preceding.

106.

IV. 106. α (Fl. 50am) Ursæ majoris sequens ad boream.

Nov. 17, Double. About 1¾ degree n. following α, in a line
1782. parallel to β Ursæ et κ Draconis; the last of three in a row. Extremely unequal. Both r. Distance 18″ 55‴; very inaccurate. Position 44° 33′ f. following. A third small star in view.

107. Fl. 79a Pegasi australior et præcedens.

Nov. 20, Double. About ¾ degree f. preceding the 79th,
1781. towards γ Pegasi; at the center of a trefoil. Very unequal. L. r.; S. d. Distance with 278, 26″ 12‴. Position 50° 21′ n. following.

108. Fl. 69a Ursæ majoris australior.

Nov. 20, Double. Near 2 degrees f. of the 69th, towards
1782. the 63d Ursæ majoris. A very little unequal. Both r. Distance 19″ 15‴; very inaccurate. Position 10° 12′ n. following.

109. Fl. 62 Tauri.

Nov. 25, Double. Considerably unequal. L. w.; S. r. Dis-
1782. tance 28″ 5‴. Position 21° 12′ n. preceding.

110. β (Fl. 112a) Tauri borealior et sequens.

Dec. 24, Double. About 1¾ degree n. following β Tauri,
1782. towards θ Aurigæ; the second in that direction. Very unequal. L. r.; S. d. Distance 16″ 1‴. Position 74° 54′ n. preceding.

111. Fl. 54 Cancri.

Dec. 28, Double. A little unequal. Both rw. S. a little
1782. darker. Distance 17″ 14‴. Position 29° 0′ f. following.

112. γ (Fl. 15am) Crateris sequens ad boream.

Jan. 1, Double. About 1 degree n. following γ Crateris, in
1783. a line parallel to δ Corvi and Spica. Equal. Both pr. Distance

IV. Distance 26″ 15‴; too low for accuracy. Position 58° 42′ n. preceding or s. following.

113. Fl. 61ª Cygni borealior et præcedens.

Jan. 6, 1783. Double. About 1½ degree n. preceding the 61st, in a line parallel to υ and α Cygni. Very or extremely unequal. L. r.; S. db. Distance with 278, 17″ 30‴. Position 28° 24′ n. preceding. A third star in view.

114. τ (Fl. 12ª) Virginis australior.

Jan. 8, 1783. Double. About 1½ degree s. of τ Virginis. Very unequal. L. pr.; S. d. Distance 23″ 21‴. Position 15° 54′ n. preceding.

115. φ (Fl. 11ᵃᵐ) Herculis præcedens ad austrum.

Jan. 10, 1783. Double. About 2½ degrees s. of, and a little preceding φ, in a line parallel to φ and ζ Herculis; the largest of three or four. Extremely unequal. L. r.; S. b. Distance 20″ 54‴. Position 43° 48′ n. following.

116*. Fl. 83ᵃᵐ Pegasi sequens ad boream.

Jan. 13, 1783. Double. Equal. Both w. Distance 28″ 59‴. Position 68° 21′. Mr. C. MAYER, in 1777, settled its place ♃ 0ʰ. 52′ 53″ in time, and 20° 17′ 53″ in declination N.

117. Fl. 42ª Eridani australior.

Jan. 31, 1783. Double. About 1½ degree s. of the 42d Eridani, in a line parallel to Rigel and μ Leporis; the most south and following of three. Very unequal. L. r.; S. r. Distance 19″ 32‴. Position 31° 48′ s. preceding.

118. ι (Fl. 48ᵃᵐ) Cancri sequens.

Feb. 5, 1783. Double. Full ½ degree following the 48th, in a line parallel to δ Cancri and ε Leonis; a very small star, next to two more which are nearer to ι. A little unequal.

IV. qual. Distance 24″ 6‴. Position about 25° n. following.

119. ι (Fl. 68am) Virginis præcedens ad austrum.

Feb. 7, 1783. Double. About 1 degree s. preceding the 68th, in a line parallel to the 99th and α Virginis. Extremely unequal. Distance 21″ 49‴. Position 36° 54′ n. preceding.

120. Fl. 82am Piscium sequens ad boream.

Feb. 27, 1783. Double. About ¾ degree n. following the 82d Piscium, in a line parallel to α and β Trianguli; the largest of two. Considerably unequal. L. rw.; S pr. Distance 18″ 19‴. Position 21° 0′ s. preceding. A third star in view.

121. σ Scorpii Fl. 20 præcedens trium lucidarum in corpore.

Mar. 8, 1783. Double. Very unequal. L. whitish; S. r. Distance 21″ 40‴. Position 0° 0′ (or perhaps 1°) n. preceding.

122. Fl. 32a Ophiuchi borealior et præcedens.

Mar. 7, 1783. Double. Near 1 degree n. of, and a little preceding the 32d Ophiuchi, in a line parallel to α and η Herculis. Very unequal. Distance 21″ 3‴. Position 25° 3′ s. preceding.

123. Fl. 19 Ophiuchi.

Mar. 9, 1783. Double. The most south of two. Very unequal. L. pr.; S. d. Distance 20″ 27‴. Position 3° 9′ s. following.

124. ψ (Fl. 4am) Ophiuchi præcedens ad austrum.

Mar. 24, 1783. Double. About ¾ degree preceding and a little s. of ψ, in a line parallel to ψ Ophiuchi and ω Scorpii; in the base of a triangle, the nearest to ψ. A little unequal.

 Both

IV. Both inclining to r. Distance 15″ 24‴. Position 62° 54′ n. following.

125. FL. 29 Camelopardali.

April 2, 1783. Double. Very unequal. L. pr.; S. d. Distance 22″ 26‴; very inaccurate. Position 47° 36′. f. following; a little inaccurate.

126. λ (FL. 22ª) Cephei borealior et præcedens.

April 20, 1783. Double. Less than ½ degree n. preceding λ, in a line almost parallel to δ and ζ Cephei; a considerable star. A little unequal. Both dw. Distance 18″ 50‴. Position 45° 39′ n. preceding.

127 †. λ (FL. 16ᵃᵐ) Aquilæ sequens ad boream.

May 21, 1783. Double. About 2½ degrees n. following the farthest of two which are about 1½ degree from λ, in a line parallel to λ and δ Aquilæ. Very unequal. L. rw.; S. dr. Distance 17″ 14‴; more exact with 932, 15″ 52‴. Position 69° 54′. n. preceding. Mr. PIGOTT, who favoured me with it, gives its place Æ 18ʰ 52′½ ±, Declination 1° 0′ S.

128. γ (FL. 57ᵃᵐ) Andromedæ præcedens ad austrum.

July 28, 1783. Double. About 1⅓ degree f. preceding γ almost towards β Andromedæ; more exact towards σ Piscium; one not in a row of stars which are near that place. Considerably unequal. L. pr.; S. dr. Distance 15″ 42‴. Position 24° 12′ n. following.

129. FL. 59 Andromedæ.

July 28, 1783. Double. A little unequal. L. rw.; S. pr. Distance 15″ 15‴. Position 55° 9′ n. following. A third star in view about 58° or 60° f. preceding.

IV. 130. η (Fl. 99[i]) Piſcium borealior et ſequens.

Aug. 2, 1783. Double. About 1½ degree n. of, and a little following η Piſcium, in a line parallel to β Arietis and β Trianguli; the laſt of four in a crooked row. Very unequal. L. r.; S. darker r. Diſtance with 278, 15″ 49‴. Poſition 62° 15′ n. following.

131. Fl. 100 Piſcium.

Aug. 2, 1783. Double. Pretty unequal. L. pr.; S. r. Diſtance 15″ 52‴. Poſition 5° 0′ n. following.

132. Fl. 46[am] Aquilæ ſequens ad boream.

Aug. 6, 1783. Double. About ½ degree n. following 46 Aquilæ, in a line parallel to α and γ Sagittæ. Very unequal. L. r.; S. db. Diſtance 22″ 44‴. Poſition 41° 24′ n. preceding.

FIFTH CLASS OF DOUBLE STARS.

V. 52. Secunda a ν Geminorum μ verſus.

Dec. 27, 1781. Double. The ſecond ſtar from ν towards μ Geminorum. Pretty unequal. L. r.; S. b. Diſtance 35″; inaccurate.

53. ρ Geminorum. Fl. 63. In inguine ſequentis II[i].

Dec. 27, 1781. Double. The brighteſt of two. Extremely unequal. L. pr.; S. d. Diſtance 44″ 15‴.

54. θ Hydræ. Fl. 22. Duarum in eductione cervicis ſequens.

Jan. 20, 1782. Double. Exceſſively unequal. L. w.; S. a point. Diſtance near 1 minute, too obſcure for meaſures, and

V. not visible till after having looked a good while at θ. Position about 75° s. following.

55. Ad Fl. 12am Geminorum. In pede II^i præcedentis sinistro.

Jan. 30, Treble. A small star near the place of the 12th
1782. Geminorum. The two nearest a little unequal. Distance less than 1′.

56. Fl. 15 Geminorum. Dextrum prioris II^i pedem attingens.

Jan. 30, Double. Considerably or very unequal. L. r.; S. d.
1782. Distance 32″ 39‴. Position near 60° s. preceding.

57. Fl. 9a Orionis borealior et sequens. In exuviarum summo.

Feb. 4, Treble. More than 1 degree n. following the 9th
1782. Orionis, towards the 113th Tauri; the largest of two. The two nearest considerably unequal. L. rw.; S. rw. Distance with 278, 36″ 26‴. Position 33° 36′. The farthest very unequal. S. r. Distance Vth Class. Position following.

58. Fl. 7 Leonis. Supra pedem borealem anteriorem.

Feb. 4, Double. Very unequal. L. rw.; S. r. Distance
1782. 42″ 25‴. Position 8° 36′ n. following.

59. θ Cancri. Fl. 31. In quadrilatero circa Nubem.

Feb. 6, Double. Extremely unequal. L. r.; S. d. Dis-
1782. tance 44″ 52‴. Position n. following.

60. o (Fl. 95am) Leonis præcedens; ad caudam.

Feb. 9, Double. Near ¾ degree s. preceding the 95th, in a
1782. line parallel to β and ρ Leonis. Very unequal. L. rw.; S. d. Distance 37″ 15‴. Position 70° 48′ n. following.

61. Fl. 81 Leonis. In clune.

Feb. 9, Double. Extremely unequal. L. rw.; S. r. Dis-
1782. tance 57″ 23‴. Position

V. 62. Fl. 57 Leonis. E posteriores pedes præcedentibus.

Feb. 11, 1782. Double. Very unequal. Distance 33″ 16‴.

63. Fl. 25 Leonis. In infimo pectore.

Feb. 17, Double. The largest of two. Extremely unequal.
1782. L. pr.; S. d. Distance 52″ 46‴. Position

64. Fl. 43ª Leonis australior. Ad sinistrum anteriorem cubitum.

Feb. 17, Double. Near 1 degree f. of the 43d, in a line pa-
1782. rallel to ψ and α Leonis. Extremely unequal. L. w.
inclining to r.; S. db. Distance 59″ 40‴. Position

65. Secunda ad π Canis majoris. Fl. 17. In pectore.*

Mar. 3, Treble. The two nearest very unequal. L. rw.;
1782. S. r. Distance 44″ 52‴. Position 64° 12′ f. following.
The two farthest very or extremely unequal. S. r.
Distance Vth Class. Position about 85° f. preceding.
The three stars form a rectangle, the hypotenuse of
which contains the largest and smallest.

66. *p* (Fl. 63ª) Geminorum borealior.

Mar. 3, Double. About ¾ degree n. of, and a little pre-
1782. ceding *p*, in a line parallel to υ and α Geminorum.
Very unequal. L. pr.; S. d. Distance 34″ 39‴.
Position 1° or 2° n. preceding.

67. Pollucem prope. In capite sequentis II[i].

Mar. 3, Double. Near 1 degree n. following β, in a line
1782. from δ continued through β Geminorum nearly; the
farthest and smallest of three. Considerably unequal.
L. r.; S. dr. Distance 47″ 37‴.

68. Fl. 75ᵃᵐ Leonis præcedens ad boream.

Mar. 5, Treble. One of two n. preceding the 75th, in a
1782. line parallel to the 84th and 59th Leonis. The two

V. nearest very unequal. Distance 54″ 37‴. The farthest extremely unequal.

69. FL. 7 Leonis minoris. In extremo anteriore pede.

Mar. 12, 1782. Double. The largest of two. Extremely unequal. L. pr.; S. r. Distance 58″ 18‴.

70. FL. 2nd Bootis præcedens ad boream.

April 5, 1782. Double Near 3 degrees n. preceding the 2d Bootis, towards the 43d Comæ Ber.; the preceding of three in a line parallel to α and η Bootis. A little unequal. L. r.; S. darker r. Distance 56″ 56‴. Position 7° 0′ f. preceding.

71. Prope γ (FL. 24^{am}) Geminorum.

April 15, 1782. Double. Three or four minutes n. preceding γ Geminorum. Of the Vth Class. More in view.

72. † *m* Herculis. FL. 36 et 37. In sinistro Serpentarii brachio.

May 18, 1782. Double. A little unequal. L. bluish w. S. reddish w. Distance 59″ 59‴. Position 36° 57′ f. preceding *.

73. r Ursæ majoris. FL. 14. Duarum in collo præcedens.

June 11, 1782. Double. Extremely unequal. L. w.; S. d. Distance 54″ 46‴. Position about 45° n. following.

74. S (FL. 72^{d}) Serpentarii borealior.

June 16, 1782. Double. More than 1 degree n. following the 56th double star of the IIId Class; nearly in a line parallel to the 62d and 72d Serpentarii, Very unequal. L. rw.; S. r. Distance 40″ 54‴. Position 39° 15′; inaccurate.

* One of these stars, at least, seems to have changed its place since the time of FLAMSTEED, who makes their difference in R.A. 45″, and in P.D. 1′ 35″, Position f. preceding; hence we have the hypotenuse or distance above 1′ 45″, instead of 59″ 59‴, and position 69° 46′ instead of 36° 57′.

V. 75. E telescopicis ε Coronæ borealis sequentibus.

July 18, 1782. Double. About 1 degree s. following ε, in a line parallel to θ and ε Coronæ; the preceding of three forming an arch. Extremely unequal. L. r.; S. darker r. Distance 41″ 12‴. Position 16° 0′ s. following.

76. β Aquarii. Fl. 22. In sinistro humero.

July 20, 1782. Double. Excessively unequal. L. w.; S. d. Distance about 33″ 16‴; very inaccurate. Position 55° 48′.

77. *d* (Fl. 43ᵃ) Sagittarii borealior et sequens.

Aug. 4, 1782. Double. A few minutes n. following the 43d, in a line parallel to *o* and π Sagittarii; the nearest of two. Extremely unequal. L. w.; S. d. Distance with 278, 36″ 3‴. Position 78° 45′ s. following.

78. ζ Sagittarii. Fl. 38. Trium super costis sub axilla.

Aug. 4, 1782. Double. Extremely unequal. L. r.; S. d. Distance Vth Class. Position 28° 6′ n. preceding. A third star. Distance about four times as far as the former. Position also n. preceding.

79. Fl. 9 :: Cassiopeiæ.

Aug. 25, 1782. Double. Of two in a line parallel to β and γ, that towards γ Cassiopeiæ. Very unequal. L. w.; S. pr. Distance 52″ 39‴. Position 50° 36′ n. preceding.

80. τ Aquarii. Fl. 69. Duarum in dextra tibia borealior.

Aug. 28, 1782. Double. Very unequal. L. rw.; S. d. Distance 36″ 47‴. Position 19° 54′ s. following.

81. Fl. 35 :: Cassiopeiæ. In sinistro crure.

Aug. 28, 1782. Double. Considerably unequal. L. rw.; S. br. Distance 42″ 35‴. Position 85° 12′ n. following.

82.

V. 82. ν (FL. 25am) Cassiopeiæ præcedens. In sinistra manu.
Aug. 28, Double. Near ¼ degree n, preceding ν, in a line pa-
1783. rallel to α and β Cassiopeiæ. Nearly equal. Both pr.
Distance 43″ 26‴. Position 7° 48′ n. following.

83. ψ Cassiopeiæ. FL. 36. Sub pede sinistro.
Aug. 28, Double. Very unequal. L. pr.; S. r. Distance
1782. 33″ 25‴. Position 10° 12′ s. following.

84. FL. 47 :: Cassippeiæ. Ex obscurioribus infra pedes.
Aug. 29, Double. The largest of three forming a rectangular
1782. triangle on, or near, the place of the 47th Cassiopeiæ. A
little unequal. L. rw.; S. pr. Distance 50″ 58‴.
Position 3° 33′ n. preceding.

85. ρ (FL. 27a) borealior et præcedens. In dextro brachio.
Aug. 29, Double. About ⅓ degree n. preceding ρ Andromedæ
1782. θ versus. Very unequal. L. rw.; S. r. Distance 30″
57‴. Position 79° 24′ n. following.

86. FL. 12 Ursæ minoris.
Sept. 4, Treble. Extremely unequal. All three r. The
1782. nearest is the smallest, Position some degrees s. follow-
ing. The farthest also south, but more following.

87. σ Capricorni. FL. 7. Sub oculo dextro.
Sept. 5, Double. Very, or almost extremely unequal. L. r.;
1782. S. d. bluish. Distance 50″ 7‴. Position 85° 12′ s.
following.

88. λ (FL. 15a) Aurigæ borealior. In sinistra manu.
Sept. 5, Double. About 3′ or 4′ n. following the 15th Au-
1782. rigæ. Very unequal. Distance 34″ 15‴, mean mea-
sure. Position 54° 6′ s. preceding.

89.

V. 89. θ Aurigæ. Fl. 37. In dextro carpo.

Sept. 5, 1782. Double. Excessively unequal. L. fine w.; S reddish. Distance with 460, 35″ 18‴, narrow measure. Position 16° 0′ n. preceding. A third star in view.

90. ν Aurigæ. Fl. 32. In dextri brachii ancone.

Sept. 5, 1782. Double. Excessively unequal. L. orange w.; S. r. Distance 53″ 43‴. Position 61° 48′ f. preceding. S. not visible till after some minutes attention.

91. β (Fl. 34ᵉ) Aurigæ adjecta. In dextro humero.

Sept. 5, 1782. Double. Near ½ degree f. following β, in a line from the 27th continued through β Aurigæ; a considerable star. Very or extremely unequal. L. pr.; S. d. Distance 30″ 3‴. Position 45° 6′ n. preceding.

92. Fl. 3ᵃ Arietis borealior.

Sept. 10, 1782. Double. Full ½ degree f. following the 3d Arietis, in a line parallel to α Arietis and δ Ceti; the most south of two. Equal. Both reddish. Distance 51″ 16‴. Position 52° 45′ n. preceding or f. following.

93. Fl. 103ᵃᵐ Herculis sequens ad austrum.

Sept. 19, 1782. Double. About 1¼ degree f. following the 103d Herculis, in a line parallel to the 1st and 10th Lyræ; the nearest of two. Equal, perhaps the following the smallest. Both r. Distance 47″ 46‴. Position 45° 42′ f. following.

94. Duarum Fl. 31ᵃᵐ Cephei sequentium austrina.

Sept. 30, 1782. Double. About ¾ degree n. of the 31st Cephei, towards α Polaris. Pretty unequal. Both pr. Distance 41″ 40‴. Position 45° 15′ f. following.

95. Fl. 51 Aquarii. In dextro cubito.

Oct. 2, 1782. Double. Excessively unequal. L. rw.; S. d. Distance Vth Class. Position n. preceding. Two other

V. other ſtars in view; the neareſt of them extremely unequal. Poſition about 80 or 90° ſ. preceding. The fartheſt very unequal. Poſition about 30° ſ. following.

96. *υ* (FL. 59am) Aquarii ſequens ad auſtrum.

Oct. 2, 1782. Double. About ½ degree ſ. following *υ*, in a line parallel to *δ* and *c* Aquarii. Extremely unequal. Diſtance Vth Claſs near. Poſition 15 or 20° ſ. preceding.

97. FL. 10 Lacertæ.

Oct. 4, 1782. Double. Very unequal. L. w.; S. r. Diſtance with 278, 52″ 34‴. Poſition 38° 45′ n. following.

98. FL. 3 Pegaſi.

Oct. 4, 1782. Double. Pretty unequal. L. wr.; S. dr. Diſtance 34″ 43‴. Poſition 82° 48′ n. preceding. Beſides II. 62. another ſtar in view. Poſition following.

99. FL. 33 Pegaſi.

Oct. 4, 1782. Double. Conſiderably unequal. L. pr.; S. r. Diſtance with 278, 45″ 3‴. Poſition 89° 12′ n. following.

100. FL. 59 Orionis.

Oct. 4, 1782. Double. The following of two. Extremely unequal. L. w.; S. a point requiring ſome attention to be ſeen. Diſtance 37″ 15‴. Poſition about 65° ſ. preceding.

101. *υ* (FL. 36am) Orionis præcedens.

Oct. 4, 1782. Double. About ¾ degree preceding *υ*, nearly in a line parallel to *κ* and *β* Orionis; the ſecond from *υ*. Extremely unequal. L. w.; S. r. Diſtance 44″ 15‴. Poſition about 15° ſ. following.

102.

V. 102. Fl. 61 Ceti.

Oct. 12, 1782. Double. Extremely unequal. L. rw.; S. dr. Distance with 278, 37″ 53‴. Position 76° 21′ f. preceding. A third star at some distance. A little unequal. Position n. following.

103. Ab ι (Fl. 18ª) Lyræ β versus.

Oct. 24, 1782. Double. Full ⅓ degree f. preceding ι, nearly towards β Lyræ. Extremely unequal. L. w.; S. r. Distance with 278, 45″ 32‴. Position 29° 12′ n. following.

104. ε (Fl. 4ª) Sagittæ australior et præcedens.

Nov. 6, 1782. Double. Full ½ degree f. preceding ε, in a line parallel to γ Sagittæ and γ Aquilæ; the nearest of two. Extremely unequal. L. pr.; S. d. Distance Vth Class. Position 16° 18′ f. following.

105. γ (Fl. 14ª) Sagittæ australior et sequens.

Nov. 6, 1782. Double. About ⅓ degree f. following γ Sagittæ, in a line parallel to Sagitta and Delphinus. Considerably unequal. L. pr.; S. r. Distance 38″ 36‴. Position 74° 15′ f. following.

106. γ (Fl. 12ª) Sagittæ borealior et præcedens.

Nov. 6, 1782. Double. About 1¾ degree n. preceding γ Sagittæ, towards the 6th Vulpeculæ; a considerable star. Equal. Both rw. Distance 38″ 54‴. Position 60° 42′ n. preceding or f. following.

107. Fl. 56 Aurigæ.

Nov. 6, 1782. Double. Considerably unequal. L. w.; S. pr. Distance 52″ 57‴. Position 72° 36′ n. following.

108. κ (Fl. 13ª) Canis majoris borealior.

Nov. 6, 1782. Double. About ¾ degree n. of κ Canis majoris. A little unequal. L. dw.; S. d. Distance 42″ 53‴. Position 23° 18′ n. following.

V. 109. Inter β Cancri et δ Hydræ.

Nov. 6, 1782. Double. A large ſtar not in FLAMSTEED, between β Cancri and δ Hydræ. Exceſſively unequal. Diſtance 35″ 24‴. Poſition 55° 0′ n. preceding.

110. FL. 111 Tauri.

Nov. 13, 1782. Double. Very unequal. L. rw.; S. r. Diſtance 46″ 42‴. Poſition 3° 48′ n. preceding.

111. FL. 42ᵈ Urſæ majoris auſtralior et ſequens.

Nov. 20, 1782. Double. Full 1 degree ſ. following the 42d, in a line parallel to the 29th and 48th Urſæ majoris; the middle of three forming an arch. Conſiderably unequal. L. wr.; S. r. Diſtance 30″ 40‴. Poſition 51° 27′ n. following.

112. * Ex obſcurioribus μ and ν Geminorum ſequentibus.

Dec. 1, 1782. Double. Forms almoſt an iſoſceles triangle with μ and ν Geminorum. Nearly equal. The preceding pr. the following wr. Diſtance Vth Claſs far.

113. * FL. 9ᵃᵐ inter et 11ᵃᵐ Orionis,

Dec. 7, 1782. Treble. About 1½ degree ſ. preceding the 11th Orionis, towards ι Tauri. The two largeſt conſiderably unequal. L. w.; S. pr. Diſtance 37″ 51‴. Poſition 33° 54′ n. preceding. The third farther off and ſmaller. S. r. Poſition n. following.

114. FL. 103 Tauri.

Dec. 7, 1782. Double. Exceſſively unequal. L. rw.; S. d. Diſtance with 278 and 625, 30″ 2‴, mean meaſure. Poſition 72° 24′.

115. ο Tauri. FL. 114.

Dec. 7, 1782. Double. Exceſſively unequal. L. w.; S. a point. Diſtance 5[illegible]″ 34‴. Poſition 77° 54′ ſ. preceding.

V. Two other small stars following, and a third to the north.

116. Fl. 41 Arietis.

Dec. 23, 1782. Treble. The two nearest excessively unequal. L. w.; S. a point. Distance with 278, 39″ 20‴. Position 80° 48′ f. preceding. For the distance of the farthest, see VI. 5. *.

117. ζ (Fl. 58am) Arietis præcedens ad boream.

Dec. 23, 1782. Double. About 1¾ n. preceding ζ, towards the 41st Arietis; the following of four forming an arch. Very unequal. Both dr. Distance 34″ 48‴. Position 47° 33′ n. preceding.

118. ε (Fl. 46a) Orionis borealior et præcedens.

Dec. 28, 1782. Double. The most n. of three preceding ε Orionis, towards μ Tauri. More north is another set of three; care must be taken not to mistake one of them for this. Extremely unequal. L. rw.; S. d. Distance Vth Class. Position 13° 6′ f. preceding. Two more following, excessively unequal; one about 1′, the other about 1½ minute.

119. ε (Fl. 46a) Orionis australior et præcedens.

Dec. 28, 1782. Double. Full ¾ degree f. preceding ε, in a line parallel to ε Orionis, and h Eridani; the smallest and most f. of two. Very unequal. L. w.; S. r. Distance 30″ 12‴; a little inaccurate. Position 21° 33 f preceding. A third star 2 or 3° f. following.

* The star VI. 5. in the place referred to is called Flamsteed's 35th Arietis. With so many stars and measures it was hardly possible to avoid several errors, I have therefore now added to the errata already given at the end of vol. LXXII. and LXXIII. of the Phil. Trans. some others, that have since been detected by a careful review of the double stars, and believe that no more will be found.

V. 120. FL. 15 Hydræ.

Dec. 28, 1782. Double. Extremely unequal. L. w.; S. r. Distance 43″ 2‴. Position about 70° n. preceding.

121. *e* Comæ Berenices. FL. 12.

Jan. 1, 1783. Double. Considerably unequal. L. rw.; S. pr. Distance 58″ 55‴. Position about 77° f. following.

122. FL. 44ª Bootis australior et præcedens.

Jan. 8, 1783. Double. Near ⅔ degree f. preceding the 44th, towards the 38th Bootis. Very unequal. L. bw.; S. pr. Distance 34″ 21‴. Position 67° 6′ f. preceding.

123. * In Andromedæ pectore.

Jan. 8, 1783. Double. Equal. Both rw. or pr. Distance 45″ 1‴. Position 32° 24′ f. preceding. Its place, as determined in 1777 by C. MAYER, is AR 0^h 34′ 33″ in time, and 29° 45′ 3″ declination north.

124. *g* (FL. 2ᵃᵐ) Centauri sequens ad austrum.

Jan. 31, 1783. Double. About 1½ degree f. following *g* Centauri, in a line parallel to *γ* Serpentis and *θ* Centauri; the most f. of two. Considerably unequal. Distance 54″ 1‴; too low for accuracy.

125. FL. 46ᵃᵐ Bootis sequens ad boream.

Feb. 3, 1783. Double. Near 2 degrees n. following the 46th, in a line parallel to *ζ* Bootis and *β* Coronæ; the third star about that direction. Considerably unequal. L. r.; S. darker r. Distance 33″ 53′. Position 37° 33′ f. preceding.

126. *r* (FL. 5ᵃᵐ) Herculis præcedens ad austrum.

Feb. 3, 1783. Double. Near ½ degree f. preceding *r* Herculis, in a line parallel to *γ* and *δ* Serpentis; a small star. A little unequal. Both pr. Distance 37″ 51‴, rather full measure. Position 52° 6′ f. preceding.

127.

V. 127. (Fl. 41am) Herculis præcedens ad boream.

Feb. 5, 1783. Double. About ¾ degree n. preceding the 41st Herculis, in a line parallel to κ Serpentarii and β Herculis. Pretty unequal. Both r. Distance 48″ 40‴. Position 19° 45′ n. preceding.

128. ι (Fl. 68am) Virginis sequens.

Feb. 7, 1783. Double. About 1½ degree following ι Virginis, in a line parallel to Spica and β Libræ. A little unequal. L. pr.; S. r. Distance 41″ 58‴.

129. f (Fl. 25am) Virginis sequens ad boream.

Feb. 7, 1783. Double. About 1½ degree n. following f, in a line parallel to γ and ε Virginis; a large star. Very unequal. L. r.; S. dark r. Distance 46″ 42‴. Position 6 or 7° s. following. A double star of the Vth Class in view, preceding.

130. Fl. 35 Comæ Berenices.

Feb. 26, 1783. Double. Very unequal. L. r.; S. d. Distance 31″ 17‴. Position 36° 51′ s. following.

131. Fl. 24am Libræ sequens ad boream.

Mar. 1, 1783. Double. About 1½ degree n. following the 24th Libræ, in a line parallel to π and β Scorpii. Considerably unequal. L. rw.; S. r. Distance 47″ 46‴.

132. Fl. 29am inter et 30am Libræ.

Mar. 1, 1783. Double. Of two between the 29th and 30th Libræ that nearest to the 30th. Very unequal. L. w.; S. d. Distance 39″ 59‴; very inaccurate.

133. Fl. 60 Herculis.

Mar. 7, 1783. Double. Extremely unequal. L. w.; S. d. Distance 48″ 40‴. Position 37° 0′ n. preceding.

134.

V. 134. ψ (FL. 4am) Ophiuchi præcedens ad auſtrum.

Mar. 24, 1783. Double. About 1 degree preceding and a little ſ. of ψ, in a line parallel to ψ Ophiuchi and ω Scorpii; the fartheſt of two in the baſe of a triangle. Equal. Diſtance 45″ 47‴.

135. Ad FL. 49am Camelopardali.

April 4, 1783. Double. The ſmalleſt and moſt ſ. of two that are about 20′ aſunder. A little unequal. Both r. Diſtance with 278, 38″ 18‴. Poſition 85° 0′ ſ. preceding.

136. θ (FL. 65a) Aquilæ borealior.

Sept. 12, 1783. Double. About ⅔ degree n. of θ, in a line parallel to η and β Aquilæ; a conſiderable ſtar. Conſiderably unequal. L. pr.; S. r.; Diſtance with 278, 47″ 5‴. Poſition 65° 48′ ſ. preceding.

137. χ (FL. 17a) Cygni borealior.

Sept. 22, 1783. Double. About 1½ degree n. of χ, towards δ Cygni; a conſiderable ſtar. Conſiderably unequal. L. garnet; S. r. Diſtance with 278, 35″ 1‴. Poſition 57° 3′ n. following.

SIXTH CLASS OF DOUBLE STARS.

VI. 67. η Orionis. FL. 28. In extremo enſis manubrio.

Dec. 27, 1781. Double. Exceſſively unequal. L. w.; S. d. Diſtance 1′ 50″ 57‴. Poſition 35° 12′ n. following.

68.

VI. 68. η (Fl. 28ª) Orionis auſtralior.

Dec. 27, 1781. Double. About ¼ degree ſ. of, and a little following η, in a line nearly parallel to δ and θ Orionis. Very unequal. L. r.; S. d. Diſtance 2′ 0″ 11‴. Poſition 7° 54′ n. preceding.

69. Fl. 14 Arietis. Supra caput.

Dec. 27, 1781. Double. Very unequal. L. pr.; S. dr. Diſtance 1′ 29″ 28‴. Poſition 11° 12′ n. preceding.

70. *o* Geminorum. Fl. 70. Supra caput prioris II[i].

Dec. 27, 1781. Treble. Or two ſmall ſtars in view; the neareſt a little more than 1 minute; the other not much farther.

71. τ Hydræ. Fl. 31. Trium in flexu colli auſtraliſſima.

Jan. 20, 1782. Double. Pretty unequal. L. w. inclining to roſe colour. S. pr. Diſtance 1′ 1″ 40‴. Poſition 88° 36′ n. preceding.

72. Ad Fl. 68ᵃᵐ Orionis. In fuſte.

Jan. 30, 1782. Double. The moſt n. of two that are 1 degree aſunder. Very unequal. L. w.; S. dr. Diſtance with 278, 1′ 12″ 50‴. Poſition 41° 0′ ſ. preceding.

73. ε Geminorum. Fl. 27. In boreali genu præcedentis II[i].

Feb. 2, 1782. Double. L. w. Diſtance 1′ 50″ 30‴.

74. Fl. 51 Geminorum.

Feb. 2, 1782. Has two very obſcure ſtars in view. L. r.; S. r. S. r. The neareſt about 1½, the next 2 minutes. Poſition of both about 40 or 50° n. following.

75. ω Cancri. Fl. 4. Ad primum borealem forficem.

Feb. 2, 1782. Has a very obſcure ſtar in view. L. pr. Diſtance about 1¼ minute. Poſition about 30° n. preceding. A third about 2′. Poſition more north.

76.

VI. 76. *o* Leonis. FL. 14.

Feb. 2, 1782. Double. Extremely unequal. L. rw.; S. r. Distance 1′ 3″ 29‴. Position 49° 36′ n. following.

77. *τ* Virginis. FL. 93.

Feb. 4, 1782. Double. Very unequal. L. w.; S. dr. Distance 1′ 8″ 22‴.

78. *ζ* (FL. 16am) Cancri sequitur.

Feb. 8, 1782. Double. About ½ degree following *ζ* Cancri, towards *η* Leonis. Extremely unequal. Distance 1′ 3″ 47‴.

79. *φ* Leonis. FL. 74,

Feb. 9, 1782. Double. Very unequal. L. w.; S. pr. Distance 1′ 38″ 35‴. Position about 10 or 12° n. preceding.

80. FL. 93 Leonis.

Feb. 9, 1782. Double. Very unequal. L. w.; S. db. Distance 1′ 10″ 13″.

81. FL. 27 Virginis. In ala dextra.

Feb. 9, 1782. Double. Extremely unequal. L. w. Distance 1′ 28″ 48‴.

82. FL. 31 Monocerotis. In media cauda.

Feb. 9, 1782. Double. Very unequal. L. rw.; S. db. Distance 1′ 10″ 13‴. Position 40° 0′ n. preceding.

83. Prope FL. 1am Orionis.

Feb. 9, 1782. Double. A few minutes s. following the 1st, towards the belt of Orion. Considerably unequal. L. pr.; S. r. Distance 1′ 20″ 58‴. Position 88° 15′ n. following.

84. FL. 14 Canis minoris.

Feb. 9, 1782. Treble. The nearest extremely unequal. L. rw.; S. d. Distance 1′ 5″ 28‴. Position 26° 24′ n. following.

VI. lowing. The third forms an angle, a little larger than a rectangle, with the other two. Position s. following.

85. Fl. 27 Hydræ.

Feb. 9. 1782. Double. Very unequal. L. rw.; S. pr. Distance VIth Class far. Position about 60° s. preceding.

86. Prima ad σ Cancri. Fl. 51.

March 5, 1782. Double. Extremely unequal. L. w.; S. d. Position n. following.

87. Tertia ad σ Cancri. Fl. 64.

March 5, 1782. Double. Very unequal. L. rw.; S. dr. Distance 1′ 25″ 45‴. Position 25° 12′ n. preceding.

88. β Aurigæ. Fl. 34. In dextro humero.

March 5, 1782. Double. Extremely or excessively unequal. L. fine bluish w.; S. d. Distance 2′ 49″ 6‴. Position 54° 12′ n. following. A third farther off, Very unequal. About 40 or 50° n. following.

89. Fl. 6ª Bootis adjecta.

Mar. 12, 1782. Double. Just following the 6th Bootis. A little unequal. L. r.; S. deeper r. Distance 1′ 19″ 39‴. Position 58° 6′ s. preceding.

90. Fl. 61 Virginis.

Apr. 3, 1782. Double. Very unequal. L. w.; S. d. Distance 1′ 13″ 15‴. Position about 75° n. preceding.

91. Prope γ (Fl. 24ᵃᵐ) Geminorum.

Apr. 15, 1782. Double. Three or four minutes n. of γ Geminorum. Considerably unequal. Both small; too obscure for measures with 7-feet; my 20-feet shews a third star between them with 12 inches aperture.

VI. 92. ξ (FL. 1[i]) Capricorni borealior.

June 14, 1782. Double. About ½ degree n. of ξ Capricorni. Very unequal. Both r. Distance 1′ 2″ 16‴. Position 2° 3′ f. preceding.

93. ρ Coronæ borealis. FL. 15. Ad summum.

July 18, 1782. Double. Very unequal. L. w.; S. d. Distance 1′ 27″ 44‴; a little inaccurate. Position 54° 27′ f. following.

94. λ Coronæ borealis. FL. 12.

July 18, 1782. Double. Extremely unequal. L. w.; S. r. Distance 1′ 35″ 14‴. Position 33° 12′ n. following.

95. η Bootis. FL. 8. Trium in sinistro crure borea.

Aug. 3, 1782. Double. Extremely unequal. L. w. inclining to orange; S. r. Distance about 1½ minute. Position about 25 or 30° f. following.

96. ζ Persei. FL. 44. In pede sinistro.

Aug. 25, 1782. Treble. The nearest extremely unequal. L. w.; S. r. Distance 1′ 11″ 26‴. Position 66° 36′ f. preceding. The farthest very unequal. S. r. about 1½ minute. 70 or 75° f. preceding.

97. Secunda ad τ Aquarii. FL. 71. In dextro crure.

Aug. 28, 1782. Double. Very unequal. L. r.; S. d. Distance 2′ 3″ 36‴, mean measure. Position 18° 30′ n. preceding.

98. FL. 46[am] Tauri sequens ad austrum.

Sept. 7, 1782. Double. About 1½ degree f. following the 46th, nearly in a line parallel to the 38th Tauri and the 42d Eridani. A little unequal. L. pr.; S. r. Distance 1′ 2″ 34‴. Position 43° 48′ n. preceding. A double star of the Vth Class in view, following within 3′. Equal.

VI. Equal. Both ſmall and r. Almoſt ſimilarly ſituated with the above, but poſition more n. preceding.

99. *m* Perſei. Fl. 57. In dextri pedis talo.

Sept. 7, 1782. Double. Pretty unequal. L. r.; S. rw. Diſtance 1′ 36″ 27‴. Poſition 71° 51′ ſ. preceding.

100. *ι* (Fl. 32am) Cephei ſequens.

Sept. 30, 1782. Double. About 1¼ degree n. following *ι* nearly towards *γ* Cephei. A little unequal. Both pr. Diſtance 1′ 1″ 54‴. Poſition 8° 9′ n. preceding.

101. *δ* Tauri. Fl. 68.

Oct. 31, 1782. Has two ſtars in view. The neareſt exceſſively unequal. L. w.; S. d. Diſtance with 2,8, 1′ 3″ 18‴. Poſition 35° 24′ ſ. preceding. The fartheſt extremely unequal. S. r. About 1½ minute. Poſition about 50° n. preceding.

102. Fl. 5 Lyncis.

Nov. 13, 1782. Double. The largeſt of a ſmall triangle. Very unequal. L. r.; S. garnet. Diſtance 1′ 28″ 20‴. Poſition 2° 0′ n. preceding.

103. *ε* Pegaſi. Fl. 8.

Nov. 20, 1782. Double. Very unequal. L. pr.; S. dr. Diſtance 1′ 30″ 56‴. Poſition 52° 45′ n. preceding.

104. *ζ* Bootis. Fl. 30. In dextro calcaneo.

Nov. 29, 1782. Has a very obſcure ſtar in view. Extremely unequal. L. w. inclining to r.; S. d. Diſtance about 1½ minute. Poſition almoſt directly preceding.

105. Fl. 105 Tauri.

Dec. 7, 1782. Double. Very unequal. L. pr.; S. r. Diſtance 1′ 41″ 29‴. Poſition 18° 0′ ſ. preceding.

VI. 106. *b* Eridani. FL. 62.

Dec. 7, 1782. Double. Considerably unequal. L. w.; S. pr. Distance 1′ 0″ 26‴. Position 15° 9′ n. following.

107. FL. 31ª Monocerotis australior et præcedens.

Dec. 21, 1782. Double. About 1¼ degree s. of, and a little preceding the 31st Monocerotis, in a line parallel to ζ Hydræ and the 31st Monocerotis; the most south of two. Considerably unequal. L. r.; S. deeper r. Distance about 1½ minute. Position 50 or 60° s. following.

108. θ (FL. 22ª) Hydræ borealior et præcedens.

Dec. 28, 1782. Double. About ½ degree n. of, and a little preceding θ, nearly in a line parallel to α and θ Hydræ. Very unequal. L. r.; S. blackish r. VIth Class far. Position 1 or 2° n. preceding. A third star preceding.

109. FL. 22 an 26 Cancri incertum.

Dec. 29, 1782. Double. One of the two being lost *, it does not appear which is the remaining star. Very unequal. L. r.; S. dr.

110. Telescopica ad *o* Ceti.

Jan. 2, 1783. Double. Looking for *o* Ceti, which was invisible to the naked eye, I mistook this for it. Pretty unequal. L. rw. of about the eighth magnitude; S. r. Distance 1′ 20″ 52‴. Position 33° 42′.

111. α Hydræ. FL. 30. Duarum contiguarum lucidior.

Jan. 8, 1783. Has two stars within about 2 minutes; the nearest excessively unequal; the farthest extremely unequal. Both s. following.

112. FL. 13 Bootis.

Jan. 8, 1783. Double. Extremely unequal. L. r.; S. dr. Distance 1′ 17″ 58‴. Position 7° 24′ n. preceding.

* See Phil. Trans. vol. LXXIII. p. 252.

113.

VI. 113. FL. 4 Virginis.

Jan. 8, 1783. Double. Extremely unequal. L. wr.; S. dr. Distance 2′ 25″ 44‴; too obscure for accuracy.

114. FL. 69am Orionis præcedens ad austrum.

Jan. 9, 1783. Double. About ¾ degree s. preceding the 69th, nearly towards λ Orionis. Considerably unequal. L. pr.; S. d. Distance 1′ 30″ 38‴. Position 22° 6′ s. following.

115. FL. 21ap Crateris sequens ad austrum.

Jan. 10, 1783. Double. About 2½ degree s. following the 21st, in a line parallel to the 12th Crateris and 4th Corvi. Very unequal. L. w.; S. r. Position 12° 12′ n. following.

116. FL. 43 Herculis.

Jan. 10, 1783. Double. Very unequal. L. inclining to garnet; S. r. Distance 1′ 14″ 37‴. Position 38° 48′ s. preceding.

117. FL. 12a Libræ borealior et præcedens.

Jan. 10, 1783. Double. About 1¼ degree n. preceding the 12th Libræ, towards Spica. Very unequal. L. rw.; S. r. Position about 40° s. preceding.

118. FL. 30 Monocerotis.

Feb. 12, 1783. Double. Very or extremely unequal. Distance 3′ 30″ 54‴ *.

119. ε (FL. 18a) Piscis austrini australior et præcedens.

July 28, 1783. Double. About 1¼ degree s. of, and a little preceding ε Piscis austrini, in a line from δ Aquarii continued

* On account of the change in the magnitudes of the 1st and 2d Hydræ, this small star may be of use to ascertain whether the 30th Monocerotis, which is situated between them, has any considerable proper motion. See Phil. Trans. vol. LXXIII. p. 255.

through

through ε Piſcis. Pretty unequal. L. dpr. S. dr. Diſtance 1′ 26″ 58‴. Poſition 67° 46′ ſ. following.

120. FL. 43rd Sagittarii ſequens ad auſtrum.

Aug. 16, 1783. Double. Near 1 degree ſ. following the 43d, in a line parallel to ξ and *o* Sagittarii; a conſiderable ſtar. Very unequal. Both dr. Diſtance with 278, 1′ 14″ 9‴. Poſition 37° 0′ n. preceding.

121. FL. 12 Lacertæ.

Aug. 18, 1783. Double. Very unequal. L. w.; S. r. Diſtance with 278, 1′ 0″ 10‴. Poſition 73° 0′ n. following.

Add the following errata of the Catalogue of Double Stars in vol. LXXII. to thoſe already noticed at the end of the LXXIId and LXXIIId volumes.

Page.	Line.	For	Read
133	22	25.	25*.
140	3	19″ 14‴	19″ 26‴
145	26	35″ 48‴	36″ 9‴
153	7	ϖ Capricorni. FL. 10.	ϱ Capricorni. FL. 11.
153	11	33° 42′	61° 23′
156	4	FL. 5.	FL. 4.

VII. *Observations of a new variable Star. In a Letter from* Edward Pigott, *Esq. to Sir* H. C. Englefield, *Bart. F. R. S. and A. S.*

Read December 23, 1784.

DEAR SIR,

FOR some years past I have been employed in verifying all the stars suspected to be variable, in order that hereafter we may know with certainty what to depend upon. This undertaking, which is nearly completed, has already proved of use in detecting many mistakes, and producing some discoveries; among which, the following is one of the most important. September 10, 1784, I first perceived a change in the brightness of the star η Antinoi, and by a series of observations made ever since, I find it subject to a variation very similar to that of Algol, though not exactly the same in any one particular.

η Antinoi, when brightest, is of the third or fourth magnitude, being between δ and β Aquilæ; and at its least brightness of the fourth or fifth magnitude, it then being between that of ι Antinoi and μ Aquilæ; therefore, its greatest variation in brightness may be called about one magnitude; and the changes it undergoes, though probably not nicely ascertained from so few observations, are nearly these:

At its greatest brightness	44 ± hours.
In decreasing - -	62 ± hours.
At its least brightness	30 ± hours.
In increasing - -	36 ± hours.

All these changes, which hitherto seem to be regular and constant, are performed in 7 days 4 hours 38—minutes; this I shall stile its period, and hereafter will shew how it is determined with such exactness.

The stars to which η Antinoi was compared are in order thus: δ Aquilæ third magnitude, β Aquilæ and θ Serpentis fourth magnitudes, ι Antinoi fourth or fifth magnitude, and μ Aquilæ a bright fifth. I find, by several years observation, that β Aquilæ retains the same brightness. ι Antinoi, which has been examined with particular attention by Mr. GOODRICKE and myself, is suspected by us both to be subject to a small variation, but not sufficiently apparent, so as to affect materially these comparisons, and possibly it may be only the effect of some optical illusion; for I have frequently remarked, that both in the twilight and moon-light, or when the air is in the least hazy, there is a greater difference between the brightness of many of the stars, than in a dark night and clear sky.

In the following journal of observations of η Antinoi, the Greek letters β, δ, μ, belong to Aquila, and ι, ν, to Antinous; secondly, the magnitudes marked in column the third are by estimation, and can be of no further use than merely to give, at first sight, an idea of the star's brightness; and lastly, the lines distinguished by inverted commas, are extracts from Mr. GOODRICKE's journal, whose friendly assistance I have often experienced, and was the more welcome on this occasion, because repeated attention and great exactness were requisite.

5 Dates.

Dates. 1783.	Hours.	Magnitude.	Journal of the comparative brightness of η Antinoi.
July 17	10±	3 · 4	Less than δ Aquilæ and brighter than θ Serpentis (β Aquilæ and θ Serpentis are equal) weather hazy.
19	10±	4	Rather brighter than β Aquilæ and θ Serpentis.
27	10±	4	If any difference, less than β Aquilæ.
1784			N. B. These times are from recollection, and cannot err more than 1½ hour.
Sept. 10	10±	4	Less than β Aquilæ and θ Serpentis.
12	7½	4 · 5	Much less than β, equal to ι.
—	9	4 · 5	"A little brighter than ι, air clear."
13	7½ / 9½	3 · 4	Less than δ, brighter than β, and much brighter than ι.
—	8	3 · 4	"Brighter than ι and β."
15	8	4	Rather brighter than β, and much brighter than ι.
18	9½ / 11	4 · 5	"Less than β and ι."
19	7½	4 · 5	Much less than β, and equal to ι.
—	7 / 9	4 · 5	"Less than β and ι."
20	8	3 · 4	Brighter than β and ι; at 11 h. it seemed to have increased.
23	7½ / 8	3 · 4	Less than δ, rather brighter than β; thought it rather less at 11½ h.; moon near.
28 / 29	9±	3 · 4	Brighter than β; moon-light.
30	9±	3 · 4	If any difference, rather brighter than β.
—	9±	3 · 4	"Rather brighter than β."
Oct. 1	7 / 9	4	Less than β, brighter than ι; air clear, moon-light.
2	8	4 · 5	Equal to ι, much less than β.
—	8	4 · 5	"Less than ι."
5	8	3 · 4	Between the brightness of β and δ.
6	9±	3 · 4	"Brighter than β and ι."
7	7½ / 9¾	3 · 4	Rather brighter than β.
—	8±	3 · 4	"Much the same as yesterday."
8	8±	4	Brighter than ι; think it not less than β; this observation doubtful, occasioned by intervening clouds.
—	8±	4 · 5	"Believe it less than ι; weather bad."
9	8±	4 · 5	"Certainly less than β; weather bad."
10	11	4 · 5	"Less than ι; rather a doubtful observation."
11	10½	4	Rather less than β, and brighter than ι.
15	10	4	Equal to β.
—	8±	4	"Rather brighter than β."
16	8	4	Less than β, brighter than ι.

Dates. 1784	Hours.	Magnitude.	Journal continued.
Oct. 16	$6\frac{1}{2}$	4 · 5	"Less than β and ι."
17	7, 8	4 · 5	Undoubtedly less than ι.
18	8	4 · 5	Less than ι, brighter than μ.
—	$6\frac{1}{2}$	4 · 5	"Less than ι."
19	$7\frac{1}{2}$	3 · 4	Evidently brighter than β.
20	8±	3 · 4	"Much brighter than β."
22	8±	3 · 4	"Brighter than β."
23	$6\frac{1}{2}$	4	Less than β.
—	8	4	"Not so bright as β, brighter than ι."
24	7	4 · 5	Equal to ι, much less than β; moon-light, air clear.
—	$6\frac{1}{2}$, 7	4 · 5	"Less than ι; rather, though very little, brighter than μ."
25	$6\frac{3}{4}$	4 · 5	Much less than β, equal to ι, brighter than μ.
26	$6\frac{1}{2}$	4	Sometimes seemed rather less, but generally equal to β.
—	$9\frac{1}{4}$	4	Equal, if not rather brighter than β.
—		4	"At $6\frac{1}{2}$ rather less, at $8\frac{1}{2}$ nearly equal, and at $9\frac{1}{2}$ "rather brighter than β.".
27	6	3 · 4	Remarkably bright, nearer δ than β; moon-light, air clear.
—	$6\frac{1}{2}$	3 · 4	"Nearer to β than to δ."
31	$8\frac{1}{4}$	4	Seemed equal to β; air not very clear.
Nov. 3	$5\frac{1}{2}$	3 · 4	"Rather brighter than β."
6	$8\frac{1}{2}$	4	Evidently less than β.
7	9	4 · 5	Much less than β.
—	$7\frac{1}{2}$	4 · 5	"Less than β and ι."
11	7	3 · 4	Brighter than β, much less than δ.
12	$6\frac{1}{2}$	3 · 4	Rather brighter than β, certainly equal.
—	$8\frac{1}{2}$	3 · 4	"Rather brighter than β and ι."
13	$5\frac{1}{2}$, 7	4 · 5	Less than β, equal to ι.
—	7	4 · 5	"Less than β, and rather less than ι."
16	$5\frac{3}{4}$	4	Evidently less than β, and rather brighter than ι; at 8 it seemed increased, and about
—	8	4	Between its least and full brightness.
—	7	4 · 5	"Less than β, and something less than ι."
17	$5\frac{3}{4}$, $7\frac{1}{4}$	3 · 4	Brighter than β.
19	6	4	If any difference, rather brighter than β; clouds cov-
—	8	4	ered the moon: at 8h. if any difference rather less than β; moon-light, and air not so clear as at 6.
—	6	4	"Rather brighter than β, brighter than ι."
20	7	4	Rather less than β, brighter than ι.
21	$6\frac{3}{4}$	4	Less than β, rather brighter than ι; moon-light.
25	7	3 · 4	"Brighter than β."
Dec. 4	$6\frac{1}{2}$	4	If any difference, less than β.

In

In order to obtain a point of comparison, for settling the periodical changes of η Antinoi, which I suppose to be constant, it is natural to fix upon that phasis, which can be determined with the greatest precision; and this seems to be at the time when it is between its least and greatest brightness, as *almost the whole* increase of brightness is completed in less than 24 hours, though the perfect completion is performed only in 36 ± hours; thus having settled this necessary point, and found roughly the length of a single period, the computations, in order to obtain greater exactness, are as follows.

Time when η Antinoi was between its least and greatest brightness.	Intervals between the observations.		Number of periods in ditto.		Length of a single period.	
Hours.	Days.	Hours.			Days.	Hours.
1784, Sept. 12. at 20 / Oct. 11. at 11	28	15	4	each of	7	3¾
Sept. 12. at 20 / Oct. 18. at 20	36	0	5	D°	7	4¼ +
Sept. 12. at 20 / Oct. 26. at 00	43	4	6	D°	7	4½ −
Sept. 12. at 20 / Nov. 16. at 8	64	12	9	D°	7	4
Sept. 19. at 20 / Oct. 18. at 20	29	0	4	D°	7	6
Sept. 19. at 20 / Oct. 26. at 00	36	4	5	D°	7	5½ +
Sept. 19. at 20 / Nov. 16. at 8	57	12	8	D°	7	4½
Oct. 11. at 11 / Nov. 16. at 8	35	21	5	D°	7	4½ −
Oct. 18. at 20 / Nov. 16. at 8	28	12	4	D°	7	3
Length of a single period, on a mean,					7	4 30

Perhaps other astronomers may not exactly agree with me, in fixing the times as set down in column the first; for my part, I determined them without paying any regard to the results, by taking a medium between the times when η Antinoi had

had rather passed its least brightness, being nearly equal to η Antinoi, and when it was a little, but undoubtedly, brighter than β Aquilæ. Though it does not appear, as I have already said, that any of the other phases can be settled with equal precision, different comparisons nevertheless may prove satisfactory towards corroborating the above; I have therefore also deduced its period from the best and most distant observations, made when at its least brightness; they are thus: 7 days 10 hours and 7 days 5 hours. These results I reject, and retain the mean given by the first set, with which we may proceed on to gain a much greater exactness; let one period be subtracted from the observation of July 27th, 1783, and it will appear, that η Antinoi had varied in brightness during the following four days, though at that time it did not strike me.

1783, July 17th, decidedly brighter than β Aquilæ.
— 18th, not observed.
— 19th, rather brighter than β Aquilæ.
— 20th (answering to the 27th) equal or rather less than β Aquilæ.

As it is therefore evident, that on July 19th and 27th, 1783, η Antinoi was *decreasing* in brightness, I shall compare those days observations to corresponding ones made in 1784.

		Hours.	
1784,	Sept. 30.	at 6	Similar observations to that of 1783 July 19th, at 10 h. ±, η Antinoi being rather brighter than β Aquilæ.
	Oct. 7.	at 16	
	Oct. 15.	at 6	
	Oct. 22.	at 12	
	Nov. 12.	at 2	
	Nov. 19.	at 00	

1784,

	Hours.	
1784, Sept. 30.	at 18	Similar Observations to that of 1783, July 27th, at 10 h. ±, η Antinoi being equal to or rather less than β Aquilæ.
Oct. 15.	at 14	
Oct. 22.	at 19	
Nov. 12.	at 14	
Nov. 19.	at 14	

In estimating the above times, I paid much attention to the observations of the preceding and following days; however, a few hours more or less do not make a material difference. The results of these comparisons are,

D.	H.	M.
7	4	39½
7	4	44½
7	4	53⅓
7	4	54⅔
7	4	32
7	4	26½
7	4	32
7	4	42½
7	4	43
7	4	26
7	4	21½
On a mean 7	4	38 — length of a single period.

As this approaches the most to the preceding result, it may be assumed as nearest the truth, provided the changes be uniformly periodical.

Hitherto the opinion of astronomers concerning the changes of Algol's light seem to be very unsettled; at least none are universally adopted, though various are the hypotheses to account for it; such, as supposing the star of some other than a spherical

aſpherical form, or a large body revolving round it, or with ſeveral dark ſpots or ſmall bright ones on its ſurface, alſo giving an inclination to its axis, &c.; though moſt of theſe conjectures with regard to Algol be attended with difficulties, ſome of them combined do, I think, account for the variation of η Antinoi.

Thoſe perſons who are accuſtomed to examine the ſtars attentively will not be ſurpriſed to find, that Mr. Goodricke and I do not always perfectly agree in our obſervations; theſe ſmall differences in the magnitudes of the ſtars are very difficult to be aſcertained with the naked eye, which has often made me lament, we had not ſome contrivance for determining their relative brightneſs, and even I attempted ſeveral methods, but did not purſue them with ſufficient attention and diligence to obtain any ſatisfactory reſults; nevertheleſs I ſhall juſt mention them, as perhaps ſomebody elſe may overcome thoſe difficulties, which to me appeared ſo very conſiderable.

1. In 1778 I had ſmall pieces of fine glaſs ſtained with different ſhades, which being applied to the eye end of a teleſcope, I could eaſily find what degree of ſhade was requiſite to efface ſtars of different brightneſs; and thus I obſerved ſome of the ſtars and planets.

2. Diaphragms were attempted; but, beſides other difficulties, they did not efface ſtars of the firſt magnitude.

3. A method which pleaſed me much, and perhaps may not prove unſucceſsful, is, by putting the ſtars out of the focus of a teleſcope till they become inviſible; this is performed by drawing the eye-tube of a refractor either in or out; the point of focal diſtance being previouſly determined, the brighter the ſtar the greater length of tube muſt be ſlid either in or out to efface it; thus I was in hopes of determining their magni-

tudes, and for that purpose had in 1776 divisions engraved on the eye-tube of a refractor; but found that its high magnifying powers prevented stars of the first and second magnitude becoming invisible.

Lastly, I am inclined to think the following method practicable, *viz.* to reflect in a telescope, by means of an illuminator, different degrees of light in a known proportion, so that stars of all magnitudes may be obliterated.

The changeable state of the weather will perhaps be thought a considerable obstacle to these contrivances, and to throw doubt on the observations; but this may be sufficiently obviated by attending to small telescopic stars, which according to the clearness of the atmosphere are more or less distinctly seen.

I beg the favour of you, dear Sir, to present these observations to the Royal Society; and believe me, with the greatest regard, &c.

York, Dec. 5, 1784. EDWARD PIGOTT.

VIII. *Astronomical Observations. In two Letters from M.* Francis de Zach, *Professor of Mathematics, and Member of the Royal Academies of Sciences at* Marseilles, Dijon, *and* Lyons, *to Mr.* Tiberius Cavallo, *F. R. S.*

Read December 23, 1784.

SIR, Lyons, April 4, 1783.

I SEND you the account of the observations on the eclipse of the moon, which I have made together with the rev. Father LE FEVRE, Astronomer at Lyons, in the Observatory called *au grand Collège*; to which I shall add the observations of the vernal equinox; some observations on Jupiter's satellites, made at Marseilles by M. SAINT JACQUES DE SYLVABELLE; and, lastly, a new solution of a problem that occurs in computing the orbits of comets. If you think that these observations do in any way deserve the notice of the Royal Society, I shall be very glad you would communicate them. In order to ascertain the going of the pendulum clock, I took several corresponding altitudes of the sun, which you will find in the following table. On the day of the eclipse the sky was very serene, nothing could be finer, and it continued so during the observation. I determined to use an achromatic telescope of 3½ feet length, that shews objects in their natural position, because the diluted and uncertain termination of the true shadow of the earth appears more perfectly defined by small than by

large telescopes, which magnify too much, and give too great a transit between the penumbra and the true dark shadow. On that account some celebrated astronomers advise to use for the eclipses of the moon no greater telescopes than of four or five feet length. It was remarked at Paris, that in an eclipse of the moon, observed through a telescope of DOLLOND, the focus of its object lens being 30 inches, and likewise through a telescope of five feet length; the eclipse appeared to begin 4′ 7″ sooner, and to end 4′ 7″ later, through the small than through the long telescope; the like has been remarked by several others, and it has been also observed by myself. As to my observations I am tolerably satisfied with them, as they do not differ materially from those of Father LE FEVRE, though it is known that in eclipses of the moon no greater exactness than that of a minute can be obtained. The moon's spots were carefully observed; for it is known, that the mean of the observations of the moon's spots is sufficient to ascertain the longitude of a place to 4″ or 5″ nearly. M. DE LA LANDE comparing the observations of the moon's spots in an eclipse, made the 22d of November, 1760, in Vienna, by the Imperial Astronomer Abbé HELL, with those made at the same time in Paris by M. MESSIER, finds the difference of meridians to be 56′ 13″, which agrees very exactly with that ascertained by other means.

Correspondent

Correſpondent altitudes of the Sun taken with a quadrant of three-feet radius, in order to adjuſt the pendulum clock to apparent time.

18th March, 1783.	Sun's altit.	Iſt obſervation.	Sun's altit.	IId obſervation.	Sun's altit.	IIId obſervation.	Sun's altit.	IVth obſervation.	Sun's altit.	Vth obſervation.
	° ′	h. ′ ″	° ′	h. ′ ″	° ′	h. ′ ″	° ′	h. ′ ″	° ′	h. ′ ″
The ſun's upper limb at the horizontal wire, eaſtern ſide	25 40	8 25 2	26 10	8 28 25	26 40	8 31 43	27 20	8 36 16	28	8 40 53
Sun's upper limb at the ſame altitude, weſtern ſide		15 12 24		15 9 1		15 5 39		15 1 10		14 56 29
Dividing the ſum by 2		23 37 26		23 37 26		23 37 22		23 37 26		23 37 22
Sun's center on the meridian as marked by the clock		11 48 43		11 48 43		11 48 41		11 48 43		11 48 41
Equation of the day		12 8 15		12 8 15		12 8 15		12 8 15		12 8 15
Clock ſlower than equated ſolar time		0 19 32		0 19 32		0 19 34		0 19 32		0 19 34

The mean of which I put 19′ 32″

19th March, 1783.	Sun's Altit.	Ist observation.	Sun's Alt.	IId observation.
	° ′	h. ′ ″	°	h. ′ ″
The sun's upper limb at the horizontal wire of the first telescope on the eastern side - - -	31 30	9 1 56	33	9 13 9
Sun's upper limb at the same altitude on the western side of the meridian -		14 31 47		14 20 33
Dividing the sum by 2 - -		23 33 43		23 33 42
Sun's center on the meridian as marked by the pendulum clock - -		11 46 51½		11 46 51
Equation of the day		12 7 57		12 7 57
Clock slower than equated solar time		0 21 6		0 21 6

		′	″
Clock slower than equated solar time	19th March	21	6
—— —— ——	18th March	19	32
Retarding of the clock upon 23 h. 58′ 8″ -		1	34

I observed too the mid-day at the great gnomon of the observatory, and found at the same time the meridian line erroneous by 19″, as you will find in the following tables.

		h. ′ ″		h. ′ ″		h. ′ ″
When the center of the sun's image was on the meridian the time pointed by the clock was	the 17th Mar.	11 50 50	the 18th Mar.	11 48 56	the 19th Mar.	11 47 3
Equations of those days -		12 8 33		12 8 15		12 7 57
Retarding on equated solar time		17 43		19 19	the	20 54
Retarding the 17th - -				17 43	18th	19 19
Retarding of the clock during those 24 hours				1 36		1 35

I fixed therefore the retarding of the clock 1′ 35″.

True

		h. ′ ″		h. ′ ″
True mid-day concluded by the sun's correspondent altitudes as the clock marked	the 18th Mar.	11 48 42	the 19th Mar.	11 46 51
Equation of the mid-day - -		— 18		— 18
Retarding of the clock at the rate of 1′ 35″ per 24 hours - - -		+ 13		+ 11
True mid-day the pendulum clock should have marked - - - -		11 48 37		11 46 44
Mid-day concluded at the gnomon of the observatory - - - -		11 48 56		11 47 3
Difference, the error of the meridian line or gnomon - - -		19		19

From thence I concluded,

	h. ′ ″		h. ′ ″
Mid-day at true solar time	11 59 60	Mid-day at equated solar time	12 8 15
Mid-day the clock should have marked on the 18th	11 48 37	Mid-day the clock should have marked on the 18th	11 48 37
Retarding upon true solar time - -	11 23	Retarding upon equated solar time -	19 38

Observations

Observations of the moon's eclipse the 18th March, 1783.

My observations with an achromatic telescope of 3½ feet length.	Time marked by the clock.	True or apparent time.
	h. ′ ″	h. ′ ″
IMMERSIONS.		
The beginning of the eclipse very doubtful.		
Shadow touches Grimaldi	7 41 45	7 53 39
Grimaldi all in the shadow	7 42 54	7 54 48
Shadow touches Mare Humorum	7 49 32	8 1 27
——— Copernicus	8 0 21	8 12 16
Copernicus all in the shadow	8 2 29	8 14 24
Tycho touches the shadow	8 6 18	8 18 13
Eudoxus ———	8 12 19	8 24 15
——— all in the shadow	8 13 43	8 25 39
Mare Serenitatis touches the shadow	8 16 7	8 28 3
——— the shadow in the middle	8 21 31	8 33 27
——— all in the shadow	8 25 54	8 37 50
Proclus touches the shadow	8 32 21	8 44 18
Mare Crisium touches the shadow	8 33 29	8 45 26
——— shadow in the middle	8 35 36	8 47 33
——— all in the shadow	8 36 56	8 48 53
Total immersion	8 38 57	8 50 55
EMERSIONS.		
Beginning of the emersion	10 19 57	10 32 2
Grimaldi emerging	10 23 33	10 35 38
——— all out of the shadow	10 24 9	10 36 14
Mare Humorum emerging	10 29 34	10 41 39
Total emersion of Mare Humorum	10 35 37	10 47 43
Copernicus all out of the shadow	10 43 6	10 55 12
Mare Serenitatis all emerged	10 57 32	11 9 39
Mare Crisium all emerged	11 15 44	11 27 51
End of the eclipse	11 20 10	11 32 18
Total duration		3 39 0

Father

Father LE FEVRE's observations with a reflector 55 inches focal length, magnifying 300 times.

	Time by the clock.	Apparent time.
IMMERSIONS.	h. ′ ″	h. ′ ″
Grimaldi touches the shadow	7 41 43	7 53 37
Kepler touches the shadow	7 52 2	8 3 57
—— all in the shadow	7 53 24	8 5 19
Copernicus touches the shadow	8 0 22	8 12 17
—— all in the shadow	8 2 26	8 14 21
Mare Serenitatis touches the shadow	8 16 7	8 28 3
—— all in the shadow	8 26 2	8 37 58
Mare Crisium touches the shadow	8 33 28	8 45 25
—— all in the shadow	8 36 56	8 48 53
Total immersion	8 38 54	8 50 52
EMERSIONS.		
Beginning of the emersion	10 19 42	10 31 47
Grimaldi emerged	10 23 24	10 35 29
Kepler all out of the shadow	10 35 43	10 47 49
Copernicus all out	10 43 4	10 55 10
Mare Serenitatis all out	10 57 19	11 9 26
—— Crisium all out	11 15 50	11 27 57
End of the eclipse	11 20 22	11 32 30
Total duration		3 39 20

The observation of the vernal equinox was made at the gnomon. The height of this gnomon, taken from the center of the hole by which the beams of the sun come in, is 1878 lines of a French inch; the distance from the bottom of the gnomon to the equinoctial point is 1928; the distance from the upper limb of the sun's image to the equinoctial point was found 16,7; the distance from the under limb 23,4; the diameter of the hole = 6; therefore the distance from the bottom to the upper limb $1928 - 16,7 + 3 = 1914,3$, to the under limb $1928 + 23,4 - 3 = 1948,4$; which gives the time the equinox happened the 20th of March, 5 h. 56″ 52″ P.M.

4 Observations

Obſervations of Jupiter's ſatellites at Marſeilles.

1782		Apparent time.	Obſervation.
		h. ′ ″	
April 3	Immerſion of the Iſt ſatellite at	2 22 56	good
May 19	Imm. Iſt ſat.	2 48 12	good
June 7	Imm. IVth ſat. was not total, but its light diminiſhed ſenſibly without ever diſappearing; the ſky was ſerene, and Jupiter had ſix belts very diſtinctly.		
20	Emerſion of the Iſt ſatellite	1 29 46	good
July 5	Em. Iſt ſat.	11 43 59	good
13	Em. IId ſat.	9 17 28	good
20	Em. IIId ſat.	9 27 13	good
20	Em. IId ſat.	11 51 59	good
21	Em. Iſt ſat.	10 1 11	good
27	Imm. IIId ſat.	10 40 33	good
Aug. 6	Em. Iſt ſat.	8 21 20	good
13	Em. Iſt ſat.	10 18 49	good
14	Em. IId ſat.	8 55 39	good
Sept. 1	Em. IIId ſat.	9 40 44	doubtful
14	Em. Iſt ſat.	8 6 48	good

IT is known, that the indirect method to calculate the orbits of comets in a conic ſection, by means of three obſervations given, is rendered more eaſy and expeditious if there is a poſſibility of drawing a graphical figure that repreſents nearly the orbit under conſideration, by means of which the calculation is directed, and the required elements of the comet's path may be rigorouſly determined. To draw the orbit of a comet that moves in a parabola or ellipſis, the problem is reduced to find the poſition of the axis and the perihelial diſtance; this poſition of the axis will be determined as ſoon as the angle is known, that the axis forms with another line, whoſe poſition is given; this line may be an ordinate to a given point of the curve, or a tangent, or a radius vector, &c. The latter is to

7 be

be employed in preference, because the perihelial distance being a constant quantity, the angle of position then becomes the true anomaly of the comet; but as the data of this problem are only geocentric longitudes and latitudes of the comet, deduced from the immediate observations of right ascension and declination, the heliocentric longitudes and latitudes must first be calculated; but as those data are not sufficient, what is not given must be arbitrarily supposed, *viz.* the shortened distances (*distantias curtatas*). This supposition is changed and altered until the calculation will agree with the three observations, then the difference between two longitudes is the angle comprehended between the two shortened distances in the plane of the ecliptic; the whole reduced to the plane of the comet's orbit by means of the heliocentric latitude, gives the difference between the anomalies comprehended by two radius vectors, the problem then is reduced: two radius vectors being given, with the angle comprehended, to find the two true anomalies, the perihelial distance, and the time the comet puts in running its anomalies.

Let therefore ♈ ♋ ♎ ♑ represent the ecliptic at an infinite distance; QPR the apparent elliptical or parabolical path of a comet; S the sun's center; P the comet's perihelion; T the place of the earth when the comet was first observed in C; I the earth's place when the comet was observed in K; $ST = d$, $SI = \delta$, the distances from the earth to the sun at the first and second observation known by astronomical tables; let C*m* and K*n* be two perpendiculars to the plane of the ecliptic, it will be $Sm = u$, $Sn = v$ the two shortened distances.

The observed geocentric longitude of the comet in $T = a =$ arc ♈ ♑ G;
the observed geocentric longitude of the comet in $I = \alpha =$ arc ♈ ♑ H;
the geocentric longitude of the sun by tables in $T = b =$ arc ♈ ♑ ♎ A;
the geocentric longitude of the sun by tables in $I = \beta =$ arc ♈ ♑ ♎ B.

Now for the first observation the angle of elongation is $b - a$; for the angle ATG = arc AG = ♈ ♍ ♎ A − ♈ ♍ G ♎ = long. ☉ − long. comet $= b - a$;

the angle of the annual parallax $SmT = \frac{\text{sin.}(b-a)d}{u} = e$;

the angle of commutation $mST = \overset{0}{\underset{180}{}}^{\circ} - e + (b - a) = f$;

from whence the heliocentric longitude of the comet $= b - 180^{\circ} + f = g$.

The same at the second observation in I.

Angle of elongation $= \beta - \alpha$;

Angle of annual parallax $\varepsilon = \frac{\text{sin.}(\beta-\alpha)\delta}{v}$;

Angle of commutation $\varphi = \overset{0}{\underset{180}{}}^{\circ} - \varepsilon + (\beta - \alpha)$;

heliocentric longitude of the comet in I $= \beta - 180^{\circ} + \varphi = \gamma$;

putting now the heliocentric latitude seen from S $= k$;

the geocentric latitude seen from T $= l$;

the heliocentric latitude will be $\frac{\text{sin.} f \,.\, \text{tang.} l}{\text{sin.}(b-a)} = \text{tang.}\, k$;

the same with Kn it will be $\frac{\text{sin.}\, \varphi \,.\, \text{tang.}\, \lambda}{\text{sin.}(\beta-\alpha)} = \text{tang.}\, \varkappa$ heliocentric latitude in K.

Having thus determined the heliocentric latitudes of two observations, the radius vectors will easily be found in the supposition made for the shortened distances, for they are in the same ratio to the radius vectors as the cosine of the heliocentric latitudes are to the radius $= 1$; therefore the radius vector m of the first observation will be $= \frac{u}{\text{cos.}\, k}$ and the radius vector of the second observation $\mu = \frac{v}{\text{cos.}\, \varkappa}$.

Taking now the difference between the found heliocentric longitudes, we get the heliocentric motion of the comet upon the ecliptic between two shortened distances, which is to be reduced upon the comet's orbit, this heliocentric motion is therefore $\gamma - g = m$. Now to reduce this motion we have, first, sinus

sinus totus = 1 is to cosine m :: as cotangent k is to the tangent of an angle which I put = n, and $90° - k \mp n$ will give an angle which I put = q. Lastly, the analogy cos. n : cos. q :: sin. k : will give the cosine of an angle ψ, which is the required motion upon the orbit, or the angle comprehended between the two radius vectors m and μ. Let therefore ECPMND be the apparent parabolic path of a comet; S the sun's center; M and N two places of the comet, the angle MSN equal to its motion in longitude, or the comprehended angle ψ; P the perihelion; it is required to find the two anomalies PM, PN, that is, PSM and PSN, the perihelial distance SP, and the time the comet employed to come from its perihelion P to M and N.

Resolution.

SM = m	In the right-angled triangle SMR and SNV we have
SN = μ	MR = OS = m sin. $(\psi \pm x)$ NV = QS = μ sin. x;
MSN = ψ	therefore OP = $\frac{1}{4}p - m$ (sin. $\psi \pm x$) and PQ =
NSB = x	$\frac{1}{4}p \mp \mu$ sin. x; but by the nature of the parabola
MSB = $(\psi \pm x)$	we have SM = AP + PO and SN = AP + PQ; that is
Parameter = p	$m = \frac{1}{2}p - m$ (sin. $\psi \pm x$) $\mu = \frac{1}{2}p \mp \mu$ sin. x
	$m + m$ (sin. $\psi \pm x$) = $\frac{1}{2}p$ $\mu \pm \mu$ sin. $x = \frac{1}{2}p$
	m (1 + sin. $\psi \pm x$) = $\frac{1}{2}p$ μ (1 $\mp$ sin. x) = $\frac{1}{2}p$
	and 1 + sin. $(\psi \pm x) = \frac{p}{2m}$ 1 $\pm$ sin. $x = \frac{p}{2\mu}$; by

putting into a sum 1 + sin. $(\psi \pm x)$ + 1 $\pm$ sin. $x = \frac{p}{2m} + \frac{p}{2\mu}$; reduction made 2 $\pm$ sin. x + sin. $(\psi \pm x) = \left(\frac{m+\mu}{2m\mu}\right)p$; but by trigonometrical formulæ we have sin. $(\psi \pm x)$ = sin. ψ cos. $x \pm$ sin. x cos. ψ. Substituting this expression in its place we obtain, 2 $\pm$ sin. x + sin. ψ cos. $x \pm$ sin. x cos. $\psi = \left(\frac{m+\mu}{2m\mu}\right)p$. By the same formulæ we have cos.^{2} x = 1 − sin.^{2} x and cos. $x = \sqrt{1 - \text{sin.}^2 x}$.

Substituting it comes out, $2 \pm \sin. x + \sin. \psi \sqrt{1 - \sin.^2 x} \pm \sin. x \cos. \psi = \left(\frac{m+\mu}{2m\mu}\right) p$ and $\sin. \psi \sqrt{1 - \sin.^2 x} = \left(\frac{m+\mu}{2m\mu}\right) p - 2 \mp \sin. x \mp \sin. x \cos. \psi$. By casting away the sign of the square root, and reducing to 0,

$$\left(\frac{m+\mu}{4m^2\mu^2}\right)^2 p^2 + 4 + \sin.^2 x + \sin.^2 x \cos.^2 \psi - 2\left(\frac{m+\mu}{m\mu}\right) p \mp \left(\frac{m+\mu}{m\mu}\right) p \,.\, \sin. x \mp \left(\frac{m+\mu}{m\mu}\right) p \,.\, (\sin. x \cos. \psi)$$

$$\pm 4 \sin. x \cos. \psi + 2 \sin.^2 x \cos. \psi - \sin.^2 \psi + \sin.^2 x \sin.^2 \psi = 0;$$ disentangling the equation,

$$\frac{m^2p^2 + 2m\mu p^2 + \mu^2 p^2}{4m^2\mu^2} + 4 + \sin.^2 x + \sin.^2 x \cos.^2 \psi - \frac{2mp - 2\mu p}{m\mu} \mp \frac{mp \sin. x \mp \mu p \sin. x}{m\mu} \mp \frac{mp \sin. x \cos. \psi \mp \mu p \sin. x \cos. \psi}{m\mu}$$

$$\pm 4 \sin. x \pm 4 \sin. x \cos. \psi + 2 \sin.^2 x \cos. \psi - \sin.^2 \psi + \sin.^2 x \sin.^2 \psi = 0.$$ But p being equal to $2\mu \pm 2\mu \sin. x$, substituting this value, we have

$$\frac{4m^2\mu^2 \pm 8m^2\mu^2 \sin. x + 4m^2\mu^2 \sin.^2 x + 8m\mu^3 + 8m\mu^3 \sin.^2 x \pm 16m\mu^3 \sin. x + 4\mu^4 + 4\mu^4 \sin.^2 x \pm 8\mu^4 \sin. x}{4m^2\mu^2} + 4 + \sin.^2 x +$$

$$+ \sin.^2 x \cos.^2 \psi - \frac{4m\mu \mp 4m\mu \sin. x - 4\mu^2 \mp 4\mu^2 \sin. x \mp 2m\mu \sin. x - 2m\mu \sin.^2 x \mp 2\mu^2 \sin. x - 2\mu^2 \sin.^2 x \mp}{m\mu}$$

$$\frac{\mp 2m\mu \sin. x \cos. \psi - 2m\mu \sin.^2 x \cos. \psi \mp 2\mu^2 \sin. x \cos. \psi - 2\mu^2 \sin.^2 x \cos. \psi}{m\mu} \pm 4 \sin. x \pm 4 \sin. x \cos. \psi + 2 \sin.^2 x \cos. \psi - \sin.^2 \psi +$$

$+ \sin.^2 x \sin.^2 \psi = 0$. Dividing the numerator and denominator of the first fraction by $4\mu^2$, and of the second by μ, and putting the whole to the common denominator m^2 it comes

$$\frac{m^2 \pm 2m^2 \sin. x + m^2 \sin.^2 x + 2m\mu + 2m\mu \sin.^2 x \pm 4m\mu \sin. x + \mu^2 + \mu^2 \sin.^2 x \pm 2\mu^2 \sin. x + 4m^2 + m^2 \sin.^2 x \cos.^2 \psi + m^2 \sin.^2 x -}{m^2}$$

$- 4m^2$

$$\frac{-4m^3 \mp 4m^2 \text{ sin. } x - 4m\mu \mp 4m\mu \text{ sin. } x \mp 2m^3 \text{ sin. } x - 2m^2 \text{ sin.}^2 x \mp 2m\mu \text{ sin. } x - 2m\mu \text{ sin.}^2 x \mp 2m^2 \text{ sin. } x \text{ cos.} \psi - 2m^2 \text{ sin.}^2 x \text{ cos.} \psi \mp}{m^2}$$

$$\frac{\mp 2m\mu \text{ sin. } x \text{ cos.} \psi - 2m\mu \text{ sin.}^2 x \text{ cos.} \psi \pm 4m^2 \text{ sin. } x \pm 4m^2 \text{ sin. } x \text{ cos.} \psi + 2m^2 \text{ sin.}^2 x \text{ cos} \psi - m^2 \text{ sin.}^2 \psi + m^2 \text{ sin.}^2 x \text{ sin.}^2 \psi}{m^2} = 0.$$

Casting away m^2, and reducing, we have

$$m^2 + \mu^2 + \mu^2 \text{ sin.}^2 x \pm 2\mu^2 \text{ sin. } x + m^2 \text{ sin.}^2 x \text{ cos.}^2 \psi - 2m\mu \mp 2m\mu \text{ sin. } x \text{ cos.} \psi - 2m\mu \text{ sin.}^2 x \text{ cos.} \psi - m^2 \text{ sin.}^2 \psi + m^2 \text{ sin.}^2 x \text{ sin.}^2 \psi \mp$$
$$\mp 2m\mu \text{ sin. } x \pm 2m^2 \text{ sin. } x \text{ cos.} \psi = 0.$$

Putting together $\text{sin.}^2 x$ and $\text{sin. } x$

$$(\mu^2 + m^2 \text{cos.}^2 \psi - 2m\mu \text{ cos.} \psi + m^2 \text{ sin.}^2 \psi) \text{ sin.}^2 x \pm (2\mu^2 \mp 2m\mu \text{ cos.} \psi \mp 2m\mu \pm 2m^2 \text{ cos.} \psi) \text{ sin. } x + m^2 - 2m\mu + \mu^2 - m^2 \text{ sin.}^2 \psi = 0.$$

Substituting $\text{cos}^2 \psi = 1 - \text{sin.}^2 \psi$ we recover a quadratic equation,

$$(\mu^2 + m^2 - 2m\mu \text{cos.} \psi) \text{sin.}^2 x \pm (2\mu^2 \mp 2m\mu \text{cos.} \psi \pm 2m^2 \text{cos.} \psi \mp 2m\mu) \text{sin. } x + m^2 - 2m\mu + \mu^2 - m^2 \text{sin.}^2 \psi = 0,$$

and $\text{sin.}^2 x \pm \frac{(2\mu^2 \mp 2m\mu \text{ cos.} \psi \pm 2m^2 \text{cos.} \psi \mp 2m\mu)}{(\mu^2 + m^2 - 2m\mu \text{ cos.} \psi)} \text{ sin. } x = \frac{2m\mu - m^2 - \mu^2 + m^2 \text{ sin}^2 \psi}{\mu^2 + m^2 - 2m\mu \text{ cos.} \psi}$.

This equation resolved gives

$$\text{sin. } x \pm \frac{\mu^2 \mp m\mu \text{ cos.} \psi \pm m^2 \text{ cos.} \psi \mp m\mu}{\mu^2 + m^2 - 2m\mu \text{ cos.} \psi} = \sqrt{\frac{2m\mu - m^2 - \mu^2 + m^2 \text{ sin.}^2 \psi}{\mu^2 + m^2 - 2m\mu \text{ cos.} \psi} \pm \frac{(\mu^2 \mp m\mu \text{ cos.} \psi \pm m^2 \text{ cos.} \psi \mp m\mu)^2}{(\mu^2 + m^2 - 2m\mu \text{ cos.} \psi)^2}}$$ which gives farther,

$$\text{sin.} x = \frac{\sqrt{(2m\mu - m^2 - \mu^2 + m^2 \text{ sin.}^2 \psi) \cdot (\mu^2 + m^2 - 2m\mu \text{cos.} \psi) \pm (\mu^2 \mp m\mu \text{cos.} \psi \pm m^2 \text{cos.} \psi \mp m\mu)^2}}{\mu^2 + m^2 - 2m\mu \text{ cos.} \psi} \mp \frac{\mu^2 \mp m\mu \text{cos.} \psi \pm m^2 \text{cos.} \psi \mp m\mu}{\mu^2 + m^2 - 2m\mu \text{ cos.} \psi}.$$

Putting $1 - \text{sin.}^2 \psi$ instead of $\text{cos.}^2 \psi$, after all reduction made, we recover

$$\text{sin. } x = \frac{\sqrt{2m^3\mu \text{ sin}^2 \psi - 2m^3 \mu \text{ cos.} \psi \text{ sin.}^2 \psi}}{m^2 + \mu^2 - 2m\mu \text{ cos.} \psi} \pm \left(\mp \frac{\mu^2 \pm \mu m \text{ cos.} \psi \mp m^2 \text{ cos.} \psi \pm m\mu}{\mu^2 + m^2 - 2m\mu \text{ cos.} \psi}\right);$$ that is,

$\text{sin. } x = (\pm\mu \mp m) \cdot (m \text{ cos.} \psi - \mu) \pm \sqrt{2m^3\mu \text{ sin.}^2 \psi (1 - \text{cos.} \psi)}$, which gives at last the simple expression $\text{sin. } x = \frac{(\pm\mu \mp m) \cdot (m \text{ cos.} \psi - \mu) \pm m \text{ sin. } \psi \sqrt{2m\mu (1 - \text{cos.} \psi)}}{m^2 + \mu^2 - 2m\mu \text{ cos.} \psi}$.

The angle x defines therefore the position of the axis and the two anomalies required, the perihelial distance being $p = 2\mu \pm 2\mu$ sin. x, it will be known also by the angle x.

In order to find the time the comet employs in running its anomalies, let the perihelial distance just now investigated p be equal to the radius of the earth's orbit, the parabolic area swept by the radius vector will be by the nature of the parabola $\frac{2}{3}$ PO × OM + $\frac{1}{3}$ SO × OM = $\frac{4\text{PO} \times \text{OM} + 3\text{SO} \times \text{OM}}{6}$. Now the periphery of the earth's orbit is $7 : 22 :: 2p : \frac{44}{7}p$; therefore the whole area $\frac{44}{7}p . \frac{1}{2}p = \frac{22}{7}p^2$. It is known that the velocity of a heavenly body moved in a circular path, is to the velocity in a parabolic path in the ratio $\sqrt{2} : 1$. If the parabolic area of the comet is divided by $\sqrt{2}$ it comes out $\frac{4\text{PO} \times \text{OM} + 3\text{SO} \times \text{MO}}{6\sqrt{2}}$ equal to an area that the earth describes in the very same time; put therefore A equal to the time of a sidereal year, we shall recover the analogy; the whole area of the earth's orbit is to the time in which it is described as the parabolic area is to the time consumed in sweeping it; therefore $\frac{22}{7}p^2 : \text{A} :: \frac{(4\text{PO} + 3\text{SO})\ \text{MO}}{6\sqrt{2}} : \frac{7\text{A}\ (4\text{PO} + 3\text{SO})\ \text{MO}}{72p^2\sqrt{2}}$; but OM = SM . sin. anom. PSM and OS = SM . cos. anom. PSM; let the anomaly be $= \delta$, we have OM $= m$ sin. δ, and OS $= m$ cos. δ; therefore PO $= p - m$ cos. δ. Substituting we obtain $\frac{7\text{A}\ (4p - 4m \text{ cos. } \delta + 3m \text{ cos. } \delta)\ m \text{ sin. } \delta}{72p^2\sqrt{2}}$ which is $\frac{7\text{A}\ (4p - m \text{ cos. } \delta)\ m \text{ sin. } \delta}{72p^2\sqrt{2}}$, whereby the time is found in parts of a sidereal year.

I am, &c.

SIR,

SIR, Lyons, May 4, 1783.

LATELY I received from the Observatory at Marseilles the observation of the transit of Mercury, which happened the 12th Nov. 1782. The sky not being very favourable, only the two internal contacts were observed; the first internal contact was observed by M. ST. JACQUES DE SYLVABELLE, at 3 h. 18′ 30″ apparent time; the last internal contact by M. ST. JACQUES, at 4 h. 30′ 16″; by M. BERNARD, his Adjunctus, at 4 h. 29′ 13″. The nearest distances of Mercury's limb to that of the sun in the northern part of its disk were at

h.	′	″		
3	33	14	31	parts of the micrometer.
3	42	57	34	
4	22	17	19	

The apparent diameter of the sun was 2174 parts of this micrometer: I suppose the before-mentioned 2174 parts = 32′ 26″,9. I conclude farther, by the observations, the middle of the transit at 3 h. 54′ 7″,25, whereas I fix, by interpolation, the distances of the limbs at 3 h. 54′ 7″,25 = 35″,6; I have therefore semi-diameter of the sun = 16′ 13″,4 − 35″,6 = 15′ 37″,8 + semi-diameter of Mercury = 6″ = 15′ 43″,8 = to the least distance of centers of the sun and Mercury. By M. DE LA LANDE's tables it is 15′ 42″, only a difference of 1″,8.

M. WALLOT at Paris has observed this transit at the Royal Observatory,

	h.	′	″
First external contact	2	56	28
First internal contact	3	2	3
Second - -	4	17	18
Second external -	4	22	53

I only add an important remark upon the diameter of Mercury, which the astronomers supposed in this transit = 12″.

Let ABC represent the sun's disk; in P an external in Q an internal contact; ANC the apparent path of Mercury over the sun.

The semi-diameter of the sun = 972″, this of Mercury in our supposition = 6″, MN = 942″ the least distances of the centers. In the right-angled triangle MNP it is MP = 972″ + 6″ = 978″, MQ = 972″ − 6″ = 966″; therefore NP will be found = 260″ and NQ = 210″: now NP − NQ = PQ = 50″, which converted into time gives 8′ 14″ for the time the diameter of Mercury employed to run over the limb of the sun; but by the observations of M. WALLOT I find this time constantly in both contacts 5′ 35″; therefore 8′ 14″ : 12″ :: 5′ 35″ : 8″,137, which should be the diameter of Mercury; and indeed M. WALLOT, by an immediate measure, taken with an excellent wire-micrometer, finds this apparent diameter not greater than 9″, which sufficiently shews that this diameter supposed 7″ in the mean distance is also too great.

I am, &c.

IX. *Observations of a new Variable Star.* By John Goodricke, *Esq.*; *communicated by Sir* H. C. Englefield, *Bart. F. R. S. and A. S.*

Read January 27, 1785.

TO SIR H. C. ENGLEFIELD, BART.

DEAR SIR, York, Jan. 10, 1785.

THE account that has been lately given of the regular variation of Algol's light, and the notice astronomers have been pleased to take of it, are well known. It is natural therefore to suppose, that the relation of other similar phænomena may also meet with the same favourable reception. Of this kind is the following, which I beg the favour of you to present to the Royal Society.

On the 10th of September, 1784, whilst my attention was directed towards that part of the heavens where β Lyræ was situated, I was surprised to find this star much less bright than usual, whereupon I suspected that it might be a variable star: my suspicions were afterwards confirmed by a series of observations, which have been regularly continued since that time, and which will presently follow in their proper place. At first I thought the light of this star subject to a periodical variation of nearly *six days and nine hours*, though the degree of its diminution did not then appear to be constant; but now, upon a more close examination of the observations themselves, I am

 inclined

inclined to think, that the extent of its variation is *twelve days and nineteen hours*, during which time it undergoes the following changes.

1. It is of the third magnitude for about two days.

2. It diminishes in about one day and a quarter.

3. It is between the fifth and fourth magnitude for less than a day.

4. It increases in about two days.

5. It is of the third magnitude for about three days.

6. It diminishes in about one day.

7. It is something larger than a star of the fourth magnitude for little less than a day.

8. It increases in about one day and three quarters to the first point, and so completes a whole period.

These eight points of the variation are perhaps inaccurately ascertained; and indeed it cannot be expected to be otherwise in estimations of this nature, where it is very possible to err even several hours.

The relative brightness of β Lyræ, at its obscuration in the third and seventh points, is nearly as follows. When in that of the third point, it is less than ζ and κ, and nearly equal to δ Lyræ; and when in that of the seventh point, it is rather less than ξ and θ Herculis, and much brighter than ζ, κ, and δ Lyræ. At its greatest brightness in the first and fifth points, it is sometimes brighter than γ Lyræ, but less than β Cygni, and sometimes only nearly equal to it; but in those points it seems to alter in its brightness several times in the same night, and that generally in a pretty considerable degree. However, this may perhaps be only owing to some fallacy of observation; for I have often perceived, that the relative brightness of stars is affected not only by the different states of the air, but also by their

their change of position occasioned by the earth's diurnal motion, and that particularly in stars of a great altitude.

The magnitudes of the stars, to which β Lyræ was compared during the progress of its variation, are as follows. β Cygni and γ Lyræ of the third magnitude; ξ and θ Herculis of between the fourth and third magnitude; ο Herculis is something less than a star of the fourth magnitude; ζ, κ, and δ Lyræ are stars of between the fourth and fifth magnitude, if not nearer the fifth. The relative brightness of these stars follows the order in which they are set down.

Observations of the brightness and magnitude of β Lyræ.

1784, Sept. 10. At 11 h. ±, much less than γ Lyræ; nearly equal to, if not rather brighter than ζ, κ, and δ Lyræ, and not so bright as ξ, θ, and ο Herculis; between the fourth and fifth magnitude.

Sept. 11. At 8½ h. nearly the same as it was last night, if not brighter; indifferent observation.

Sept. 12. At 8½ h. and 9 h. between the third and fourth magnitude; less than γ Lyræ, brighter than θ, ξ, and ο Herculis, and much brighter than ζ, κ, and δ Lyræ. Mr. E. PIGOTT agrees with me nearly.

Sept. 13, 15, 18, 19, and 20. It was at or near its greatest brightness.

Sept. 23. At 7½ h. it was nearly equal to ζ, κ, and δ Lyræ, and much less than ξ, θ, and ο Herculis.

At 10½ h. the air being extremely clear, I compared it more attentively to the neighbouring stars, and found it as follows: rather a little brighter than δ, a little less than ζ, and rather

less than $\varkappa$ Lyræ. Mr. E. PIGOTT thought it had rather increased from 8½ to 11 h.

Sept. 24. At 13½ h. certainly brighter than it was last night, but intervening clouds precluded all further observation.

Sept. 28. At 10 h. not quite so bright as γ Lyræ, but rather brighter than θ and ξ Herculis. Mr. E. PIGOTT thought it nearly equal to γ Lyræ.

Sept. 29. At 7½ h. not so bright as γ Lyræ.

At 8½ h. to 10½ h. nearly equal to ξ and θ Herculis; but if any thing it seemed rather less than ξ, and rather brighter than θ; about the fourth magnitude.

At 11½ h. to 12¾ h. the same, if not less; I could not compare it well to ξ and θ, because they were low; moon-light, but the air was clear.

Sept. 30. At 7 h. rather less than θ, if not equal to it; a little less than ξ, and brighter than o Herculis; about the fourth magnitude.

At 11 h. and 12¾ h. it seemed to be on its increase, being for the most part larger than ξ and θ Herculis.

Oct. 1 and 2. About its greatest brightness, but less than γ Lyræ. Mr. E. PIGOTT thought it brighter on the 2d than on the 1st, being on the 2d nearly equal to γ Lyræ.

Oct. 4. At 10½ h. I thought it rather less, but the weather was hazy.

Oct. 5. At 6½ h. not so bright as ξ and θ Herculis; a little brighter than ζ, and brighter than δ and $\varkappa$ Lyræ; air clear.

At 9½ h. nearly equal to ζ, and a little brighter than δ and $\varkappa$ Lyræ.

At 12½ h. a little less than ζ, nearly equal to $\varkappa$, and rather a little brighter than δ Lyræ; between the fourth and fifth magnitude; air very clear.

Oct. 6. At 6¾ h. and 7¾ h. less than ζ and κ, and a little less than δ Lyræ; between the fifth and fourth magnitude.

Oct. 7. At 6¾ h. between the fourth and third magnitude; a little brighter than θ, and nearly equal to ξ Herculis; much brighter than ζ, κ, and δ Lyræ; I observed it till 12½ h. when it was certainly increased.

At 7½ h. Mr. E. Pigott thought it brighter than ξ and θ Herculis.

Oct. 8. At 8 h. nearly equal to γ Lyræ; on account of the intervening clouds, I could not perceive which was largest; third magnitude.

Oct. 9. At 7 h. rather less than γ Lyræ.

Oct. 10. At 7 h. 11½ h. and 12 h. nearly equal to γ, if not rather less.

Oct. 11. At 8 h. 10 h. and 12 h. rather less than γ; at 12 h. if any difference, less than it was last night.

Oct. 15. At 8 h. = nearly equal to, though rather less than, γ Lyræ.

Oct. 16. At 6½ h. and 9½ h. little less than γ, if not equal to it.

At 11 h. rather larger than γ, but the weather was foggy. Mr. E. Pigott agrees with me in both observations.

Oct. 17. At 6½ h. and 7 h. somewhat less than γ Lyræ.

Oct. 18. At 6¾ h. between the fourth and fifth magnitude; brighter than κ and δ, and rather brighter than ζ Lyræ; good observation.

At 9½ h. I thought it was decreased, being equal to ζ and rather brighter than κ Lyræ. Mr. E. Pigott also thought it was decreasing.

Oct. 19. At 6½ h. it was rather less than ζ and κ, and brighter than δ Lyræ.

At

At 8¼ h. nearly the same, if not increased.

Oct. 20. At 6¼ h. rather brighter than ξ and θ Herculis, and between the fourth and third magnitude.

At 8¼ h. and 11 h. I thought it was increased, but it was less than γ Lyræ; between the third and fourth magnitude.

Oct. 22. At 6 h. 8 h. and 9 h. nearly equal to γ Lyræ.

Oct. 23. At 6 h. 8 h. and 11 h. rather less than γ, though nearly equal to it.

Oct. 24. At 6¼ h. and 11 h. less than γ Lyræ, and brighter than ξ and θ Herculis; at 8 h. Mr. E. PIGOTT thought it rather less than γ Lyræ.

Oct. 25. At 6 h. 8 h. and 11 h. nearly, though perhaps not quite equal to θ Herculis; less than ξ Herculis, and brighter than ζ and δ Lyræ; about the fourth magnitude. At 6¼ h. Mr. E. PIGOTT thought it rather brighter than θ and o Herculis.

Oct. 26. At 6 h. and 11 h. brighter than θ and ξ Herculis, but less than γ Lyræ.

Oct. 27. At 6 h. and 8¼ h. brighter than it was last night, but still less than γ Lyræ; much brighter than ξ and θ Herculis; the moon was at its full.

Oct. 28. At 8 h. ± rather less than γ Lyræ.

Oct. 29. At 9¼ h. nearly equal to, though rather brighter than γ Lyræ; I saw them but for a short time on account of clouds coming on.

Oct. 31. At 8 h. between the fifth and fourth magnitude; less than ζ and κ, and brighter than δ Lyræ. Mr. E. PIGOTT thought it equal to ζ Lyræ at 8¼ h.

Nov. 1. At 6¼ h. between the fourth and fifth magnitude; rather brighter than ζ, and brighter than κ and δ Lyræ.

Nov. 3. At 5¼ h. little less than γ Lyræ.

Nov.

Nov. 6. At 8 h. rather less than γ Lyræ, and brighter than θ Herculis. Mr. E. PIGOTT thought it nearly equal to γ Lyræ.

Nov. 7. At 7 h. and 10½ h. much less than γ Lyræ; nearly equal to, if not rather brighter than, θ Herculis, and rather less than ξ Herculis; between the fourth and third magnitude.

Nov. 10. At 10½ h. nearly equal to γ Lyræ. Mr. E. PIGOTT thought it not quite so bright as γ at 11 h.

Nov. 11. At 5½ h. and 7 h. a little brighter than γ Lyræ; afterwards I rather thought them equal, though β appeared for the most part something brighter. At 11 h. and 12 h. they appeared nearly equal. At 7 h. Mr. E. PIGOTT thought it was less than γ, if there was any difference.

Nov. 12. At 6½ h. 8½ h. and 10 h. much less than γ Lyræ, but brighter than ξ and θ Herculis; between the fourth and third magnitude.

Nov. 13. At 6½ h. and 10 h. equal to, if not rather less than ζ, less than κ, and brighter than δ Lyræ; between the fifth and fourth magnitude. At 5½ h. Mr. E. PIGOTT thought it rather brighter than ζ Lyræ.

Nov. 16. At 7½ h. little less than γ. At 10 h. certainly a little brighter than it.

Nov. 17. At 6 h. rather brighter than γ. At 8½ h. 9½ h. and 10½ h. brighter than γ, and less than β Cygni.

Nov. 18. At 9 h. 10 h. and 19 h. just the same.

Nov. 19. At 6½ h. and 8 h. less than γ Lyræ, and brighter than θ and ξ Herculis; between the third and fourth magnitude. At 10 h. something brighter than θ Herculis.

Nov. 20. At 7 h. 8 h. and 10½ h. rather less than ξ, and rather brighter than θ Herculis; between the fourth and third

magnitude. At 18¾ h. I thought it was increased; observed in twilight.

Nov. 21. At 7 h. something brighter than θ and ξ Herculis.

Nov. 25. At 7 h. less than γ Lyræ, and brighter than θ Herculis; between the fourth and third magnitude. At 9¼ h. I thought it was decreased, being now of the fourth magnitude.

Nov. 26. At 9 h. ± much less than γ, and of between the fourth and fifth magnitude; but the weather was too hazy, and the moon-light too strong, to observe well.

Nov. 29. At 7½ h. and 8 h. rather brighter than γ Lyræ. Mr. EDW. PIGOTT thought it nearly equal to γ at 8 h.

Nov. 30. At 8¼ h. and 10¾ h. brighter than γ Lyræ, and less than β Cygni; air clear.

Dec. 4. At 5½ h. 6½ h. and 10½ h. less than γ Lyræ, and brighter than θ Herculis; between the third and fourth magnitude. Mr. E. PIGOTT thought it nearly equal to γ at 6½ h.

Dec. 9. At 8 h. much less than γ Lyræ, and brighter than ζ Lyræ; about between the fourth and fifth magnitude. At 18½ h. it was increased, and nearly equal to o Herculis; but less than θ and ξ; not quite of the fourth magnitude.

Dec. 11. At 6 h. and 8 h. less than γ Lyræ, and brighter than θ and ξ Herculis. At 8½ h. 9½ h. and 18½ h. nearly equal to, though rather less than γ.

Dec. 12. At 5 h. and 6 h. nearly equal to γ, though rather less.

Dec. 13. At 5½ h. and 9¼ h. something brighter than γ.

Dec. 14. At 7 h. and 8½ h. rather brighter than γ.

Dec. 17. At 5½ h. less than γ Lyræ, and brighter than θ and ξ Herculis. At 7¾ h. nearly equal to γ, though rather less.

Dec.

Dec. 19. At 9 h. I believe it was brighter than γ, but the weather was not very favourable.

At 19 h. little less than γ.

Dec. 20. At 5¼ h. less than γ Lyræ, and brighter than θ and ξ Herculis. At 6¼ h. nearly equal, though rather less than γ Lyræ.

Dec. 21. At 8 h. much less than γ, and considerably brighter than ζ Lyræ; not quite of the fourth magnitude.

At 18 h. a little brighter than ζ and κ, and brighter than δ Lyræ; between the fourth and fifth magnitude.

Dec. 28. At 6 h. less than γ and brighter than θ Herculis; between the third and fourth magnitude. At 8 h. nearly equal to θ Herculis; between the fourth and third magnitude.

1785, Jan. 5. At 5¼ h. about equal to θ Herculis; fourth magnitude.

Jan. 6. At 5¼ h. between γ Lyræ and θ Herculis, but rather nearer γ. At 8½ h. it seemed a little brighter than γ.

From the above series of observations I have deduced all the conclusions relative to the eight points of the variation, as they are stated in the beginning of this paper. However, as at first it may not clearly appear, that the star has a more considerable diminution in the third point than in the seventh, it will not be improper to add a few words relating to that circumstance: for proof of it, therefore, I refer to an attentive comparison of the observations of Sept. 10. Sept. 23. Oct. 5 and 6. Oct. 18 and 19. &c. corresponding to the third point of the variation with those of Sept. 29 and 30. Oct. 25. Nov. 7 and 19, &c. corresponding to the seventh point of the variation. It may be objected, that in some of the observations of the seventh point, the star might have become still more diminished in the intermediate hours; but this is not probable,

 because

because in that point the star has been observed of about the fourth magnitude at intervals much shorter than in the third point, so that, if it had continued to diminish, its diminution would have proceeded at a more rapid rate, which still shews that there is at least a difference between these two points.

With regard to the period of the variation, it is evident from a collation of the preceding observations in a coarse way, that it is nearly twelve days and three quarters. To determine it with greater accuracy is a subject of considerable difficulty, in the present case; for unless we can obtain very exact points of comparison, the period would come out erroneous, especially if deduced from intervals consisting of only a very few periods, as is the case here. However, as I have been able to obtain a few observations of the middle of its obscuration in the third point accurate enough for our purpose, I have formed the following calculation.

Times of the middle of its obscuration in the third point.

			h.		d.	h.
1784,	Oct.	6	1	only a single period of	12	21
	—	18	22			
	Oct.	18	22	D° —	12	17
	—	31	15			
	Oct.	6	1	two periods, each of	12	19
	—	31	15			
				Hence the period on a mean is	12	19

In ascertaining the above times, I attended particularly to the nearest observations both preceding and following. In the manner above stated the period may also be deduced from the middle of its obscuration in the seventh point; but as these observations are not so exact as the above, I shall only, as a further

further confirmation, compare two of the most distant of them, *viz.* Sept. 29. 22 h. and Nov. 20. 6 h. which interval I find contains six periods, each of 12 d. 20 h. ±.

I have it in my intention to pursue the subject further, and when I have got a sufficient number of observations, it will be easy to determine the period with greater exactness, and also at the same time to ascertain the other particulars of the variation with more precision. In the mean while I wish that this account may be considered as being yet imperfect; but I was induced to send it in its present state, in hopes that other astronomers may contribute by their observations to the elucidation of this phænomenon.

As β Lyræ is a quadruple star, N° 3. of Mr. HERSCHEL's VIth Class of Double Stars *, I was desirous to see if any of the small stars near it would be affected by its different changes; but they seemed not to suffer any alteration, either when it was at its greatest or at its least brightness. I attended to this the more particularly because the loss of the star's light was very considerable, and the phænomenon seemed to be occasioned by a rotation on the star's axis, under a supposition that there are several large dark spots upon its body, and that its axis is inclined to the earth's orbit.

I must not omit mentioning here that Mr. HERSCHEL, amongst those stars which he supposes to have undergone an alteration, reckons β or γ Lyræ; because he observed that γ was much larger than β, while FLAMSTEED marks both of the same magnitude †. It may also be added, as shewing that β Lyræ varied in former times, that HEVELIUS, in his Catalogue, differs from FLAMSTEED, and marks γ of the third magnitude,

* Phil. Trans. for 1782, p. 142.

† Phil. Trans. for 1783, p. 256.

and β of between the fourth and third. I have, however, ſome doubts whether the variation of this ſtar does not entirely ceaſe or become leſs viſible in certain years. Theſe doubts ariſe from ſome obſervations of CASSINI in Phil. Tranſ. N° 73. p. 2198. where I find that in obſerving the new ſtar, which then appeared near the beak of the Swan, he compared it very frequently for upwards of a month to β and γ Lyræ, yet without perceiving, or even ſuſpecting, that β was variable, though it was eaſy for him to have perceived it, if the variation had then been even leſs than it is now.

I am, &c.

JOHN GOODRICKE.

X. *On the Motion of Bodies affected by Friction.* By the Rev. Samuel Vince, *A. M. of* Cambridge; *communicated by* Anthony Shepherd, *D. D. F. R. S. Plumian Professor of Astronomy and experimental Philosophy at* Cambridge.

Read November 25, 1784.

THE subject of the paper which I have now the honour of presenting to the Royal Society, seems to be of a very considerable importance both to the practical mechanic and to the speculative philosopher; to the former, as a knowledge of the laws and quantity of the friction of bodies in motion upon each other will enable him at first to render his machines more perfect, and save him in a great measure the trouble of correcting them by trials; and to the latter, as those laws will furnish him with principles for his theory, which when established by experiments will render his conclusions applicable to the real motion of bodies upon each other. But, however important a part of mechanics this subject may constitute, and however, from its obvious uses, it might have been expected to have claimed a very considerable attention both from the mechanic and philosopher, yet it has, of all the other parts of this branch of natural philosophy, been the most neglected. The law by which the motions of bodies are retarded by friction has never, that I know of, been truly established. MUSSCHENBROEK says, that in small velocities the friction varies very nearly as the velocity, but that in great velocities the friction increases; he has also attempted to prove, that by increasing

the weight of a body the friction does not always increase exactly in the same ratio; and that the same body, if by changing its position you change the magnitude of the surface on which it moves, will have its quantity of friction also changed. HELSHAM and FERGUSON, from the same kind of experiments, have endeavoured to prove, that the friction does *not* vary by changing the quantity of surface on which the body moves; and the latter of these asserts, that the friction increases very nearly as the velocity; and that by increasing the weight, the friction is increased in the *same* ratio. These different conclusions induced me to repeat their experiments, in order to see how far they were conclusive in respect to the principles deduced from them: when it appeared, that there was another cause operating besides friction, which they had not attended to, and which rendered all their deductions totally inconclusive. Of those who have written on the theory, no one has established it altogether on true principles: EULER (whose theory is extremely elegant, and which, as he has so fully considered the subject, would have precluded the necessity of offering anything further, had its principles been founded on experiments) supposes the friction to vary in proportion to the velocity of the body, and its pressure upon the plane, neither of which are true: and others, who have imagined that friction is a uniformly retarding force (and which conjecture will be confirmed by our experiments), have still retained the other supposition, and therefore rendered their solutions not at all applicable to the cases for which they were intended. I therefore endeavoured by a set of experiments to determine,

1st, *Whether friction be a uniformly retarding force.*

2dly, *The quantity of friction.*

3dly,

3dly, *Whether the friction varies in proportion to the pressure or weight.*

4thly, *Whether the friction be the same on whichever of its surfaces a body moves.*

The experiments, in which I was assisted by my ingenious friend the Rev. Mr. JONES, Fellow of Trinity College, were made with the utmost care and attention, and the several results agreed so very exactly with each other, that I do not scruple to pronounce them to be conclusive.

2. A plane was adjusted parallel to the horizon, at the extremity of which was placed a pulley, which could be elevated or depressed in order to render the string which connected the body and the moving force parallel to the plane. A scale accurately divided was placed by the side of the pulley perpendicular to the horizon, by the side of which the moving force descended; upon the scale was placed a moveable stage, which could be adjusted to the space through which the moving force descended in any given time, which time was measured by a well regulated pendulum clock vibrating seconds. Every thing being thus prepared, the following experiments were made to ascertain the law of friction. But let me first observe, that if friction be a uniform force, the difference between it and the given force of the moving power must be also uniform, and therefore the moving body must descend with a uniformly accelerated velocity, and consequently the spaces described from the beginning of the motion must be as the squares of the times, just as when there was no friction, only they will be diminished on account of the friction.

3. EXP. 1. A body was placed upon the horizontal plane, and a moving force applied, which from repeated trials was found to descend 52¼ inches in 4″, for by the beat of the clock and

the ſound of the moving force when it arrived at the ſtage, the ſpace could be very accurately adjuſted to the time; the ſtage was then removed to that point to which the moving force would deſcend in 3″, upon ſuppoſition that the ſpaces deſcribed by the moving power were as the ſquares of the times; and the ſpace was found to agree very accurately with the time; the ſtage was then removed to that point to which the moving force ought to deſcend in 2″, upon the ſame ſuppoſition, and the deſcent was found to agree exactly with the time; laſtly, the ſtage was adjuſted to that point to which the moving force ought to deſcend in 1″, upon the ſame ſuppoſition, and the ſpace was obſerved to agree with the time. Now, in order to find whether a difference in the time of deſcent could be obſerved, by removing the ſtage a little above and below the poſitions which correſponded to the above times, the experiment was tried, and the deſcent was always found too ſoon in the former, and too late in the latter caſe; by which I was aſſured that the ſpaces firſt mentioned correſponded exactly to the times. And, for the greater certainty, each deſcent was repeated eight or ten times; and every caution uſed in this experiment was alſo made uſe of in all the following.

EXP. 2. A ſecond body was laid upon the horizontal plane, and a moving force applied which deſcended 41¾ inches in 3″; the ſtage was then adjuſted to the ſpace correſponding to 2″, upon ſuppoſition that the ſpaces deſcended through were as the ſquares of the times, and it was found to agree accurately with the time; the ſtage was then adjuſted to the ſpace correſponding to 1″, upon the ſame ſuppoſition, and it was found to agree with the time.

EXP. 3. A third body was laid upon the horizontal plane, and a moving force applied, which deſcended 59⅞ inches in 4″; the

ſtage

ſtage was then adjuſted to the ſpace correſponding to 3″, upon ſuppoſition that the ſpaces deſcended through were as the ſquares of the times, and it was found to agree with the time; the ſtage was then adjuſted to the ſpace correſponding to 2″, upon the ſame ſuppoſition, and it was found to agree with the time; the ſtage was then adjuſted to the ſpace correſponding to 1″, and was found to agree with the time.

Exp. 4. A fourth body was then taken and laid upon the horizontal plane, and a moving force applied, which deſcended 55 inches in 4″; the ſtage was then adjuſted to the ſpace through which it ought to deſcend in 3″, upon ſuppoſition that the ſpaces deſcended through were as the ſquares of the times, and it was found to agree with the time; the ſtage was then adjuſted to the ſpace correſponding to 2″, upon the ſame ſuppoſition, and was found to agree with the time; laſtly, the ſtage was adjuſted to the ſpace correſponding to 1″, and it was found to agree exactly with the time.

Beſides theſe experiments, a great number of others were made with hard bodies, or thoſe whoſe parts ſo firmly cohered as not to be moved *inter ſe* by the friction; and in each experiment bodies of very different degrees of friction were choſen, and the reſults all agreed with thoſe related above; we may therefore conclude, that *the friction of hard bodies in motion is a uniformly retarding force.*

But to determine whether the ſame was true for bodies when covered with cloth, woollen, &c. experiments were made in order to aſcertain it; when it was found in all caſes, that the retarding force increaſed with the velocity; but, upon covering bodies with paper, the conſequences were found to agree with thoſe related above.

4. Having proved that the retarding force of all hard bodies arising from friction is uniform, the quantity of friction, considered as equivalent to a weight without inertia drawing the body on the horizontal plane backwards, or acting contrary to the moving force, may be immediately deduced from the foregoing experiments. For let $M =$ the moving force expressed by its weight; $F =$ the friction; $W =$ the weight of the body upon the horizontal plane; $S =$ the space through which the moving force descended in the time t expressed in seconds; $r = 16\frac{1}{12}$ feet; then the whole accelerative force (the force of gravity being unity) will be $\frac{M-F}{M+W}$; hence, by the laws of uniformly accelerated motions, $\frac{M-F}{M+W} \times rt^2 = S$, consequently $F = M - \frac{\overline{M+W} \times S}{rt^2}$. To exemplify this, let us take the case of the last experiment, where $M = 7$, $W = 25\frac{1}{4}$, $S = 4\frac{7}{12}$ feet, $t = 4''$; hence $F = 7 - \frac{32\frac{3}{4} \times 4\frac{7}{12}}{16\frac{1}{12} \times 16} = 6.417$; consequently the friction was to the weight of the rubbing body as 6.4167 to 25.75. And the great accuracy of determining the friction by this method is manifest from hence, that if an error of 1 inch had been made in the descent (and experiments carefully made may always determine the space to a much greater exactness) it would not have affected the conclusion $\frac{1}{600}$dth part of the whole.

5. We come in the next place to determine, whether friction, *cæteris paribus*, varies in proportion to the weight or pressure. Now if the whole quantity of the friction of a body, measured by a weight without inertia equivalent to the friction drawing the body backwards, increases in proportion to its weight, it is manifest, that the retardation of the velocity of the body arising from the friction will not be altered; for the

7 retardation

retardation varies as $\frac{\text{Quantity of friction}}{\text{Quantity of matter}}$; hence, if a body be put in motion upon the horizontal plane by any moving force, if both the weight of the body and the moving force be increaſed in the ſame ratio, the acceleration ariſing from that moving force will remain the ſame, becauſe the accelerative force varies as the moving force divided by the whole quantity of matter, and both are increaſed in the ſame ratio; and if the quantity of friction increaſes alſo as the weight, then the retardation ariſing from the friction will, from what has been ſaid, remain the ſame, and therefore the whole acceleration of the body will not be altered; conſequently the body ought, upon this ſuppoſition, ſtill to deſcribe the ſame ſpace in the ſame time. Hence, by obſerving the ſpaces deſcribed in the ſame time, when both the body and the moving force are increaſed in the ſame ratio, we may determine whether the friction increaſes in proportion to the weight. The following experiments were therefore made in order to aſcertain this matter.

Exp. 1. A body weighing 10 oz. by a moving force of 4 oz. deſcribed in 2″ a ſpace of 51 inches; by loading the body with 10 oz. and the moving force with 4 oz. it deſcribed 56 inches in 2″; and by loading the body again with 10 oz. and the moving force with 4 oz. it deſcribed 63 inches in 2″.

Exp. 2. A body, whoſe weight was 16 oz. by a moving force of 5 oz. deſcribed a ſpace of 49 inches in 3″; and by loading the body with 64 oz. and the moving force with 20 oz. the ſpace deſcribed in the ſame time was 64 inches.

Exp. 3. A body weighing 6 oz. by a moving force of 2½ oz. deſcribed 28 inches in 2″; and by loading the body with 24 oz. and the moving force with 10 oz. the ſpace deſcribed in the ſame time was 54 inches.

EXP. 4. A body weighing 8 oz. by a moving force of 4 oz. described 33¼ inches in 2″; and by loading the body with 8 oz. and the moving force with 4 oz. the space described in the same time was 47 inches.

EXP. 5. A body whose weight was 9 oz. by a moving force of 4½ oz. described 48 inches in 2″; and by loading the body with 9 oz. and the moving force with 4½ oz. the space described in the same time was 60 inches.

EXP. 6. A body weighing 10 oz. by a moving force of 3 oz. described 20 inches in 2″; by loading the body with 10 oz. and the moving force with 3 oz. the space described in the same time was 31 inches; and by loading the body again with 30 oz. and the moving force with 9 oz. the space described was 34 inches in 2″.

From these experiments, and many others which it is not necessary here to relate, it appears, that the space described is always increased by increasing the weight of the body and the accelerative force in the same ratio; and as the acceleration arising from the moving force continued the same, it is manifest, that the retardation arising from the friction must have been diminished, for the whole accelerative force must have been increased on account of the increase of the space described in the same time; and hence (as the retardation from friction varies as $\frac{\text{Quantity of friction}}{\text{Quantity of matter}}$) *the quantity of friction increases in a less ratio than the quantity of matter or weight of the body.*

6. We come now to the last thing which it was proposed to determine, that is, whether the friction varies by varying the surface on which the body moves. Let us call two of the surfaces A and *a*, the former being the greater, and the latter the less. Now the weight on every given part of *a* is as much greater

2 than

than the weight on an equal part of A, as A is greater than *a*; if therefore the friction was in proportion to the weight, *cæteris paribus*, it is manifest, that the friction on *a* would be equal to the friction on A, the whole friction being, upon such a supposition, as the weight on any given part of each surface multiplied into the number of such parts or into the whole area, which products, from the proportion above, are equal. But from the last experiments it has been proved, that the friction on any given surface increases in a less ratio than the weight; consequently the friction on any given part of *a* has a less ratio to the friction on an equal part of A than A has to *a*, and hence the friction on *a* is less than the friction on A, that is, the smallest surface has always the least friction. But as this conclusion is contrary to the generally received opinion, I have thought it proper to confirm the same by a set of experiments. But before I proceed to relate them, I will beg leave to recommend to those, who may afterwards be induced to repeat them, the following cautions, which are extremely necessary to be attended to. Great care must be taken that the two surfaces have exactly the same degree of roughness; in order to be certain of which, such bodies must be chosen as have no knots in them, and whose grain is so very regular that when the two surfaces are planed with a fine rough plane, their roughness may be the same, which will not be the case if the body be knotty, or the grain irregular, or if it happens not to run in the same direction on both surfaces. When you cannot depend on the surfaces having the same degree of roughness, the best way will be to paste some fine rough paper on each surface, which perhaps will give a more equal degree of roughness than can be obtained by any other method. Now as the proof which I have already given depends only on the motion of

of the body upon the *same* surface, it is not liable to any inaccuracy of the kind which the preceding cautions have been given to avoid, nor indeed to any other, and therefore it must be perfectly conclusive. In the following experiments the cautions mentioned above were carefully attended to.

EXP. 1. A body was taken whose flat surface was to its edge as 22 : 9, and with the same moving force the body described on its flat side 33¼ inches in 2″, and on its edge 47 inches in the same time.

EXP. 2. A second body was taken whose flat surface was to its edge as 32 : 3, and with the same moving force it described on its flat side 32 inches in 2″, and on its edge it described 37½ inches in the same time.

EXP. 3. I took another body and covered one of its surfaces, whose length was 9 inches, with a fine rough paper, and by applying a moving force, it described 25 inches in 2″; I then took off some paper from the middle, leaving only $\frac{3}{8}$ of an inch at the two ends, and with the same moving force it described 40 inches in the same time.

EXP. 4. Another body was taken which had one of its surfaces, whose length was 9 inches, covered with a fine rough paper, and by applying a moving force it described 42 inches in 2″; some of the paper was then taken off from the middle, leaving only 1$\frac{3}{8}$ inches at the two ends, and with the same moving force it described 54 inches in 2″; I then took off more paper, leaving only ¼ of an inch at the two ends, and the body then described, by the same moving force, 60 inches in the same time.

In the two last experiments the paper which was taken off the surface was laid on the body, that its weight might not be altered.

EXP.

Exp. 5. A body was taken whose flat surface was to its edge as 30 : 17; the *flat* side was laid upon the horizontal plane, a moving force was applied, and the stage was fixed in order to stop the moving force, in consequence of which the body would then go on with the velocity acquired until the friction had destroyed all its motion; when it appeared from a mean of 12 trials that the body moved, after its acceleration ceased, $5\frac{2}{3}$ inches before it stopped. The *edge* was then applied, and the moving force descended through the same space, and it was found, from a mean of the same number of trials, that the space described was $7\frac{1}{8}$ inches before the body lost all its motion, after it ceased to be accelerated.

Exp. 6. Another body was then taken whose flat surface was to its edge as 60 : 19, and, by proceeding as before, on the flat surface it described, at a mean of 12 trials, $5\frac{1}{3}$ inches, and on the edge $6\frac{11}{12}$ inches, before it stopped, after the acceleration ceased.

Exp. 7. Another body was taken whose flat surface was to its edge as 26 : 3, and the spaces described on these two surfaces, after the acceleration ended, were, at a mean of 10 trials, $4\frac{2}{3}$ and $7\frac{7}{10}$ inches respectively.

From all these different experiments it appears, that the smallest surface had always the least friction, which agrees with the consequence deduced from the consideration that the friction does not increase in so great a ratio as the weight; we may therefore conclude, that *the friction of a body does not continue the same when it has different surfaces applied to the plane on which it moves, but that the smallest surface will have the least friction.*

7. Having thus established, from the most decisive experiments, all that I proposed relative to friction, I think it proper, before

before I conclude, to give the result of my examination into the nature of the experiments which have been made by others; which were repeated, in order to see how far they were conclusive in respect to the principles which have been deduced from them. The experiments which have been made by all the authors that I have seen, have been thus instituted. To find what moving force would *just* put a body at rest in motion: and they concluded from thence, that the accelerative force was then equal to the friction; but it is manifest, that any force which will put a body in motion must be *greater* than the force which opposes its motion, otherwise it could not overcome it; and hence, if there were no other objection than this, it is evident, that the friction could not be very accurately obtained; but there is another objection which totally destroys the experiment so far as it tends to show the quantity of friction, which is the strong cohesion of the body to the plane when it lies at rest; and this is confirmed by the following experiments. 1st, A body of 12¾ oz. was laid upon an horizontal plane, and then loaded with a weight of 8 lb. and such a moving force was applied as would, when the body was just *put* in motion, continue that motion without any acceleration, in which case the friction must be just equal to the accelerative force. The body was then stopped, when it appeared, that the same moving force which had *kept* the body in motion before, would not *put* it in motion, and it was found necessary to take off 4¼ oz. from the body before the same moving force *would* put it in motion; it appears, therefore, that this body, when laid upon the plane at rest, acquired a very strong cohesion to it. 2dly, A body whose weight was 16 oz. was laid at rest upon the horizontal plane, and it was found that a moving force of 6 oz. would just *put* it in motion; but that a moving force of 4 oz.

would

would, when it was just put in motion, *continue* that motion without any acceleration, and therefore the accelerative force must *then* have been equal to the friction, and not when the moving force of 6 oz. was applied.

From these experiments therefore it appears, how very considerable the cohesion was in proportion to the friction when the body was in motion; it being, in the latter case, almost $\frac{1}{3}$, and in the former it was found to be very nearly equal to the whole friction. All the conclusions therefore deduced from the experiments, which have been instituted to determine the friction from the force necessary to *put* a body in motion (and I have never seen any described but upon such a principle) have manifestly been totally false; as such experiments only shew the resistance which arises from the cohesion and friction conjointly.

8. I shall conclude this part of the subject with a remark upon Art. 5. It appears from all the experiments which I have made, that the proportion of the increase of the friction to the increase of the weight was different in all the different bodies which were made use of; no general rule therefore can be established to determine this for *all* bodies, and the experiments which I have hitherto made have not been sufficient to determine it for the *same* body. At some future opportunity, when I have more leisure, I intend to repeat the experiments in order to establish, in some particular cases, the law by which the quantity of friction increases by increasing the weight. Leaving this subject therefore for the present, I shall proceed to establish a theory upon the principles which we have already deduced from our experiments.

PROPOSITION I.

Let e, f, g, (fig. 1.) *represent either a cylinder, or that circular section of a body on which it rolls down the inclined plane* CA *in consequence of its friction, to find the time of descent and the number of revolutions.*

As it has been proved in Art. 5. that the friction of a body does not increase in proportion to its weight or pressure, we cannot therefore, by knowing the friction on any other plane, determine the friction on CA; the friction therefore on CA can only be determined by experiments made upon *that* plane, that is, by letting the body descend from rest, and observing the space described in the first second of time; call that space a, and then, as by Art. 3. friction is a uniformly retarding force, the body must be uniformly accelerated, and consequently the whole time of descent in seconds will be $= \sqrt{\frac{AC}{a}}$. Now to determine the number of revolutions, let s be the center of oscillation to the point of suspension a *; then, because no force acting at a can affect the motion of the point s, that point, notwithstanding the action of the friction at a, will always have a motion parallel to CA uniformly accelerated by a force equal to that with which the body would be accelerated if it had no friction; hence, if $2m = 32\frac{1}{6}$ feet, the velocity acquired by the point s in the first second will be $= \frac{2m \times CB}{CA}$; now the excess of the ve-

* a and s are not fixed points in the body, but the former always represents that point of the body in contact with the plane, and the latter the corresponding center of oscillation.

locity of the point s above that of r (r being the center) is manifestly the velocity with which s is carried about r; hence the velocity of s about the center $= \frac{2m \times CB}{CA} - 2a = \frac{2m \times CB - 2a \times CA}{CA}$, consequently $rs : ra :: \frac{2m \times CB - 2a \times CA}{CA} : \frac{2m \times ra \times CB - 2a \times ra \times CA}{rs \times CA}$ = the velocity with which a point of the circumference is carried about the center, and which therefore expresses the force which accelerates the rotation; now as $2a$ expresses the accelerative force of the body down the plane, and the spaces described in the same time are in proportion to those forces, we have $2a : CA :: \frac{2m \times ra \times CB - 2a \times ra \times CA}{rs \times CA} : \frac{m \times ra \times CB - a \times ra \times CA}{a \times rs}$ the space which any point of the circumference describes about the center in the whole time of the body's descent down CA; which being divided by the circumference $p \times ra$ (where $p = 6.283$ &c.) will give $\frac{m \times BC - a \times AC}{p \times a \times rs}$ for the whole number of revolutions required.

Cor. 1. If $a \times CA = m \times BC$, the number of revolutions $= 0$, and therefore the body will then only slide; consequently the friction vanishes.

Cor. 2. Let $a'r's'$ (fig. 2.) be the next position of ars, and draw $tr'b$ parallel to sa, then will $s't$ represent the retardation of the center r arising from friction, and $a'b$ will represent the acceleration of a point of the circumference about its center; hence the retardation of the center : acceleration of the circumference about the center :: $s't : a'b$ :: (by sim. $\triangle$'s) $tr' : br' :: rs : ra$.

Cor. 3. If a' coincides with a, the body does not *slide* but only *roll*; now in this case $ss' : rr' :: as : ar$; but as ss' and rr' represent the ratio of the velocities of the points s and r,

they will be to each other as $\frac{2m \times BC}{CA} : 2a$ or as $m \times CB : a \times CA$; hence, when the body *rolls* without *sliding*, $as : ar :: m \times CB : a \times CA$.

Cor. 4. The time of descent down CA is $= \sqrt{\frac{AC}{a}}$; but by the last Cor. when the body *rolls* without *sliding*, $a = \frac{m \times ra \times BC}{sa \times AC}$, hence the time of descent in that case $= AC \sqrt{\frac{sa}{m \times ra \times BC}}$; now the time of descent, if there were no friction, would be $= \frac{AC}{\sqrt{m \times BC}}$, hence the time of descent, when the body *rolls* without *sliding* : time of free descent $:: \sqrt{sa} : \sqrt{ra}$.

Cor. 5. By the last Cor. it appears, that when the body just *rolls* without *sliding*, or when the friction is just equal to the accelerative force, the time of descent $= AC \sqrt{\frac{sa}{m \times ra \times BC}}$; now it is manifest, that the time of descent will continue the same, if the friction be increased, for the body will still freely roll, as no increase of the friction acting at a can affect the motion of the point s.

If the body be projected from C with a velocity, and at the same time have a rotatory motion, the time of descent and the number of revolutions may be determined from the common principles of uniformly accelerated motions, as we have already investigated the accelerative force of the body down the plane and of its rotation about its axis; it seems therefore unnecessary to lengthen out this paper with the investigations.

PRO-

PROPOSITION II.

Let the body be projected on an horizontal plane LM (fig. 3.) *with a given velocity, to determine the space through which the body will move before it stops, or before its motion becomes uniform.*

CASE I. 1. Suppose the body to have no rotatory motion when it begins to move; and let a = the velocity of projection *per* second measured in feet, and let the retarding force of the friction of the body, measured by the velocity of the body which it can destroy in one second of time, be determined by experiment and called F, and let x be the space through which the body would move by the time its motion was all destroyed when projected with the velocity a, and retarded by a force F; then, from the principles of uniformly retarded motion, $x = \frac{a^2}{2F}$, and if t = time of describing that space, we have $t = \frac{a}{F}$, and hence the space described in the first second of time $= \frac{2a - F}{2}$. Now it is manifest, that when the rotatory motion of the body about its axis is equal to its progressive motion, the point a will be carried backwards by the *former* motion as much as it is carried forwards by the *latter*; consequently the point of contact of the body with the plane will then have no motion in the direction of the plane, and hence the friction will at that instant cease, and the body will continue to *roll* on uniformly without *sliding* with the velocity which it has at that point. Put therefore z = the space described from the commencement of the motion till it becomes uniform, then the body being uniformly retarded, the spaces from the end of

the motion vary as the squares of the velocities, hence $\frac{a^2}{2F} : a^2 \; (:: 1 : 2F) :: \frac{a^2}{2F} - z : a^2 - 2Fz =$ square of the progressive velocity when the motion becomes uniform; therefore the velocity destroyed by friction $= a - \sqrt{a^2 - 2Fz}$; hence, as the velocity generated or destroyed in the same time is in proportion to the force, we have by Cor. 2. Prop. 1. $rs : ra :: a - \sqrt{a^2 - 2Fz} : \frac{ra}{rs} \times \overline{a - \sqrt{a^2 - 2Fz}}$ the velocity of the circumference efg generated about the center, consequently $\sqrt{a^2 - 2Fz} = \frac{ra}{rs} \times \overline{a - \sqrt{a^2 - 2Fz}}$, and hence $z = \frac{\overline{rs^2 + 2rs \times ra} \times a^2}{as^2 \times 2F}$ the space which the body describes before the motion becomes uniform.

2. If we substitute this value of z into the expression for the velocity, we shall have $a \times \frac{ra}{rs}$ for the velocity of the body when its motion becomes uniform; hence therefore it appears, that the velocity of the body, when the friction ceases, will be the same whatever be the quantity of the friction. If the body be the circumference of a circle, it will always lose half the velocity before its motion becomes uniform.

CASE II. 1. Let the body, besides having a progressive velocity in the direction LM (fig. 3.) have also a rotatory motion about its center in the direction gfe, and let v represent the initial velocity of any point of the circumference about the center, and suppose it first to be less than a; then friction being a uniformly retarding force, no alteration of the velocity of the point of contact of the body upon the plane can affect the quantity of friction; hence the progressive velocity of the body will be the same as before, and consequently the rotatory velo-

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city generated by friction will also be the same, to which if we add the velocity about the center at the beginning of the motion, we shall have the whole rotatory motion; hence therefore, $v + \frac{ra}{rs} \times \overline{a - \sqrt{a^2 - 2Fz}} = \sqrt{a^2 - 2Fz}$, consequently $z = \frac{a^2 \times as^2 - \overline{v \times rs + a \times ra}^2}{2F \times as^2}$ the space described before the motion becomes uniform.

2. If this value of z be substituted into the expression for the velocity, we shall have $\frac{v \times rs + a \times ra}{as}$ for the velocity when the friction ceases.

3. If $v = a$, then $z = o$, and hence the body will continue to move uniformly with the first velocity.

4. If v be greater than a, then the rotatory motion of the point a on the plane being greater than its progressive motion and in a contrary direction, the absolute motion of the point a upon the plane will be in the direction ML, and consequently friction will now act in the direction LM in which the body moves, and therefore will accelerate the *progressive* and retard the *rotatory* motion; hence it appears, that *the progressive motion of a body may be* ACCELERATED *by friction.* Now to determine the space described before the motion becomes uniform, we may observe, that as the progressive motion of the body is now accelerated, the velocity after it has described any space z will be $= \sqrt{a^2 + 2Fz}$, hence the velocity acquired $= \overline{\sqrt{a^2 + 2Fz} - a}$, and consequently the rotatory velocity destroyed $\frac{ra}{rs} \times \overline{\sqrt{a^2 + 2Fz} - a}$, hence $v - \frac{ra}{rs} \times \overline{\sqrt{a^2 + 2Fz} - a} = \sqrt{a^2 + 2Fz}$, therefore $z = \frac{rs \times v + ra \times a^2 - a^2 \times as^2}{2F \times as^2}$ the space required.

5. If

5. If $a = o$, or the body be placed upon the plane without any progressive velocity, then $z = \frac{rs^2 \times v^2}{2F \times as}$.

CASE III. 1. Let the given rotatory motion be in the direction $g\,e\,f$; then as the friction must in this case always act in the direction ML, it must continually tend to destroy both the progressive and rotatory motion. Now as the velocity destroyed in the same time is in proportion to the retarding force, and the force which retards the *rotatory* is to the force which retards the *progressive* velocity by Cor. 2. Prop. 1. as $r\,a : r\,s$, therefore if v be to a as $r\,a$ is to $r\,s$, then the retarding forces being in proportion to the velocities, both motions will be destroyed together, and consequently the body, after describing a certain space, will rest; which space, being that described by the body uniformly retarded by the force F, will, from what was proved in Case I. be equal to $\frac{a^2}{2F}$.

2. If v bears a greater proportion to a than $r\,a$ does to $r\,s$, it is manifest, that the rotatory motion will not be all destroyed when the progressive is; consequently the body, after it has described the space $\frac{a^2}{2F}$, will return back in the direction ML; for the progressive motion being then destroyed, and the rotatory motion still continuing in the direction $g\,e\,f$, will cause the body to return with an accelerative velocity until the friction ceases by the body's beginning to roll, after which it will move on uniformly. Now to determine the space described before this happens, we have $r\,s : r\,a :: a : \frac{ra \times a}{rs}$ the rotatory velocity destroyed when the progressive is all lost; hence $v - \frac{ra \times a}{rs} = \frac{v \times rs - a \times ra}{rs}$ = the rotatory velocity at that time, which

being

being ſubſtituted for v in the laſt article of Caſe II. gives $\frac{\overline{v+rs-a\times ra}^2}{2F\times as^2}$ for the ſpace deſcribed before the motion becomes uniform.

3. If v has a leſs proportion to a than ra has to rs, it is manifeſt, that the *rotatory* motion will be deſtroyed before the *progreſſive*; in which caſe a rotatory motion will be generated in a contrary direction until the two motions become equal, when the friction will inſtantly ceaſe, and the body will then move on uniformly. Now $ra : rs :: v : \frac{v\times rs}{ra}$ the progreſſive velocity deſtroyed when the rotatory velocity ceaſes, hence $a-\frac{v\times rs}{ra}=\frac{a\times ra-v\times rs}{ra}$ = progreſſive velocity when it begins its rotatory motion in a contrary direction; ſubſtitute therefore this quantity for a in the expreſſion for z in Caſe I. and we have $\frac{\overline{rs^2+2rs\times ra}\times\overline{a\times ra-v\times rs}^2}{as^2\times ar^2\times 2F}$ for the ſpace deſcribed after the rotatory motion ceaſes before the motion of the body becomes uniform. Now to determine the ſpace deſcribed before the rotatory motion was all deſtroyed, we have (as the ſpace from the end of a uniformly retarded motion varies as the ſquare of the velocity) $a^2 : \frac{a^2}{2F} :: \frac{\overline{a\times ra-v\times rs}^2}{ra^2} : \frac{\overline{a\times ra-v\times rs}^2}{2F\times ra^2}$ the ſpace that could have been deſcribed from the time that the rotatory velocity was deſtroyed, until the progreſſive motion would have been deſtroyed had the fricton continued to act; hence $\frac{a^2}{2F}-\frac{\overline{a\times ra-v\times rs}^2}{2F\times ra^2}=\frac{2av\times ra\times rs-v^2\times rs^2}{2F\times ra^2}$ = the ſpace deſcribed when the rotatory motion was all deſtroyed, hence

$$\frac{\overline{rs^2+2rs\times ra}\times\overline{a\times ar-v\times r}^2}{as^2\times ar^2\times 2F}+\frac{2av\times ra\times rs-v^2\times rs^2}{2F\times ra^2}=$$ whole ſpace deſcribed by the body before its motion becomes uniform.

DEFINITION.

The CENTER *of* FRICTION *is that point in the base of a body on which it revolves, into which if the whole surface of the base, and the mass of the body were collected, and made to revolve about the center of the base of the given body, the angular velocity destroyed by its friction would be equal to the angular velocity destroyed in the given body by its friction in the same time.*

PROPOSITION III.

To find the center of friction.

Let FGH (fig. 4.) be the base of a body revolving about its center C, and suppose about *a*, *b*, *c*, &c. to be indefinitely small parts of the base, and let A, B, C, &c. be the corresponding parts of the solid, or the prismatic parts having *a*, *b*, *c*, &c. for their bases; and P the center of friction. Now it is manifest, that the decrement of the angular velocity must vary as the whole diminution of the momentum of rotation caused by the friction *directly*, and as the whole momentum of rotation or effect of the inertia of all the particles of the solid *inversely*; the *former* being employed in diminishing the angular velocity, and the *latter* in opposing that diminution by the endeavour of the particles to persevere in their motion. Hence, if the effect of the friction varies as the effect of the inertia, the decrements of the angular velocity in a given time will be equal. Now as the quantity of friction (as has been proved from experiments) does not depend on the velocity, the effect of the friction of the elementary parts of the base *a*, *b*, *c*, &c.

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will be as $a \times aC$, $b \times bC$, $c \times cC$, &c. also the effect of the inertia of the corresponding parts of the body will be as $A \times aC^2$, $B \times bC^2$, $C \times cC^2$, &c. Now when the whole surface of the base and mass of the body are concentrated in P, the effect of the friction will be as $\overline{a+b+c+}$ &c. $\times$ CP, and of the inertia as $\overline{A+B+C+}$ &c. $\times$ CP^2; consequently $a \times aC + b \times bC + c \times cC$ + &c. : $\overline{a+b+c+}$ &c. $\times$ CP :: $A \times aC^2 + B \times bC^2 + C \times cC^2$ + &c. : $\overline{A+B+C+}$ &c. $\times$ CP^2; and hence

$$CP = \frac{\overline{A \times aC^2 + B \times bC^2 + C \times cC^2 +} \text{\&c.} \times \overline{a+b+c+} \text{\&c.}}{a \times aC + b \times bC + c \times cC + \text{\&c.} \times \overline{A+B+C+} \text{\&c.}} =$$ (if S = the sum of the products of each particle into the square of its distance from the axis of motion, T = the sum of the products of each part of the base into its distance from the center, s = the area of the base, t = the solid content of the body) $\frac{S \times s}{T \times t}$.

PROPOSITION IV.

Given the velocity with which a body begins to revolve about the center of its base, to determine the number of revolutions which the body will make before all its motion be destroyed.

Let the friction, expressed by the velocity which it is able to destroy in the body if it were projected in a right line horizontally in one second, be determined by experiment, and called F; and suppose the initial velocity of the center of friction P about C to be a. Then conceiving the whole surface of the base and mass of the body to be collected into the point P, and (as has been proved in Prop. II.) $\frac{a^2}{2F}$ will be the space which the body so concentrated will describe before all its motion be destroyed;

 hence

hence if we put $z = PC$, $p =$ the circumference of a circle whose radius is unity, then will $pz =$ circumference described by the point P; consequently $\frac{a^2}{2pzF} =$ the number of revolutions required.

Cor. If the solid be a cylinder and r be the radius of its base, then $z = \frac{3r}{4}$, and therefore the number of revolutions $= \frac{2a^2}{3prF}$.

PROPOSITION V.

To find the nature of the curve described by any point of a body affected by friction, when it descends down any inclined plane.

Let efg (fig. 5.) be the body, the points a, r, s, as in Prop. I. and conceive st, rn, to be two indefinitely small spaces described by the points s and r in the same time, and which therefore will represent the velocities of those points; but from Prop. I. the ratio of these velocities is expressed by $m \times CB : a \times CA$, hence $st : rn :: m \times CB : a \times CA$. With the center r let a circle vw be described touching the plane LM which is parallel to AC at the point b, and let the radius of this circle be such that, conceiving it to descend upon the plane LM along with the body descending on CA, the point b may be at rest, or the circle may roll without sliding. To determine which radius, produce rs to x, parallel to which draw ndy, and produce nt to z; now it is manifest, that in order to answer the conditions above-mentioned, the velocity of the point x must be to the velocity of the point r as $2 : 1$, that is, $zx : yx :: 2 : 1$, hence $zy = yx = nr$. Now $zy : dt$ $(:: ny : nd)$ $:: rx : rs$; therefore $dt = \frac{rs}{rx} \times zy = \frac{rs}{rx} \times nr$, hence ts $(= td + ds = td + nr =$

$\frac{rs}{rx} \times nr + nr) = \frac{rs+rx}{rx} \times nr$, consequently $\frac{rs+rx}{rx} : 1 :: ts : nr ::$ (from what is proved above) $m \times CB : a \times CA$; therefore $a \times CA \times rs + a \times CA \times rx = m \times CB \times rx$, hence $rx = \frac{a \times CA \times rs}{m \times CB - a \times CA}$ the radius of the circle which rolling down the inclined plane LM, and carrying the body with it, will give the true ratio of its progressive to its rotatory motion, and consequently that point of the circle which coincides with any given point of the body will, as the circle revolves upon the line LM, describe the same curve as the corresponding point of the body; but as the nature of the curve described by any point of a circle revolving upon a straight line is already very well known, it seems unnecessary to give the investigation.

By a method of reasoning, not very different, may the nature of the curve, which is described by any point of a body moving upon an horizontal plane, and affected by friction, be determined.

XI. *Obſervations and Experiments on the Light of Bodies in a State of Combuſtion. By the Rev. Mr.* Morgan; *communicated by the Rev.* Richard Price, *LL.D. F.R.S.*

Read January 27, 1785.

THE diſcuſſion which I now wiſh to lay before the Royal Society is nothing more than a ſeries of facts, and of concluſions which ſeem to flow from thoſe facts, and from an attention to the following data.

I. That light is a body, and like all other bodies ſubject to the laws of attraction.

II. That light is an heterogeneous body, and that the ſame attractive power operates with different degrees of force on its different parts.

III. That the light which eſcapes from combuſtibles when decompoſed by heat, or by any other means, was, previous to its eſcape, a component part of thoſe ſubſtances.

It is an obvious concluſion from theſe data, that when the attractive force, by which the ſeveral rays of light are attached to a body, is weakened, ſome of thoſe rays will eſcape

escape sooner than others. Those which are united with the least degree of power will escape first, and those which adhere to it most strongly will (if I may be allowed the expression) be the last to quit their basis. We may here have recourse to a familiar fact, which is analogous to this, and will illustrate it. If a mixture, consisting of equal parts of water, of spirits of wine, and of other more fixed bodies, be placed over a fire; the first influence of that heat, to which all the ingredients are alike exposed, will carry off the spirits of wine only. The next will carry off the spirits of wine blended with particles of water. A still greater degree of heat will blend with the vapour which escapes a part of the more fixed bodies, till at length what evaporates will be a mixture of all the ingredients which were at first exposed to the fire. In like manner, when the surface of a combustible is in a state of decomposition, those parts which are the least fixed, or which are united to it with the least force, will be separated first. Amongst these the indigo rays of light will make the earliest appearance. By increasing the heat we shall mix the violet with the indigo. By increasing it still more we shall add the blue and the green to the mixture, till at length we reach that intensity of heat which will cause all the rays to escape at the same instant, and make the flame of a combustible perfectly white. It is not my present design to shew why the most refrangible rays are the first which escape from a burning body, but to enumerate the several facts which seem to shew, that such a general law takes place in combustion; and that the various colours of bodies in this state are uniformly regulated by that decrease of attractive force now described.

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By examining the flame of a common candle we may obſerve, that its loweſt extremities, or the part in which the black colour of the wick terminates, diſcharges the leaſt heat; and that, as the vertex of the flame is approached, a ſucceſſive order of parts is paſſed through, in which the loweſt is continually adding to the heat of what is juſt above it, till we come to the top of the flame, near which all the heat is collected into a focus. At the loweſt extremity, however, where the heat is inconſiderable, a blue colour may be always obſerved; and from this appearance, amongſt others, it may, I think, be ſafely concluded, that the blue rays are ſome of thoſe which eſcape from combuſtibles in an early period of their decompoſition; and that if the decompoſition could be examined in a period ſtill more early, the colour of their flame would be violet. By an *a priori* deduction of this kind, I was led to watch the appearances of a candle more attentively; whence I found that to the external boundary of a common candle is annexed a filament of light, which, if proper care be taken to prevent the eſcape of too much ſmoke, will appear moſt beautifully coloured with the violet and indigo rays. To the preceding inſtance of a common candle many facts may be added, which ſpeak a ſimilar language. If ſulphur or æther is burned, or any of thoſe combuſtibles whoſe vapour is kindled in a ſmall degree of heat, a blue flame will appear, which, if examined by the priſm, will be found to conſiſt of the violet, the indigo, the blue, and ſometimes a ſmall quantity of the green rays. The beſt mode, however, of ſhewing the eſcape of ſome rays by that degree of heat which will not ſeparate others till increaſed, is the following. Give a piece of brown paper a ſpherical form, by preſſing it upon

upon any hard globular substance. Gradually bring the paper, thus formed, to that distance from the candle at which it will begin to take fire. In this case a beautiful blue flame may be seen, hanging as it were by the paper till a hole is made in it, when the flame, owing to the increased action of the air upon all parts of it, becomes white, though the edges still continue of a blue or violet colour. As a confirmation of what I have concluded from the preceding facts, it may be observed, that the very flame which, when exposed to a certain degree of heat, emitted the most refrangible rays only, will, if exposed to a greater degree of heat, emit such as are less refrangible. The flames of sulphur, spirits of wine, &c. when suddenly exposed to the heat of a reverberatory, change their blue appearance for that which is perfectly white. But to gain a more striking diversity of this fact, I adopted Mr. MELVILL's mode of examining bodies whilst on fire. I darkened my room, and placed between my eye and the combustible a sheet of pasteboard, in the center of which I made a small perforation. As the light of the burning body escaped through this perforation, I examined it with a prism, and observed the following appearances. When the spirits of wine were set on fire, all the rays appeared in the perforation; but the violet, the blue, and the green, in the greatest abundance. When the combustion of the spirits was checked by throwing some sal ammoniac into the mixture, the red rays disappeared; but when, by the long continuance of the flame, the sal ammoniac was rendered so hot as to increase, rather than diminish the combustion, the red rays again appeared at the perforation. If the screen was managed so that the different parts of the flame might be examined separately, I always observed that

the colours varied according to the degree of heat. At the base of the flame, or where the heat was least, the indigo, the violet, and a very small tinge of the blue and green appeared. As I approached the vertex of the flame, the rays which escaped became more and more numerous till I reached the top, when all the rays appeared in the prism. It should be attended to, that when the red rays first made their appearance, their quantity was small, and gradually increased as the eye in its examination approached that part where the heat was greatest. Mr. MELVILL, when he made some of the preceding experiments, observed, that the yellow rays frequently escaped in the greatest abundance; but this singularity proceeded from some circumstances which escaped his attention. In consequence of mixing acids or salts with the burning spirits, a very dense fume of unignited particles arises, and before the rays of the burning body arrive at the perforation where the prism catches them, they must pass through a medium which will absorb a great part of the indigo and the violet. On the other hand, owing to the imperfection of the decomposition, very few of the red rays are separated from their basis, and consequently the yellow and the orange rays are those alone which pass through the unburnt smoke of the flame.

I would now proceed with observing, that, besides the increase or decrease of heat, there are other modes of retarding or accelerating the combustion of bodies, by which also may be examined some of the preceding illustrations.

1. A candle burns most rapidly and brilliantly in dephlogisticated air.

2. The

2. The blue colour of a sulphureous flame in pure air is changed into a dazzling white.

3. The flame of inflammable air, when mixed with nitrous air, is green. It is white strongly tinged with the indigo and violet when mixed with common air; but when mixed with dephlogisticated air, or surrounded by it, the brilliancy of its flame is most singularly beautiful.

If the preceding facts prove that light, as an heterogeneous body, is gradually decomposed during combustion; if they prove, likewise, that the indigo rays escape with the least heat, and the red with the greatest; I think we may rationally account for several singularities in the colours of different flames. If a piece of paper, impregnated with a solution of copper in the nitrous acid, be set on fire, the bottom and sides of the flame are always tinged with green. Now this flame is evidently in that weak state of decomposition, in which the most refrangible rays escape in the greatest abundance; but of these rays the green escape most plentifully through the unignited vapour and that portion of the atmosphere which separates the eye from the flame. The peculiarity which I have now endeavoured to account for may be observed in the greatest perfection in brass founderies. The heat in this instance, though very strong, is scarcely adequate to the decomposition of the metallic vapour which escapes from the melted brass. A very singular flame therefore appears to the eye; for while its edges are green, its body is such as to give the objects around a very pallid or ghastly appearance, which is the consequence of its wanting that portion of red rays which is necessary to make a perfect white.

The most singular phænomenon attending a burning body is, perhaps, the red appearance it assumes in its last stage of combustion. The preceding facts and observations may, I think, help us to explain it.

1. After a body has continued to burn for some time, its external surface is to be regarded as having lost a great portion if not the whole of those rays which the first application of heat was able to separate. But these rays were the indigo, the violet, the blue, and perhaps the green. Nothing, therefore, will remain to be separated, but the yellow, the orange, and the red. Consequently, the combustion of the body, in its last state of decomposition, can assume no other than a reddish appearance. But

2. Let us consider the external surface of the combustible as annexed to an inner surface, which may be partly, but not so perfectly decomposed as itself: for the violence of the heat will be found to lessen in its effects the nearer it approaches to the center of the substance which is exposed to it. Hence we are to consider the parts which are just covered by the external surface as having lost less of their component light than the external surface itself. Or the former may retain the green rays when the latter has lost both indigo, violet, blue, and green.

3. Those parts which are nearer the center of the body than either of the preceding must, as they are further from the greatest violence of the heat, have lost proportionably fewer of their rays. Or while the more external parts may have lost all but the red, these may have lost only the indigo and violet.

4. The most central parts may be unaffected by the heat; and whenever the fire does reach these parts, they will immediately discharge their indigo rays, and be decomposed in the gradual

gradual manner which I have already described. A piece of rotten wood, whilst burning, will exemplify and confirm the preceding illustration. When influenced by the external air only, if examined through a prism, no rays will be found to escape but the orange and the red. By blowing upon the burning wood with a pair of bellows, the combustion, being increased, will affect those internal parts of the body which were not acted upon before. These parts, therefore, will begin to lose their light, and a prism will shew the green, the blue, the violet, and indigo, all appearing in succession. Appearances similar to the preceding may be observed in a common kitchen fire. When it is faintest, its colour is most red, the other rays having been emitted, and the combustion at a stand; but by blowing upon it in this state, its brightness will be increased, and more and more of the rays which are yielded by the internal parts of the body will come to the eye, till at length, by continuing to blow, the combustion will be made so complete as to yield all the rays, or to make it appear perfectly white.

Many are the varieties discoverable in the flames and in the appearances of fixed burning bodies to which the preceding observations may be applied; but, to avoid unnecessary amplification I will take notice only of what appears to me an imperfection in Sir Isaac Newton's definition of flame. He conjectures, that it may be a vapour heated red-hot. I think I should rather say, that flame is an instance of combustion whose colour will be determined by the degree of decomposition which takes place. If it be very imperfect, the most refrangible rays only will appear. If it be very perfect, all the rays will appear, and its flame will be brilliant in

proportion

proportion to this perfection. There are flames, however, which consist of burning particles, whose rays have partly escaped before they ascended in the form of vapour. Such would be the flame of a red-hot coal, if exposed to such a heat as would gradually disperse it into vapour. When the fire is very low under the furnace of an iron foundery, at the upper orifice of the chimney a red flame of this kind may be seen, which is different from the flame that appears immediately after fresh coals have been thrown upon the fire; for, in consequence of adding such a supply to the burning fuel, a vast column of smoke ascends, and forms a medium so thick as to absorb most of the rays excepting the *red*.

Experiments on electric light.

If we would wish to procure any degree of certainty in any hypothesis which we may form concerning electrical light, perhaps the following general deductions may be of some service to us.

1. There is no fluid or solid body in its passage through which the electric fluid may not be made luminous. In water, spirits, oil, animal fluids of all kinds, the discharge of a Leyden phial of almost any size will appear very splendid, provided we take care to place them in the circuit, so that the fluid may not pass through too great a quantity of them. My general method is to place the fluid, on which I mean to make the experiment, in a tube three-quarters of an inch in diameter, and four inches long. I stop up the orifices of the tube with two corks, through which I push two pointed wires, so that the points may approach within one-eighth of an inch to each

other. The fluid in passing through the interval which separates the wires is always luminous, if a force be used sufficiently strong. I should observe, that the glass tube, if not very thick, always breaks when this experiment succeeds. To make the passage of the fluid luminous in the acids, they must be placed in capillary tubes, and two wires introduced, as in the preceding experiment, whose points shall be very near each other. It is a well known fact, that the discharge of a small Leyden phial in passing over a strip of gold, silver, or Dutch metal leaf, will appear very luminous. By conveying the contents of a jar, measuring two gallons, over a strip of gold leaf one-eighth of an inch in diameter, and a yard long, I have frequently given the whole a dazzling brightness. I cannot say, that a much greater length might not have been made very splendid, nor can I determine to what length the force of a battery might be made luminous in this manner. We may give this experiment a curious diversity, by laying the gold or silver leaf on a piece of glass, and then placing the glass in water; for the whole gold leaf will appear most brilliantly luminous in the water by exposing it, thus circumstanced, to the explosion of a battery.

2. The difficulty of making any quantity of the electrical fluid luminous in any body increases as the conducting power of that body increases.

EXP. I. In order to make the contents of a jar luminous in boiling water, a much higher charge is necessary than would be sufficient to make it luminous in cold water, which is universally allowed to be the worst conductor.

EXP. II. I have various reasons for believing the acids to be very good conductors. If therefore into a tube, filled with water, and circumstanced as I have already described, a few drops

drops of either of the mineral acids are poured, it will be almoſt impoſſible to make the fluid luminous in its paſſage through the tube.

EXP. III. If a ſtring*, whoſe diameter is one-eighth of an inch and whoſe length is ſix or eight inches, is moiſtened with water, the contents of a jar will paſs through it luminouſly, but no ſuch appearance can be produced by any charge of the ſame jar, provided the ſame ſtring be moiſtened with one of the mineral acids. To the preceding inſtance we may add the various inſtances of metals which will conduct the electrical fluid without any appearance of light, in circumſtances the ſame with thoſe in which the ſame force would have appeared luminous in paſſing through other bodies whoſe conducting power is leſs. But I proceed to obſerve,

III. That the eaſe with which the electrical fluid is rendered luminous in any particular body is increaſed by increaſing the rarity of the body. The appearance of a ſpark, or of the diſcharge of a Leyden phial, in rarefied air is well known. But we need not reſt the truth of the preceding obſervation on the ſeveral varieties of this fact; ſimilar phænomena attend the rarefaction of æther, of ſpirits of wine, and of water.

EXP. IV. Into the orifice of a tube, 48 inches long, and two-thirds of an inch in diameter, I cemented an iron ball, ſo as to bear the weight which preſſed upon it when I filled the tube with quickſilver, leaving only an interval at the open end, which contained a few drops of water. Having inverted the tube, and plunged the open end of it into a baſon of mercury, the mercury in the tube ſtood nearly half an inch lower than it

* The thickneſs and diameter of the ſtring ſhould be regulated by the force we employ.

did

did in a barometer at the same instant, owing to the vapour which was formed by the water. But through this rarefied water the electrical spark passed as luminously as it does through air equally rarefied.

EXP. V. If, instead of water, a few drops of spirits of wine are placed on the surface of the mercury, phænomena similar to those of the preceding experiment will be discovered, with this difference only, that as the vapour in this case is more dense, the electrical spark in its passage through it is not quite so luminous as it is in the vapour of water.

EXP. VI. Good æther substituted in the room of the spirits of wine will press the mercury down so low as the height of 16 or 17 inches. The electrical fluid in passing through this vapour (unless the force be very great indeed) is scarcely luminous. But if the pressure on the surface of the mercury in the bason be gradually lessened by the aid of an air-pump, the vapour will become more and more rare, and the electric spark in passing through it more and more luminous.

EXP. VII. I could not discover that any vapour escaped from the mineral acids when exposed *in vacuo*. To give them, therefore, greater rarity or tenuity, I found different methods necessary. With a fine camel-hair pencil, dipped in the vitriolic, the nitrous, or the marine acid, I drew upon a piece of glass a line about one-eighth of an inch broad. In some instances I extended this line to the length of 27 inches, and found that the contents of an electric battery, consisting of 10 pint phials coated, would pass over the whole length of this line with the greatest brilliancy. If by widening the line, or by laying on a drop of the acid, its quantity was increased in any particular part, the charge, in passing through that part, never appeared luminous. Water, spirits of wine, circum-

ſtanced ſimilarly to the acids in the preceding experiment, were attended with ſimilar, but not equal effects, becauſe, in conſequence of the inferiority of their conducting power, it was neceſſary to make the line through which the charge paſſed conſiderably ſhorter.

4. The brilliancy or ſplendor of the electric fluid in its paſſage through any body is always increaſed by leſſening the dimenſions of that body. I would explain my meaning by ſaying, that a ſpark, or the diſcharge of a battery which we might ſuppoſe equal to a ſphere one quarter of an inch in diameter, would appear much more brilliant if the ſame quantity of fluid is compreſſed into a ſphere one-eighth of an inch in diameter. This obſervation is the obvious conſequence of many known facts. If the machine be large enough to afford a ſpark whoſe length is nine or ten inches, this ſpark may be ſeen ſometimes forming itſelf into a bruſh, in which ſtate it occupies more room, but appears very faintly luminous. At other times the ſame ſpark may be ſeen dividing itſelf into a variety of ramifications which ſhoot into the ſurrounding air. In this caſe, likewiſe, the fluid is diffuſed over a large ſurface, and in proportion to the extent of that ſurface, ſo is the faintneſs of the appearance. A ſpark, which in the open air cannot exceed one quarter of an inch in diameter, will appear to fill the whole of an exhauſted receiver four inches wide and eight inches long. But in the former caſe it is brilliant, and in the latter it grows fainter and fainter as the ſize of the receiver increaſes. To prove the obſervation, which I think may be juſtified by the preceding facts, I made the following experiments.

EXP. VIII. To an inſulated ball, four inches in diameter, I fixed a ſilver thread, about four yards long. This thread, at the end which was remoteſt from the ball, was fixed to another

5 inſulated

insulated substance. I brought the ball within the striking distance of my conductor, and the spark in passing from the conductor to the ball appeared very brilliant; but the whole length of the silver thread appeared faintly luminous at the same instant. In other words, when the spark was confined within the dimensions of a sphere one-eighth of an inch in diameter, it was bright, but, when diffused over the surface of air which received it from the thread, its light became so faint as to be seen only in a dark room. If I lessened the surface of air which received the spark by shortening the thread, I never failed to increase the brightness of the appearance.

EXP. IX. To prove that the faintness of the electric light *in vacuo* depends on the enlarged dimensions of the space through which it is diffused, we have nothing more to do than to introduce two pointed wires into the vacuum, so that the fluid may pass from the point of the one to the point of the other, when the distance between them is not more than the one-tenth of an inch. In this case we shall find a brilliancy as great as in the open air.

EXP. X. Into a Torricellian vacuum, 36 inches in length, I conveyed as much air as would have filled two inches only of the exhausted tube, if it were inverted in water. This quantity of air afforded resistance enough to condense the fluid as it passed through the tube into a spark 38 inches in length. The brilliancy of the spark in condensed air, in water, and in all substances through which it passes with difficulty, depends on principles similar to those which account for the preceding facts. I would now proceed to shew,

5. That in the appearances of electricity, as well as in those of burning bodies, there are cases in which all the rays of light do not escape; and that the most refrangible rays are those

which escape first or most easily. The electrical brush is always of a purple or bluish hue. If you convey a spark through a Torricellian vacuum, made * *without* boiling the mercury in the tube, the brush will display the indigo rays. The spark, however, may be divided and weakened even in the open air, so as to yield the most refrangible rays only.

EXP. XI. To an insulated metallic ball, four inches in diameter, I fixed a wire a foot and a half long. This wire terminated in four ramifications, each of which was fixed to a metallic ball half an inch in diameter, and placed at an equal distance from a metallic plate, which communicated by metallic conductors with the ground. A powerful spark, after falling on the large ball at one extremity of the wire, was divided in its passage from the four small balls to the metallic plate. When I examined this division of the fluid in a dark room, I discovered some little ramifications which yielded the indigo rays only: indeed, at the edges of all weak sparks the same purple appearance may be discovered. We may likewise observe, that the nearer we approach the center of the spark, the greater is the brilliancy of its colour. But I would now wish to shew

6. That the influence of different media on electrical light is analogous to their influence on solar light, and will help us to account for some very singular appearances.

EXP. XII. Let a pointed wire, having a metallic ball fixed to one of its extremities, be forced obliquely into a piece of wood, so as to make a small angle with the surface of the wood, and to make

* If the Torricellian vacuum is made with mercury perfectly purged of air, it becomes a perfect non-conductor. This, I believe, will be proved decisively by some experiments which I hope will be soon communicated to the Royal Society.

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the point lie about one-eighth of an inch below the surface. Let another pointed wire, which communicates with the ground, be forced in the same manner into the same wood, so that its point likewise may lie about one-eighth of an inch below the surface, and about two inches distant from the point of the first wire. Let the wood be insulated, and a strong spark which strikes on the metallic ball will force its passage through the interval of wood which lies between the points, and appear as red as blood. To prove that this appearance depends on the wood's absorption of all the rays but the red, I would observe, that the greater the depth of the points is below the surface, the less mixed are the red rays. I have been able sometimes, by increasing or diminishing the depth of the points, to give the spark the following succession of colours. When they were deepest below the surface, the red only came to the eye through a prism. When they were raised a little nearer the surface, the red and orange appeared. When nearer still, the yellow; and so on till, by making the spark pass through the wood very near its surface, all the rays were at length able to reach the eye. If the points be only one-eighth of an inch below the surface of soft deal wood, the red, the orange, and the yellow rays will appear as the spark passes through it. But when the points are at an equal depth in a harder piece of wood (such as box) the yellow, and perhaps the orange, will disappear. As a farther proof that the phænomena I am describing are owing to the interposition of the wood, as a medium which absorbs some of the rays and suffers others to escape, it may be observed, that when the spark strikes very brilliantly on one side of the piece of deal, on the other side it will appear very red. In like manner a red appearance may be given to a spark which strikes

brilliantly

brilliantly over the inſide of a tube, merely by ſpreading ſome pitch very thinly over the outſide of the ſame tube.

EXP. XIII. I would now give another fact, whoſe ſingularities depend very much on the influence of the medium through which the electrical light is made to paſs. If into a Torricellian vacuum, of any length, a few drops of æther are conveyed, and both ends of the vacuum are ſtopped up with metallic conductors, ſo that a ſpark may paſs through it, the ſpark in its paſſage will aſſume the following appearances. When the eye is placed cloſe to the tube, the ſpark will appear perfectly white. If the eye is removed to the diſtance of two yards, it will appear green; but at the diſtance of ſix or ſeven yards, the colour of the ſpark will be reddiſh. Theſe changes evidently depend on the quantity of medium through which the the light paſſes; and the red light more particularly, which we ſee at the greateſt diſtance from the tube, is accounted for on the ſame principle as the red light of a diſtant candle or a beclouded ſun.

EXP. XIV. Dr. PRIESTLEY long ago obſerved the red appearance of the ſpark when paſſing through inflammable air. But this appearance is very much diverſified by the quantity of medium, through which you look at the ſpark. When at a very conſiderable diſtance, the red comes to the eye unmixed; but, if the eye is placed cloſe to the tube, the ſpark appears white and brilliant. In confirmation, however, of ſome of my concluſions, I would obſerve, that by increaſing the quantity of fluid which is conveyed through any portion of inflammable air, or by condenſing that air, the ſpark may be entirely deprived of its red appearance, and made perfectly brilliant. I have only to add, that all weak exploſions and ſparks,

ſparks, when viewed at a diſtance, bear a reddiſh hue. Such are the exploſions which have paſſed through water, ſpirits of wine, or any bad conductor, when confined in a tube whoſe diameter is not more than an inch. The reaſon of theſe appearances ſeems to be, that the weaker the ſpark or exploſion is, the leſs is the light which eſcapes; and the more viſible the effect of any medium which has a power to abſorb ſome of that light.

The preceding obſervations concerning electrical light were the reſult of my attempts to arrange, under general heads, the principal ſingularities attending it. They may, perhaps, aſſiſt others in determining how far they may have led my mind aſtray in giving birth to a theory which I would now briefly deſcribe in a few queries.

I. If we conſider all bodies as compounds, whoſe conſtituent parts are kept together by attracting one another with different forces, can we avoid concluding, that the operations of that attractive force are regulated, not only by the quality, but the quantity likewiſe of thoſe component parts? If an union of a certain number of one kind of particles, with a certain number of a ſecond and third kind of particles, forms a particular body, muſt not the bond which keeps that body together be weakened or ſtrengthened by increaſing or diminiſhing any one of the different kinds of particles which enter into its conſtitution?

II. When, to the natural ſhare of the electric fluid already exiſting in the body, a freſh quantity of the ſame fluid is added, muſt not ſome of the component parts of that body eſcape; or muſt not that attractive force which kept all together be ſo far weakened as to let looſe ſome conſtituent parts,

parts, and amongst these the particles of light in particular?

III. Must not this separation of parts be great in proportion to the quantity of extraneous particles which are added to the body? Or (agreeable to the 4th observation) must not the spark be more splendid and brilliant, the more the electrical fluid is concentered in any given space?

IV. In the diminution or alteration of that attractive force on which depends the constitution of bodies, may there not be a gradation which, in the present case, as well as in that of burning bodies, will cause the escape of some rays sooner than others?

Observations on phosphoric light.

It is obvious, from Mr. B. WILSON's experiments, that there are many curious diversities in the appearances of phosphori. Some shells, prepared agreeably to his directions, after exposure to the sun or to the flash of a battery, emit a purple, others a green, and others a reddish light. If with Mr. WILSON we suppose, that these shells are in a state of slow combustion, may we not conclude, that some are just beginning to burn, and therefore, agreeably to what I have observed on combustible bodies, emitting the most refrangible rays; whilst others are in a more advanced state of combustion, and therefore emitting the least refrangible. If this conclusion be right, the shells which are emitting the purple, or the green, must still retain the yellow, the orange, and the red, which will also make their appearance as soon as the combustion is sufficiently increased.

EXP.

EXP. XV. Place a shell whilst emitting its green rays on a warm shovel, and the appearance of the shell will be soon changed into that of a yellow mixed with red. To Mr. WILSON's theory, however, of slow combustion the following objections may be opposed.

1°. If phosphoric shells owe their light to this cause, we must consider the word combustion when applied to them as implying in its signification all those circumstances which are the usual attendants of a body whilst on fire. Amongst other necessary consequences in such a case, the increase of heat must increase the decomposition of the combustible; whereas we discover an effect the very opposite to this in the appearance of a phosphoric body, which never fails to lose its light entirely in a certain degree of heat, without losing the power of becoming phosphoric again when it has been sufficiently cooled. Besides, when a phosphoric shell has been made very hot, and while it has continued so, I have conveyed the most brilliant discharge of a battery over it without effect. In other words, heat, or the very cause which promotes combustion in all other instances, in this particular case puts an end to it. Mr. WILSON, in his Treatise on Phosphori, has described an experiment similar to the preceding. But the result he mentions is different from that here mentioned. However, from a regard to his authority, I have so frequently repeated my trials that I cannot justly suspect myself of any inaccuracy. 2°. When bodies are wasted by combustion, they can never be made to re-assume the appearances which they previously displayed. No power can give to ashes the phænomena of a burning coal. But phosphoric bodies are very different in this respect; for a shell may be made to lose all its light by exposure to heat, and again

may be made as luminous as ever by exposure to the sun. But 3°. It is observable, that some bodies, which are most beautifully phosphoric, or which, according to Mr. WILSON's theory, are in the best state of slow combustion; it is observable, I say, that the same bodies are the most obstinate in resisting the fire. The diamond, which to be decomposed requires the force of a most powerful furnace, is, according to this theory, wasting away, owing to a separation of parts which is promoted by the weakest influence of the sun's rays.—Without determining whether the preceding objections be valid, let us now see the consequence of admitting the common hypothesis, that the detention of those rays which fall upon phosphori is owing to some force which prevents their immediate reflection, but is not adequate to their entire absorption. This force, whatever it be, cannot well be supposed to operate with equal power on all the rays. And if this be not the case, I think we cannot avoid concluding, that phosphoric shells will assume different colours, owing to the earlier and later escape of the different rays of light. This conclusion is justified by an experiment which I have already appealed to. When the force is such as to admit of the escape of the purple, the blue, and the green, we have only to lessen that force by warming the body, and the yellow, the orange, and red escape. It is proved by BECCARIA's extensive experience on this subject, that there is scarcely any body which is not phosphoric, or which may not be made so by heat. But as the phosphoric force is most powerful when the purple rays only escape, so we are to conclude, that it is weakest when it is able to retain the red rays only. This conclusion is agreeable to several facts. Chalk, oyster-shells, together with those phosphoric bodies whose goodness

has been very much impaired by long keeping; when finely powdered and placed within the circuit of an electrical battery, will exhibit by their scattered particles a shower of light; but these particles will appear reddish, or their phosphoric power will be sufficient only to detain the yellow, orange, and red rays. When spirits of wine are in a similar manner brought within the circuit of a battery, a similar effect may be discovered; its particles diverge in several directions, displaying a most beautiful golden appearance. The metallic calces are, of all bodies, those which are rendered phosphoric with the greatest difficulty. But even these may be scattered into a shower of red luminous particles by the electric stroke.

Norwich, Oct. 7, 1784.

POSTSCRIPT by the Rev. Dr. PRICE.

BY the *phosphoric* force mentioned in the last paragraph of this paper, Mr. MORGAN appears to mean, not the force with which a phosphoric body *emits*, but the force with which it *absorbs* and *retains* light. This last force is proportioned to the degree of attraction between the phosphoric body and light; and therefore must (as Mr. MORGAN observes) be *weakest* when it emits so freely the light it has imbibed as not to retain those rays which adhere to it most strongly. According to Mr. MORGAN's theory, these rays are those which

are least refrangible. The observations and experiments in this paper seem to render this theory probable. It is, however, an objection to it, that the less refrangibility of rays seems to imply a less force of attraction between them and the substances which refract them; but it should be considered, that, possibly, the force of cohesion, which unites the rays of light to bodies, may be a different power from that which refracts them.

XII. *On the Construction of the Heavens.*

By William Herschel, *Esq. F. R. S.*

Read February 3, 1785.

THE subject of the Construction of the Heavens, on which I have so lately ventured to deliver my thoughts to this Society, is of so extensive and important a nature, that we cannot exert too much attention in our endeavours to throw all possible light upon it; I shall, therefore, now attempt to pursue the delineations of which a faint outline was begun in my former paper.

By continuing to observe the heavens with my last constructed, and since that time much improved instrument, I am now enabled to bring more confirmation to several parts that were before but weakly supported, and also to offer a few still further extended hints, such as they present themselves to my present view. But first let me mention that, if we would hope to make any progress in an investigation of this delicate nature, we ought to avoid two opposite extremes, of which I can hardly say which is the most dangerous. If we indulge a fanciful imagination and build worlds of our own, we must not wonder at our going wide from the path of truth and nature; but these will vanish like the Cartesian vortices, that soon gave way when better theories were offered. On the other hand, if we add observation to observation, without attempting to draw not only certain conclusions, but also conjectural views,

views from them, we offend against the very end for which only obſervations ought to be made. I will endeavour to keep a proper medium; but if I ſhould deviate from that, I could wiſh not to fall into the latter error.

That the milky way is a moſt extenſive ſtratum of ſtars of various ſizes admits no longer of the leaſt doubt; and that our ſun is actually one of the heavenly bodies belonging to it is as evident. I have now viewed and gaged this ſhining zone in almoſt every direction, and find it compoſed of ſtars whoſe number, by the account of theſe gages, conſtantly increaſes and decreaſes in proportion to its apparent brightneſs to the naked eye. But in order to develop the ideas of the univerſe, that have been ſuggeſted by my late obſervations, it will be beſt to take the ſubject from a point of view at a conſiderable diſtance both of ſpace and of time.

Theoretical view.

Let us then ſuppoſe numberleſs ſtars of various ſizes, ſcattered over an indefinite portion of ſpace in ſuch a manner as to be almoſt equally diſtributed throughout the whole. The laws of attraction, which no doubt extend to the remoteſt regions of the fixed ſtars, will operate in ſuch a manner as moſt probably to produce the following remarkable effects.

Formation of nebulæ.

Form I. In the firſt place, ſince we have ſuppoſed the ſtars to be of various ſizes, it will frequently happen that a ſtar, being conſiderably larger than its neighbouring ones, will attract them more than they will be attracted by others that are immediately

immediately around them; by which means they will be, in time, as it were, condensed about a center; or, in other words, form themselves into a cluster of stars of almost a globular figure, more or less regularly so, according to the size and original distance of the surrounding stars. The perturbations of these mutual attractions must undoubtedly be very intricate, as we may easily comprehend by considering what Sir ISAAC NEWTON says in the first book of his Principia, in the 38th and following problems; but in order to apply this great author's reasoning of bodies moving in ellipses to such as are here, for a while, supposed to have no other motion than what their mutual gravity has imparted to them, we must suppose the conjugate axes of these ellipses indefinitely diminished, whereby the ellipses will become straight lines.

Form II. The next case, which will also happen almost as frequently as the former, is where a few stars, though not superior in size to the rest, may chance to be rather nearer each other than the surrounding ones; for here also will be formed a prevailing attraction in the combined center of gravity of them all, which will occasion the neighbouring stars to draw together; not indeed so as to form a regular or globular figure, but however in such a manner as to be condensed towards the common center of gravity of the whole irregular cluster. And this construction admits of the utmost variety of shapes, according to the number and situation of the stars which first gave rise to the condensation of the rest.

Form III. From the composition and repeated conjunction of both the foregoing forms, a third may be derived, when many large stars, or combined small ones, are situated in long extended, regular, or crooked rows, hooks, or branches; for they will also draw the surrounding ones, so as to produce figures

of

of condenſed ſtars coarſely ſimilar to the former which gave riſe to theſe condenſations.

Form IV. We may likewiſe admit of ſtill more extenſive combinations; when, at the ſame time that a cluſter of ſtars is forming in one part of ſpace, there may be another collecting in a different, but perhaps not far diſtant quarter, which may occaſion a mutual approach towards their common center of gravity.

V. In the laſt place, as a natural conſequence of the former caſes, there will be formed great cavities or vacancies by the retreat of the ſtars towards the various centers which attract them; ſo that upon the whole there is evidently a field of the greateſt variety for the mutual and combined attractions of the heavenly bodies to exert themſelves in. I ſhall, therefore, without extending myſelf farther upon this ſubject, proceed to a few conſiderations, that will naturally occur to every one who may view this ſubject in the light I have here done.

Objections conſidered.

At firſt ſight then it will ſeem as if a ſyſtem, ſuch as it has been diſplayed in the foregoing paragraphs, would evidently tend to a general deſtruction, by the ſhock of one ſtar's falling upon another. It would here be a ſufficient anſwer to ſay, that if obſervation ſhould prove this really to be the ſyſtem of the univerſe, there is no doubt but that the great Author of it has amply provided for the preſervation of the whole, though it ſhould not appear to us in what manner this is effected. But I ſhall moreover point out ſeveral circumſtances that do manifeſtly tend to a general preſervation; as, in the firſt place, the indefinite extent of the ſidereal heavens,

which muſt produce a balance that will effectually ſecure all the great parts of the whole from approaching to each other. There remains then only to ſee how the particular ſtars belonging to ſeparate cluſters will be preſerved from ruſhing on to their centers of attraction. And here I muſt obſerve, that though I have before, by way of rendering the caſe more ſimple, conſidered the ſtars as being originally at reſt, I intended not to exclude projectile forces; and the admiſſion of them will prove ſuch a barrier againſt the ſeeming deſtructive power of attraction as to ſecure from it all the ſtars belonging to a cluſter, if not for ever, at leaſt for millions of ages. Beſides, we ought perhaps to look upon ſuch cluſters, and the deſtruction of now and then a ſtar, in ſome thouſands of ages, as perhaps the very means by which the whole is preſerved and renewed. Theſe cluſters may be the *Laboratories* of the univerſe, if I may ſo expreſs myſelf, wherein the moſt ſalutary remedies for the decay of the whole are prepared.

Optical appearances.

From this theoretical view of the heavens, which has been taken, as we obſerved, from a point not leſs diſtant in time than in ſpace, we will now retreat to our own retired ſtation, in one of the planets attending a ſtar in its great combination with numberleſs others; and in order to inveſtigate what will be the appearances from this contracted ſituation, let us begin with the naked eye. The ſtars of the firſt magnitude being in all probability the neareſt, will furniſh us with a ſtep to begin our ſcale; ſetting off, therefore, with the diſtance of Sirius or Arcturus, for inſtance, as unity, we will at preſent ſuppoſe, that thoſe of the ſecond magnitude are at double, and

 thoſe

those of the third at treble the distance, and so forth. It is not necessary critically to examine what quantity of light or magnitude of a star intitles it to be estimated of such or such a proportional distance, as the common coarse estimation will answer our present purpose as well; taking it then for granted, that a star of the seventh magnitude is about seven times as far as one of the first, it follows, that an observer, who is inclosed in a globular cluster of stars, and not far from the center, will never be able, with the naked eye, to see to the end of it: for, since, according to the above estimations, he can only extend his view to about seven times the distance of Sirius, it cannot be expected that his eyes should reach the borders of a cluster which has perhaps not less than fifty stars in depth every where around him. The whole universe, therefore, to him will be comprised in a set of constellations, richly ornamented with scattered stars of all sizes. Or if the united brightness of a neighbouring cluster of stars should, in a remarkable clear night, reach his sight, it will put on the appearance of a small, faint, whitish, nebulous cloud, not to be perceived without the greatest attention. To pass by other situations, let him be placed in a much extended stratum, or branching cluster of millions of stars, such as may fall under the III[d] form of nebulæ considered in a foregoing paragraph. Here also the heavens will not only be richly scattered over with brilliant constellations, but a shining zone or milky way will be perceived to surround the whole sphere of the heavens, owing to the combined light of those stars which are too small, that is, too remote to be seen. Our observer's sight will be so confined, that he will imagine this single collection of stars, of which he does not even perceive the thousandth part, to be the whole contents of the heavens. Allowing him now the use of a

common telescope, he begins to suspect that all the milkiness of the bright path which surrounds the sphere may be owing to stars. He perceives a few clusters of them in various parts of the heavens, and finds also that there are a kind of nebulous patches; but still his views are not extended so far as to reach to the end of the stratum in which he is situated, so that he looks upon these patches as belonging to that system which to him seems to comprehend every celestial object. He now increases his power of vision, and, applying himself to a close observation, finds that the milky way is indeed no other than a collection of very small stars. He perceives that those objects which had been called nebulæ are evidently nothing but clusters of stars. He finds their number increase upon him, and when he resolves one nebula into stars he discovers ten new ones which he cannot resolve, He then forms the idea of immense strata of fixed stars, of clusters of stars and of nebulæ (*a*); till, going on with such interesting observations, he now perceives that all these appearances must naturally arise from the confined situation in which we are placed. *Confined* it may justly be called, though in no less a space than what before appeared to be the whole region of the fixed stars; but which now has assumed the shape of a crookedly branching nebula; not, indeed, one of the least, but perhaps very far from being the most considerable of those numberless clusters that enter into the construction of the heavens.

Result of Observations.

I shall now endeavour to shew, that the theoretical view of the system of the universe, which has been exposed in the

(*a*) See a former paper on the Construction of the Heavens.

foregoing part of this paper, is perfectly consistent with facts, and seems to be confirmed and established by a series of observations. It will appear, that many hundreds of nebulæ of the first and second forms are actually to be seen in the heavens, and their places will hereafter be pointed out. Many of the third form will be described, and instances of the fourth related. A few of the cavities mentioned in the fifth will be particularised, though many more have already been observed; so that, upon the whole, I believe, it will be found, that the foregoing theoretical view, with all its consequential appearances, as seen by an eye inclosed in one of the nebulæ, is no other than a drawing from nature, wherein the features of the original have been closely copied; and I hope the resemblance will not be called a bad one, when it shall be considered how very limited must be the pencil of an inhabitant of so small and retired a portion of an indefinite system in attempting the picture of so unbounded an extent.

But to proceed to particulars: I shall begin by giving the following table of gages that have been taken. In the first column is the right ascension, and in the second the north polar distance, both reduced to the time of FLAMSTEED's Catalogue. In the third are the contents of the heavens, being the result of the gages. The fourth shews from how many fields of view the gages were deduced, which have been ten or more where the number of the stars was not very considerable; but, as it would have taken too much time, in high numbers, to count so many fields, the gages are generally single. Where the stars happened to be uncommonly crouded, no more than half a field was counted, and even sometimes only a quadrant; but then it was always done with the precaution of fixing on some row of stars that would point out the division of the field,

so

so as to prevent any considerable mistake. When five, ten, or more fields are gaged, the polar distance in the second column of the table is that of the middle of the sweep, which was generally from 2 to 2½ degrees in breadth; and, in gaging, a regular distribution of the fields, from the bottom of the sweep to the top, was always strictly attended to. The fifth column contains occasional remarks relating to the gages.

I. Table of Star-Gages.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
0 1 41	78 47	9,9	10	
0 4 55	65 36	20,0	10	
0 7 54	74 13	11,3	10	Most of the stars extremely small.
0 8 24	49 7	60	1	
0 9 52	113 17	4,1	10	* The gages marked with an asterisk
0 12 52	113 17	3,2	10	* are those by which fig. 4. tab.
0 16 48	67 44	11,9	10	VIII. has been delineated.
0 21 52	113 17	3,9	10	*
0 22 21	87 10	5,9	10	
0 28 26	46 54	60	1	
0 31 38	46 54	40	1	
0 33 33	65 32	20,4	10	
0 34 22	56 38	20	1	
0 35 22	55 38	24	1	
0 36 39	76 32	11,3	10	
0 39 56	78 43	8,1	10	
0 40 29	48 43	60	½	
0 44 21	87 10	7,6	10	
0 46 22	69 51	11	10	
0 46 33	65 32	13	10	
0 48 42	58 47	40	1	
0 48 50	58 13	17	1	
0 53 18	67 41	9,8	10	A little hazy.
0 53 40	45 37	73	1	
0 54 10	75 16	13	1	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
0 55 10	73 16	14	1	
0 56 4	74 0	15	1	
0 57 52	113 17	3,8	10	*
0 59 10	74 25	14	1	
1 0 16	74 16	11,1	10	
1 1 10	74 5	11,2	10	
1 1 18	111 0	5,2	10	Very clear for this altitude.
1 2 52	52 0	28,1	10	Most of the stars very small.
1 3 52	113 17	2,8	10	*
1 4 15	94 52	7,5	10	
1 4 33	65 32	11,0	10	
1 5 55	78 31	9,2	10	
1 7 27	45 23	58	1	
1 12 0	58 37	20	1	
1 12 48	60 19	13	1	
1 13 4	94 50	6,3	10	
1 15 51	48 40	30	1	
1 18 21	48 40	58	1	
1 23 21	48 40	44	1	
1 27 30	65 42	12,9	10	
1 31 21	87 7	5,8	10	
1 32 4	94 50	7,3	10	
1 33 10	100 8	6,4	10	
1 33 32	92 35	7,1	10	
1 34 52	60 8	17	1	
1 43 30	65 42	14,4	10	
1 45 24	69 43	7,1	10	
1 48 4	100 12	4,9	10	
1 54 24	76 28	12,1	10	
1 58 55	61 55	15,0	10	
2 4 28	87 5	6,4	10	
2 4 36	78 38	9,3	10	
2 7 12	94 56	7,8	10	
2 8 0	83 3	7,3	10	
2 10 4	100 12	4,3	10	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
2 11 30	65 45	14,8	10	
2 16 27	110 54	4,2	10	*
2 19 27	76 24	9,9	10	
2 22 17	45 31	82	1	
2 23 6	60 16	14	1	
2 23 19	113 8	4,2	10	*
2 24 6	58 30	15	1	
2 27 40	115 21	3,0	10	* The situation too low for great accuracy.
2 30 0	94 56	6	10	
2 31 23	76 22	13,8	10	
2 35 14	87 2	5,6	10	
2 38 0	94 56	6,6	10	
2 42 7	61 50	14,8	10	
2 47 32	74 3	11,1	10	Most of the stars exceedingly small.
2 49 22	92 55	9,0	10	
2 49 30	110 55	6,1	10	*
2 50 0	94 56	6,8	10	
2 54 53	76 22	9,2	10	
2 59 56	81 10	6,1	10	
3 1 53	78 37	4,1	10	
3 1 56	81 10	5,1	10	
3 4 53	78 37	3,5	10	
3 10 20	100 2	6,8	10	
3 11 6	59 29	7,0	5	In a part of the heavens which looks pretty full of stars to the naked eye.
3 13 6	59 29	6,1	10	
3 15 6	59 29	9,4	10	
3 22 57	83 1	10,3	10	
3 23 21	92 49	10,1	10	
3 29 41	46 35	55	1	
3 35 0	62 1	15	1	About 15 stars generally in the field.
3 35 12	100 3	7,4	10	
3 36 1	113 3	4,9	10	*
3 42 49	46 10	54	1	
3 48 16	99 59	8,1	10	
3 55 11	74 2	11,0	10	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
4 1 24	92 48	13,8	10	
4 6 18	82 57	13,4	10	
4 8 31	114 55	4,2	10	*
4 12 41	69 33	15,3	10	And many more, extremely small,
4 16 34	112 45	6,2	10	* suspected.
4 26 34	112 45	8,8	10	*
4 27 11	70 41	25	1	
4 28 41	70 1	17	1	
4 29 5	69 24	30	1	
4 30 14	99 50	9,7	10	
4 31 19	67 33	15,6	10	
4 32 29	69 2	36	1	
4 33 31	114 55	8,1	10	*
4 42 14	86 27	19,9	10	
4 53 22	72 59	56	1	
4 57 45	83 22	38	1	
4 58 45	84 36	35	1	
5 1 16	69 23	34	1	
5 3 45	83 29	17,7	6	
5 10 52	69 22	74	1	
5 11 22	96 37	24	1	
5 17 22	96 15	8,9	8	
5 18 0	80 46	30	1	About 30 stars in the field, not very
5 20 7	92 52	19,1	10	exactly gaged.
5 24 12	66 5	36	1	
5 27 3	68 52	58	1	
5 27 48	110 40	17,7	10	*
5 33 4	76 10	65	1	
5 33 12	66 26	86	1	
5 33 17	114 59	13,5	10	*
5 34 45	70 33	50	1	
5 36 30	62 1	20—30		From 20 to 30 stars in the fields, not
5 37 4	74 26	140	½	very exactly gaged.
5 38 45	70 8	73	1	
5 41 12	66 43	60	1	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
5 44 0	116 43	11,5	10	*
5 45 30	83 30	50	1	
5 47 34	112 34	19,3	10	*
5 48 30	62 1	30	1	About 30 ſtars in the field; not very
5 48 44	92 51	22,4	5	exactly gaged.
5 49 0	80 5	50	1	
5 52 14	93 14	44	1	
5 52 30	83 30	60	1	
5 53 0	80 5	110	1	
5 55 4	92 56	57	1	
5 56 40	70 27	73	1	
5 57 0	80 5	60	1	
5 57 37	110 33	19,6	10	*
5 58 51	88 36	90	1	
5 59 30	83 30	80	1	
6 0 23	86 38	24,1	10	
6 1 0	80 5	70	1	
6 4 0	80 5	90	1	
6 5 4	67 17	120	¼	Very unequally ſcattered.
6 6 14	96 16	52	1	
6 6 30	83 30	80	1	
6 6 30	80 5	70	1	
6 6 38	91 45	54	1	Like the reſt, or many ſuch fields.
6 6 40	68 24	56	1	
6 9 0	80 5	74	1	
6 9 34	113 35	26	1	*
6 11 0	62 1	30—40	1	Between.
6 11 0	80 5	63	1	The leaſt number of ſtars in the field I
6 11 34	112 5	33	1	* could find in this neighbourhood.
6 11 37	90 15		1	About 60 or 70 generally.
6 14 4	68 11	178	¼	
6 14 38	90 15	77	1	
6 17 45	62 1	50	1	
6 18 14	96 12	38	1	Very unequally ſcattered.
6 19 14	93 59	72	1	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
6 26 17	114 59	15,9	10	
6 27 14	94 30	132	½	*
6 27 32	70 23	50	1	
6 31 48	115 40	40	1	
6 34 44	92 25	94	1	
6 34 55	79 5	50	1	Generally about 50 ſtars.
6 36 0	94 56	62	1	Twilight.
6 37 15	75 5	70	1	Generally about 70 ſtars.
6 39 8	99 7	50	1	*
6 40 0	116 43	31,3	10	
6 43 25	79 5	67	1	
6 44 28	100 30	67	1	*
6 49 5	87 21	120	½	*
6 49 30	77 31	50	1	Many fields like this.
6 49 44	92 33	120	½	
6 51 8	98 33	78	1	*
6 52 0	116 21	48	1	
6 52 25	79 5	60	1	About 60 ſtars.
6 52 44	92 59	98	1	
6 54 9	111 11	45	1	*
6 57 8	100 1	34	1	*
6 57 38	98 50	83	1	*
6 58 39	112 48	84	1	*
7 0 25	79 5	70	1	
7 4 0	92 3	102	1	*
7 4 38	98 59	70	1	*
7 5 9	111 11	70	1	*
7 8 9	112 15	62	1	*
7 12 8	100 5	118	½	*
7 15 38	98 12	112	½	*
7 19 0	91 51	58	1	*
7 20 0	78 59	48	1	
7 25 9	111 21	168	¼	* One of the richeſt fields.
7 28 9	112 34	204	¼	* A field like the reſt.
7 33 3	115 28	86	1	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
7 41 9	113 26	108	½	*
7 53 4	86 39	28,3	10	*
8 1 4	111 15	80	1	*
8 3 4	113 31	66	1	
8 6 38	100 5	40	1	
8 7 38	99 3	45	1	*
8 11 8	99 25	24,2	10	*
8 12 34	112 15	52	1	*
8 22 4	111 30	35	1	*
8 31 4	112 1	33	1	
8 32 24	112 7	30	1	
8 35 4	112 17	24	1	
8 35 14	111 19	20	1	
8 40 4	111 11	22	1	*
8 45 4	113 22	13	1	
8 46 39	91 26	20,3	10	*
8 48 4	112 23	16,2	10	
8 57 25	66 20	8,3	10	*
9 5 38	91 22	13,8	10	*
9 10 4	115 17	14,0	10	
9 20 4	112 23	15,8	10	
9 20 40	99 12	11,1	10	
9 20 58	88 7	11,5	10	*
9 35 4	112 23	13,0	10	
9 38 4	115 17	10,1	10	
9 38 8	90 23	7,9	10	*
9 42 16	86 16	7,7	10	*
9 45 49	112 21	13,2	10	Strong twilight.
10 0 4	115 17	9,1	10	
10 16 8	88 8	7,2	10	*
10 19 32	91 14	6,5	10	
10 25 8	88 8	4,9	10	*
10 26 0	81 41	5,6	7	*
11 4 4	81 38	5,3	6	*
11 7 36	91 14	5,6	10	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
11 10 6	115 23	6,5	10	Twilight.
11 16 52	81 38	3,1	8	*
11 20 37	91 17	4,9	10	
11 53 43	81 39	6,0	5	*
12 5 6	78 57	2,2	13	*
12 30 40	79 3	3,4	11	*
12 46 51	81 40	4,6	13	*
12 48 19	79 4	3,9	13	*
12 53 45	101 45	9,3	10	Twilight.
12 57 8	99 56	8,1	10	Pretty ſtrong day-light.
13 1 19	79 4	3,8	12	*
13 17 27	101 45	8,6	10	Twilight.
13 22 49	100 1	8,4	10	Some day-light.
13 27 57	101 45	11,3	10	
13 31 10	75 55	5—6	1	* Generally about 5 or 6 ſtars in the field.
13 38 53	104 27	8,5	10	
13 48 49	100 1	9,2	10	Strong twilight.
13 51 27	101 45	10,0	10	
13 55 44	58 11	7,4	10	* Twilight.
13 57 53	104 27	12,3	10	Moſt very ſmall.
14 9 49	100 1	11,2	10	Twilight.
14 13 52	113 4	9,7	10	
14 14 57	101 45	8,8	10	
14 24 49	81 53	2,7	6	
14 29 45	100 5	13,3	10	
14 30 7	66 3	8,8	10	* All ſizes.
14 30 8	80 38	3,5	13	
14 33 22	58 7	8,9	10	* Chiefly ſmall.
14 33 52	113 4	10,3	10	
14 39 57	101 45	14,0	10	All ſizes.
14 40 36	64 47	6,4	10	
14 44 11	114 54	10,3	10	
14 49 52	113 4	12,8	10	
14 51 14	58 10	9,2	10	* Twilight.
14 52 58	60 41	4,4	10	* Strong Aurora borealis.

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
14 53 7	65 15	9,0	10	Chiefly large.
14 55 36	64 47	6,6	10	Most very small.
14 59 11	114 54	8,8	10	
15 2 42	62 48	8,3	10	
15 3 7	66 15	9,5	10	
15 4 36	64 47	5,0	10	
15 8 37	113 0	14,1	10	
15 8 45	93 5	9,4	12	Very small.
15 13 42	62 48	8,9	10	
15 15 44	58 17	10,0	10	* Twilight.
15 19 48	60 40	4,9	10	* Strong Aurora borealis, so as to affect the gages.
15 20 0	75 52	9,5	4	
15 21 0	93 5	10,9	12	
15 26 7	81 53	11,0	5	
15 28 48	99 51	13,1	10	
15 29 7	66 15	10,6	10	All sizes.
15 29 44	58 17	8,9	10	* Twilight.
15 32 0	75 51	6	6	
15 33 52	111 32	12,8	10	
15 35 0	75 51	6,5	6	
15 42 2	58 14	13,1	10	* Twilight.
15 42 3	116 56	18,6	10	
15 42 53	113 47	32,5	2	The stars too small for the gage.
15 46 30	93 5	10,8	12	
15 48 37	113 0	17,1	10	
15 48 46	63 4	12,4	10	
15 49 52	111 32	18,1	10	The situation so low that it requires attention to see the stars.
15 50 20	114 55	9,2	10	
15 57 3	116 56	7,2	10	
16 0 2	58 14	12,2	10	* Twilight.
16 0 3	116 56	6,1	10	
16 0 12	114 57	1,6	10	
16 3 12	114 57	2,0	10	
16 4 0	75 43	13	6	All sizes.
16 4 19	113 6	,5	10	Perfectly clear. See p. 256.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
16 4 46	63 4	12,0	10	Most small.
16 4 52	99 57	14,6	10	Moon and twilight.
16 6 28	113 4	,7	10	Perfectly clear.
16 7 12	66 15	13,3	10	
16 8 6	115 1	3,8	6	
16 8 11	93 9	12,2	12	
16 8 16	116 48	11,6	10	
16 9 28	113 4	1,1	10	Perfectly clear. See p. 256.
16 11 28	113 4	1,4	10	The same.
16 13 28	113 4	1,8	10	*g* Serpentarii and 19 Scorpii visible to the naked eye.
16 13 52	58 24	14,2	10	* Most small.
16 14 42	63 7	15,1	10	Most very small.
16 15 37	80 40	9,7	12	All sizes.
16 17 28	113 4	4,7	10	
16 20 51	81 57	13,8	6	
16 23 0	73 43	24	1	
16 23 28	113 4	13,6	10	
16 24 11	93 9	13,6	12	Require attention to be seen.
16 25 7	80 40	14,6	13	
16 27 32	68 23	21,6	10	Twilight.
16 29 16	116 48	50,4	10	
16 30 37	80 40	34	1	
16 31 12	66 15	18,4	10	Strong twilight.
16 32 28	113 4	20,3	10	Most extremely small.
16 32 52	58 24	15,6	10	* Most small.
16 35 42	63 7	16,5	10	*
16 35 48	93 15	18,6	12	All sizes.
16 38 12	66 15	20,1	10	Strong twilight.
16 38 45	107 57	19,9	10	Strong twilight.
16 40 51	113 14	41,1	8	
16 45 32	68 23	19,0	4	Hazy.
16 51 45	107 57	29,8	10	
16 52 22	66 26	16,6	10	Day-light pretty strong.
16 55 42	63 7	26,6	10	* Strong twilight.
17 1 34	58 11	18,8	10	* Strong day light.

R.A.

R.A.	P.D.	Stars.	Fields	Memorandums.
H. M. S.	D. M.			
17 3 22	66 26	35	1	* Day-light too strong for gaging.
17 6 36	98 38	13,7	10	Most small, and more suspected.
17 9 30	116 55	7,6	10	
17 9 32	68 23	32,3	10	
17 11 10	66 26	38	1	* Day-light pretty strong.
17 13 24	63 21	32,8	10	* Strong day-light.
17 17 36	111 47	15,3	10	Moon and day-light.
17 25 7	108 5	23	10	
17 27 29	116 48	25	1	
17 28 32	68 23	42,2	5	* Twilight.
17 30 29	116 48	42	1	
17 33 29	116 48	52	1	Day-light very strong.
17 34 36	98 38	18,5	10	Very strong twilight.
17 39 34	120 0	84	1	Most large.
17 40 41	114 52	77	1	Day-light very strong.
17 41 29	116 48	82	1	Day-light very strong.
17 43 45	105 3	80	1	Flying clouds.
17 48 0	61 18	25,6	5	Most large.
17 50 4	56 16	27,2	10	Twilight.
17 50 7	108 5	59	1	Like the rest in this part of the heaven.
17 52 7	108 5	118	1	Many such fields just by.
17 52 17	98 43	7,6	10	
17 52 30	62 12	40	1	Most large.
17 52 32	68 19	54	1	* Strong day-light.
17 55 7	108 5	232	½	
17 55 15	106 6	112	1	Many such fields.
17 55 38	112 54	112	½	
17 57 30	60 28	38	1	Most large.
17 58 37	103 24	35	1	
17 58 41	118 57	64	1	
17 58 49	122 17	17	1	
17 59 1	108 8	320	½	
17 59 19	104 24	68	1	
18 0 13	122 11	27	1	
18 3 49	120 42	19	1	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S	D. M.			
18 5 17	98 47	65	1	
18 6 37	90 36	9,4	10	Too ſoon for gaging, not having been long enough out in the dark.
18 7 4	62 14	40	1	Moſt large.
18 7 4	56 16	38,2	5	
18 7 37	103 25	88,0	3	
18 10 7	120 58	20	1	
18 10 52	61 8	78	1	Chiefly large.
18 11 49	104 6	170	½	
18 13 37	104 16	238	½	
18 13 52	93 11	2,0	7	
18 14 46	56 20	48	1	
18 15 28	92 42	3,4	7	
18 16 52	92 42	8,9	7	
18 18 40	92 42	13,8	7	
18 19 37	102 34	9,5	2	
18 20 7	103 18	19	1	
18 20 46	92 42	25,8	6	
18 21 1	103 55	22	1	
18 21 12	90 41	8,6	10	
18 21 31	103 36	24	1	
18 22 4	62 7	48	1	Large and ſmall.
18 22 4	56 16	39,6	5	
18 22 19	104 6	14	1	
18 22 37	103 45	30	1	
18 24 3	115 10	35	1	
18 24 4	109 35	35	1	Twilight.
18 24 7	102 31	30	1	
18 24 10	92 59	88	1	
18 24 43	103 39	25	1	
18 25 37	102 34	39	1	
18 26 17	98 3	111	1	
18 26 25	103 57	60	1	
18 26 47	97 43	250	1	
18 27 1	120 58	30	1	
18 27 55	120 44	32	1	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
18 28 7	102 51	13	1	Extremely ſmall.
18 28 8	91 44	39	1	Moſt ſmall.
18 28 25	103 9	20	1	Extremely ſmall.
18 28 37	122 25	12	1	
18 29 25	103 24	20	1	Extremely ſmall.
18 29 47	97 50	150	1	
18 29 49	121 39	24	1	
18 30 34	57 18	62	1	
18 31 10	92 42	13,7	7	
18 31 10	108 53	74	1	Twilight.
18 31 13	103 19	112	1	All ſizes.
18 31 17	97 53	188	½	Many more ſuſpected.
18 31 34	62 34	76	1	* Large and ſmall.
18 31 49	121 39	19,3	10	
18 33 4	108 43	88	1	Twilight.
18 33 7	103 53	146	½	
18 34 5	98 34	130	1	
18 34 47	71 53	78	1	*
18 34 58	60 41	80	1	Large and ſmall.
18 36 34	110 12	83	1	Twilight.
18 36 34	91 37	176	¼	
18 36 47	72 28	224	½	*
18 37 34	93 29	5	1	
18 38 1	104 14	118	½	
18 39 40	93 52	116	¼	
18 40 28	92 47	10	1	
18 40 47	71 48	236	¼	*
18 41 22	91 37	156	¼	
18 42 49	121 39	15,2	10	Very clear for this altitude.
18 43 17	72 8	368	¼	*
18 43 33	119 21	21	1	
18 44 34	112 43	53	1	
18 44 34	60 34	84	1	All ſizes.
18 47 32	91 14	328	¼	
18 48 4	110 12	83		

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
18 50 16	60 55	136	½	Many of them small.
18 51 4	57 26	84	1	
18 51 32	108 26	36,8	5	Strong twilight.
18 52 49	115 30	26,2	5	
18 54 4	57 18	93	1	
18 54 8	91 14	328	¼	
18 54 55	104 23	180	½	
18 55 4	108 41	80	1	
18 55 16	62 31	206	½	
18 59 8	91 14	328	¼	
18 59 26	72 37	40	1	Too soon for gaging.
19 1 2	71 40	75	1	
19 1 34	56 47	127	1	Moonlight.
19 2 29	74 53	204	¼	* Twilight.
19 2 37	103 16	160	½	
19 2 49	121 39	14,1	10	
19 3 34	55 56	146	½	☽
19 6 34	61 8	196	½	And many small besides.
19 7 34	56 56	130	½	☽
19 7 52	57 59	116	½	
19 8 38	92 8	120	½	
19 9 37	109 1	60	1	
19 9 40	56 51	130	1	☽
19 12 59	75 21	58	1	*
19 13 50	59 59	256	¼	
19 13 52	59 29	158	½	
19 14 2	72 15	60	1	*
19 14 4	61 21	279	⅓	Too crowded for accuracy.
19 14 55	103 36	64	1	Changeable focus.
19 15 40	55 26	160	1	☽ bright.
19 16 50	60 43	296	¼	
19 16 59	73 23	56	1	*
19 17 44	108 12	50	1	
19 18 23	78 9	196	¼	*
19 18 28	61 21	279	⅓	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
19 19 52	57 14	180	½	
19 19 56	108 12	55	1	
19 20 51	60 55	384	¼	
19 21 1	78 47	472	¼	*
19 21 34	55 17	208	½	☽ bright.
19 22 27	62 29	320	¼	
19 24 36	56 49	224	¼	
19 24 49	104 24	36	1	Changeable focus.
19 24 50	60 43	296	¼	
19 24 53	113 51	18,3	10	
19 25 4	57 9	190	½	☽ bright.
19 25 16	64 18	280	¼	
19 25 22	59 36	340	¼	
19 25 37	103 50	55	1	Changeable focus.
19 27 36	72 34	424	¼	* Too small and too crowded to be cer-
19 27 44	61 8	240	⅓	[tain of the number.
19 28 1	103 30	45	1	Changeable focus.
19 28 6	56 49	288	¼	
19 28 52	59 26	344	¼	
19 28 52	56 47	186	½	☽ very bright.
19 29 46	65 10	34	1	
19 30 36	74 33	588	¼	*
19 30 36	54 53	312	¼	
19 31 33	92 34	62,2	5	*
19 32 9	109 44	23,8	10	
19 32 15	62 35	296	¼	
19 33 4	55 34	212	½	☽
19 33 7	103 12	50	1	Changeable focus.
19 33 14	61 8	240	⅓	
19 33 20	58 59	232	¼	
19 34 51	115 44	14,1	10	
19 35 34	63 19	256	¼	
19 36 6	54 57	384	¼	
19 36 37	102 31	68	1	Changeable focus.
19 36 50	60 35	296	¼	

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
19 40 33	63 0	296	¼	
19 40 46	59 12	192	¼	
19 40 48	74 33	588	¼	*
19 42 33	73 14	352	¼	*
19 43 30	57 23	130	½	☽
19 43 56	64 27	124	½	Most large.
19 45 36	77 58	140	½	* Faint ☽.
19 45 37	103 3	50	1	
19 46 21	73 14	252	¼	*
19 46 51	115 44	12,8	10	Strong twilight.
19 47 8	60 35	312	¼	
19 47 18	109 46	20,9	10	
19 47 22	57 38	312	¼	Very unequally scattered.
19 49 6	57 13	268	¼	
19 49 48	56 51	120	½	☽
19 50 5	92 39	39,2	5	* Most small.
19 51 37	62 37	51	1	
19 52 0	57 15	220	½	☽
19 53 1	60 36	80	1	
19 53 28	63 40	52	½	
19 53 40	54 59	306	¼	
19 53 49	121 39	7,7	10	
19 54 0	55 12	160	½	☽
19 54 12	78 3	120	½	* Faint ☽.
19 54 22	59 58	136	¼	
19 55 7	62 41	48	1	
19 56 19	60 44	112	½	
19 56 22	57 17	192	¼	
19 57 19	62 34	45	1	
19 57 40	58 29	104	½	
19 59 49	62 37	41	1	
20 0 21	79 3	56	1	* Strong ☽.
20 0 24	55 12	184	½	☽
20 0 25	60 33	80	1	Most of the stars extremely small.
20 0 51	115 44	12,2	10	Twilight.

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
20 1 39	79 34	68	1	* Strong ☽
20 5 26	56 34	46	1	☽
20 5 27	72 56	280	¼	
20 6 23	107 27	22,6	10	
20 6 43	62 32	75	1	Many small.
20 8 26	56 27	47,4	5	☽
20 8 27	72 56	280	¼	
20 8 58	103 37	38	1	
20 9 6	109 40	24,2	5	
20 9 52	102 48	31	1	
20 12 22	58 14	76	½	
20 17 20	76 12	184	¼	Some twilight.
20 18 51	115 44	10,6	10	Twilight.
20 20 58	61 27	88	1	
20 21 36	71 28	104	½	Hazy.
20 22 56	56 27	66	1	☽
20 22 58	103 26	20	1	
20 24 51	115 44	9,3	10	Twilight.
20 25 58	103 26	22,8	10	Changeable focus.
20 25 59	67 27	248	¼	
20 26 1	92 44	30,8	5	*
20 26 46	109 37	16,7	10	Not clear.
20 26 49	121 39	7,7	10	A little hazy.
20 27 33	96 7	39	1	Most small.
20 34 51	115 44	9,5	10	☽
20 35 53	61 20	142	½	
20 37 18	58 28	108	½	
20 37 34	97 6	26,6	10	*
20 38 1	92 44	28,2	5	*
20 39 42	66 37	78	½	
20 40 22	56 21	192	¼	
20 41 11	67 54	108	½	
20 41 56	74 33	116	½	
20 42 59	62 14	112	½	
20 43 1	70 29	76	1	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
20 43 30	54 47	260	¼	Most of the stars of the same size.
20 44 59	70 6	80	1	
20 47 13	60 46	120	½	
20 49 1	92 44	27,0	5	*
20 49 10	57 11	248	¼	Most of a size.
20 50 59	103 26	17,2	3	
20 51 23	68 30	70	1	
20 53 29	103 26	17,4	5	
20 54 1	107 47	10,3	10	
20 56 59	103 26	14,9	10	Most extremely small.
20 57 55	61 25	64	1	Twilight.
20 59 1	92 44	21,4	5	*
21 1 6	96 43	40	1	* Most small.
21 3 29	66 39	80	½	
21 3 53	73 9	55	1	
21 6 13	69 23	40	1	A little hazy.
21 6 55	103 32	11,1	10	
21 7 49	109 45	12,8	10	
21 7 59	64 58	110	½	
21 9 25	61 36	75	1	Strong twilight.
21 10 13	60 39	70	1	Strong twilight.
21 11 17	73 18	50	1	
21 11 42	96 13	25	1	*
21 12 1	92 44	16,4	5	
21 15 3	109 56	15,3	10	
21 16 43	59 7	76	½	
21 18 54	57 20	50	1	
21 20 18	96 43	24	1	*
21 21 0	107 49	8,1	10	
21 22 14	76 33	30,0	5	
21 25 31	92 44	8,0	5	
21 29 12	83 11	21,6	5	
21 30 58	78 57	18,9	10	
21 32 10	57 14	25	1	
21 33 1	92 44	15,4	5	Strong twilight.

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
21 34 55	97 17	13,6	10	*
21 36 38	65 55	42	1	
21 38 20	65 38	60	1	
21 39 55	96 17	18	1	*
21 41 52	58 42	44	1	
21 43 22	109 55	11,5	10	*
21 45 4	59 39	52	1	
21 48 22	59 30	29	1	
21 51 52	58 56	61	1	
21 51 55	97 17	11,5	10	*
21 54 22	109 55	12,8	10	*
21 57 49	59 37	60	½	
21 58 4	75 7	33	1	
21 58 19	59 6	40	½	
21 58 43	58 34	32,6	5	☽
21 58 49	58 20	34	1	
22 2 25	60 9	42,6	5	
22 2 52	109 55	7,4	10	*
22 3 56	71 48	25,1	10	
22 7 22	109 55	8,9	10	*
22 10 28	75 2	26	1	
22 11 32	97 14	10,7	10	* Twilight.
22 11 35	65 48	26,6	5	
22 18 32	97 14	9,1	10	* Twilight.
22 20 35	109 58	8,3	10	*
22 20 55	78 54	11,7	10	Bright ☽.
22 27 41	95 4	8,1	10	
22 30 35	109 58	5,0	10	*
22 31 28	73 59	17,3	10	
22 33 6	76 52	16,5	10	
22 34 40	61 56	20,1	10	
22 35 35	109 58	7,1	10	*
22 36 49	71 57	18,5	10	
22 39 41	82 5	19	1	
22 40 5	65 48	21,3	10	

R.A.

R.A.	P.D.	Stars.	Fields.	Memorandums.
H. M. S.	D. M.			
22 43 55	60 9	26,7	10	Faint ☽
22 45 3	80 47	13,2	10	
22 45 30	58 38	17,2	10	☽
22 48 49	71 57	13,4	10	
22 52 9	78 43	8,2	10	☽
22 52 41	95 4	8,9	10	
22 55 40	71 54	11,6	10	
22 56 55	67 53	12,1	10	
22 58 19	78 42	9,2	10	☽
23 0 27	113 12	4,4	10	
23 0 30	58 38	18,7	10	☽
23 2 59	65 50	21,3	10	
23 5 35	109 58	7,3	10	☽
23 8 52	95 1	7,5	10	Most extremely small.
23 10 4	64 55	26	1	
23 11 40	61 48	21,1	10	
23 12 40	71 54	11,9	10	
23 17 50	81 0	9,7	10	
23 23 58	69 48	12,1	10	
23 25 32	113 12	3,1	10	*
23 32 2	69 51	9,5	10	
23 33 20	79 45	10	1	
23 43 2	69 51	10,9	10	
23 44 47	45 24	50	1	
23 46 52	113 17	4,2	10	*
23 46 55	65 36	15,3	10	
23 59 21	87 10	5,6	10	
23 59 56	95 4	7,8	10	

PRO-

PROBLEM.

The stars being supposed to be nearly equally scattered, and their number, in a field of view of a known angular diameter, being given, to determine the length of the visual ray.

Here, the arrangement of the stars not being fixed upon, we must endeavour to find which way they may be placed so as to fill a given space most equally. Suppose a rectangular cone cut into frustula by many equidistant planes perpendicular to the axis; then, if one star be placed at the vertex, and another in the axis at the first intersection, six stars may be set around it so as to be equally distant from one another and from the central star. These positions being carried on in the same manner, we shall have every star within the cone surrounded by eight others, at an equal distance from that star taken as a center. Fig. 1. (tab. VIII.) contains four sections of such a cone distinguished by alternate shades, which will be sufficient to explain what sort of arrangement I would point out.

The series of the number of stars contained in the several sections will be 1 . 7 . 19 . 37 . 61 . 91 . &c. which continued to n terms, the sum of it, by the differential method, will be $na + n \cdot \frac{n-1}{2} d' + n \cdot \frac{n-1}{2} \cdot \frac{n-2}{3} d''$, &c.: where a is the first term d', d'', d''', &c. the 1st, 2d, and 3d differences. Then, since $a = 1$, $d' = 6$, $d'' = 6$, $d''' = 0$, the sum of the series will be n^3. Let S be the given number of stars; 1, the diameter of the base of the field of view; and B, the diameter of the base of the great rectangular cone; and, by trigonometry, we shall have $B = \frac{\text{Radius.}}{\text{Tang. } \frac{1}{2} \text{ field}}$. Now, since the

field of view of a telescope is a cone, we shall have its solidity to that of the great cone of stars, formed by the above construction as the square of the diameter of the base of the field of view, to the square of the diameter of the base of the great cone, the height of both being the same; and the stars in each cone being in the ratio of the solidity, as being equally scattered (*b*), we have $n = \sqrt[3]{B^2S}$. And the length of the visual ray $= n - 1$, which was to be determined.

(*b*) We ought to remark, that the periphery and base of the cone of the field of view, in gaging, would in all probability seldom fall exactly on such stars as would produce a perfect equality of situation between the stars contained in the small and the great cone; and that, consequently, the solution of this problem, where we suppose the stars of one cone to be to those of the other in the ratio of the solidity on account of their being equally scattered, will not be strictly true. But it should be remembered, that in small numbers, where the different terminations of the fields would most affect this solution, the stars in view have always been ascertained from gages that were often repeated, and each of which consisted of no less than ten fields successively taken, so that the different deviations at the periphery and base of the cone would certainly compensate each other sufficiently for the purpose of this calculation. And that, on the other hand, in high gages, which could not have the advantage of being so often repeated, these deviations would bear a much smaller proportion to the great number of stars in a field of view; and therefore, on this account, such gages may very justly be admitted in a solution where practical truth rather than mathematical precision is the end we have in view. It is moreover not to be supposed that we imagine the stars to be actually arranged in this regular manner, and, returning therefore to our general hypothesis of their being equally scattered, any one field of view promiscuously taken may, in this general sense, be supposed to contain a due proportion of them; so that the principle on which this solution is founded may therefore be said to be even more rigorously true than we have occasion to insist upon in an argument of this kind.

The

The same otherwise.

If a different arrangement of the stars should be selected, such as that in fig. 2. where one star is at the vertex of a cone; three in the circumference of the first section, at an equal distance from the vertex and from each other; six in the circumference of the next section, with one in the axis or center; and so on, always placing three stars in a lower section in such a manner as to form an equilateral pyramid with one above them: then we shall have every star, which is sufficiently within the cone, surrounded by twelve others at an equal distance from the central star and from each other. And by the differential method, the sum of the two series equally continued, into which this cone may be resolved, will be $2n^3 + 1\frac{1}{2}n^2 + \frac{1}{2}n$; where n stands for the number of terms in each series. To find the angle which a line vx, passing from the vertex v over the stars v, n, b, l, &c. to x, at the outside of the cone, makes with the axis; we have, by construction, vs in fig. 3. representing the planes of the first and second sections $= 2 \times \text{cos.}30° = \varphi$, to the radius ps, of the first section $= 1$. Hence it will be $\sqrt{\varphi^2 - 1} = vp = \frac{1}{2}vm$; or $vm = 2\sqrt{\varphi^2 - 1}$: and, by trigonometry, $\frac{R\varphi}{2\sqrt{\varphi^2-1}} = T$. Where T is the tangent of the required angle to the radius R (*c*); and putting $t =$ tangent of

(*c*) In finding this angle we have supposed the cone to be generated by a revolving rectangular triangle of which the line vx, fig. 2. is the hypotenuse; but the stars in the second series will occasion the cone to be contained under a waving surface, wherefore the above supposition of the generation of the cone is not strictly true; but then these waves are so inconsiderable, that, for the present purpose, they may safely be neglected in this calculation.

half the given field of view, it will be $\frac{T}{t} = B$, the base of the cone. And $\frac{\sqrt{\varphi^2 - 1}}{\varphi} = d$, will be an expression for vp, in terms of vs, which is the mutual distance of the scattered stars. Then having $\frac{B^2S}{2} = n^3 + \frac{3}{4}n^2 + \frac{1}{4}n$, we may find n; whence $2dn - d$, the visual ray, will be obtained.

The result of this arrangement gives a shorter ray than that of the former; but since the difference is not so considerable as very materially to affect the conclusions, I shall, on account of the greater convenience, make use of the first.

We inhabit the planet of a star belonging to a Compound Nebula of the third form.

I shall now proceed to shew that the stupendous sidereal system we inhabit, this extensive stratum and its secondary branch, consisting of many millions of stars, is, in all probability, *a detached Nebula.* In order to go upon grounds that seem to me to be capable of great certainty, they being no less than an actual survey of the boundaries of our sidereal system, which I have plainly perceived, as far as I have yet gone round it, every where terminated, and in most places very narrowly too, it will be proper to shew the length of my sounding line, if I may so call it, that it may appear whether it was sufficiently long for the purpose.

In the most crowded part of the milky way I have had fields of view that contained no less than 588 stars (*d*), and these were continued for many minutes, so that in one quarter of an hour's time there passed no less than 116000 stars through the field of

(*d*) See the table of Gages, p. 235.

view

view of my telescope (*e*). Now, if we compute the length of the visual ray by putting $S = 588$, and the diameter of the field of view fifteen minutes, we shall find $n = \sqrt[3]{B^2 S} = 498$; so that it appears the length of what I have called my sounding line, or $n - 1$, was probably not less than 497 times the distance of Sirius from the sun. The same gage calculated by the second arrangement of stars gives $\sqrt{\varphi^2 - 1} = 1.41421$; $\frac{R\varphi}{2\sqrt{\varphi^2 - 1}} =$ tangent of $31^\circ\ 28'\ 55'',77$; $\frac{T}{t} = B = 280,69$; $\frac{\sqrt{\varphi^2 - 1}}{\varphi} = d = ,81649$; $\frac{B^2 S}{2} = 23163409,7 = n^3 + \frac{3}{4} n^2 + \frac{1}{4} n$; where $n = 284,8$ nearly; and $2dn - 1 = 464$, the visual ray.

It may seem inaccurate that we should found an argument on the stars being equally scattered, when in all probability there may not be two of them in the heavens, whose mutual distance shall be equal to that of any other two given stars; but it should be considered, that when we take all the stars collectively there will be a mean distance which may be assumed as the general one; and an argument founded on such a supposition will have in its favour the greatest probability of not being far short of truth. What will render the supposition of an equal distribution of the stars, with regard to the gages, still less exposed to objections is, that whenever the stars happened either to be uncommonly crowded or deficient in number, so as very sud-

(*e*) The breadth of my sweep was $2^\circ\ 26'$, to which must be added $15'$ for two semi-diameters of the field. Then, putting $161 = a$, the number of fields in 15 minutes of time; $,7854 = b$, the proportion of a circle to 1, its circumscribed square; $\varphi =$ sine of $74^\circ\ 22'$, the polar distance of the middle of the sweep reduced to the present time; and $588 = S$, the number of stars in a field of view, we have $\frac{a\varphi S}{b} = 116076$ stars.

denly

denly to paſs over from one extreme to the other, the gages were reduced to other forms, ſuch as the border-gage, the diſtance-gage, &c. which terms, and the uſe of ſuch gages, I ſhall hereafter find an opportunity of explaining. And none of thoſe kinds of gages have been admitted in this table, which conſiſts only of ſuch as have been taken in places where the ſtars apparently ſeemed to be, in general, pretty evenly ſcattered; and to increaſe and decreaſe in number by a certain gradual progreſſion. Nor has any part of the heavens containing a cluſter of ſtars been put in the gages; and here I muſt obſerve, that the difference between a crowded place and a cluſter may eaſily be perceived by the arrangement as well as the ſize and mutual diſtance of the ſtars: for in a cluſter they are generally not only reſembling each other pretty nearly in ſize, but a certain uniformity of diſtance alſo takes place; they are more and more accumulated towards the center, and put on all the appearances which we ſhould naturally expect from a number of them collected into a group at a certain diſtance from us. On the other hand, the rich parts of the milky way, as well as thoſe in the diſtant broad part of the ſtratum, conſiſt of a mixture of ſtars of all poſſible ſizes, that are ſeemingly placed without any particular apparent order. Perhaps we might recollect, that a greater condenſation towards the center of our ſyſtem than towards the borders of it ſhould be taken into conſideration; but, with a nebula of the third form, containing ſuch various and extenſive combinations, as I have found to take place in ours, this circumſtance, which in one of the firſt form would be of conſiderable moment, may, I think, be ſafely neglected. However, I would not be underſtood to lay a greater ſtreſs on theſe and the following calculations than the principles on which they are founded will permit; and if hereafter

after we shall find reason, from experience and observation, to believe that there are parts of our system where the stars are not scattered in the manner here supposed, we ought then to make proper exceptions.

But to return: if some other high gage be selected from the table, such as 472 or 344, the length of the visual ray will be found 461 and 415. And although, in consequence of what has been said, a certain degree of doubt may be left about the arrangement and scattering of the stars, yet when I recollect, that in those parts of the milky way where these high gages were taken, the stars were neither so small, nor so crowded, as they must have been on a supposition of a much farther continuance of them, when certainly a milky or nebulous appearance must have come on, I need not fear to have over-rated the extent of my visual ray. And indeed every thing that can be said to shorten it will only contract the limits of our nebula, as it has in most places been of sufficient length to go far beyond the bounds of it. Thus, in the sides of the stratum opposite to our situation in it, where the gages often run below 5, our nebula cannot extend to 100 times the distance of Sirius; and the same telescope, which could shew 588 stars in a field of view of 15 minutes, must certainly have presented me also with the stars in these situations as well as the former, had they been there. If we should answer this by observing that they might be at too great a distance to be perceived, it will be allowing that there must at least be a vacancy amounting to the length of a visual ray not short of 400 times the distance of Sirius; and this is amply sufficient to make our nebula a detached one. It is true, that it would not be consistent confidently to affirm that we were on an island unless we had actually found ourselves every where bounded by the ocean,

ocean, and therefore I ſhall go no farther than the gages will authoriſe; but conſidering the little depth of the ſtratum in all thoſe places which have been actually gaged, to which muſt be added all the intermediate parts that have been viewed and found to be much like the reſt, there is but little room to expect a connection between our nebula and any of the neighbouring ones. I ought alſo to add, that a teleſcope with a much larger aperture than my preſent one, graſping together a greater quantity of light, and thereby enabling us to ſee farther into ſpace, will be the ſureſt means of compleating and eſtabliſhing the arguments that have been uſed: for if our nebula is not abſolutely a detached one, I am firmly perſuaded, that an inſtrument may be made large enough to diſcover the places where the ſtars continue onwards. A very bright milky nebuloſity muſt there undoubtedly come on, ſince the ſtars in a field of view will increaſe in the ratio of n^3, greater than that of the cube of the viſual ray. Thus, if 588 ſtars in a given field of view are to be ſeen by a ray of 497 times the diſtance of Sirius; when this is lengthened to 1000, which is but little more than double the former, the number of ſtars in the ſame field of view will be no leſs than 4774: for when the viſual ray r is given, the number S of ſtars will be $=\frac{n^3}{B^2}$; where $n=r+1$; and a teleſcope with a three-fold power of extending into ſpace, or with a ray of 1500, which, I think, may eaſily be conſtructed, will give us 16096 ſtars. Now, theſe would not be ſo cloſe but that a good power applied to ſuch an inſtrument might eaſily diſtinguiſh them; for they need not, if arranged in regular ſquares, approach nearer to each other than 6″,27; but what would produce the milky nebuloſity which I have mentioned is the numberleſs ſtars beyond them, which in one

respect

respect the visual ray might also be said to reach. To make this appear we must return to the naked eye, which, as we have before estimated, can only see the stars of the seventh magnitude so as to distinguish them; but it is nevertheless very evident that the united lustre of millions of stars, such as I suppose the nebula in Andromeda to be, will reach our sight in the shape of a very small, faint nebulosity; since the nebula of which I speak may easily be seen in a fine evening. In the same manner my present telescope, as I have argued, has not only a visual ray that will reach the stars at 497 times the distance of Sirius so as to distinguish them (and probably much farther), but also a power of shewing the united lustre of the accumulated stars that compose a milky nebulosity, at a distance far exceeding the former limits; so that from these considerations it appears again highly probable, that my present telescope, not shewing such a nebulosity in the milky way, goes already far beyond its extent: and consequently, much more would an instrument, such as I have mentioned, remove all doubt on the subject, both by shewing the stars in the continuation of the stratum, and by exposing a very strong milky nebulosity beyond them, that could no longer be mistaken for the dark ground of the heavens.

To these arguments, which rest on the firm basis of a series of observation, we may add the following considerations drawn from analogy. Among the great number of nebulæ which I have now already seen, amounting to more than 900, there are many which in all probability are equally extensive with that which we inhabit; and yet they are all separated from each other by very considerable intervals. Some indeed there are that seem to be double and treble; and though with most of these it may be, that they are at a very great distance from each

other, yet we allow that ſome ſuch conjunctions really are to be found; nor is this what we mean to exclude. But then theſe compound or double nebulæ, which are thoſe of the third and fourth forms, ſtill make a detached link in the great chain. It is alſo to be ſuppoſed, that there may ſtill be ſome thinly ſcattered ſolitary ſtars between the large interſtices of nebulæ, which, being ſituated ſo as to be nearly equally attracted by the ſeveral cluſters when they were forming, remain unaſſociated. And though we cannot expect to ſee theſe ſtars, on account of their vaſt diſtance, yet we may well preſume, that their number cannot be very conſiderable in compariſon to thoſe that are already drawn into ſyſtems; which conjecture is alſo abundantly confirmed in ſituations where the nebulæ are near enough to have their ſtars viſible; for they are all inſulated, and generally to be ſeen upon a very clear and pure ground, without any ſtar near them that might be ſuppoſed to belong to them. And though I have often ſeen them in beds of ſtars, yet from the ſize of theſe latter we may be certain, that they were much nearer to us than thoſe nebulæ, and belonged undoubtedly to our own ſyſtem.

Uſe of the gages.

A delineation of our nebula, by an application of the gages in the manner which has been propoſed to be done in my former paper, may now be attempted, and the following table is calculated for this purpoſe. It gives us the length of the viſual ray for any number of ſtars in the field of view contained in the third column of the foregoing table of gages from $\frac{1}{10}$ to 100000. If the number required is not to be found in the firſt

column of this table, a proportional mean may be taken between the two neareſt rays in the ſecond column, without any material error, except in the few laſt numbers. The calculations of reſolvable and milky nebuloſity, at the end of the table, are founded, the firſt, on a ſuppoſition of the ſtars being ſo crowded as to have only a ſquare ſecond of ſpace allowed them; the next aſſigning them only half a ſecond ſquare. However, we ſhould conſider that in all probability a very different accumulation of ſtars may take place in different nebulæ; by which means ſome of them may aſſume the milky appearance, though not near ſo far removed from us; while cluſters of ſtars alſo may become reſolvable nebulæ from the ſame cauſe. The diſtinctneſs of the inſtrument is here alſo concerned; and as teleſcopes with large apertures are not eaſily brought to a good figure, nebulous appearances of both ſorts may probably come on much before the diſtance annexed to them in the table.

TABLE II.

Stars in the field.	Visual ray.	Stars	Ray.	Stars.	Ray.	Stars.	Ray.	Stars.	Ray.
		31	186	71	245	210	352	700	527
0,1	27	32	188	72	246	220	358	800	551
0,2	34	33	190	73	247	230	363	900	573
0,3	39	34	192	74	249	240	368	1000	593
0,4	43	35	193	75	250	250	374	10000	1280
0,5	46	36	195	76	251	260	378	100000	2758
0,6	49	37	197	77	252	270	383		
0,7	52	38	199	78	253	280	388		
0,8	54	39	201	79	254	290	393		
0,9	56	40	202	80	255	300	397		
1	58	41	204	81	256	310	401		
2	74	42	206	82	257	320	406	636175 or resolvable nebulosity	5112
3	85	43	207	83	258	330	410		
4	93	44	209	84	259	340	414		
5	101	45	210	85	260	350	418		
6	107	46	212	86	261	360	422		
7	113	47	214	87	262	370	426		
8	118	48	215	88	263	380	430		
9	123	49	217	89	264	390	433		
10	127	50	218	90	265	400	437		
11	131	51	219	91	266	410	441		
12	135	52	221	92	267	420	444	2544700 or milky nebulosity	8115
13	139	53	222	93	268	430	448		
14	142	54	224	94	269	440	451		
15	146	55	225	95	270	450	455		
16	149	56	226	96	271	460	458		
17	152	57	228	97	272	470	461		
18	155	58	229	98	273	480	464		
19	158	59	230	99	274	490	468		
20	160	60	232	100	275	500	471		
21	163	61	233	110	284	510	474		
22	166	62	234	120	291	520	477		
23	168	63	236	130	300	530	480		
24	170	64	237	140	308	540	483		
25	173	65	238	150	315	550	486		
26	175	66	239	160	322	560	489		
27	177	67	240	170	328	570	492		
28	180	68	242	180	335	580	495		
29	182	69	243	190	341	590	498		
30	184	70	244	200	347	600	500		

Section

Section of our sidereal system.

By taking out of this table the visual rays which answer to the gages, and applying lines proportional to them around a point, according to their respective right ascensions and north polar distances, we may delineate a solid by means of the ends of these lines, which will give us so many points in its surface; I shall, however, content myself at present with a section only. I have taken one which passes through the poles of our system, and is at rectangles to the conjunction of the branches which I have called its length. The name of poles seemed to me not improperly applied to those points which are 90 degrees distant from a circle passing along the milky way, and the north pole is here assumed to be situated in R.A. 186° and P.D. 58°. The section represented in fig. 4. is one which makes an angle of 35 degrees with our equator, crossing it in 124½ and 304½ degrees. A celestial globe, adjusted to the latitude of 55° north, and having σ Ceti near the meridian, will have the plane of this section pointed out by the horizon, and the gages which have been used in this delineation are those which in table I. are marked by asterisks. When the visual rays answering to them are taken out of the second table, they must be projected on the plane of the horizon of the latitude which has been pointed out; and this may be done accurately enough for the present purpose by a globe adjusted as above directed; for as gages, exactly in the plane of the section, were often wanting, I have used many at some small distance above and below the same, for the sake of obtaining more delineating points; and in the figure the stars at the borders which are larger than the rest are those pointed out by the gages. The

intermediate parts are filled up by ſmaller ſtars arranged in ſtraight lines between the gaged ones. The delineating points, though pretty numerous, are not ſo cloſe as we might wiſh; it is however to be hoped that in ſome future time this branch of aſtronomy will become more cultivated, ſo that we may have gages for every quarter of a degree of the heavens at leaſt, and theſe often repeated in the moſt favourable circumſtances. And whenever that ſhall be the caſe, the delineations may then be repeated with all the accuracy that long experience may enable us to introduce; for, this ſubject being ſo new, I look upon what is here given partly as only an example to illuſtrate the ſpirit of the method. From this figure however, which I hope is not a very inaccurate one, we may ſee that our nebula, as we obſerved before, is of the third form; that is: *A very extenſive, branching, compound Congeries of many millions of ſtars*; which moſt probably owes its origin to many remarkably large as well as pretty cloſely ſcattered ſmall ſtars, that may have drawn together the reſt. Now, to have ſome idea of the wonderful extent of this ſyſtem, I muſt obſerve that this ſection of it is drawn upon a ſcale where the diſtance of Sirius is no more than the 80th part of an inch; ſo that probably all the ſtars, which in the fineſt nights we are able to diſtinguiſh with the naked eye, may be comprehended within a ſphere, drawn round the large ſtar near the middle, repreſenting our ſituation in the nebula, of leſs than half a quarter of an inch radius.

The Origin of nebulous Strata.

If it were poſſible to diſtinguiſh between the parts of an indefinitely extended whole, the nebula we inhabit might be

ſaid

said to be one that has fewer marks of profound antiquity upon it than the rest. To explain this idea perhaps more clearly, we should recollect that the condensation of clusters of stars has been ascribed to a gradual approach; and whoever reflects on the numbers of ages that must have past before some of the clusters, that will be found in my intended catalogue of them, could be so far condensed as we find them at present, will not wonder if I ascribe a certain air of youth and vigour to many very regularly scattered regions of our sidereal stratum. There are moreover many places in it where there is the greatest reason to believe that the stars, if we may judge from appearances, are now drawing towards various secondary centers, and will in time separate into different clusters, so as to occasion many sub-divisions. Hence we may surmise that when a nebulous stratum consists chiefly of nebulæ of the first and second form, it probably owes its origin to what may be called the decay of a great compound nebula of the third form; and that the sub-divisions, which happened to it in length of time, occasioned all the small nebulæ which sprung from it to lie in a certain range, according as they were detached from the primary one. In like manner our system, after numbers of ages, may very possibly become divided so as to give rise to a stratum of two or three hundred nebulæ; for it would not be difficult to point out so many beginning or gathering clusters in it (*f*). This view of the present subject throws a considerable light upon the appearance of that remarkable collection of many

(*f*) Mr. Michell has also considered the stars as gathered together into groups (Phil. Transf. vol. LVII. p. 249.); which idea agrees with the sub-division of our great system here pointed out. He founds an elegant proof of this on the computation of probabilities, and mentions the Pleiades, the Præsepe Cancri, and the nebula (or cluster of stars) in the hilt of Perseus's sword, as instances.

hundreds

hundreds of nebulæ which are to be ſeen in what I have called the nebulous ſtratum of Coma Berenices. It appears from the extended and branching figure of our nebula, that there is room for the decompoſed ſmall nebulæ of a large, reduced, former great one to approach nearer to us in the ſides than in other parts. Nay, poſſibly, there might originally be another very large joining branch, which in time became ſeparated by the condenſation of the ſtars; and this may be the reaſon of the little remaining breadth of our ſyſtem in that very place: for the nebulæ of the ſtratum of the Coma are brighteſt and moſt crowded juſt oppoſite our ſituation, or in the pole of our ſyſtem. As ſoon as this idea was ſuggeſted, I tried alſo the oppoſite pole, where accordingly I have met with a great number of nebulæ, though under a much more ſcattered form.

An Opening in the heavens.

Some parts of our ſyſtem indeed ſeem already to have ſuſtained greater ravages of time than others, if this way of expreſſing myſelf may be allowed; for inſtance, in the body of the Scorpion is an opening, or hole, which is probably owing to this cauſe. I found it while I was gaging in the parallel from 112 to 114 degrees of north polar diſtance. As I approached the milky way, the gages had been gradually running up from 9,7 to 17,1; when, all of a ſudden, they fell down to nothing, a very few pretty large ſtars excepted, which made them ſhew 0,5, 0,7, 1,1, 1,4, 1,8; after which they again roſe to 4,7, 13,5, 20,3, and ſoon after to 41,1. This opening is at leaſt 4 degrees broad, but its height I have not yet aſcertained. It is remarkable, that the 80 *Nebuleuſe ſans étoiles* of the *Connoiſſance des Temps*, which is one of the richeſt and moſt compreſſed

preſſed cluſters of ſmall ſtars I remember to have ſeen, is ſituated juſt on the weſtern border of it, and would almoſt authoriſe a ſuſpicion that the ſtars, of which it is compoſed, were collected from that place, and had left the vacancy. What adds not a little to this ſurmiſe is, that the ſame phænomenon is once more repeated with the fourth cluſter of ſtars of the *Connoiſſance des Temps*; which is alſo on the weſtern border of another vacancy, and has moreover a ſmall, miniature cluſter, or eaſily reſolvable nebula of about 2½ minutes in diameter, north following it, at no very great diſtance.

Phænomena at the Poles of our Nebula.

I ought to obſerve, that there is a remarkable purity or clearneſs in the heavens when we look out of our ſtratum at the ſides; that is, towards Leo, Virgo, and Coma Berenices on one hand, and towards Cetus on the other; whereas the ground of the heavens becomes troubled as we approach towards the length or height of it. It was a good while before I could trace the cauſe of theſe phænomena; but ſince I have been acquainted with the ſhape of our ſyſtem, it is plain that theſe troubled appearances, when we approach to the ſides, are eaſily to be explained by aſcribing them to ſome of the diſtant, ſtraggling ſtars, that yield hardly light enough to be diſtinguiſhed. And I have, indeed, often experienced this to be actually the cauſe, by examining theſe troubled ſpots for a long while together, when, at laſt, I generally perceived the ſtars which occaſioned them. But when we look towards the poles of our ſyſtem, where the viſual ray does not graze along the ſide, the

ſtraggling ſtars of courſe will be very few in number; and therefore the ground of the heavens will aſſume that purity which I have always obſerved to take place in thoſe regions.

Enumeration of very compound Nebulæ or Milky-Ways.

As we are uſed to call the appearance of the heavens, where it is ſurrounded with a bright zone, the Milky-Way, it may not be amiſs to point out ſome other very remarkable Nebulæ which cannot well be leſs, but are probably much larger than our own ſyſtem; and, being alſo extended, the inhabitants of the planets that attend the ſtars which compoſe them muſt likewiſe perceive the ſame phænomena. For which reaſon they may alſo be called milky-ways by way of diſtinction.

My opinion of their ſize is grounded on the following obſervations. There are many round nebulæ, of the firſt form, of about five or ſix minutes in diameter, the ſtars of which I can ſee very diſtinctly; and on comparing them with the viſual ray calculated from ſome of my long gages, I ſuppoſe, by the appearance of the ſmall ſtars in thoſe gages, that the centers of theſe round nebulæ may be 600 times the diſtance of Sirius from us.

In eſtimating the diſtance of ſuch cluſters I conſulted rather the comparatively apparent ſize of the ſtars than their mutual diſtance; for the condenſation in theſe cluſters being probably much greater than in our own ſyſtem, if we were to overlook this circumſtance and calculate by their apparent compreſſion, where, in about ſix minutes diameter, there are perhaps ten or more ſtars in the line of meaſures, we ſhould find, that on the ſuppoſition of an equal ſcattering of the ſtars throughout all nebulæ, the diſtance of the center of ſuch a cluſter from us could not be leſs than 6000 times the diſtance of

of Sirius. And, perhaps, in putting it, by the apparent size of the stars, at 600 only, I may have considerably under-rated it; but my argument, if that should be the case, will be so much the stronger. Now to proceed,

Some of these round nebulæ have others near them, perfectly similar in form, colour, and the distribution of stars, but of only half the diameter: and the stars in them seem to be doubly crowded, and only at about half the distance from each other: they are indeed so small as not to be visible without the utmost attention. I suppose these miniature nebulæ to be at double the distance of the first. An instance, equally remarkable and instructive, is a case where, in the neighbourhood of two such nebulæ as have been mentioned, I met with a third, similar, resolvable, but much smaller and fainter nebula. The stars of it are no longer to be perceived; but a resemblance of colour with the former two, and its diminished size and light, may well permit us to place it at full twice the distance of the second, or about four or five times that of the first. And yet the nebulosity is not of the milky kind; nor is it so much as difficultly resolvable, or colourless. Now, in a few of the extended nebulæ, the light changes gradually so as from the resolvable to approach to the milky kind; which appears to me an indication that the milky light of nebulæ is owing to their much greater distance. A nebula, therefore, whose light is perfectly milky, cannot well be supposed to be at less than six or eight thousand times the distance of Sirius; and though the numbers here assumed are not to be taken otherwise than as very coarse estimates, yet an extended nebula, which in an oblique situation, where it is possibly fore-shortened by one-half, two-thirds, or three-fourths of its length, subtends a degree or more in

 diameter,

diameter, cannot be otherwiſe than of a wonderful magnitude, and may well outvie our milky-way in grandeur.

The firſt I ſhall mention is a milky Ray of more than a degree in length. It takes *k* (FL. 52.) Cygni into its extent, to the north of which it is crookedly bent ſo as to be convex towards the following ſide; and the light of it is pretty intenſe. To the ſouth of *k* it is more diffuſed, leſs bright, and loſes itſelf with ſome extenſion in two branches, I believe; but for want of light I could not determine this circumſtance. The northern half is near two minutes broad, but the ſouthern is not ſufficiently defined to aſcertain its breadth.

The next is an extremely faint milky Ray, above ¾ degree long, and 8 or 10′ broad; extended from north preceding to ſouth following. It makes an angle of about 30 or 40 degrees with the meridian, and contains three or four places that are brighter than the reſt. The ſtars of the Galaxy are ſcattered over it in the ſame manner as over the reſt of the heavens. It follows ε Cygni 11,5 minutes in time, and is 2° 19′ more ſouth.

The third is a branching Nebuloſity of about a degree and a half in right aſcenſion, and about 48′ extent in polar diſtance. The following part of it is divided into ſeveral ſtreams and windings, which, after ſeparating, meet each other again towards the ſouth. It precedes ζ Cygni 16′ in time, and is 1° 16′ more north. I ſuppoſe this to be joined to the preceding one; but having obſerved them in different ſweeps, there was no opportunity of tracing their connection.

The fourth is a faint, extended milky Ray of about 17′ in length, and 12′ in breadth. It is brighteſt and broadeſt in the middle, and the ends loſe themſelves. It has a ſmall, round, very faint nebula juſt north of it; and alſo, in another place, a ſpot, brighter than the reſt, almoſt detached enough to form a different

a different nebula, but probably belonging to the great one. The Ray precedes α Trianguli 18′,8 in time, and is 55′ more north. Another observation of the same, in a finer evening, mentions its extending much farther towards the south, and that the breadth of it probably is not less than half a degree; but being shaded away by imperceptible gradations, it is difficult exactly to assign its limits.

The fifth is a Streak of light about 27′ long, and in the brightest part 3 or 4′ broad. The extent is nearly in the meridian, or a little from south preceding to north following. It follows β Ceti 5′,9 in time, and is 2° 43′ more south. The situation is so low, that it would probably appear of a much greater extent in a higher altitude.

The sixth is an extensive milky Nebulosity divided into two parts; the most north being the strongest. Its extent exceeds 15′; the southern part is followed by a parcel of stars which I suppose to be the 8th of the *Connoissance des Temps*.

The seventh is a wonderful, extensive Nebulosity of the milky kind. There are several stars visible in it, but they can have no connection with that nebulosity, and are, doubtless, belonging to our own system scattered before it. It is the 17th of the *Connoissance des Temps*.

In the list of these must also be reckoned the beautiful Nebula of Orion. Its extent is much above one degree; the eastern branch passes between two very small stars, and runs on till it meets a very bright one. Close to the four small stars, which can have no connection with the nebula, is a total blackness; and within the open part, towards the north-east, is a distinct, small, faint nebula, of an extended shape, at a distance from the border of the great one, to which it runs in a parallel

direction, resembling the shoals that are seen near the coasts of some islands.

The ninth is that in the girdle of Andromeda, which is undoubtedly the nearest of all the great nebulæ; its extent is above a degree and a half in length, and, in even one of the narrowest places, not less than 16′ in breadth. The brightest part of it approaches to the resolvable nebulosity, and begins to shew a faint red colour; which, from many observations on the colour and magnitude of nebulæ, I believe to be an indication that its distance in this coloured part does not exceed 2000 times the distance of Sirius. There is a very considerable, broad, pretty faint, small nebula near it; my Sister discovered it August 27, 1783, with a Newtonian 2-feet sweeper. It shews the same faint colour with the great one, and is, no doubt, in the neighbourhood of it. It is not the 3rd of the *Connoissance des Temps*; which is a pretty large round nebula, much condensed in the middle, and south following the great one; but this is about two-thirds of a degree north preceding it, in a line parallel to β and ν Andromedæ.

To these may be added the nebula in Vulpecula: for, though its appearance is not large, it is probably a double stratum of stars of a very great extent, one end whereof is turned towards us. That it is thus situated may be surmised from its containing, in different parts, nearly all the three nebulosities; *viz.* the resolvable, the coloured but irresolvable, and a tincture of the milky kind. Now, what great length must be required to produce these effects may easily be conceived when, in all probability, our whole system, of about 800 stars in diameter, if it were seen at such a distance that one end of it might assume the resolvable nebulosity, would not, at the other end, present us

us with the irresolvable, much less with the colourless and milky sort of nebulosities.

A Perforated Nebula, or Ring of Stars.

Among the curiosities of the heavens should be placed a nebula, that has a regular, concentric, dark spot in the middle, and is probably a Ring of stars. It is of an oval shape, the shorter axis being to the longer as about 83 to 100; so that, if the stars form a circle, its inclination to a line drawn from the sun to the center of this nebula must be about 56 degrees. The light is of the resolvable kind, and in the northern side three very faint stars may be seen, as also one or two in the southern part. The vertices of the longer axis seem less bright and not so well defined as the rest. There are several small stars very near, but none that seem to belong to it. It is the 57th of the *Connoissance des Temps.* Fig. 5. is a representation of it.

Planetary Nebulæ.

I shall conclude this paper with an account of a few heavenly bodies, that from their singular appearance leave me almost in doubt where to class them.

The first precedes ν Aquarii 5′,4 in time, and is 1′ more north. Its place, with regard to a small star Sept. 7, 1782, was, Distance 8′ 13″ 51‴; but on account of the low situation, and other unfavourable circumstances, the measure cannot be very exact. August 25, 1783, Distance 7′ 5″ 11‴, very exact, and to my satisfaction; the light being thrown in by an opaque-microscopic-illumination (g). Sept. 20, 1783, Position 41° 24′ south

(g) It may be of use to explain this kind of illumination for which the Newtonian reflector is admirably constructed. On the side opposite the eye-piece an opening is to be made in the tube, through which the light may be thrown in, so as to fall on some reflecting body, or concave perforated mirror, within the eye-piece,

south preceding the same star; very exact, and by the same kind of illumination. Oct. 17, 1783, Distance 6′ 55″ 7‴; a second measure 6′ 56″ 11‴, as exact as possible. Oct. 23, 1783, Position 42° 57′; a second measure 42° 45′; single lens; power 71; opaque microscopic-illumination. Nov. 14, 1783, Distance 7′ 4″ 35‴. Nov. 12, 1784, Distance 7′ 22″ 35‴; Position 38° 39′. Its diameter is about 10 or 15″. I have examined it with the powers of 71, 227, 278, 460, and 932; and it follows the laws of magnifying, so that its body is no illusion of light. It is a little oval, and in the 7-feet reflector pretty well defined, but not sharp on the edges. In the 20-feet, of 18,7 inch aperture, it is much better defined, and has much of a planetary appearance, being all over of an uniform brightness, in which it differs from nebulæ: its light seems however to be of the starry nature, which suffers not nearly so much as the planetary disks are known to do, when much magnified.

The second of these bodies precedes the 13th of FLAMSTEED's Andromeda about 1′6 in time, and is 22′ more south. It has a round, bright, pretty well defined planetary disk of about 12″ diameter, and is a little elliptical. When it is viewed with a 7-feet reflector, or other inferior instruments, it is not nearly so well defined as with the 20-feet. Its situation with regard to a pretty considerable star is, Distance (with a compound glass of a low power) 7′ 51″ 34‴. Position 12° 0′ f. preceding. Diameter taken with 278, 14″ 42‴.

The third follows B (FL. 44.) Ophiuchi 4′,1 in time, and is 23′ more north. It is round, tolerably well defined, and pretty bright; its diameter is about 30″.

piece, that may throw it back upon the wires. By this means none of the direct rays can reach the eye, and those few which are reflected again from the wires do not interfere sensibly with the faintest objects, which may thus be seen undisturbed.

The

The fourth follows η Sagittæ 17′,1 in time, and is 2′ more north. It is perfectly round, pretty bright, and pretty well defined; about ¾ min. in diameter.

The fifth follows the 21ſt Vulpeculæ 2′,1 in time, and is 1° 46′ more north. It is exactly round, of an equal light throughout, but pretty faint, and about 1′ in diameter.

The ſixth precedes *h* (Fl. 39.) Cygni 8′,1 in time, and is 1° 26′ more ſouth. It is perfectly round, and of an equal light, but pretty faint; its diameter is near 1′, and the edges are pretty well defined.

The planetary appearance of the two firſt is ſo remarkable, that we can hardly ſuppoſe them to be nebulæ; their light is ſo uniform, as well as vivid, the diameters ſo ſmall and well defined, as to make it almoſt improbable they ſhould belong to that ſpecies of bodies. On the other hand, the effect of different powers ſeems to be much againſt their light's being of a planetary nature, ſince it preſerves its brightneſs nearly in the ſame manner as the ſtars do in ſimilar trials. If we would ſuppoſe them to be ſingle ſtars with large diameters we ſhall find it difficult to account for their not being brighter; unleſs we ſhould admit that the intrinſic light of ſome ſtars may be very much inferior to that of the generality, which however can hardly be imagined to extend to ſuch a degree. We might ſuſpect them to be comets about their aphelion, if the brightneſs as well as magnitude of the diameters did not oppoſe this idea; ſo that after all, we can hardly find any hypotheſis ſo probable as that of their being Nebulæ; but then they muſt conſiſt of ſtars that are compreſſed and accumulated in the higheſt degree. If it were not perhaps too hazardous to purſue a former ſurmiſe of a renewal in what I figuratively called the Laboratories of the univerſe, the ſtars forming theſe extraordinary nebulæ, by ſome decay or waſte of nature, being no longer

 fit

fit for their former purpofes, and having their projectile forces, if any fuch they had, retarded in each others atmofphere, may rufh at laft together, and either in fucceffion, or by one general tremendous fhock, unite into a new body. Perhaps the extraordinary and fudden blaze of a new ftar in Caffiopea's chair, in 1572, might poffibly be of fuch a nature. But left I fhould be led too far from the path of obfervation, to which I am refolved to limit myfelf, I fhall only point out a confiderable ufe that may be made of thefe curious bodies. If a little attention to them fhould prove that, having no annual parallax, they belong moft probably to the clafs of nebulæ, they may then be expected to keep their fituation better than any one of the ftars belonging to our fyftem, on account of their being probably at a very great diftance. Now to have a fixed point fomewhere in the heavens, to which the motions of the reft may be referred, is certainly of confiderable confequence in Aftronomy; and both thefe bodies are bright and fmall enough to anfwer that end (*b*).

Datchet near Windfor,
January 1, 1785.

W. HERSCHEL.

(*b*) Having found two more of thefe curious objects, I add the place of them here, in hopes that thofe who have fixed inftruments may be induced to take an early opportunity of obferving them carefully.

Feb. 1, 1785. A very bright, planetary nebula, about half a minute in diameter, but the edges are not very well defined. It is perfectly round, or perhaps a very little elliptical, and all over of an uniform brightnefs: with higher powers it becomes proportionally magnified. It follows γ Eridani 16′ 16″ in time, and is 49′ more north than that ftar.

Feb. 7, 1785. A beautiful, very brilliant globe of light; a little hazy on the edges, but the hazinefs goes off very fuddenly, fo as not to exceed the 20th part of the diameter, which I fuppofe to be from 30 to 40″. It is round, or perhaps a very little elliptical, and all over of an uniform brightnefs: I fuppofe the intenfity of its light to be equal to that of a ftar of the ninth magnitude. It precedes the third *b* (FL. 6.) Crateris 28′ 36″ in time, and is 1° 25′ more north than that ftar.

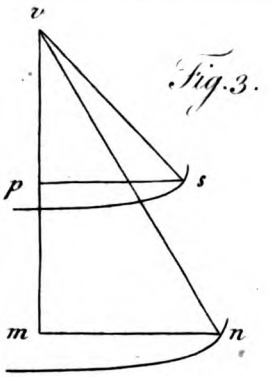


Fig. 3.

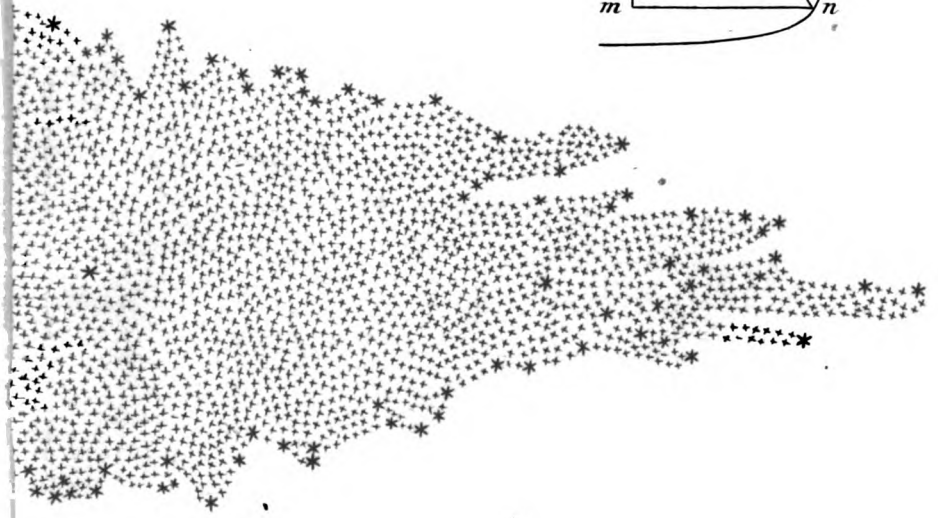


Fig. 5.

Sgitre Sc.

XIII. *Remarks on ſpecific Gravities taken at different Degrees of Heat, and an eaſy Method of reducing them to a common Standard.* *By* Richard Kirwan, *Eſq. F. R. S.*

Read February 17, 1785.

THAT a comparative view of the weights of equal volumes of water and all other ſubſtances is highly uſeful on many occaſions, is too well known to require any proof; but that a principal uſe reſulting from this compariſon, when properly made, is unattainable by a peruſal of the common tables, I ſhall here endeavour to ſhew, and at the ſame time point out a remedy for this defect.

One capital advantage derivable from a table of ſpecific gravities, is the knowledge of the abſolute weight of any ſolid meaſure of the ſubſtances therein contained, or that of the ſolid meaſure of a given weight of thoſe ſubſtances, a cubic foot of water being ſuppoſed to weigh 1000 ounces avoirdupois, and conſequently a cubic inch of water weighing 253,182 grains. But the authors who have diſcovered this equation of weight and meaſure, and all thoſe who have ſince treated this ſubject, have neglected to inform us of the temperature at which this agreement takes place; yet that it cannot take place in all temperatures is evident from the experiments of Dr. HALLEY and others, who have found, that from a few degrees above the freezing to the boiling point, water is dilated about $\frac{1}{20}$ of its bulk; and, conſequently, if 1000 ounces at the freezing point be equal to one cubic foot, they muſt be equal at the boiling

point to one cubic foot and 66,46 cubic inches. And if the dilatations are proportional to the degrees of heat throughout the ſcale, there muſt be an augmentation of 3,136 cubic inches *per* cubic foot, produced by every 10 degrees of heat. Both theſe points remain, therefore, to be determined; firſt, at what temperature a cubic foot of water weighs exactly 1000 ounces avoirdupois; and, 2dly, whether the dilatations produced by ſucceſſive degrees of heat are proportional to the degrees that produce them. This laſt point has indeed been handled by others, but with different views; and their determinations are not eaſily applicable to the preſent queſtion.

To examine this matter experimentally, I ordered a hollow tinned iron cone to be made, of four inches diameter in the baſe, one-tenth of an inch diameter in the ſummit inſide, and 10 inches perpendicular height, whoſe ſolid contents ſhould be 42,961 cubic inches, but by a ſlight diminution of the diameter, and a protuberance ariſing from the ſoldering, I found it to contain, in the temperature of 62°, but 42,731 cubic inches, according to the eſtimation of 1000 ounces to the cubic foot; and having filled it by immerſion in boiling water, and taken it up at different degrees of heat, and weighed it when cold, I found its contents as expreſſed in the following table; the firſt column of which ſhews the degrees of heat at which it was taken up; the ſecond, the weight of the water contained in it; the third, the diminution of weight occaſioned by thoſe degrees of heat; the fourth, the ſum of the diminutions of weight in the cubic foot, by the preceding degrees of heat; the fifth ſhews the weight of a cubic inch of water in each of thoſe degrees of heat; and the ſixth, the augmentation of bulk in the cubic foot by every 20° of heat. The horizontal lines, marked thus *, I have added from the experiments of Mr.

BLADH, in the Memoirs of the Academy of Stockholm for the year 1776, whose determinations, as far as they reached, agreed very nearly with mine. The water I used was common water well boiled and filtered. The experiments were for the most part three times repeated, and the difference in each trial amounted to a very few grains.

I.	II.	III.	IV.	V.	VI.	
Degrees	Contents of the cone in grains.	Dimin. in grains.	Sum of dim. in a cubic foot.	Weight of a cubic inch.	Increase in cubic inches.	
			Grs.			
212	10418,75	29,5	16589	243,8	4,892	
202	10448,25	77,5	15354	244,51	12,818	
182	10525,75	71,75	12133	246,33	11,533	
162	10596,00	62,60	9171	247,97	10,209	
142	10658,60	56,15	6602	249,43	9,103	
122	10714,75	49,00	4310	250,75	7,920	
102	10763,75	35,5	2226	251,89	5,7	
82	10799,25	19,5	788	252,72	3,120	
*75	- - -	- -	- - -	252,8	- - -	
*70	- - -	- -	- - -	252,97	- - -	
*66	- - -	- -	- - -	253,06	- - -	
62	10818,75	0	0	253,182	0	Total increase of bulk from 62° to 212°=65,327 cubic inches.
*56	- - -	- -	- - -	253,3	- - -	
		Increase	Increase			
*50	- - -	- -	- - -	253,46	- - -	
					Decrease	Total from 36° to 212=67,327 cubic inches.
42	10830,75	12	485,3	253,463	1,936	
*36,5	- - -	- -	- - -	253,5	0,064	

Hence we see, that a cubic foot of water weighs 485,3 grains more at 42° than at 62°, and consequently is equal to 1001,109 avoirdupois ounces, and in the temperature of 82° it weighs less than at 62° by 788,5 grains, and therefore is equal to 998,198 ounces. At the boiling point it wants 16589 grains, or 37,915 ounces of the weight it possesses at 62°, and consequently weighs but 962,085 ounces, &c.

In

In this calculation I take no account of the difference arising from the expansion of the vessel, it being only 0,067 of an inch at the boiling point; for, according to BOUGUER, iron is dilated 0,00055 of its bulk from the freezing to the boiling point; consequently 42,961 cubic inches gain only 0,067 of an inch, augmenting the diameter and perpendicular height of this frustum of a cone at the boiling point in that proportion.

Hence also we see, that the expansions of water are not proportional to the degrees of heat; for by 20 degrees of heat from 62° to 82° a cubic foot of water is dilated only 3,12 inches, but by the next 20 degrees of heat, that is, from 82° to 102°, it is expanded 5,7 inches, &c.

Mr. BLADH found the volume of water at 32° to be equal to that at 53°,6; but that this irregular expansion ceased at 36°6, and, according to Mr. DE LUC (who first discovered it) at 43°.

As the expansion of liquids by equal degrees of heat is much greater than that of solids, it happens, that the specific gravities of the same solid taken at different temperatures will be different; and, what appears more extraordinary, the same solid will appear specifically heavier in higher than in lower temperatures; for the same volume of water being lighter in higher than in lower temperatures, the solid will lose less of its weight in it in the former than in the latter case: this mistake we may remedy by inspecting the fifth column of the foregoing table and the following analogy: as the weight of a cubic inch of water at the temperature of 62° is to the weight of a cubic inch of water at *n* degrees of temperature, so is the specific gravity found at *n* degrees of temperature to that which will be found at 62°.

Thus, if 1000 grains of iron be weighed in water of the temperature of 62°, and it loses therein 13,333 grains, if the same

ſame piece of iron be weighed in water of the temperature of 75°, it will loſe but 13,313 grains; for the loſſes of weight will be as the weights of equal volumes of water at thoſe temperatures, which, as we have ſeen, are as 253,18 to 252,8; therefore, its ſpecific gravity in water of the temperature of 62° will be 7,49; and in water of the temperature of 75°. 7,511; but we may correct this by the above analogy, for ∺ 253,8 . 252,18 :: 7,511 . 7,49.

By this means we obtain the advantage of diſcovering the true weight of a cubic foot of any ſubſtance whoſe ſpecific gravity is known, which it is now plain cannot be known when bodies are hydroſtatically weighed at any temperature a few degrees above or below 62°, without ſuch reduction, or ſubtracting the quantities in the fourth column.

This method is equally applicable, and with equal neceſſity, to other means of finding ſpecific gravities, as areometers, the compariſon of the weights of equal meaſures of liquids, the different loſſes of weight of the ſame ſolid, when weighed in different liquids, &c. In all which caſes the weight of water at 62°, or the loſs of weight of a ſolid in water at 62°, ſhould be found by the above analogy.

Dr. HALES and ſome others have eſtimated the weight of a cubic inch of water at 254 grains, which is an evident miſtake, as it is true in no degree of temperature, and produces an error of more than three ounces in the cubic foot.

XIV. *Electrical Experiments made in order to ascertain the non-conducting Power of a perfect Vacuum, &c. By Mr.* William Morgan; *communicated by the Rev.* Richard Price, *LL.D. F.R.S.*

Read February 24, 1785.

THE non-conducting power of a perfect vacuum is a fact in electricity which has been much controverted among philosophers. The experiments made by Mr. WALSH, F.R.S. in the double barometer tube clearly demonstrated the impermeability of the electric *light* through a vacuum; nor was it, I think, precipitate to conclude from them the impermeability of the electric *fluid* itself. But this conclusion has not been universally admitted, and the following experiments were made with the view of determining its truth or fallacy. When I first attended to the subject, I was not aware that any other attempts had been made besides those of Mr. WALSH; and though I have since found myself to have been in part anticipated in one of my experiments, it may not perhaps be improper to give some account of them, not only as they are an additional testimony in support of this fact, but as they led to the observation of some phænomena which appear to be new and interesting.

A mercurial gage B (see tab. IX. fig. 1.) about 15 inches long, carefully and accurately boiled till every particle of air was expelled from the inside, was coated with tin-foil five inches down from its sealed end (A), and being inverted into mercury

mercury through a perforation (D) in the brafs cap (E) which covered the mouth of the ciftern (H), the whole was cemented together, and the air was exhaufted from the infide of the ciftern through a valve (C) in the brafs cap (E) juft mentioned, which producing a perfect vacuum in the gage (B) afforded an inftrument peculiarly well adapted for experiments of this kind. Things being thus adjufted (a fmall wire (F) having been previoufly fixed on the infide of the ciftern to form a communication between the brafs cap (E) and the mercury (G) into which the gage was inverted) the coated end (A) was applied to the conductor of an electrical machine, and notwithftanding every effort, neither the fmalleft ray of light, nor the flighteft charge, could ever be procured in this exhaufted gage. I need not obferve, that if the vacuum on its infide had been a conductor of electricity, the latter at leaft muft have taken place, for it is well known (and I have myfelf often made the experiment) that if a glafs tube be exhaufted by an air-pump, and coated on the outfide, both light and a charge may very readily be procured. If the mercury in the gage be imperfectly boiled, the experiment will not fucceed; but the colour of the electric light, which, in air rarefied by an exhaufter, is always violet or purple, appears in this cafe of a beautiful green, and, what is very curious, the degree of the air's rarefaction may be nearly determined by this means; for I have known inftances, during the courfe of thefe experiments, where a fmall particle of air having found its way into the tube (B), the electric light became vifible, and as ufual of a green colour; but the charge being often repeated, the gage has at length cracked at its fealed end, and in confequence the external air, by being admitted into the infide, has gradually produced a change in the electric light from green to blue, from blue to indigo, and

so on to violet and purple, till the medium has at last become so dense as no longer to be a conductor of electricity. I think there can be little doubt from the above experiments of the non-conducting power of a perfect vacuum; and this fact is still more strongly confirmed by the phænomena which appear upon the admission of a very minute particle of air into the inside of the gage. In this case the whole becomes immediately luminous upon the slightest application of electricity, and a charge takes place, which continues to grow more and more powerful in proportion as fresh air is admitted, till the density of the conducting medium arrives at its maximum, which it always does when the colour of the electric light is indigo or violet. Under these circumstances the charge may be so far increased as frequently to break the glass. In some tubes, which have not been completely boiled, I have observed, that they will not conduct the electric fluid when the mercury is fallen very low in them, yet upon letting in air into the cistern (H), so that the mercury shall rise in the gage (B), the electric fluid, which was before latent in the inside, shall now become visible, and as the mercury continues to rise, and of consequence the medium is rendered less rare, the light shall grow more and more visible, and the gage shall at last be charged, notwithstanding it has not been near an electrical machine for two or three days. This seems to prove, that there is a limit even in the rarefaction of air, which sets bounds to its conducting power; or, in other words, that the particles of air may be so far separated from each other as no longer to be able to transmit the electric fluid; that if they are brought within a certain distance of each other, their conducting power begins, and continually increases till their approach also arrives at its limit, when the particles again become so near as to resist the passage

of the fluid entirely, without employing violence, which is the case in common and condensed air, but more particularly in the latter. These experiments, however, belong to another subject, and may possibly be communicated at some future time.

It is surprising to observe, how readily an exhausted tube is charged with electricity. By placing it at ten or twelve inches from the conductor the light may be seen pervading its inside, and as strong a charge may sometimes be procured as if it were in contact with the conductor: nor does it signify how narrow the bore of the glass may be; for even a thermometer tube, having the minutest perforation possible, will charge with the utmost facility; and in this experiment the phænomena are peculiarly beautiful.

Let one end of a thermometer tube be sealed hermetically. Let the other end be cemented into a brass cap with a valve, or into a brass cock, so that it may be fitted to the plate of an air-pump. When it is exhausted, let the sealed end be applied to the conductor of an electrical machine, while the other end is either held in the hand or connected to the floor. Upon the slightest excitation the electric fluid will accumulate at the sealed end, and be discharged through the inside in the form of a spark, and this accumulation and discharge may be incessantly repeated till the tube is broken. By this means I have had a spark 42 inches long, and, had I been provided with a proper tube, I do not doubt but that I might have had a spark of four times that length. If, instead of the sealed end, a bulb be blown at that extremity of the tube, the electric light will fill the whole of that bulb, and then pass through the tube in the form of a brilliant spark, as in the foregoing experiment; but in this case I have seldom been able to repeat the trials above three or four

times before the charge has made a small perforation in the bulb. If again a thermometer filled with mercury be inverted into a cistern, and the air exhausted in the manner I have described for making the experiment with the gage, a Torricellian vacuum will be produced; and now the electric light in the bulb, as well as the spark in the tube, will be of a vivid green; but the bulb will not bear a frequent repetition of charges before it is perforated in like manner as when it has been exhausted by an air-pump. It can hardly be necessary to observe, that in these cases the electric fluid assumes the appearance of a spark *, from the narrowness of the passage through which it forces its way. If a tube, 40 inches long, be fixed into a globe 8 or 9 inches in diameter, and the whole be exhausted, the electric fluid, after passing in the form of a brilliant spark throughout the length of the tube, will, when it gets into the inside of the globe, expand itself in all directions, entirely filling it with a violet and purple light, and exhibiting a striking instance of the vast elasticity of the electric fluid.

I cannot conclude this paper without acknowledging my obligations to the ingenious Mr. BROOK, of Norwich, who, by communicating to me his method of boiling mercury, has been the chief cause of my success in these experiments †. I have lately learned

* By cementing the string of a guittar into one end of a thermometer tube, a spark may be obtained as well as if the tube had been sealed hermetically.

† Mr. BROOK's method of making mercurial gages is nearly as follows. Let a glass tube L (see fig. 2.), sealed hermetically at one end, be bent into a right-angle within two or three inches of the other end. At the distance of about an inch or less from the angle let a bulb (K), of about ¾ of an inch in diameter, be blown in the curved end, and let the remainder of this part of the tube be drawn out (I) for

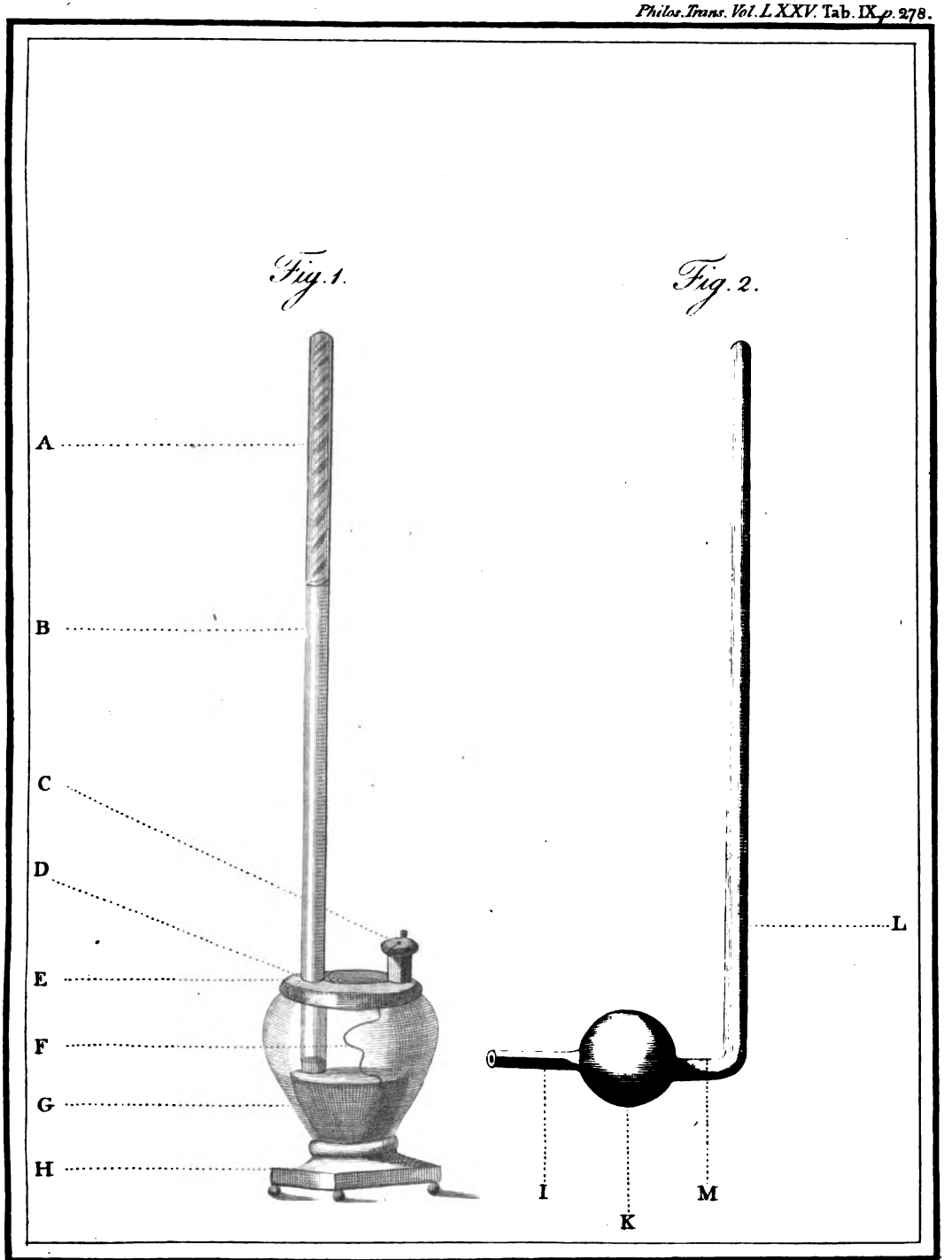
learned from him, that he has alſo aſcertained the non-conducting power of a perfect vacuum; but what ſteps he took for that purpoſe I know not. Of his accuracy, however, I am ſo well convinced, that had I never made an experiment myſelf, I ſhould, upon his teſtimony alone, have been equally aſſured of the fact. To moſt of the preceding experiments Dr. PRICE, Mr. LANE, and ſome others of my friends, have been eye-witneſſes, and I believe that they were as thoroughly ſatisfied as myſelf with the reſults of them. I muſt beg leave to obſerve to thoſe who wiſh to repeat them, that the firſt experiment requires ſome nicety, and no inconſiderable degree of labour and patience. I have boiled many gages for ſeveral hours together without ſucceſs,

ſo as to be ſufficiently long to take hold of, when the mercury is boiling. The bulb (K) is deſigned as a receptacle for the mercury, to prevent its boiling over, and the bent figure of the tube is adapted for its inverſion into the ciſtern; for by breaking off the tube at (M) within $\frac{1}{8}$ or $\frac{1}{4}$ of an inch of the angle, the open end of the gage may be held perpendicular to the horizon when it is dipped into the mercury in the ciſtern, without obliging us to bring our finger, or any other ſubſtance, into contact with the mercury in the gage, which never fails to render the inſtrument imperfect. It is neceſſary to obſerve, that if the tube be fourteen or fifteen inches long, I have never been able to boil it effectually for the experiments mentioned in this paper in leſs than three or four hours, although Mr. BROOK ſeems to preſcribe a much ſhorter time for the purpoſe; nor will it even then ſucceed, unleſs the greateſt attention be paid that no bubbles of air lurk behind, which to my own mortification I have frequently found to have been the caſe; but experience has at length taught me to guard pretty well againſt this diſappointment, particularly by taking care that the tube be completely dry before the mercury is put into it; for if this caution be not obſerved, the inſtrument can never be made perfect. There is, however, one evil which I have not yet been able to remedy; and that is, the introduction of air into the gage, owing to the unboiled mercury in the ciſtern; for when the gage has been a few times exhauſted, the mercury which originally filled it becomes mixed with that into which it is inverted, and in conſequence the vacuum is rendered leſs and leſs perfect, till at laſt the inſtrument is entirely ſpoiled. I have juſt conſtructed a gage ſo as to be able to boil the mercury in the ciſtern, but have not yet aſcertained its ſucceſs.

and

and was for ſome time diſpoſed to believe the contrary of what I am now convinced to be the truth. Indeed, if we reaſon *a priori*, I think we cannot ſuppoſe a perfect vacuum to be a perfect conductor without ſuppoſing an abſurdity: for if this were the caſe, either our atmoſphere muſt have long ago been deprived of all its electric fluid by being every where ſurrounded by a boundleſs conductor, or this fluid muſt pervade every part of infinite ſpace, and conſequently there can be no ſuch thing as a perfect vacuum in the univerſe. If, on the contrary, the truth of the preceding experiments be admitted, it will follow, that the conducting power of our atmoſphere increaſes only to a certain height, beyond which this power begins to diminiſh, till at laſt it entirely vaniſhes; but in what part of the upper regions of the air theſe limits are placed, I will not preſume to determine. It would not, perhaps, have been difficult to have applied the reſults of ſome of theſe experiments to the explanation of meteors, which are probably owing to an accumulation of electricity. It is not, however, my preſent deſign to give looſe to my imagination. I am ſenſible, that by indulging it too freely, much harm is done to real knowledge; and therefore, that one fact in philoſophy well aſcertained is more to be valued than whole volumes of ſpeculative hypotheſes.

Chatham-Place, Feb. 12, 1785.



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XV. *Experiments and Obſervations relating to Air and Water.* *By the Rev.* Joſeph Prieſtley, *LL.D. F.R.S.*

Read February 24, 1785.

EVER ſince the diſcovery of the diminution of reſpirable air in thoſe proceſſes which are generally called *phlogiſtic*, it has been a great object with philoſophers to find what becomes of the air which diſappears in them. Among others, I have made and publiſhed a variety of experiments with that view; but though by this means ſome farther progreſs was made in the philoſophy of air, and conſequently our knowledge of the principles, or conſtituent parts, of natural ſubſtances was extended, I did not by any means ſucceed to my ſatisfaction with reſpect to the immediate object of my reſearches. Others, however, were more ſucceſsful, and their ſucceſs has at length enabled me to reſume my experiments with more advantage; by which means I have been led both to confirm their concluſions, and, by diverſifying the experiments, to throw conſiderable light upon various other chemical proceſſes. The reſult of theſe obſervations I ſhall lay before the Society, with as much brevity and diſtinctneſs as I can.

In the experiments of which I ſhall now give an account, I was principally guided by a view to the opinions which have lately been advanced by Mr. CAVENDISH, Mr. WATT, and M. LAVOISIER. Mr. CAVENDISH was of opinion, that when *air* is decompoſed, *water* only is produced; and Mr. WATT concluded

concluded from ſome experiments, of which I gave an account to the Society, and alſo from ſome obſervations of his own, that water conſiſts of dephlogiſticated and inflammable air, in which Mr. CAVENDISH and M. LAVOISIER concur with him; but Mr. LAVOISIER is well known to maintain, that there is no ſuch thing as what has been called *phlogiſton*, affirming inflammable air to be nothing elſe but one of the elements or conſtituent parts of water. In the following experiments I alſo had a particular view to a concluſion which I had drawn from thoſe experiments, of which an account is given in my laſt communications to the Royal Society; *viz.* that inflammable air is pure phlogiſton in the form of air, at leaſt with the element of *heat*; and that fixed air conſiſts of dephlogiſticated and inflammable air; both which doctrines had been firſt advanced by Mr. KIRWAN, before I had made the experiments which I then thought clearly proved them.

Such were the hypotheſes to which I had a view when I began the following courſe of experiments, which I hope will be an admonition to myſelf, as well as to others, to adhere as rigorouſly as poſſible to *actual obſervations*, and to be extremely careful not to overlook any circumſtance that may poſſibly contribute to any particular reſult. I ſhall have occaſion to notice my own miſtakes with reſpect to *concluſions*, though all the *facts* were ſtrictly as I have repreſented them. But whilſt philoſophers are faithful narrators of what they obſerve, no perſon can juſtly complain of being miſled by them; for to *reaſon* from the facts with which they are ſupplied is no more the province of the perſon who diſcovers them, than of him to whom they are diſcovered.

One of the moſt ſimple of all phlogiſtic proceſſes is that in which metals are ignited in dephlogiſticated air. I therefore

began with this, with a view to ascertain whether any *water* is produced when the air is made to disappear in it. Accordingly, into a glass vessel containing 7 ounce measures of pretty pure dephlogisticated air, I introduced a quantity of iron turnings (which is iron in small thin pieces, exceedingly convenient for these and many other experiments) having previously made them, together with the vessel, the air, and the mercury by which it was confined, as dry as I possibly could. Also, to prevent the air from imbibing any moisture, I received it immediately in the vessel in which the experiment was made, from the process of procuring it from red precipitate; so that it had never been in contact with any water.

I then fired the iron, by means of a burning lens, and presently reduced the 7 ounce measures of air to .65; but I found no more water after this process than I imagined it had not been possible for me to exclude, as it bore no proportion to the air which had disappeared. Examining the residuum of the air, I found one-fifth of it to be fixed air, and when I tried the purity of that which remained by the test of nitrous air, it did not appear that any phlogisticated air had been produced in the process: for though it was more impure than I suppose the air with which I began the experiment must have been, it was not more so than the phlogisticated air of the 7 ounce measures, which had not been affected by the process, and which must have been contained in the residuum, would necessarily make it. In this case one measure of this residuum and two of nitrous air occupied the space of .32.

In another experiment of this kind, ten ounce measures of dephlogisticated air were reduced to .8, and by washing in lime water to .38. In another experiment, in which 7½ ounce measures of dephlogisticated air were reduced to half an ounce

measure, of which one-fifth was fixed air, the residuum was quite as pure as the air with which I began the experiment, the test with nitrous air, in the proportions above-mentioned, giving .4 in both cases. To what circumstance the difference might be owing I cannot tell.

In these experiments the fixed air must, I presume, have been formed by the union of the phlogiston from the iron and the dephlogisticated air in which it was ignited; but the quantity of it was very small in proportion to the air which had disappeared, and at that time I had no suspicion that the iron, which had been melted, and gathered into round balls, could have imbibed it; a melting heat having been sufficient, as I had imagined, to expel every thing that was capable of assuming the form of air from any substance whatever. I was therefore intirely at a loss about what must have become of the air.

Sensible, however, that such a quantity of air must have been imbibed by *something* to which it must have given a very perceivable addition of weight, and seeing nothing else that could have imbibed it, it occurred to me to weigh the calx into which the iron had been reduced; and I presently found, that the dephlogisticated air had actually been imbibed by the melted iron, in the same manner as inflammable air, in my former experiments, had been imbibed by the melted calces of metals, however impossible such an absorption might have appeared to me *a priori*. In the first instance, about twelve ounce measures of dephlogisticated air had disappeared, and the iron had gained six grains in weight. Repeating the experiment very frequently, I always found, that other quantities of iron, treated in the same manner, gained similar additions of weight, which was always very nearly that of the air which had disappeared.

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This calx of iron, I then concluded, was by no means what I had before taken it to be, *viz.* a *pure calx* or *ſlag*, but either the calx, or the iron itſelf, ſaturated with pure air. This calciform ſubſtance I found, by various experiments, to be the ſame thing with the *ſcales* that fly from iron when it is made red-hot, or the ſubſtance into which it runs in a very intenſe heat, in an open fire.

Concluding from the preceding experiment, that iron, ſufficiently heated, was capable of ſaturating itſelf with pure air, extracted from the maſs of the atmoſphere, I then proceeded to melt it with the heat of a burning lens in the open air; and I preſently found, that perfect iron was eaſily fuſed in this way, and continued in this fuſion a certain time, exhibiting the appearance of *boiling* or *throwing out* air, whereas it was on the contrary *imbibing* air; and when it was ſaturated the fuſion ceaſed, and the heat of my lens could not make any farther impreſſion upon it. When this was the caſe, I always found that it had gained weight in the proportion of 7½ to 24, which is very nearly *one-third* of its original weight. The ſame was the effect when I melted *ſteel* in the ſame circumſtances, and alſo every kind of iron on which the experiment could be tried. But I have ſome reaſon to think, that with a greater degree of heat than I could apply, the iron might have been kept in a ſtate of fuſion ſomewhat longer, and by that means have imbibed more air, even more than one-third of its original weight.

There was a peculiar circumſtance attending the melting of *caſt iron* with a burning lens, which made it impoſſible to aſcertain the addition that was made to its weight, and at the ſame time afforded an amazing ſpectacle; for the moment that any quantity of it was melted, and gathered into a round ball, it began

to diſperſe in a thouſand directions, exhibiting the appearance of a moſt beautiful fire-work, ſome of the particles flying to the diſtance of half a yard from the place of fuſion; and the whole was attended with a conſiderable hiſſing noiſe. Some of the largeſt pieces which had been diſperſed in this manner I was able to collect, and having ſubjected them to the heat of the lens, they exhibited the ſame appearance as the larger maſs from which they had been ſcattered.

When I melted this caſt iron in the bottom of a deep glaſs receiver, in order to collect all the particles that were diſperſed, they firmly adhered to the glaſs, melting it ſuperficially, though without making it crack, ſo that it was ſtill impoſſible to collect and weigh the particles. However, I generally found that, notwithſtanding the copious diſperſion, what remained after the experiment rather exceeded than fell ſhort of the original weight of the iron. Sometimes a piece of common iron, and eſpecially ſteel, would make a little hiſſing in the fuſion, and a particle or two would fly off; but this was never conſiderable *.

Having now procured what I thought to be a new calx of iron, or a calx ſaturated with pure air, I endeavoured to revive it by making it imbibe inflammable air, in the ſame manner that I had before made iron, and various other metals, by melting them in a veſſel containing inflammable air. In this I ſucceeded; but in the courſe of the experiment a new and very unexpected appearance occurred. I took a piece of iron which I had ſaturated with pure air, and putting it into a glaſs veſſel

* On being informed of the above-mentioned phænomena, Mr. WATT concluded, that the baſis of the dephlogiſticated air united to the phlogiſton of the iron, and formed *water*, which was attracted by, and remained ſo firmly united to the calx of iron, as to reſiſt the effects of heat to ſeparate them.

containing

containing inflammable air, confined by water, threw upon it the focus of the lens, and presently perceived the inflammable air to disappear, and without thinking of any thing escaping from the calx of iron (which had been subjected to a greater heat before) I imagined that I should have found the addition of the weight of air in the iron, and the result might be an iron different from the common sort. But I found, to my surprise, that the iron, which had exhibited no new appearance in this mode of treatment, had lost weight, instead of gaining any. The piece of iron on which I made this first experiment weighed 11½ grains, and 7½ ounce measures of inflammable air had disappeared while the iron had lost 2½ grains.

Considering the quantity of inflammable air that had disappeared, *viz.* 7½ ounce measures, and the dephlogisticated air which had been expelled from the iron, *viz.* 2½ grains, which is equal to about 4.1 ounce measures, I found that they were very nearly in the proper proportion to saturate each other, when decomposed by the electrical spark, *viz.* two measures of inflammable air to one of dephlogisticated air. I therefore had now no doubt but that the two kinds of air had united, and had formed either *fixed air* or *water*; but which it was I could not tell, having had water on the receiver in which the experiment was made, and having neglected to examine the state of the air that remained, except in a general way, by which I found, that it was still, to appearance, as inflammable as ever.

With a view to determine whether *fixed air*, or *water*, would be the produce of this mode of combining inflammable and dephlogisticated air, I repeated the experiment in a vessel in which the inflammable air was confined by mercury, and both the vessel and the mercury had been previously made as dry as possible. I had no sooner begun to heat the iron, or rather *slag*, in

in these circumstances, than I perceived the air to diminish, and at the same time the inside of the vessel to grow very cloudy, with particles of dew, that covered almost the whole of it. These particles by degrees gathered into drops, and ran down the sides of the vessel in all places, except where it was heated by the sun-beams; so that it then appeared to me very evident, that *water*, with or without fixed air, was the produce of the inflammable air, and the pure air let loose from the iron in this mode of operation; though afterwards I was taught by Mr. WATT to correct this hypothesis, and to account for this result in a different manner. When I had examined the remaining air, it was as inflammable as ever, without containing any mixture of fixed air at all.

When I collected the water which was produced in this experiment by means of a piece of filtering paper, carefully introduced to absorb it, I found it to be, as nearly as possible, of the same weight with that which had been lost by the iron: and also, in every experiment of this kind, in which I attended to this circumstance, I found that the quantity of inflammable air which had disappeared was about double to that of the dephlogisticated air set loose from the iron, supposing that weight to have been reduced into air. Thus at one time I made a piece of this slag imbibe 5½ ounce measures of inflammable air, while it lost as much as the weight of about 3 ounce measures of dephlogisticated air, and the water collected weighed 2 grains. Another time a piece of slag lost 1.5 grains, and the water produced was 1.7 grains; but perfect accuracy is not to be expected. I shall only mention one more experiment of this kind, in which 6½ ounce measures of inflammable air were reduced to .92 ounce measure, and the iron had lost 2 grains, equal in weight to 3.3 ounce measures of dephlogisticated air.

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In all the above-mentioned experiments, the inflammable air was that which is produced by the solution of iron in acids.

As before I had finished this course of experiments I had satisfied myself that inflammable air always contains a portion of water, and also, that when it has been some time confined by water, it imbibes more, so as to be increased in its specific gravity by that means, I repeated the experiment with inflammable air which had not been confined by water, but which was received in a vessel of dry mercury from the vessel in which it was generated; but I presently perceived that water was produced in this case also, and to appearance as copiously as in the former experiment. Indeed, the quantity of water produced, which so greatly exceeded the weight of all the inflammable air, is sufficient to prove that it must have had some other source than any constituent part of that air, or the whole of it, together with the water contained in it, without taking into consideration the corresponding loss of weight in the iron.

I must here observe, that the iron slag which I had treated in this manner, and which had thereby lost the weight which it had acquired by melting in dephlogisticated air, became *perfect iron* as at first, and was then capable of being melted by the burning lens again; so that the same piece of iron would serve for these experiments as long as the operator should chuse. It was evident, therefore, that if the iron had lost its phlogiston in the preceding fusion, it had acquired it again from the inflammable air which it had absorbed; and I do not see how the experiment can be accounted for in any other way, which necessarily implies the reality of phlogiston as a constituent principle in bodies. This, at least, is the most natural way of accounting for the appearances.

Having had this ſucceſs with the calx, or ſcales of *iron*, I tried the calx of *copper*, or thoſe ſcales which fly from it when it is made red-hot; and I found water produced in the inflammable air in the ſame manner as when I uſed the ſcales of iron in the ſame circumſtances. I alſo had the ſame reſult when I revived *precipitate per ſe* in inflammable air; but having at that time a very weak winter's ſun, I could not make the experiment with ſo much advantage as I could have wiſhed.

Iron, I found, acquired this additional weight by melting in an earthen retort, as well as in the open air by the ſun-beams, if it were poſſible for it to attract air, or whatever elſe it is that is the immediate cauſe of its additional weight. Three ounces of common iron filings, expoſed to a ſtrong heat in an earthen retort, gained 11 dwts, or 264 grains, and yet was very far from having been completely fuſed. Having a glaſs tube communicating with the retort, in order to collect any air that the iron filings might give out, I found that when they were very hot, the water aſcended within the tube; which ſhews that the iron was then in a ſtate of abſorbing, and not of giving out any air.

Seeing ſo much water produced in theſe experiments with inflammable air, I was particularly led to reflect on the relation which they bore to each other, and eſpecially to Mr. CAVENDISH's ideas on the ſubject. He had told me that, notwithſtanding the experiments of which I had given an account to the Royal Society, and from which I had concluded that inflammable air was pure phlogiſton, he was perſuaded that *water* was eſſential to the production of it, and even entered into it as a conſtituent principle. At that time I did not perceive the force of the arguments which he ſtated to me, eſpecially as, in the experiments with charcoal, I totally diſperſed any quantity of

of it with a burning lens *in vacuo*, and thereby filled my receiver with nothing but inflammable air. I had no ſuſpicion that the wet leather on which my receiver ſtood could have any influence in the caſe, while the piece of charcoal was ſubject to the intenſe heat of the lens, and placed ſeveral inches above the leather. I had alſo procured inflammable air from charcoal in a glazed earthen retort two whole days ſucceſſively, in which it had given inflammable air without intermiſſion. Alſo iron filings in a gun-barrel, and a gun-barrel itſelf, had always given inflammable air whenever I tried the experiment.

Theſe circumſtances, however, deceived me, and perhaps would have deceived any other perſon; for I did not know, and could not have believed, the powerful attraction that *charcoal*, or *iron*, appear to have for *water* when they are intenſely hot. They will find, and attract it, in the midſt of the hotteſt fire, and through any pores that may be left open in a retort; and iron filings are ſeldom ſo dry as not to have moiſture enough adhering to them, capable of enabling them to give a conſiderable quantity of inflammable air. But my attention being now fully awake to the ſubject, I preſently found that the circumſtances above mentioned had actually miſled me; I mean with reſpect to the *concluſion* which I drew from the experiments, and not with reſpect to the experiments themſelves, every one of which, I doubt not, will be found to anſwer, whenever they are tried by perſons of ſufficient ſkill and properly attentive to all the circumſtances.

Being thus appriſed of the influence of unperceived moiſture in the production of inflammable air, and willing to aſcertain it to my perfect ſatisfaction, I began with filling a gun-barrel with iron filings in their common ſtate, without taking any particular precaution to dry them, and I found that they gave air as

 they

they had been used to do, and continued to do so many hours; I even got ten ounce measures of inflammable air from two ounces of iron filings in a coated glass retort. At length, however, the production of inflammable air from the gun-barrel ceased; but on putting water into it, the air was produced again, and a few repetitions of the experiment fully satisfied me that I had been too precipitate in concluding that inflammable air is pure phlogiston.

I then repeated the experiment with the charcoal, making the receiver, the stand on which I placed the charcoal, and the charcoal itself, as dry and as hot as possible, and using cement instead of a wet leather to exclude the air. In these circumstances I was not able, with the advantage of a good sun, and an excellent burning lens, to decompose quite so much as two grains of the piece of charcoal, which gave me ten ounce measures of inflammable air; and this, I imagine, was effected by means of so much moisture as was deposited from the air in its state of rarefaction, and before it could be drawn from the receiver. To the production of this kind of inflammable air I was therefore now convinced, that water is as necessary as to that from iron.

It was in this state of my experiments that I received an authentic account of those of M. LAVOISIER, on transmitting water through an hot iron tube and also through a hot copper tube containing charcoal, and thereby procuring large quantities of inflammable air, M. LAVOISIER himself having been so obliging as to send me a copy of his Memoir on that subject. I had heard an account of the experiments some months before; but it was so imperfect a one, that I own I paid little attention to them. At this time, however, I was prepared to be sufficiently sensible of their value.

In

In my last communications to the Royal Society, it will be seen that I had transmitted the vapour of several fluid substances through red-hot *earthen tubes*, and thereby procured different kinds of air. M. LAVOISIER adopted the same process, but used an *iron tube*; and by means of that circumstance made a very valuable discovery which had escaped me. I had indeed on one occasion made use of an iron tube, and transmitted steam through it; but not having at that time any view to the production of *air*, I did not collect it at all, contenting myself with observing that *water*, after being made red-hot, was still water, there being no change in its sensible properties. Being now farther instructed by the experiment of M. LAVOISIER, I was determined to repeat the process with all the attention I could give to it; but I should not have done this with so much advantage, if I had not had the assistance of Mr. WATT, who always thought that M. LAVOISIER's experiments by no means favoured the conclusion that he drew from them. As to myself, I was a long time of opinion that his conclusion was just, and that the inflammable air was really furnished by the water being decomposed in the process. But though I continued to be of this opinion for some time, the frequent repetition of the experiments, with the light which Mr. WATT's observations threw upon them, satisfied me at length that the inflammable air came principally from the charcoal, or the iron.

I shall first relate the result of the experiment that was made with *charcoal*, and then those with iron and other substances, in contact with which (when they were in a state of fusion, or at least red-hot) I made steam, or the vapour of other liquid substances, to pass. I shall only observe that, previous to this, I began to make the experiments with coated glass tubes, which

I found to anſwer very well during the proceſs, though they never failed to break in cooling. At length I procured a tube of *copper*, on which, as M. LAVOISIER diſcovered, ſteam had no effect; and at laſt I made uſe of earthen tubes, with which Mr. WEDGEWOOD, that moſt generous promoter of ſcience, liberally ſupplied me for the purpoſe; and theſe glazed on the outſide only I find far preferable to copper. They are, indeed, every thing that I could wiſh for in experiments of this kind; the reaſon of which will appear in my account of another courſe of experiments, which I hope to lay before the Society in due time.

The diſpoſition of the apparatus, with which theſe experiments were made, was as follows. The water was made to boil in a glaſs retort, which communicated with the copper or earthen tube which contained the charcoal or iron, &c. and which, being placed in an horizontal poſition, was ſurrounded with hot coals. The end of this tube oppoſite to the retort communicated with the pipe of a common *worm tub*, ſuch as is generally uſed in diſtillations, by means of which all the ſuperfluous ſteam was condenſed, and collected in a proper receptacle, while the air which had been produced, and had come along with it through the worm tub, was tranſmitted into a trough of water, where proper veſſels were placed to receive it, and aſcertain the quantity of it; after which I could examine the quality of it at leiſure.

In the experiment with *charcoal*, I found unexpected difficulties, and conſiderable variations in the reſult; the proportion between the *charcoal* and *water* expended, and alſo between each ofth em and the *air* produced, not being ſo nearly the ſame as I imagined they would have been. Alſo the quantity of fixed air that was mixed with the inflammable air varied very

much. This laſt circumſtance, however, ſome of my experiments may ſerve to explain. Whenever I had no more water than was ſufficient for the production of the air, there was never any ſenſible quantity of uncombined fixed air mixed with the inflammable air from charcoal. This was particularly the caſe when I produced the air by means of a burning lens in an exhauſted receiver, and alſo in an earthen retort with the application of an intenſe heat. I therefore preſume, that when the ſteam tranſmitted through the hot tube containing the charcoal was very copious, the fixed air in the produce was greater than it would otherwiſe have been. The extremes that I have obſerved in the proportion of the fixed to the inflammable air have been from one-twelfth to one-fifth of the whole. As I generally produced this air, the latter was the uſual proportion; and this was excluſive of the fixed air that was intimately combined with the inflammable air, and which could not be ſeparated from it except by decompoſition with dephlogiſticated air; and this combined fixed air I ſometimes found to be one-third of the whole maſs, though at other times not quite ſo much.

To aſcertain this, I mixed one meaſure of this inflammable air from charcoal (after the uncombined fixed air had been ſeparated from it by lime-water) with one meaſure of dephlogiſticated air, and then fired them by the electric ſpark. After this I always found that the air which remained made lime-water very turbid, and the proportion in which it was now diminiſhed, by waſhing in lime-water, ſhewed the quantity of fixed air that had been combined with the inflammable. That the fixed air is not *generated* in this proceſs, is evident from there being no fixed air found after the exploſion of dephlogiſticated air and inflammable air from iron.

Notwith-

Notwithstanding the above-mentioned variations, the loss of weight in the charcoal was always much exceeded by the weight of the water expended, which was generally more than double of the charcoal; and this water was intimately combined with the air; for when I received a portion of it in mercury, no water was ever deposited from it.

The experiment which, upon the whole, gave me the most satisfaction, and the particulars of which I shall therefore recite, was the following. Expending 94 grains of perfect charcoal (by which I mean charcoal made with a very strong heat, so as to expel all fixed air from it) and 240 grains of water, I procured 840 ounce measures of air, one-fifth of which was fixed air, and of the inflammable part nearly one-third more appeared to be fixed air by decomposition.

Receiving this kind of air in a variety of experiments, but not in the preceding ones in particular (for then I could not have ascertained the quantity of it) consisting of fixed and inflammable air together, I found some variations in its specific gravity, owing, I imagine, to the different proportions of fixed air contained in it; but upon the whole, I think, that the proportion of 14 grains to 40 ounce measures is pretty near the truth, when the proportion of fixed air is about one-fifth of the whole. With respect to the weight of the inflammable air after the fixed air was separated from it, I found no great difference, and think it may be estimated at 8 grains to 30 ounce measures.

Upon these principles, the whole weight of the 840 ounce measures of air will be - 294 grains

that of the charcoal will be	94	
that of the water -	240	
	334	which, considering the na-

7 ture

ture of the experiment, will perhaps be thought to be tolerably near that of the air.

If the air be analyzed, the 840 ounce measures will be found to contain - 168 of uncombined fixed air = 151 grains.
and 672 impure inflammable = 179.

so that the whole 840 will weigh - - 330

Lastly, if the 672 ounce measures of impure inflammable air be decomposed, it will be found to contain
164 ounce measures of fixed air = 147.6 grs.
and 508 inflammable - - = 30.7

so that the whole 672 will weigh - - 178.3
which is very near to 179, the weight of the whole together.

It may, however, be safely concluded from this experiment, and indeed from every other that I made with charcoal, that there was no more pure inflammable air produced than the charcoal itself may be very well supposed to have supplied.

There is, therefore, no reason for deserting the old established hypothesis of *phlogiston* on account of these experiments, since the fact is by no means inconsistent with it. The pure inflammable air with the water necessarily contained in it would weigh no more than about 30 grains, while the loss of weight in the charcoal was 94 grains. But to this must be added the phlogiston contained in 392 ounce measures of fixed air, which, according to Mr. KIRWAN's proportion, will be nearly 65 grains, and this and the 30 grains will be 95 grains.

The basis to this fixed air, as well as to the inflammable, must have been furnished by the *water*; and from this it may be concluded, that the water must have been so far altered as to be changed into fixed air, which will be thought not to be any great paradox, if it be considered that, according to the latest

lateſt diſcoveries, fixed air and water appear to conſiſt of the ſame ingredients, namely dephlogiſticated and inflammable air. However, in this change of the water we cannot be abſolutely ſure that the ſame proportion of the ingredients is contained, and therefore it cannot be abſolutely determined whether the inflammable air which it contains enters wholly into the fixed air, or not. Farther experiments, or a careful compariſon of theſe experiments with thoſe made by Mr. KIRWAN and others, may perhaps throw ſome light upon this ſubject. Whether the combined fixed air comes wholly from the charcoal, or whether the charcoal only ſupplies the phlogiſton, and the water its baſis, that is, the dephlogiſticated air, deſerves to be inveſtigated.

Before I conclude my account of the experiments with charcoal, I would obſerve, that there is another in which I place ſome dependance, in which, with the loſs of 178 grains of charcoal, and 528 grains of water, I procured 1410 ounce meaſures of air, of which the laſt portion (for I did not examine the reſt) contained one-ſixth part of uncombined fixed air. This was made in an earthen tube glazed on the outſide.

The experiments with *iron* were more ſatisfactory than thoſe with charcoal, being ſubject to leſs variation; and it is ſtill more evident from them, that the inflammable air does not come from the *water*, but only from the *iron*, as the quantity of water expended, added to the weight of the air produced, was as nearly as could be expected in experiments of this kind, found in the addition of weight gained by the iron. And though the inflammable air procured in this proceſs is between one-third and one-half more than can be procured from iron by a ſolution in acids, the reaſon may be, that much phlogiſton is retained in the ſolutions, and therefore much more may be expelled from iron, when pure water, without any acid, takes the place of it. I would further obſerve, that the produce of air,

air, and alſo the addition of weight gained by the iron, are much more eaſily aſcertained in theſe experiments than the quantity of water expended in them, on account of the great length of the veſſels uſed in the proceſs, and the different quantities that may perhaps be retained in the worm of the tub, though I did not fail to uſe all the precautions that I could think of to guard againſt any variation on theſe accounts.

Of the many experiments which I made with *iron*, I ſhall content myſelf with reciting the following reſults. With the addition of 267 grains to a quantity of iron, and the loſs of 336 grains of water, I procured 840 ounce meaſures of inflammable air; and with the addition of 140 grains to another quantity of iron, and the conſumption of 253 grains of water, I got 420 ounce meaſures of air *.

The inflammable air produced in this manner is of the lighteſt kind, and free from that very *offenſive ſmell* which is generally occaſioned by the rapid ſolution of metals in oil of vitriol, and it is extricated in as little time in this way as it is poſſible to do it by any mode of ſolution. On this account it occurred to me, that it muſt be by much the cheapeſt method that has yet been uſed of filling *balloons* with the lighteſt inflammable air. For this purpoſe it will be proper to make uſe of caſt-iron cylinders of a conſiderable length, and about three or four inches, or perhaps more, in diameter. Though the iron tube itſelf will contribute to the production of air, and therefore may become unfit for the purpoſe in time; yet, for any

* If the perfect accuracy of the former of theſe experiments may be depended on (and it may always be preſumed, that thoſe in which *little water* is expended are preferable to thoſe in which *more* is conſumed) the *water* that neceſſarily enters into this kind of inflammable air is about equal in weight to the *phlogiſton* that is in it. But I propoſe to give more particular attention to this ſubject.

thing that I know to the contrary, the ſame tube may ſerve for a very great number of proceſſes, and perhaps the change made in the inſide ſurface may protect it from any farther action of the water, if the tube be of ſufficient thickneſs; but this can only be determined by experiment.

Some eſtimate of what may be expected from this method of procuring inflammable air may be formed from the following obſervations. About twelve inches in length of a copper tube, three-fourths of an inch in diameter, filled with *iron turnings* (which are more convenient for this purpoſe than *iron filings*, as they do not lie ſo cloſe, but admit the ſteam to paſs through their interſtices) when it was heated, and a ſufficient quantity of ſteam paſſed through it, yielded thirty ounce meaſures of air in fifty ſeconds; and eighteen inches of another copper tube, an inch and a quarter in diameter, filled and treated in the ſame manner, gave two hundred ounce meaſures in one minute and twenty-five ſeconds; ſo that this larger tube gave air in proportion to its ſolid contents compared with the ſmaller; but to what extent this might be depended upon I cannot tell. However, as the heat penetrates ſo readily to ſome diſtance, the rate of giving air will always be in a greater proportion than that of the ſimple diameter of the tube.

The following experiment was made with a view to aſcertain the quantity of inflammable air that may be procured in this way from any given quantity of iron. Two ounces of iron, or 960 grains, when diſſolved in acids, will yield about 800 ounce meaſures of air; but treated in this manner it yielded 1054 ounce meaſures, and then the iron had gained 329 grains in weight, which is little ſhort of one-third of the weight of the iron.

Conſidering

Considering how little this inflammable air weighs, *viz.* the whole 1054 ounce measures not more than 63 grains, and the difficulty of ascertaining the loss of water to so small a quantity as this, it is not possible to determine, from a process of this kind, how much water enters into the composition of the inflammable air of metals. It would be more easy to determine this circumstance with respect to the inflammable air of charcoal, especially by means of the experiment made with a burning lens *in vacuo*. In this method two grains of charcoal gave at a medium thirteen ounce measures of inflammable air, which, in the proportion of 30 ounce measures to 8 grains, will weigh 3.3 grains; so that water in the composition of this kind of inflammable air is in the proportion of 1.3 to 2, though there will be some difficulty with respect to the fixed air intimately combined with this kind of inflammable air.

Since iron gains the same addition of weight by melting in *dephlogisticated air*, and also by the addition of *water* when red-hot, and becomes, as I have already observed, in all respects the same substance, it is evident, that this *air* or *water*, as existing in the iron, is the very same thing; and this can hardly be explained but upon the supposition that water consists of two kinds of air; *viz.* inflammable and dephlogisticated. I shall endeavour to explain these processes in the following manner.

When iron is melted in dephlogisticated air, we may suppose that, though part of its phlogiston escapes, to enter into the composition of the small quantity of fixed air which is then procured, yet enough remains to form *water* with the addition of dephlogisticated air which it has imbibed, so that this *calx* of iron consists of the intimate union of the pure *earth of iron* and of *water*; and therefore when the same calx, thus satu-

rated with water, is expoſed to heat in inflammable air, this air enters into it, deſtroys the attraction between the water and the earth, and revives the iron, while the water is expelled in its proper form.

Conſequently, in the proceſs with *ſteam*, nothing is neceſſary to be ſuppoſed but the entrance of the water, and the expulſion of the phlogiſton belonging to the iron, no more phlogiſton remaining in it than what the water brought along with it, and which is retained as a conſtituent part of the water, or of the new compound.

Having procured water from the ſcales of iron (which I muſt again obſerve is, in all reſpects, the ſame ſubſtance with iron melted in dephlogiſticated air, or ſaturated with ſteam by means of heat) and having thereby converted it into perfect iron again, I did not entertain a doubt but that I ſhould be able to produce the ſame effect by heating it with charcoal in a retort; and I had likewiſe no doubt but I ſhould be able to extract the additional weight which the iron had gained (*viz.* one-third of the whole) in *water*. In the former of theſe conjectures I was right; but with reſpect to the latter, I was totally miſtaken.

Having made the ſcales of iron, and alſo the powder of charcoal very hot, previous to the experiment, ſo that I was ſatisfied that no air could be extracted from either of them ſeparately by any degree of heat, and having mixed them together while they were hot, I put them into an earthen retort, glazed within and without, which was quite impervious to air. This I placed in a furnace, in which I could give it a very ſtrong heat; and connected with it proper veſſels to condenſe and collect the water which I expected to receive in the courſe of the proceſs. But, to my great ſurpriſe, not one particle

of

of *moisture* came over, but a prodigious quantity of *air*, and the rapidity of its production astonished me; so that I had no doubt but that the weight of the air would have been equal to the loss of weight both in the scales and in the charcoal; and when I examined the air, which I repeatedly did, I found it to contain one-tenth of fixed air, and the inflammable air, which remained when the fixed air was separated from it, was of a very remarkable kind, being quite as heavy as common air. The reason of this was sufficiently apparent when it was decomposed by means of dephlogisticated air; for the greatest part of it was fixed air.

The theory of this process I imagine to be, that the phlogiston from the charcoal reviving the iron, the water with which it had been saturated, being now set loose, affected the hot charcoal as it would have done if it had been applied to it in the form of *steam* as in the preceding experiments; and therefore the air produced in these two different modes have a near resemblance to each other, each containing fixed air, both combined and uncombined, though in different proportions; and in both the cases I found these proportions subject to variations. In one process with charcoal and scales of iron, the first produce contained one-fifth of uncombined fixed air, the middle part one-tenth, and the last none at all. But in all these cases the proportion of combined fixed air varied very little.

Why *air* and not *water* should be produced in this case, as well as in the preceding, when the iron is equally revived in both, I do not pretend perfectly to understand. There is, indeed, an obvious difference in the circumstances of the two experiments; as in that with charcoal the phlogiston is found in a combined state; whereas in that of inflammable air, it is

loose,

looſe, or only united to water; and perhaps future experiments may diſcover the operation of this circumſtance.

There is ſome analogy between the experiment of the calx of iron imbibing inflammable air, and the iron itſelf imbibing dephlogiſticated air. In the former caſe *water* is produced, and in the latter *fixed air*. However, this caſe of iron imbibing dephlogiſticated air more nearly reſembles the caſe of the *blood in the lungs* imbibing the ſame kind of air, and in both the caſes as dephlogiſticated air is imbibed, fixed air is formed. This, therefore, ſeems to be a confirmation of the concluſion which I drew from my former experiments on blood, *viz.* that it parts with phlogiſton in reſpiration. Only I would now add, that at the ſame time that it parts with phlogiſton it takes in dephlogiſticated air, which makes the caſe perfectly ſimilar to that of the experiment with *iron*, which likewiſe parts with phlogiſton to form fixed air, at the ſame time that it imbibes dephlogiſticated air in contact with which it is fuſed.

I propoſe to reſerve for a future communication the continuation of theſe experiments, containing an account of the application of the ſame proceſs to other ſubſtances; but it may not be amiſs juſt to mention a few of the *general reſults*, and thoſe which have the neareſt connexion with the experiments recited above.

After having tranſmitted ſteam in contact with *charcoal* and *iron* in a copper tube, I propoſed to do the ſame with other ſubſtances containing phlogiſton, and I began with *bones*, which were burnt black, and had been ſubjected to an intenſe heat, covered with ſand, in an earthen retort. From three ounces of bone thus prepared, and treated as I had done the charcoal, I got 840 ounce meaſures of air, with the loſs of 288 grains of water. The bones were by this means made perfectly white,

3 and

and had loſt 110 grains of their weight. As the air ceaſed to come a conſiderable time before all the water had been tranſmitted through the tube containing them, I concluded that the air was formed from the phlogiſton contained in the bones, and ſo much water as was neceſſary to give it the form of air.

This air differs conſiderably from any other kind of inflammable air, being in ſeveral reſpects a medium between that from charcoal and that from iron. It contains about one-fourth of its bulk of uncombined fixed air, but not quite one-tenth intimately combined with the remainder. The water that came over was blue, and pretty ſtrongly alkaline, which muſt have been occaſioned by the volatile alkali not having been intirely expelled from the bones in the former proceſs, and its having in part diſſolved the copper of the tube in which the experiment was made.

I ſubjected to the ſame proceſs a variety of ſubſtances that are ſaid not to contain phlogiſton, but I was never able to procure inflammable air by means of them; which ſtrengthens the hypotheſis of the principal element in the conſtitution of this air having been derived from the ſubſtance ſuppoſed to contain phlogiſton, and therefore that phlogiſton is a real ſubſtance, capable of aſſuming the form of air by means of water and heat.

The experiments above-mentioned relating to iron were made with that kind which is *malleable*; but I had the ſame reſult when I made uſe of ſmall nails of *caſt iron*, except that theſe were firmly faſtened together after the experiment, the ſurfaces of them being cryſtallized, and the cryſtals mixing with each other, ſo that it was with great difficulty that they could be got out of the tube after the experiment, and in general the ſolid parts of the nails were broken before they were ſeparated

from

from each other. Indeed the pieces of malleable iron adhered together after the experiment, but by no means so firmly.

Cast iron annealed (by being kept red-hot in charcoal) is remarkably different from the cast iron which has not undergone that operation, especially in its being, to an extraordinary degree, more soluble in acids. With the turnings of annealed cast iron I made the following experiment. From 960 grains of this iron, and with the loss of 480 grains of water, I got 870 ounce measures of inflammable air, and transmitting steam through them a second time, I got 150 ounce measures more. The iron had then gained 246 grains in weight, and the pieces adhered firmly together; but being thin they were easily broken and got out of the tube, whereas it had required a long time, and a sharp steel instrument, to clear the tube of the cast-iron nails.

Having got water from the scales of iron and of copper saturated with dephlogisticated air, by heating them in inflammable air, it occurred to me to make the same experiment with *precipitate per se*, and I found, that the moment that the focus of the lens fell upon this substance the mercury began to revive, the inflammable air rapidly disappeared, and *water* was formed on the sides of the vessel in which the experiment was made. For want of a better sun, I could not ascertain every circumstance relating to this process; but what I did seemed to afford a sufficient proof that mercury contains phlogiston, and that it is not revived by the mere expulsion of dephlogisticated air, as M. LAVOISIER supposes; especially as *no fixed air* was found in what remained of the inflammable air. In one of these experiments 4.5 ounce measures of inflammable air had disappeared, and 1.6 ounce measure remained; and this appeared to contain some dephlogisticated air mixed with the inflammable.

Willing to try the effect of heating iron, and other substances, in all the different kinds of air, without any particular expectation, I found that iron melted more readily in *vitriolic acid* air than in dephlogisticated air, the air was diminished as rapidly, and the inside of the vessel was covered with a *black sooty matter*, which, when exposed to heat, readily sublimed in the form of a white vapour, and left the glass quite clean. The iron, after the experiment, was quite brittle, and must, I presume, be the same thing with iron that is *sulphurated*; but I did not particularly examine it. Of seven ounce measures of vitriolic acid air, in one of these experiments, not more than three-tenths of an ounce measure remained; of this two-thirds was fixed air, and the residuum of this was inflammable. I had put three of such residuums together, in order to make the experiment with the greater certainty.

Having transmitted *steam*, or the vapour of water, through a copper tube, I was willing to try the effects of *spirit of wine* through the same tube when red-hot, having before procured inflammable air by sending the same vapour through a red-hot tobacco-pipe. In this case, the vapour of the spirit of wine had no sooner entered the hot copper tube, than I was perfectly astonished at the rapid production of air. It resembled the blowing of a pair of bellows. But I had not used four ounces of the spirit of wine before I very unexpectedly found, that the tube was perforated in several places; and presently afterwards it was so far destroyed, that in attempting to remove it from the fire it actually fell in pieces. The inside was full of a black sooty matter resembling lamp-black.

Upon this I had recourse to *earthen tubes*, and found, that by melting copper and other metals in them, and transmitting the

vapour of spirit of wine in contact with them, different substances were formed according to the metals employed. The new substances thereby formed may be said to be the several metals super-saturated with phlogiston, and may perhaps not be improperly called the *charcoal of metals*.

That this appellation is not very improper, may appear from these substances yielding inflammable air very copiously when they are made red hot, and the steam of *water* is transmitted in contact with them, just as when the charcoal of wood is treated in the same manner. The detail of these experiments I reserve for another communication, as also those of the conversion of *spirit of wine*, *æther*, and *oil*, into different kinds of inflammable air, by transmitting them, in vapour, through hot earthen tubes. In the mean time, I shall think myself happy if the communication of the preceding experiments shall give any satisfaction to the Members of the Society.

POSTSCRIPT.

BEFORE I close this paper, I wish to make a few *general inferences* from the principal of the experiments above-mentioned, especially relating to the proportional quantity of phlogiston contained in *iron* and *water*.

When any quantity of iron is melted in dephlogisticated air, it imbibes the greatest part of it, and gains an addition of weight very nearly equal to that of the air imbibed. Thus the absorption of twelve ounce measures of dephlogisticated air

gave an addition of ſix grains to the piece of iron which had been melted in it. But there was always a quantity of *fixed air* produced in this proceſs; and on the ſuppoſition that this air conſiſts of the union of dephlogiſticated and inflammable air, it proves that the dephlogiſticated air which enters the iron expels more phlogiſton than is neceſſary to conſtitute an equal weight of water, ſo that *water* does not contain ſo much phlogiſton as *iron*; but the difference is not very conſiderable.

Admitting Mr. KIRWAN's concluſion, *viz.* that 100 cubic inches of fixed air contain 8,357 grains of phlogiſton, the .13 ounce meaſure of fixed air, which (in an experiment recited in theſe papers) was found in the reſiduum of ſeven ounce meaſures of dephlogiſticated air abſorbed by iron, would not have contained more than .01 grain of phlogiſton, or about .16 ounce meaſure of inflammable air. Then, as the abſorption of 12 ounce meaſures of dephlogiſticated air occaſioned an addition of 6 grains to the weight of the iron which had abſorbed it, the abſorption of ſeven ounce meaſures muſt have occaſioned the addition of 3.5 grains to the iron which had imbibed it. But the ſame addition of weight to iron given by *ſteam* (which carries its own inflammable air along with it) would have expelled near 12 ounce meaſures of inflammable air: conſequently, about ten ounce meaſures of inflammable air (or the phlogiſton requiſite to form it) muſt, in the former experiment, have been retained in the iron, in order to compoſe the *water* which was now made by the union of the dephlogiſticated air imbibed by the iron and the phlogiſton contained in it: and therefore the proportion between the quantity of phlogiſton in *iron* to that which is contained in an equal weight of water, may be about 12 to 10, or more accurately to 10.4.

Had no fixed air at all been found in the reſiduum above-mentioned, it might have been concluded, that water had con-

tained the very ſame proportion of phlogiſton with iron. Since when iron that has been ſaturated with dephlogiſticated air is heated in inflammable air (in which proceſs an equal weight of water is produced, and the loſs of weight in the iron is equal to that of ſuch a quantity of dephlogiſticated air as would have been one-half of the bulk of the inflammable air which diſappears in that proceſs) it might have been concluded, that one-fifth of any quantity in water had been inflammable air.

For, neglecting the difference between the weight of dephlogiſticated and common air, which is not conſiderable, and eſtimating the latter $\frac{1}{800}$th part of water, and inflammable air at one-tenth of the weight of common air, an ounce meaſure of dephlogiſticated air will weigh .6 grain, and two ounce meaſures of inflammable air will weigh .12 grain, which numbers are to each other as 5 to 1 *.

Though, in conſequence of the ſmall quantity of fixed air which is found in the proceſs of melting iron in dephlogiſticated air, this concluſion is not accurate, it is pretty nearly ſo; and it is remarkable that, upon this ſuppoſition, about as much inflammable air is expelled from iron when water is com-

* It appears from the proſecution of theſe experiments, that the water which is found on heating the ſcales of iron in inflammable air, is not formed by the dephlogiſticated air expelled from them uniting with the inflammable air in the veſſel, but was the water previouſly contained in the ſcales, which is made to quit its place by the introduction of the phlogiſton from the inflammable air; yet that water carries out with it not much leſs phlogiſton than was taken in by the iron, and a little more muſt be allowed for that water which was neceſſary to make inflammable air, and which could not enter the iron when it was revived; ſo that, on the whole, the phlogiſton in the water that is found after the proceſs muſt be very nearly the ſame quantity that is imbibed by the iron, and the water is nearly the ſame that would have been produced, on the ſuppoſition of its being made from dephlogiſticated air expelled from the ſcales uniting with the inflammable air in the veſſel.

bined with it, as the water itself brings along with it, as an essential ingredient in its composition. For in one experiment 296 grains added to the weight of a quantity of iron by steam, made it to yield about 1000 ounce measures of inflammable air. This would weigh 60 grains, and one-fifth of the 296 grains of water will be 59.2 grains. Again, 267 grains added to iron by steam made it to yield 840 ounce measures of inflammable air, which would weigh 50.4 grains, and one-fifth of the 267 would be 53.4 grains.

When the experiments on the melting of iron in dephlogisticated air shall be repeated on a larger scale, which it will not be difficult to do by the help of a larger burning lens than I am at present possessed of, it will be easy to reduce these calculations to a greater certainty. All that I can do at present is to approximate to such general conclusions as I have mentioned; but they are of so much consequence in philosophy, that it will certainly be well worth while to ascertain them with as much accuracy as possible. Nice calculations would be ill bestowed on the imperfect *data* which I am as yet able to furnish. Attention must also be given to the quantity of water contained in inflammable air from iron; which not being yet ascertained is not considered in these inferences. I wish only to hint in this Postscript, that some important conclusions seems to be nearly within our reach.

END OF PART I. OF VOL. LXXV.

E R R A T A.

Page. Line.

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289. 17. *dele the whole line except* $\frac{b e^2}{a^2} \times p$. Whence,

VOL. LXXIV.

32. 26. *for* 2 ·0000000001, *read* 2 — ·0000000001.
34. 8. *for* to tangent, *read* to cotangent.

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78. 2. *for* unequal. *read* unequal;
94. 25. *for* 20′ 39‴ *read* 20″ 39‴
120. 10. *for* FL. 74, *read* FL. 74.
170. 10. *for* 16_{12} *read* $16\frac{1}{12}$
200. 15. *for* III. *read* 3.
270. 14. *for* 36°6 *read* 36°,6

N.Y. Dec. 8-1785

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OF THE

ROYAL SOCIETY

OF

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VOL. LXXV. For the Year 1785.

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PHILOSOPHICAL

TRANSACTIONS.

XVI. *Of the Rotatory Motion of a Body of any Form whatever, revolving, without Restraint, about any Axis passing through its center of Gravity.* By Mr. John Landen, F.R.S.

Read March 17, 1785.

A SPHERICAL body, uniformly dense, it is obvious, will, if made to revolve freely about any axis passing through its center, continue to revolve about the same axis; and, by what I have shewn in the *Philosophical Transactions* for the year 1777, it appears, that a *cylinder* of uniform density, whose length is to its radius as $\sqrt{3}$ to 1, will do the same. It likewise appears, by my *Mathematical Memoirs*,

that a *cone*, a *conoid*, a *prism*, or a *pyramid*, &c. of certain dimensions, will have the like property of continuing, without any restraint, to revolve about any axis passing through its center of gravity.

When the axis, about which a body may be made to revolve, is not a permanent one, the centrifugal force of its particles will disturb its rotatory motion, so as to cause it to change its axis of rotation (and consequently its poles) every instant, and endeavour to revolve about a new one: and I cannot think it will be deemed an uninteresting proposition to determine in what track, and at what rate, the poles of such momentary axis will be varied in any body whatever; as, without the knowledge to be obtained from the solution of such problem, we cannot be certain whether the earth, or any other planet, may not, from the *inertia* of its own particles, so change its momentary axis, that the poles thereof shall approach nearer and nearer to the present equator, or whether the evagation of the momentary poles, arising from that cause, will not be limited by some known lesser circle. Which certainly is an important consideration in astronomy; especially now that branch of science is carried to great perfection, and the acute astronomer endeavours to determine the motions of the heavenly bodies with the greatest exactness possible.

I do not know that the problem has before been solved by any mathematician in these kingdoms; but I am aware that it has been considered by some gentlemen, very eminent for their mathematical knowledge, in other nations. The solutions of it, given by the celebrated M. LEONHARD EULER and M. D'ALEMBERT, I have seen: and we learn from what the last mentioned gentleman has said, in his *Opuscules Mathematiques*, that

that a ſolution of it, inveſtigated by M. JOHN ALBERT EULER (after a method ſimilar to his father's) obtained the prize given by the Academy of Sciences in the year 1761. The concluſions deduced by thoſe very learned gentlemen differing greatly from mine made me ſuſpect, for ſome time, that I had ſomewhere erred in my inveſtigation, and induced me to reviſe my proceſs again and again with the greateſt circumſpection. At length my ſcrutiny has ſo removed my doubts, that, being well aſſured of the truth of my theory, I now beg leave to preſent it to the Royal Society; preſuming that it will be found not unworthy of the notice of ſuch readers, as are curious in contemplating the various motions which bodies may naturally have, in conſequence of inſtantaneous or continued impulſe.

In the *Philoſophical Tranſactions* referred to above, I gave a ſpecimen of this theory, as far as it relates to the motion of *a ſpheroid* and *a cylinder*. The improvements I have ſince made in it, enable me now to extend it to the motion of *any body whatever*, how irregular ſoever its form may be.

What I here infer therefrom will be found to differ very materially from the deductions in the ſolutions given by the gentlemen above-mentioned. They repreſent the angular velocity, and the momentum of rotation of the revolving body, as always *variable*, when the axis about which it has a tendency to revolve is a momentary one, except in a particular caſe. By my inveſtigation it appears, that the angular velocity and the momentum of rotation will always be *invariable* in any revolving body, though the axis about which it endeavours to revolve be continually varied; and the tracks of the varying poles upon the ſurface of the body are thereby determined with great facility.

It is not only obſervable, that the tracks which the varying poles take, in the ſurface of any revolving body, are ſuch that its momentum of rotation may continue the ſame whilſt its angular velocity continues the ſame; but it may be obſerved, that, in any given body, there is only one ſuch track which a momentary pole can purſue from any given point.

If the angular velocity and the momentum of rotation of a revolving body were to vary according to the computations adverted to above, it would follow, that a body might acquire an increaſe of force from its own motion, without being any way affected by any other body whatever, as the ſame percuſſive force, applied at the ſame diſtance from the momentary axis, would not always deſtroy the rotatory motion of the body, which ſurely cannot poſſibly be true. From the principles or laws of motion, which I conſider as undoubtedly true (and which indeed are no other than the common principles of mechanics), I conclude that a revolving body, not affected by any external impulſe, can no more acquire an increaſe in its momentum of rotation, than any other body, moving freely, can acquire an increaſe in its momentum, or quantity of motion, in any given direction, without being impelled by gravity or ſome other force. And the truth of this concluſion (which is hereinafter proved by other reaſoning) may be eaſily inferred from the property of the lever; ſeeing that the joint centrifugal force of the particles of the revolving body (which is the *only* diſturbing force) has no tendency to accelerate or retard their motion about the momentary axis, but only to alter the poſition of ſuch axis, the direction in which that force acts being always in a plane wherein that axis will be found.

By the theory explained in this paper, it appears that a *parallelopipedon* may always be conceived of ſuch dimenſions,

4 that

that being, by ſome force or forces, made to revolve about an axis, paſſing through its center of gravity, with a certain angular velocity, it ſhall move exactly in the ſame manner as any other given body will move, if made to revolve, by the ſame force or forces, about an axis paſſing through its center of gravity; the quantity of matter (as well as the initial angular velocity) being ſuppoſed the ſame in both bodies; and due regard being had, in the application of the moving force or forces, to the correſponding planes in the bodies. Therefore, as we may from thence always aſſign the dimenſions of a *parallelopipedon* that ſhall be affected exactly in the ſame manner as any other given body will be affected, as well with regard to the centrifugal force of the reſpective particles of the bodies, as to the action of equal percuſſive forces, or oſcillation; it will, after ſhewing how the dimenſions of ſuch *parallelopipedon* may be computed, be only neceſſary, in inveſtigating the propoſition under conſideration, to determine the tracks and velocities of the poles of the momentary axis, about which any *parallelopipedon* may be made to revolve.

Firſt then to find ſuch parallelopipedon (P), that, with reſpect to the action of ſuch forces as are mentioned above, it may be affected exactly in the ſame manner as any other given body (Q). Let it be conſidered that G (tab.X.fig.1.) being the center of gravity, N a point of ſuſpenſion, and O the correſponding center of oſcillation or percuſſion, the rectangle GN × GO will be an invariable quantity, the direction NGO continuing the ſame; and that a cylindric ſurface being deſcribed, ſuch that the center of the middle circular ſection thereof ſhall be G, and radius $=\sqrt{GN \times GO}$, and whoſe axis ſhall be perpendicular to the plane wherein the line NGO is ſuppoſed to be impelled to move; if all the matter in the body were placed

any

any where in that surface, so that G should be the center of gravity of the matter so placed, any given force or forces, acting on the body in the plane just now mentioned, would cause the line NGO in the body to move exactly in the same manner as it would move, if it were carried with the matter placed in the said surface (as before-mentioned) after having been put in motion by the action of the same force or forces. Moreover, let it be considered, that there will at least be three permanent axes of rotation in the body Q, at right angles to each other (as I have proved in my *Mathematical Memoirs*); and that, supposing NGO to coincide with those three axes in three successive cases wherein the matter in Q shall, in each case, be conceived to be placed in a cylindric surface as described above, we may conceive it possible so to place the matter of the body, that all of it shall be in each of those three surfaces, and G still continue its center of gravity. And, a computation being made accordingly, it appears, that the matter of the body Q must be placed, in equal quantities, at each of the eight angular points of a *parallelopipedon* (R) whose dimensions (length, breadth, and thickness) shall be $\sqrt{2d^2+2f^2-2e^2}$, $\sqrt{2e^2+2f^2-2d^2}$, and $\sqrt{2d^2+2e^2-2f^2}$; d, e, and f, being the three values of $\sqrt{GN \times GO}$, when NGO is successively a permanent axis of rotation, with respect to the body Q, in three directions at right angles to each other.

If Q were a parallelopipedon, it may be easily proved, that its dimensions must be $\sqrt{6d^2+6f^2-6e^2}$, $\sqrt{6e^2+6f^2-6d^2}$, and $\sqrt{6d^2+6e^2-6f^2}$, that the corresponding parallelopipedon, at the angular points whereof the matter of Q is conceived to be placed as above, may have the same dimensions as those which we have found our parallelopipedon R must have.

Whence we may infer, that the parallelopipedon (P), which we proposed to find, must have the dimensions last written; namely, length, breadth, and thickness, respectively equal to $\sqrt{6d^2+6f^2-6e^2}$, $\sqrt{6e^2+6f^2-6d^2}$, and $\sqrt{6d^2+6e^2-6f^2}$: which may be confirmed by a more strict demonstration founded on the principles made use of in my *fourth Memoir*. For it appears by what is there proved, that the centrifugal forces of the particles of any revolving body, in two directions at right angles to each other, may be expressed in terms of A, B, K, and variable quantities shewing the position of the momentary axis; and that, in a parallelopipedon whose dimensions (length, breadth, and thickness) are a, b, k; and whose mass, or content, is $=$M; A will be $=\frac{Ma^2}{12}$, B $=\frac{Mb^2}{12}$, and K $=\frac{Mk^2}{12}$. If therefore a be $=\sqrt{6d^2+6f^2-6e^2}$, $b=\sqrt{6e^2+6f^2-6d^2}$, and $k=\sqrt{6d^2+6e^2-6f^2}$; in such body,

$$\text{A will be} = \frac{M}{2}\times\overline{d^2+f^2-e^2},$$

$$\text{B} \quad = \frac{M}{2}\times\overline{e^2+f^2-d^2},$$

$$\text{K} \quad = \frac{M}{2}\times\overline{d^2+e^2-f^2}.$$

But, in any body whatever,

$$M\times d^2 \text{ is} = \text{the sum of all the } \overline{x^2+z^2}\times p,$$

$$M\times e^2 \quad = \text{the sum of all the } \overline{y^2+z^2}\times p,$$

$$M\times f^2 \quad = \text{the sum of all the } \overline{x^2+y^2}\times p,$$

and $\frac{M}{2}\times\overline{d^2+e^2+f^2}=$ the sum of all the $\overline{x^2+y^2+z^2}\times p$: x, y, and z corresponding to the place of the particle p in the body; x being measured from the center of gravity upon a permanent axis of rotation, y at right angles to x, and z at right angles to

to y in a plane to which the said axis is perpendicular. Therefore,

A, which is = the sum of all the $x^2 \times p$, will be $= \frac{M}{2} \times \overline{d^2 + f^2 - e^2}$,

B, = the sum of all the $y^2 \times p$, $= \frac{M}{2} \times \overline{e^2 + f^2 - d^2}$,

K, = the sum of all the $z^2 \times p$, $= \frac{M}{2} \times \overline{d^2 + e^2 - f^2}$,

Hence it is evident, that d, e, and f being determined from any body whatever, the values of A, B, and K will be the same in that body as in our parallelopipedon P; and that the centrifugal forces of the particles will be the same in both bodies. Consequently, their motions about successive momentary axes (whose poles are varied by the perturbation arising from those forces), will be the same in both bodies; their initial angular velocities being the same; as well as the position of their initial momentary axes, with respect to the correspondent permanent axes of rotation in each body.

Let us now proceed to find how any *parallelopipedon* will revolve about successive momentary axes passing through its center of gravity: by which means, with the help of the theorem just now investigated, we shall be enabled to define how any body whatever will revolve about such axes; which is the chief purpose of this disquisition.

Fig. 2. and 3. The length, breadth, and thickness of the revolving *parallelopipedon* (P) being $2d$, $2c$, and $2b$, conceive a spherical surface without matter, whose center is the center of gravity of the body P, to be carried about with that body during its motion; and let the said surface be orthographically projected, so that the radius upon which b is measured may be represented

represented by AB; the radius upon which d is measured may be represented by AD; and the radius AC, upon which c is measured, may be projected into the central point A. Let P be the momentary pole, and PQ the continuation of the great circle CP. Let a denote the radius AB (= AD); g and γ the sine and cosine of the arc CP; s and t the sine and cosine of the arc BQ, to the same radius a; e the angular velocity of the body and spherical surface, measured at the distance a from the momentary axis; and M the mass or content of the parallelopipedon ($=8bcd$).

Then the motive force E, urging the pole P towards Q, will (by what I have proved in my *Mathematical Memoirs*) be $=\frac{Me^2g\gamma}{3a^7}\times\overline{Ds^2-Ca^2}$; and the motive force $\ddot{E}$, urging the same pole in a direction Po, at right angles to that in which E acts, $=\frac{Me^2g}{3a^6}\times Dst$; C and D being equal to c^2-b^2 and d^2-b^2 respectively. Let Pq be to Po as E to $\ddot{E}$; complete the parallelogram oPqr, and draw the diagonal Pr. This last mentioned line will (by what I have shewn in the *Philosophical Transactions* for the year 1777) be perpendicular to the tangent to the polar track at P. Therefore P$\ddot{p}\dddot{p}$ being the projection of that track, and Pp an indefinitely small particle thereof; if pu be perpendicular to PuA, and the quantities d^2-c^2, c^2-b^2, be not negative; $\frac{\gamma}{a}\times\overline{Ds^2-Ca^2}$ will be to Dst (as Pq to Po) as pu to Pu, the triangles Por and Pup being similar, and $or=$ Pq. But with respect to our spherical surface, pu will be to Pu as $\frac{g\dot{s}}{t}$ to $-\frac{a\dot{g}}{\gamma}$; therefore, $\overline{Ca^2-Ds^2}\times\dot{g}$ will be $=Dgs\dot{s}$, and $\frac{\dot{g}}{g}=\frac{Ds\dot{s}}{Ca^2-Ds^2}$. Whence, by taking the

fluents, we have $s^2 = a^2 \times \frac{Dm^2 - C\gamma^2}{Dg^2}$, and $t^2 = a^2 \times \frac{Dn^2 - B\gamma^2}{Dg^2}$; m and n denoting the values of s and t, when g is $= a$ and $\gamma = 0$; and B being put to denote the difference $D - C = d^2 - c^2$.

If now β and δ be put to denote the cosines of BP and DP to the radius a, we shall, from what is done above, have

$$\beta = \frac{gt}{a} = \frac{\sqrt{Dn^2 - B\gamma^2}}{D^{\frac{1}{2}}},\ \delta = \frac{gs}{a} = \frac{\sqrt{Dm^2 - C\gamma^2}}{D^{\frac{1}{2}}};$$

$$\beta^2 + \gamma^2 + \delta^2 = a^2,\ \beta\dot{\beta} + \gamma\dot{\gamma} + \delta\dot{\delta} = 0;$$

$$b^2\beta^2 + c^2\gamma^2 + d^2\delta^2 = b^2n^2 + d^2m^2, \text{ and } b^2\beta\dot{\beta} + c^2\gamma\dot{\gamma} + d^2\delta\dot{\delta} = 0.$$

Drawing AR so that $D^{\frac{1}{2}} \times$ sine of BR shall be $= C^{\frac{1}{2}}a$, it is *very remarkable*, that the momentary pole (P) will run round about the point B, or about the point D, in the spherical surface, according as the initial pole shall be in the part BCR or DCR of the said surface; that is, according as Dm^2 is less or greater than Ca^2: and that, if the initial pole (P) be any where in the great circle CR, the momentary pole, keeping in the arc of that circle, will continually approach nearer and nearer to the point C in the surface of the sphere; but, by what follows, we shall find that it never can arrive at that point in any finite time!

The equation of the track of the pole in the projection to which we have hitherto referred will, it is now obvious, be $y^2 = \frac{B}{C} \times \overline{x^2 + \frac{Ca^2 - Dm^2}{B}}$; x, measured from the center A upon AD, being $= \delta$; and y, at right angles thereto, $= \beta$.

If C be $= 0$ (that is, if c be $= b$), x will be equal to the invariable quantity m; the projected track, a *right line* parallel to AB; and the track upon the surface of the sphere, a *lesser circle* in a plane parallel to the plane of the great circle BC.

If C be $=$ D, y will be equal to the invariable quantity n; the projected track, a *right line* parallel to AD; and the track on the ſurface of the ſphere, a *leſſer circle* in a plane parallel to the plane of the great circle CD.

If Dm^2 be $= Ca^2$ the projected track will be the *right line* AR, and $y = \overline{\frac{B}{C}}|^{\frac{1}{2}} \times x$; the track upon the ſurface of the ſphere being the *great circle* CR.

In all other caſes in this projection, the track will be an *hyperbola* whoſe center is A, ſemi-axis $Aa = \overline{\frac{Ca^2 \backsim Dm^2}{B}}|^{\frac{1}{2}}$, and the other ſemi-axis $= \overline{\frac{Ca^2 \backsim Dm^2}{C}}|^{\frac{1}{2}}$; the right line AR being always an *aſypmtote*.

Fig. 4. When the track is projected on a plane ACD, to which the radius AB is perpendicular (the point D being the vertex as before) the equation thereof will be $y^2 = \frac{D}{C} \times \overline{m^2 - x^2}$; x, meaſured from the center A upon AD, being $= \delta$ (as before); and y, at right angles thereto $= \gamma$. This projection of the track of the pole will therefore always be an *ellipſis* a b (or a *circle*) whoſe center is A; ſemi-axis A a $= m$; and the other ſemi-axis $= \overline{\frac{D}{C}}|^{\frac{1}{2}} \times m$: except c be $= b$; in which caſe the projected track will be a *right line* a b parallel to AC.

Fig. 5. Moreover, the equation of the track projected on the plane ABC, to which the radius AD is perpendicular, will be $y^2 = \frac{D}{B} \times \overline{n^2 - x^2}$; x, meaſured from the center A upon AB, being $= \beta$; and y, at right angles thereto, $= \gamma$. The track of the pole in this projection will therefore always be an *ellipſis*

a b (or a *circle*) whose center is A; semi-axis A a $= n$; and the other semi-axis $= \frac{D}{B}\Big]^{\frac{1}{2}} \times n$; except c be $= d$; in which case the projected track will be a *right line* a b parallel to AC.

With regard to the permanent axes of rotation of our parallelopipedon, it appears, by my *Mathematical Memoirs*, that if two of its dimensions be equal (that is, when the body is a *square prism*), any line passing through the center of gravity of the body, in a plane to which the other dimension is perpendicular, will be a permanent axis of rotation; as will the line passing through that center, at right angles to that plane. If all the three dimensions be equal (that is, when the body is a *cube*), any line whatever passing through the center of gravity of the body will be a permanent axis of rotation.

It is observable, that the momentum of rotation of the body, about the momentary axis, is found by computation always $= \frac{e}{a^2} \times \overline{b^2m^2 + c^2a^2 + d^2n^2}$, e denoting the angular velocity. But $\frac{f}{a^2} \times \overline{b^2m^2 + c^2a^2 + d^2n^2}$ is the initial momentum of rotation. Therefore, considering the momentum of rotation as invariable, the angular velocity will be invariable, e being always $= f$, which here denotes the initial angular velocity.

Our next business is to find the length of the track described by the momentary pole (P), upon the spherical surface; and the velocity of the pole in that track.

Fig. 2, 3. It appearing, that the motive force E is $= \frac{Me^2}{3a^5} \times \overline{Dm^2 - Ca^2} \times \frac{\gamma}{g}$, and the motive force $\overset{\shortparallel}{E} = \frac{Me^2}{3a^4g} \times \sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2}$; we find $F = \sqrt{E^2 + \overset{\shortparallel}{E}^2}$ (the force compounded of those two forces) $= \frac{Me^2}{3a^5} \times \sqrt{D^2m^2n^2 - BCa^2\gamma^2}$; and, F being

to E as a to the ſine of the angle pPu, it follows, that the ſine of pPu will be $= \frac{\overline{Dm^2 - Ca^2} \times a\gamma}{g\sqrt{D^2m^2n^2 - BCa^2\gamma^2}}$, and its coſine $= \frac{a^2\sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2}}{g\sqrt{D^2m^2n^2 - BCa^2\gamma^2}}$. Therefore, that coſine being to radius as $\left(\frac{a\dot{\gamma}}{g}\right)$ the fluxion of the arc PQ to ($\dot{z}$) the fluxion of the polar track on the ſpherical ſurface, $\dot{z}$ will be $= \frac{\dot{\gamma}\sqrt{D^2m^2n^2 - BCa^2\gamma^2}}{\sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2}}$. Now, P$p$LN being a quadrant of a great circle (touching the ſaid polar track at P), and NAN a diameter of that circle; if we put w to denote the diſtance of any particle (p) of the parallelopipedon from that diameter, and G to denote the acceleratives force of any ſuch particle when w is $= a$; the motive force F $\left(= \frac{Me^2}{3a^5} \times \sqrt{D^2m^2n^2 - BCa^2\gamma^2}\right)$, computed above, will be $= \frac{G}{a^2} \times$ *the ſum of all the* $w^2 \times p$; which ſum, by computation, is found $= \frac{M}{3} \times \frac{\overline{d^2 + b^2} \,.\, D^2m^2n^2 - \overline{b^2m^2 + c^2a^2 + d^2n^2} \,.\, BC\gamma^2}{D^2m^2n^2 - BCa^2\gamma^2}$. Conſequently, G will be $= \frac{e^2}{a^3} \times \frac{\overline{D^2m^2n^2 - BCa^2\gamma^2}\Big|^{\frac{3}{2}}}{\overline{d^2 + b^2} \,.\, D^2m^2n^2 - \overline{b^2m^2 + c^2a^2 + a^2n^2} \,.\, BC\gamma^2}$. But, by what I have done in the *Philoſophical Tranſactions* for the year 1777, $\frac{aG}{e}$ will be $= v =$ the velocity wherewith the momentary pole changes its place in the ſpherical ſurface to which it is referred. Therefore,

v will be $= \frac{e}{a^2} \times \frac{\overline{D^2m^2n^2 - BCa^2\gamma^2}\Big|^{\frac{3}{2}}}{\overline{d^2 + b^2} \,.\, D^2m^2n^2 - \overline{b^2m^2 + c^2a^2 + d^2n^2} \,.\, BC\gamma^2}$; and $\frac{\dot{z}}{v} = \dot{T}$, the fluxion of the time $= \frac{a^2\dot{\gamma}}{e} \times \frac{\overline{d^2 + b^2} \,.\, D^2m^2n^2 - \overline{b^2m^2 + c^2a^2 + d^2n^2} \,.\, BC\gamma^2}{\sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2} \times \overline{D^2m^2n^2 - BCa^2\gamma^2}}$

$=$

$$= \frac{b^2m^2+c^2a^2+d^2n^2}{e} \times \left\{ \frac{\dot{\gamma}}{\sqrt{Dm^2-C\gamma^2} \times \sqrt{Dn^2-B\gamma^2}} + \frac{\frac{Dm^2-Ca^2}{b^2m^2+c^2a^2+d^2n^2} \times D^2m^2n^2\dot{\gamma}}{\sqrt{Dm^2-C\gamma^2} \times \sqrt{Dn^2-B\gamma^2} \times \overline{D^2m^2n^2-BCa^2\gamma^2}} \right.$$: which,

when Dm^2 is $= Ca^2$, becomes $= \frac{\overline{d^2+b^2} \cdot a^2\dot{\gamma}}{e\sqrt{BC} \cdot \overline{a^2-\gamma^2}}$.

It is evident, that $\frac{d^2+b^2}{2e\sqrt{BC}} \times a \times$ hyp. log. of $\frac{a+\gamma}{a-\gamma}$, the value of T in that particular case, will be *infinite* when γ is $= a$; and this conclusion agrees with what is said above respecting the motion of the momentary pole along the great circle CR (fig. 2. and 3.).

I have not found, that the value of T will, in general, be assigned by the *arcs of the conic sections*; but my Tables * shew, that it will be so assigned when Dm^2 is $= Ba^2$, and in some other particular cases.

We have still to investigate the track of the momentary pole in the *immoveable* concave spherical surface, which we must conceive to surround our *moveable* convex spherical surface, supposing the center of both those surfaces to coincide with the centers of gravity of our parallelopipedon: which central point is always in this disquisition supposed at rest.

Let AL be the projection of part of a great circle CL, at right angles to the great circle PpLN; then will the sine of the arc CL be $= \frac{\overline{Dm^2-Ca^2} \times \gamma}{\sqrt{D^2m^2n^2-BCa^2\gamma^2}}$; its cosine $=$ $\frac{D^{\frac{1}{2}}\sqrt{Da^2m^2n^2-\overline{Dm^4-Ca^2m^2+Ca^2n^2} \cdot \gamma^2}}{\sqrt{D^2m^2n^2-BCa^2\gamma^2}}$; and, the fluxion of that sine being $= \frac{\overline{Dm^2-Ca^2} \times D^2m^2n^2\dot{\gamma}}{\overline{D^2m^2n^2-BCa^2\gamma^2}\}^{\frac{3}{2}}}$, the fluxion of that arc (CL) will be

* *Mathematical Memoirs*, published in 1780.

$$= \frac{\overline{Dm - Ca^2} \times D^{\frac{3}{2}} am^2 n^2 \dot{\gamma}}{D^2 m^2 n^2 - BCa^2\gamma^2 \times \sqrt{Da^2 m^2 n^2 - Dm^4 - Ca^2 m^2 + Ca^2 n^2 \cdot \gamma^2}}.$$ Consequently,

the sine of the arc PL being $= \frac{a^2 \sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2}}{D^{\frac{1}{2}} \sqrt{Da^2 m^2 n^2 - Dm^4 - Ca^2 m^2 + Ca^2 n^2 \cdot \gamma^2}}$;

and this sine being to radius as the fluxion of the arc CL to the measure of the angle of contact of the polar track on the *moveable* spherical surface with a great circle, we find that measure

$$= \frac{\overline{Dm - Ca^2} \times D^2 m^2 n^2 \dot{\gamma}}{\sqrt{Dm^2 - C\gamma^2} \times \sqrt{Dn^2 - B\gamma^2} \times \overline{D^2 m^2 n^2 - BCa^2\gamma^2}} = \frac{\overline{Dm^2 - Ca^2} \times D^2 m^2 n^2 \dot{z}}{\overline{D^2 m^2 n^2 - BCa^2\gamma^2}|^{\frac{3}{2}}}.$$

The measure of the angle of contact of the track of the momentary pole, in the *immoveable* spherical surface, with a great circle, will accordingly be

$$e\dot{T} = \frac{\overline{Dm^2 - Ca^2} \times D^2 m^2 n^2 \dot{z}}{\overline{D^2 m^2 n^2 - BCa^2\gamma^2}|^{\frac{3}{2}}} = \frac{\overline{b^2 m^2 + c^2 n^2 + d^2 n^2} \cdot \dot{z}}{\sqrt{D^2 m^2 n^2 - BCa^2\gamma^2}}$$: by means of which measure we may describe, by points, the track of the momentary pole in the spherical surface last mentioned.

There are other methods of finding that track; but I know none that is less difficult than this method, or in any respect more satisfactory.

The radius of the lesser circle, which is the circle of curvature of the polar track in our immoveable spherical surface, will be =

$$\frac{a\dot{z}}{\sqrt{\dot{z}^2 + \text{sq. of the meas. of the ang. of cont.}}} = \frac{\sqrt{D^2 m^2 n^2 - BCa^2\gamma^2}}{\sqrt{\overline{b^2 + c^2}|^2 \cdot \overline{m^2 + c^2 + d^2}|^2 \cdot n^2 - BC\gamma^2}}.$$

When B is $= 0$, or very small in comparison with D, and Dm^2 is less than Ca^2, the last mentioned radius will be equal, or nearly equal, to the invariable quantity $\frac{Dmn}{\sqrt{\overline{b^2 + d^2}|^2 \cdot m^2 + 4d^4 n^2}}$; the track of the pole in the immoveable spherical surface being then exactly, or very nearly, a lesser circle. At the same time, the

the polar track upon the moveable spherical surface will be exactly, or very nearly, a lesser circle whose radius is m.

When C is $=0$, or very small in comparison with D, and Dm^2 is greater than Ca^2, the track of the pole in the immoveable spherical surface will be exactly, or very nearly, a lesser circle whose radius is $=\frac{Dmn}{\sqrt{4c^4m^2+\overline{c^2+a^2}|^2 \cdot n^2}}$; and then the polar track upon the moveable spherical surface will be exactly, or very nearly, a lesser circle whose radius is n.

Whatever the curves may be which the momentary pole shall describe in those two spherical surfaces, the track upon the moveable surface will always touch and roll along the track in the immoveable surface (whilst the common center of both surfaces remains at rest), in the manner described in my Paper in the *Philosophical Transactions* for the year 1777; the velocity of the point of contact being equal to the value of v computed above, which velocity when B is $=0$, or very small in comparison with D, and Dm^2 is less than Ca^2, will be exactly, or very nearly, $=\frac{d^2-b^2}{d^2+b^2}\times\frac{mne}{a^2}$; and when C is $=0$, or very small in comparison with D, and Dm^2 is greater than Ca^2, that velocity will be exactly, or very nearly, $=\frac{d^2-c^2}{d^2+c^2}\times\frac{mne}{a^2}$.

The polar track upon the moveable spherical surface will always roll along the convexity of the track in the immoveable spherical surface; the convexity or concavity of the former being turned towards the convexity of the latter, according as Dm^2 is greater or less than Ca^2. Which track in the immoveable spherical surface, when it is not circular, will touch a certain circle as often as γ, during the motion, shall become $=0$; and likewise another parallel circle

circle as often as γ shall become equal to $\sqrt{\frac{A}{C}} \times m$, or $\sqrt{\frac{A}{B}} \times n$; the parts of the track between the points of contact being perfectly similar. If Dm^2 be $= Ca^2$ (Dn^2 being then $= Ba^2$, and consequently $\sqrt{\frac{D}{C}} \times m = \sqrt{\frac{D}{B}} \times n = a$), the said track will make an infinite number of revolutions about a certain point, continually approaching nearer and nearer thereto, without arriving thereat in any finite time, though the length of the spiral so described cannot exceed a certain finite qnantity.

M. Euler has computed, that if the motive forces to turn the revolving body about AB, AC, AD, be respectively denoted by H, I, K;

$$H \text{ will be} = \frac{M}{3} \cdot \frac{d^2+c^2}{a^3\dot{T}} \times \text{flux. of } e\beta - \frac{M}{3a^5} \cdot Be^2\gamma\delta,$$

$$I = \frac{M}{3} \cdot \frac{d^2+b^2}{a^3\dot{T}} \times \text{flux. of } e\gamma + \frac{M}{3a^5} \cdot De^2\beta\delta,$$

$$K = \frac{M}{3} \cdot \frac{c^2+b^2}{a^3\dot{T}} \times \text{flux. of } e\delta - \frac{M}{3a^5} \cdot Ce^2\beta\gamma;$$

γ being supposed to decrease as T increases: and he has put the value of each of those forces (H, I, K) = o. In doing so, it seems to me, that he has erroneously assumed equations as generally true, which are only so in a particular case. For $\frac{M}{3a^5} \cdot Be^2\gamma\delta$ is the motive force to turn the body about AB, arising from the centrifugal force of its particles revolving about the momentary axis AP, supposing the pole to keep its place; and $\frac{M}{3} \cdot \frac{d^2+c^2}{a^3\dot{T}} \times$ flux. of $e\beta$ is the value of the motive force requisite to cause the *whole* variation of the velocity $\left(\frac{e\beta}{a}\right)$ about AB. But the first mentioned force *alone* does not, in general,

 cause

cause *all* the variation of the velocity about AB; that velocity varies in consequence of the evagation of the pole P; and that evagation is caused by the motive forces urging the body to turn about AB, AC, AD, *conjunctly*. Therefore the motive force $\frac{M}{3a^5} \cdot Be^2\gamma\delta$ about AB *only* will not, in general, be equal to $\frac{M}{3} \cdot \frac{d^2+c^2}{a^3\dot{T}} \times$ flux. of $e\beta$, the value of the *whole* motive force requisite to cause the variation of the velocity $\frac{e\beta}{a}$, as M. EULER reckoned.

The like objection may, I conceive, be justly made to his other two equations similar to that which is here particularly adverted to.

M. D'ALEMBERT's radical errors, in treating this subject, appear to me nearly similar to M. EULER's.

Other arguments may be adduced to prove, that the equations assumed by those gentlemen are not well founded. If the forces to turn the body about the lines AB, AC, AD were each $= 0$, the velocities about those lines must each remain invariable; but it seems absolutely impossible that they can ever remain so, whilst the angles which those lines make with the momentary axis are each continually varying. Moreover, according to their conclusions, the tangent at P to the track of polar evagation, upon the moveable spherical surface, will not always be perpendicular to the direction in which the pole P will be urged to turn by the joint centrifugal force of the particles of the revolving body; whereas it is proved, I presume, beyond a doubt, in my Paper above-mentioned, that the said track will always be intersected at right angles by the direction in which the momentary pole shall, at any instant of time, be urged to turn by the force causing its evagation.

If we resolve each of the three forces H, I, K, into two others; the one to turn the body about the diameter NAN, and the other to turn it about the momentary axis PAP, at right angles to that diameter; the forces to turn it in the last mentioned direction, arising from the said forces H, I, K, will be

$$\frac{\beta H}{a} = \frac{M}{3} \cdot \frac{d^2+c^2}{a^4\dot{T}} \times \beta \text{ flux. of } e\beta - \frac{M}{3a^6} \cdot Be^2\beta\gamma\delta,$$

$$\frac{\gamma I}{a} = \frac{M}{3} \cdot \frac{d^2+b^2}{a^4\dot{T}} \times \gamma \text{ flux. of } e\gamma + \frac{M}{3a^6} \cdot De^2\beta\gamma\delta,$$

$$\frac{\delta K}{a} = \frac{M}{3} \cdot \frac{c^2+b^2}{a^4\dot{T}} \times \delta \text{ flux. of } e\delta - \frac{M}{3a^6} \cdot Ce^2\beta\gamma\delta.$$

The sum of these forces, it is obvious, must be $=0$; the direction wherein they are supposed to act being at right angles to that in which the body will be actually urged to turn by the joint centrifugal force of its particles, and that being the only force whereby the motion of the body is supposed to be affected: which sum ($B+C-D$ being $=0$) is, when divided by $\frac{M}{3a^4\dot{T}}$,

$$= \left\{ \begin{array}{l} \overline{d^2+c^2} \cdot \beta^2\dot{e} + \overline{d^2+b^2} \cdot \gamma^2\dot{e} + \overline{c^2+b^2} \cdot \delta^2\dot{e} \\ \overline{d^2+c^2} \cdot e\beta\dot{\beta} + \overline{d^2+b^2} \cdot e\gamma\dot{\gamma} + \overline{c^2+b^2} \cdot e\delta\dot{\delta} \end{array} \right\} = 0.$$

But $\beta\dot{\beta}+\gamma\dot{\gamma}+\delta\dot{\delta}$ being before found $=0$, we have $\overline{d^2+c^2+b^2} \times \overline{\beta\dot{\beta}+\gamma\dot{\gamma}+\delta\dot{\delta}} = 0$; and $b^2\beta\dot{\beta}+c^2\gamma\dot{\gamma}+d^2\delta\dot{\delta}$ being also found $=0$; it evidently follows, that

$$\overline{d^2+c^2} \cdot \beta\dot{\beta} + \overline{d^2+b^2} \cdot \gamma\dot{\gamma} + \overline{c^2+b^2} \cdot \delta\dot{\delta} \text{ will be} = 0.$$

Therefore $\overline{d^2+c^2} \cdot \beta^2\dot{e} + \overline{d^2+b^2} \cdot \gamma^2\dot{e} + \overline{c^2+b^2} \cdot \delta^2\dot{e}$ will be $=0$: consequently $\dot{e}$ will be $=0$, and e invariable; which agrees with what is said above respecting the momentum of rotation.

The other forces arising by resolution from the forces H, I, K, to turn the body about the diameter NAN, will be

$$-\frac{B\gamma\delta H}{S} = -\frac{M}{3a^3}\cdot\frac{d^2+c^2}{S\dot{T}}\cdot Be\gamma\delta\dot{\beta} + \frac{Me^2}{3a^5S}\cdot B^2\gamma^2\delta^2,$$

$$\frac{D\beta\delta I}{S} = \frac{M}{3a^3}\cdot\frac{d^2+b^2}{S\dot{T}}\cdot De\beta\delta\dot{\gamma} + \frac{Me^2}{3a^5S}\cdot D^2\beta^2\delta^2,$$

$$-\frac{C\beta\gamma K}{S} = -\frac{M}{3a^3}\cdot\frac{c^2+b^2}{S\dot{T}}\cdot Ce\beta\gamma\dot{\delta} + \frac{M}{3a^5S}\cdot C^2\beta^2\gamma^2;$$

$$S \text{ being} = \sqrt{D^2m^2n^2 - BCa^2\gamma^2}.$$

And, no external force being supposed to act on the body, it follows, that the sum of these three forces must be $=0$: therefore we may infer, that

$\dot{T}$ will be $= \frac{a^2}{e}\times\frac{\overline{a^4-c^4}\cdot\gamma\delta\dot{\beta} - \overline{d^4-b^4}\ \beta\delta\dot{\gamma} + \overline{c^4-b^4}\cdot\beta\gamma\dot{\delta}}{B^2\gamma^2\delta^2 + D^2\beta^2\delta^2 + C^2\beta^2\gamma^2}$; which agreeing with the value of $\dot{T}$ found above, the truth of our preceding process is thus confirmed.

The force $\frac{Me^2}{3a^5S}\times\overline{B^2\gamma^2\delta^2 + D^2\beta^2\delta^2 + C^2\beta^2\gamma^2}$, arising from those three forces, is the *whole* joint centrifugal force of the particles of the revolving body, to turn it about the diameter NAN the way it will actually be urged to turn by such force; the value whereof so computed will be $\left(=\frac{Me^2}{3a^5}\times\sqrt{D^2m^2n^2 - BCa^2\gamma^2} = \frac{Me^2S}{3a^5}\right)$ equal to the value of the force F computed above, both being considered as urging the body to turn in the same direction. And the quantity

$$\frac{Me}{3a^3S\dot{T}}\times\overline{\overline{d^2+c^2}\cdot\gamma\delta\dot{\beta} - \overline{d^2+b^2}\cdot D\beta\delta\dot{\gamma} + \overline{c^2+b^2}\cdot C\beta\gamma\dot{\delta}}$$

$\left(=\frac{ex}{a\dot{T}}\times \textit{the sum of all the } w^2\times p\right)$ is the value of the motive force which, acting in that very direction, is requisite to cause the momentary pole to change its place as above described. Thus we see distinctly how the equation arises, by which the value of $\dot{T}$ is just now determined. I do

I do not find that the resolving the forces H, I, K, in any other manner will conduce to the attainment of any useful conclusion.

It appears, by what is done above, that the force

$$H \text{ is} = \frac{Me}{3a^3RT} \times CD^2 m^2 \beta^2 \dot{\beta},$$

$$I \text{ is} = \frac{Me}{3a^3RT} \times BC \,.\, \overline{Cn^2 - Bm^2} \,.\, \gamma^2 \dot{\gamma},$$

$$K = \frac{-Me}{3a^3RT} \times BD^2 n^2 \delta^2 \dot{\delta};$$

$$R \text{ being} = B^2\gamma^2\delta^2 + D^2\beta^2\delta^2 + C^2\beta^2\gamma^2.$$

And it is obvious, that each of the three last mentioned forces will be $= 0$, if any two of the quantities b, c, d, be equal; two of the values of those forces then vanishing, by reason of that equality; and the third value also vanishing by either $\dot{\beta}$, $\dot{\gamma}$, or $\dot{\delta}$, being at the same time $= 0$. Therefore, in that case it happens, that M. EULER's computation agrees with mine: in every other case, I am clearly of opinion, his conclusions are not true. The same may be said of M. D'ALEMBERT's conclusions respecting the same proposition.

The evagation of the pole of a revolving body considered above, does not arise from gravity, the attraction of any other body, or any external impulse whatever; but is only the consequence of the *inertia of matter*, and must necessarily ensue, according to the theory here explained, in every body in the universe, after having been made to revolve, without restraint, about any line passing through its center of gravity, that is not *a permanent* axis of rotation.

The Earth being neither uniformly dense nor a perfect spheroid must, in strictness, be considered as having only three permanent axes of rotation, agreeably to what I have proved in my *Mathematical Memoirs*; and, as it is disturbed in its rotatory motion by the attraction of the sun and moon (and

other

other bodies in our system); it follows; that it will not continually revolve about either of those axes, but will revolve, or endeavour to revolve, about successive momentary axes, as shewn above. If then its three permanent axes of rotation be called its *first*, *second*, and *third* axes; and the poles of its *first* axis be those about which its momentary poles are carried according to our theory; the *second* and *third* axes will be in the plane of its equator, the three being at right angles to each other. Therefore, with respect to the above theory, this terrestrial mass must be considered of such a form, that its equator, and any section parallel thereto, shall rather be elliptical than circular. And, denoting its first, second, and third axes by b, c, d, respectively, observations evince, that the difference $c-b$ will be much greater than the difference $d-c$. Whence it follows, that (supposing the earth's rotatory motion to be disturbed *only* by the centrifugal force arising from the *inertia* of its own particles) the track of polar evagation with us will be nearly circular, and the radius of the limiting circle very small, whether we have regard to the moveable or immoveable spherical surface referred to above; but that, in the latter surface, such circle will be much less than in the former: and it moreover follows, that the concavity of the track upon the moveable surface will continually touch and roll along the convexity of the track in the immoveable surface.

In other planets, the tracks of polar evagation may, from a similar cause, be very different. The theory above explained evidently proves, that their axes of rotation may possibly vary greatly in position, merely through the *inertia of matter*; whilst Providence has so ordered it, that the position of the axes of rotation of this planet shall, by that cause, be but very little altered.

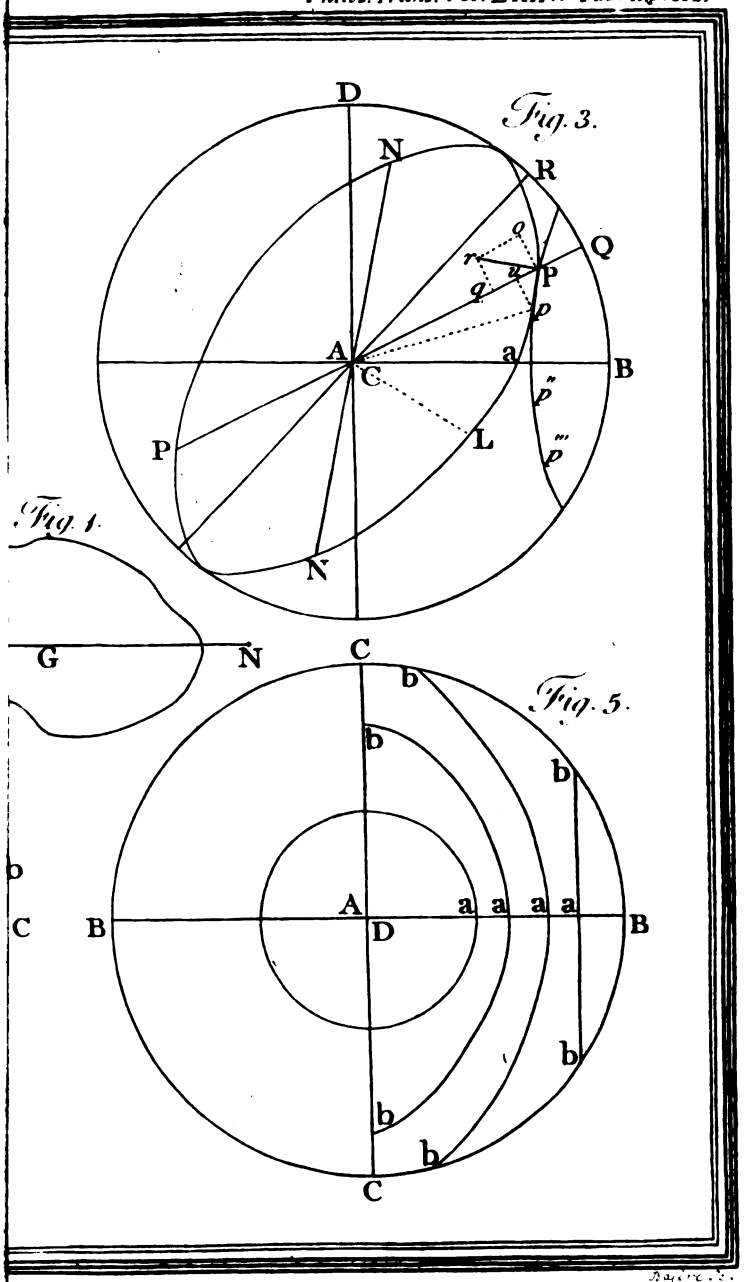
Fig. 3.
D
N
R
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Fig. 1.
G
N
Fig. 5.
C
b
A
a
D
B
C

XVII. *Description of a new Marine Animal. In a Letter from Mr.* Everard Home, *Surgeon, to* John Hunter, *Esq. F.R.S. With a Postscript by Mr.* Hunter, *containing anatomical Remarks upon the same.*

Read March 7, 1785.

TO JOHN HUNTER, ESQ. F.R.S.

DEAR SIR, Sept. 20, 1784.

I SENT you, about three years ago, a sea animal from Barbadoes, which was unlike any one I had ever seen. From the want of books and other information in that island, I was unable at the time to find out, whether it was a new acquisition, or had been described by any authors in natural history.

Since my arrival in England, I have examined the libraries of some men of science for an account of this animal, and have made other enquiries among the naturalists, without success. The specimen I sent you was found on a part of the coast which had undergone very remarkable changes, in consequence of a violent hurricane. These changes were indeed the means of its being discovered, and present a probable reason why it was not discovered before. The extraordinary circumstances which brought it within our reach, and the silence of all the authors on natural history which I have been able to consult, incline me to believe it to be a non-descript. As the peculiarities of its structure may add to the knowledge of the natural history

hiſtory of other animals of this genus, at preſent ſo little underſtood, I have drawn out a more particular account of it; which, if you think it deſerves attention, you may preſent to the Royal Society.

This animal was found on the ſouth-eaſt coaſt of Barbadoes, cloſe to Charles Fort, about a mile from Bridge Town, in ſome ſhoal water, ſeparated from the ſea by the ſtones and ſand thrown up by the dreadful hurricane, which happened in the year 1780, and did ſo much miſchief to the iſland.

The wind, in the beginning of the ſtorm, which was in the afternoon, blew very furiouſly from the north-weſt, making a prodigious ſwell in the ſea; and in the middle of the night changing ſuddenly to the ſouth-eaſt, it blew from that quarter upon the ſea, already agitated, forcing it upon the ſhore with ſo much violence, that it threw down the rampart of Fort Charles, which was oppoſed to it, although thirty feet broad, by the burſting of one ſea. It forced up, at the ſame time, immenſe quantities of large coral rocks from the bottom of the bay, making a reef along this part of the coaſt for the extent of ſeveral miles, at only a few yards diſtance from the ſhore.

The ſoundings of the harbour were found afterwards to be intirely changed, by the quantity of materials removed from the bottom in different places. In the reef of coral was found an infinite number of large pieces of brain-ſtone, containing the ſhell of this animal; but the animals had either been long dead, or more probably deſtroyed by the motion of the rocks in the ſtorm: ſome few of the brain-ſtones, however, that had been thrown beyond the reef, and lodged in the ſhoal water, receiving leſs injury, the animals were preſerved unhurt.

The animal, with the ſhell, is almoſt intirely incloſed in the brain-ſtone, ſo that at the depth in which they generally lie,

they are hardly discernible, through the water, from the common surface of the brain-stone; but when in search of food they throw out two cones, with membranes twisted round them in a spiral manner, which have a loose fringed edge, looking at the bottom of the sea like two flowers; and in this state they were discovered.

The species of Actinia called in Barbadoes the Animal Flower, and common to many parts of that island, although rarely before seen on this part of the coast, was now found in considerable numbers in this shoal water.

The animal was first observed by Captain HENDIE, the officer commanding Fort Charles, in looking for shells which were thrown up in great numbers from the bottom of the harbour. He found a piece of brain-stone containing three of them in different parts of it. Some little time after, I was lucky enough to find another brain-stone with two in it; one of them is the specimen in your possession; the other was destined for examination, of which the following is the account.

The animal, when taken out of the shell, including the two cones and their membranes, is five inches in length; of which the body is three inches and three-quarters, and the apparatus for catching its prey, which may be considered as its tentacula, about an inch and a quarter.

The body of the animal is attached to its shell, for about three-quarters of an inch in length, at the anterior part where the two cones arise, by means of two cartilaginous substances, with one side adapted to the body of the animal, the other to the internal surface of the shell: the rest of the body is unattached, of a darkish white colour, about half an inch broad, a little flattened, and rather narrower towards the tail. The muscular fibres upon its back are transverse; those on the belly

longitudinal, making a band the whole length of the body, on the edge of which the transverse fibres running across the back terminate.

The two cartilaginous substances by which the animal adheres to its shell, are placed one on each side of the body, and are joined together upon the back of the animal at their posterior edges: they are about three-quarters of an inch long, are very narrow at their anterior end, becoming broader as they go backwards; and at their posterior end they are the whole breadth of the body of the animal. Upon their external surface there are six transverse ridges, or narrow folds; and along their external edges, at the end or termination of each ridge, is a little eminence resembling the point of a hair pencil, so that on each side of the animal there are six of these little projecting studs, for the purpose of adhering to the sides of the shell in which the animal is inclosed. The internal surfaces of these cartilages are firmly attached to the body of the animal, in their middle part, by a kind of band or ligament; but the upper and lower ends are lying loose.

From the end of the body, between the two upper ends of these cartilages, arise what I suppose to be the tentacula, consisting of two cones, each having a spiral membrane twining round it: they are close to each other at their bases, and diverge as they rise up, being about an inch and a quarter in length, and nearly one-sixth of an inch in thickness at their base, and gradually diminishing till they terminate in points. The membranes which twine round these cones also take their origin from the body of the animal, and make five spiral turns and a half round each, being lost in the points of the cones; they are loose from the cone at the lowest spiral turn which they make, and are nearly half an inch in breadth; they are exceedingly

ingly delicate, and have at ſmall diſtances fibres running acroſs them from their attachment at the ſtem to the looſe edge, which gives them a ribbed appearance. Theſe fibres are continued about one-tenth of an inch beyond the membrane, having their edges finely ſerrated, like the tentacula of the Actiniæ found in Barbadoes: theſe tentacula ſhorten as the ſpiral turns become ſmaller, and are entirely loſt in that part of the membrane which terminates in the point of the cone.

Behind the origin of theſe cones ariſes a ſmall ſhell, which, for one-ſixth of an inch from its attachment to the animal, is very ſlender: it is about three quarters of an inch in length, becoming conſiderably broader at the other end, which is flat, and about one-third of an inch broad; the flattened extremity is covered with a kind of hair, and has riſing out of it two ſmall claws, about one-ſixth of an inch in length. If the hair, and mucus entangled in it, be taken away, this extremity of the ſhell becomes concave, is of a pink colour, and the two claws riſing out from its middle part have each three ſhort branches, not unlike the horns of a deer. The body of this ſhell has a ſoft cartilaginous covering, with an irregular but poliſhed ſurface: on this the cones reſt in their collapſed ſtate, in which ſtate the whole of the ſhell is drawn into the cavity of the brain-ſtone, excepting the flattened end with the two claws.

Before the cones there is a thin membrane, which appears to be of the ſame length with the ſhell juſt deſcribed. In the collapſed ſtate it lies between the cones and the ſhell in which the animal is incloſed; but, when the tentacula are thrown out, it is alſo protruded.

The ſhell of this animal is a tube, which is very thin, and adapted to its body: the internal ſurface is ſmooth, and of a

pinkish white colour: its outer surface is covered by the brain-stone in which it is inclosed, and the turnings and windings which it makes are very numerous. The end of the shell, which opens externally, rises above the surface of the stone on one side half an inch in height, for about half the circumference of the aperture, bending a little forwards over it, and becoming narrower and narrower as it goes up, terminating at last in a point just over the center of the opening of the shell; on the other side it forms a round margin to the surface of the brain-stone. This part of the shell is much thicker and stronger than that part which is inclosed in the brain-stone: its outer surface is of a darkish brown colour; its inner of a pinkish white.

The animal, when at rest, is wholly concealed in its shell; but when it seeks for food, the moveable shell is pushed slowly out with the cones and their membranes in a collapsed state; and when the whole is exposed, the moveable shell falls a little back, and the membrane round each of the cones is expanded, the tentacula at the bases of the cones having just room enough to move without touching one another. The thin membrane which lay between the cones and the inclosing shell is protruded in the form of a fold, and lies over the external shell which projects from the brain-stone.

The membranes have a slow spiral motion, which continues during the whole time of their being expanded; and the tentacula upon their edges are in constant action. The motion of the membrane of the one cone seems to be a little different from that of the other, and they change from the one kind of motion to the other alternately, a variation in the colour of the membrane at the same time taking place, either becoming a shade lighter or darker; and this change in the colour, while the

the whole is in motion, produces a pleasing effect, and is most striking when the sun is very bright. The membranes, however, at some particular times appear to be of the same colour.

While the membranes are in motion, a little mucus is often separated from the tentacula at the point of the cone. Upon the least motion being given to the water, the cones are immediately, and very suddenly, drawn in.

This apparatus for catching food is the most delicate and complicated that I have seen; but I shall not trouble you with any conjectures upon what that food may be, as I have not attained sufficient knowledge of the animal to speak with the smallest certainty.

I have endeavoured to describe the external appearances as I saw them; and have annexed two drawings of the animal in its two different states, one in search of food, and one while lying at rest; these are a little magnified, to show the parts more distinctly.

I shall not say any thing of the internal parts, or their uses, as the animal is in your possession, who are so much better able to explain its internal œconomy.

I am, &c.

EVERARD HOME.

POST-

POSTSCRIPT,

BY JOHN HUNTER, ESQ. F.R.S.

ANIMALS which come from foreign countries, and cannot be brought to England alive, must be kept in spirits to preserve them from putrefaction, which makes them less fitted for anatomical examination; for the spirits, which preserve them, produce a change in many of their properties, and alter the natural colours, and texture of the parts, so that often the structure alone of the animal can be ascertained; and where this is not naturally distinct, it becomes frequently intirely obscured, and the texture of the finer parts is wholly destroyed, requiring a very extensive knowledge of such parts in animals at large, to assist us in bringing them to light: this happens to be the case with the animal whose dissection is the subject of this Postscript.

The animal may be said to consist of a fleshy covering, a stomach and intestinal canal, and the two cones with their tentacula and moveable shell, which last may be considered as appendages.

The body of the animal is flattened, and terminates in two edges, which are intersected by rugæ, the fasciculi of transverse muscular fibres which run across the back being continued over them. Upon each of these edges is placed a row of fine hairs, which project to some distance from the skin.

The fleshy covering consists principally of muscular fibres: those upon the back are placed transversely, to contract the body

5 laterally;

laterally; those on the belly longitudinally, to shorten the animal when stretched out, and to draw it into the shell.

The stomach and intestine make one straight canal: the anterior end of this forms the mouth, which opens into the grooves made by the spiral turns of the tentacula round the stem of each of the cones; and the intestine at the posterior end opens externally, forming the anus. From the contracted state of the animal, the intestine is thrown into a number of folds.

On examining the cones and the tentacula, I at first believed that the spiral form arose from their being in a contracted state; and that, when the tentacula were erected, the cone untwisted, forming a longer cone with the tentacula arising from its sides; like the plume from the stem of a feather; and that this stem was drawn in or shortened by means of a muscle passing along the center, which threw the tentacula into a spiral line, similar to the penis's of many birds; but how far this is really the case, I have not been able to ascertain.

The internal structure of this animal, like most of those which have tentacula, is very simple; it differs, however, materially from many, in having an anus, most animals of this tribe, as the Polypi, having only one opening, by which the food is received, and the excrementitious part of it also afterwards thrown out; this we must have supposed, from analogy, to take place in the animal which is here described, more particularly since it is inclosed in a hard shell, at the bottom of which there appears to be no outlet; but as there is an anus this cannot be the case.

It is very singular, that in the Leach, Polypi, &c. where no apparent inconvenience can arise from having an anus, there is not one, while in this animal, where it would seem to be attended with many, we find one; but there being no anus in

in the Leach, Polypi, &c. may depend upon ſome circumſtance in the animal œconomy which we are at preſent not fully acquainted with.

The univalves, whoſe bodies are under ſimilar circumſtances reſpecting the ſhell with this animal, have the inteſtine reflected back, and the anus, by that means, brought near to the external opening of the ſhell, the more readily to diſcharge the excrement; and although this ſtructure, in theſe animals, appears to be ſolely intended to anſwer that purpoſe, yet when we find the ſame ſtructure in the black Snail, which has no ſhell, this reaſoning will not wholly apply, and we muſt refer it to ſome other intention in the animal œconomy.

In this animal we muſt therefore reſt ſatisfied that the diſadvantageous ſituation of the anus, with reſpect to the excrement's being diſcharged from the ſhell, anſwers ſome purpoſe in the œconomy of the animal, which more than counter-balances the inconveniences produced by it.

It would appear, from conſidering all the circumſtances, that the excrement thrown out at the anus muſt paſs from the tail along the inſide of the tube, between it and the body of the animal, till it comes to the external opening of the ſhell, as there is no other evident mode of diſcharging it.

How the tube or ſhell is formed in ſtone or coral is not eaſily aſcertained. It may be aſked, whether this animal has the power of boring backwards as the Teredo Navalis probably does, or whether the ſtone or coral is formed at the ſame time with the animal, and grows and increaſes with it: and if we conſider all the circumſtances, this laſt would appear to be moſt probable, and agree beſt with the different phænomena; for the coral is lined with a ſhell, which could not be the caſe if the animal was continually increaſing this hole, both in length and

 breadth,

breadth, in proportion to its growth; but if the coral and the animal increaſe together, it is then ſimilar to the growth of all ſhells, whether bivalve or univalve.

The animal does not appear to have the power of increaſing its canal, being only compoſed of ſoft parts. This, however, is no argument againſt its doing it, for every ſhell fiſh has the power of removing a part of its ſhell, ſo as to adapt the new and the old together; which is not done by any mechanical power, but by abſorption.

The tribe of animals which have tentacula conſiſts of an almoſt infinite variety, and many of the ſpecies have been deſcribed. Of that kind, however, which has the double cones, I believe hitherto no account has been given. It is moſt probably to be found in the ſeas ſurrounding the different iſlands in the Weſt Indies; for I received an animal, ſome years ago, from Mr. OLIVER, ſurgeon, at Tenby in Pembrokeſhire, which he had procured from a gentleman at St. Vincent's; which, upon examination, proves to be the ſame animal with that above deſcribed, only that the moveable ſhell is wanting.

Since I began this Poſtſcript, I find there is a deſcription of a double-coned Terebella, publiſhed by the rev. Mr. CORDINER, at Bamf in Scotland, which was found upon that coaſt; in which the cones have their tentacula paſſing out from the end, and when erected they ſpread from the cone as from a center. This proves that the double-coned tentacula alſo have different ſpecies.

EXPLANATION OF THE FIGURES, TAB. XI.

FIG. I.

A drawing of the animal after death, as it appeared in ſpirits, a little magnified.

A. The under ſide of the body.

BB. The cartilages which attach the animal to the ſides of the cavity in which it lies.

C. One of the cones covered by its membrane in a collapſed ſtate.

D. The loweſt ſpiral turn of the membrane and its tentacula ſpread out.

EE. The cut edges of the divided membrane, which are turned on each ſide to ſhew the cone.

F. The cone as it appears in the intervals between the ſpiral turns of the membrane.

G. The moveable ſhell, with the ſmooth cartilaginous covering, in an outſide view.

H. The flattened end of the moveable ſhell, with hair upon it.

II. The two claws that ariſe from the ſurface of the flattened end of the moveable ſhell.

K. The anus, into which a hog's briſtle is introduced.

FIG. II.

A drawing of the animal, with its tentacula expanded in ſearch of food, as it appears in the ſea; taken from a ſketch made in Barbadoes, where no draughtſman could be procured

Fig. I.

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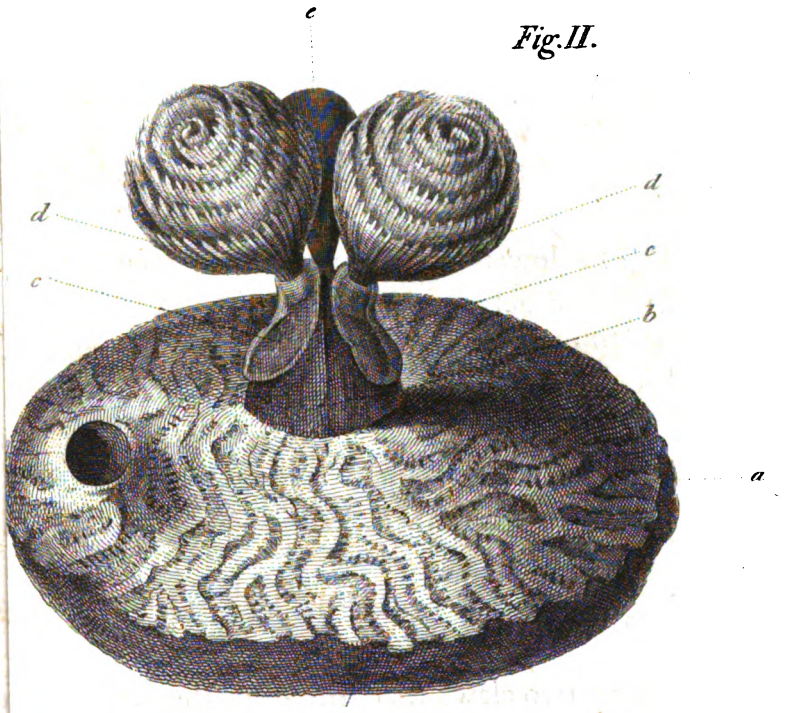
Fig. II.



while the animal was alive. This alſo is larger than the animal.

a. The ſort of brain-ſtone in which the animal was diſcovered.

b. The external prominent ſhell.

cc. The membrane which is protruded with the cones and moveable ſhell, and makes a fold over the edges of the prominent ſhell.

dd. The membranes and tentacula in a ſtate of expanſion.

e. The inner ſide of the moveable ſhell, as it appears when protruded.

f. The hole in the brain-ſtone as it appears when the prominent ſhell is broken off, and which may be ſeen in many ſpecimens of brain-ſtone.

XVIII. *A Description of a new System of Wires in the Focus of a Telescope, for observing the comparative Right Ascensions and Declinations of cœlestial Objects; together with a Method of investigating the same when observed by the Rhombus, though it happen not to be truly in an equatorial Position.* By the Rev. Francis Wollaston, *LL.B. F.R.S.*

Read April 7, 1785.

IN consequence of a paper communicated the last year to this Society, and honoured with a place in our Transactions, it may be expected of me, that I should now deliver in an account of what farther observations I have made on that constellation of which I then gave a rough map. This I readily would do, if they were in any degree worthy of the Society's notice. But as yet they are far from perfect: how much better they may succeed hereafter, time must shew.

Yet has this year perhaps not quite been lost; the difficulties which disappointed my hopes, having led to what appears to me an improvement in the instrument with which to pursue such observations.

My design, as was hinted in that Paper, was to ascertain, as well as I was able, the right ascensions and declinations of the stars I had laid down; by observing their *meridian passages* and *meridian altitudes*, where that could be done with such small instruments as mine; as also by their *comparative passages* through the field of an equatorial telescope furnished with a

ſyſtem of wires invented by Dr. BRADLEY, and called by the French *Reticule Rhomboïde*, whence it has commonly obtained in Engliſh the name of the Rhomboid.

In the former I was diſappointed by the weather; which from the time I went into the country, in the middle of May, till the end of June, when that conſtellation came to the meridian in the day-light, afforded me very few evenings fit for obſervation.

In the latter I failed, through the imperfection of my inſtrument, or my own want of ſkill in the uſe of it; for though a ſingle ſet of obſervations in any one evening would appear very good, yet when reduced by calculation, and confronted with other repeated trials, they never gave me the ſatisfaction I wiſhed.

The rhombus (for a rhombus, and not a rhomboid, it ought moſt properly to be called) is very good in theory; but very difficult to get executed with preciſion, and liable to ſome inaccuracy in the obſervation. The truth of it depends upon the longer diagonal being exactly twice the length of the ſhorter one; which requires an aukward angle ($53° 7' 48''$) at the vertex, not eaſily to be hit by the workmen, and therefore ſeldom ſufficiently true. Beſide this, as the ſides of the rhombus, on which depends the calculation for differences of declination, are but $26° 33' 54''$ declining from the perpendicular or horary wire, a very ſmall error in obſerving the paſſage of a ſtar makes a very material difference in the reſult.

This determined me upon making trial of a ſquare placed angularly (an addition to M. CASSINI's wires at 45°, as may be ſeen in tab. XII. fig. 1.) which ſeems to anſwer better. I muſt confeſs I have not yet had opportunity for trying it ſo completely as I could wiſh: but I was unwilling to let this

year

year slip by, without making it known; since, I think, from what I have done with it, I may be confident of its utility*.

The *properties* and *advantages* of such a system of wires scarcely need to be pointed out to astronomers. The whole extent of the field is employed as it is in the rhombus (the want of which was said to be Dr. BRADLEY's objection to M. CASSINI's wires); but being formed of right angles or half-right angles, to which workmen are most accustomed, they will always be apt to execute their part better; and the obliques, from the differences being just double to what they are in the rhombus, give the comparative declinations with twice the certainty. To this the number of corresponding observations in the passage of every star add considerably; since you may calculate its distance from the center C, from the angle D or E, or from one of the intermediate angles K, as you shall see occasion. The same indeed you may do in the rhombus from D or from E; or, if the rhombus be formed of wires, from the angle at L, fig. 2.; but only with half the precision. The result of a single passage of any one star (excepting towards the extremities of the field) gives the extent of the field equally in each, provided the declination of the star be known, by deducing its distance from those several angles; and such deductions serve as a still farther check upon every observation; be-

* What is here offered is by no means to be understood as recommending any system of wires in preference to actual measurement with a micrometer, but to render the use of them as convenient as may be to such gentlemen as are not provided with better instruments. The equatorial micrometer with a large field (such as I have seen at Mr. AUBERT's, of Mr. SMEATON's construction) I take to be the best instrument for taking differences of right ascension and declination out of the meridian; and far superior to any system of fixed wires, or indeed to any equatorial sector whatever.

cause,

cause, if any part of it be thought doubtful, its tallying or not tallying with the known extent of the field will shew whether there be any error, or where it lies. And, in each of them, the parallel wires will tell you whether the placing of your instrument be true or faulty; because, if truly made and truly set, the same star must take the same time in passing from one wire to its corresponding parallel; which will differ considerably, and in every star the same way, if the position be faulty.

Some of these latter remarks might have been spared, but that they may serve as hints to such gentlemen as may be inclined to lend their assistance to what was proposed the last year, and who may not have considered the many helps to be derived from a cross examination of the observations they make. For their use also it may be proper to add, what indeed is nothing new, that if the position of the instrument be found erroneous, the formula given by M. DE LA LANDE in his Astronomy will serve to rectify the observation. Calling the larger interval between the passage of any oblique and the horary wire m, and the smaller one n, $\frac{m^2n+n^2m}{m^2+n^2}$ will give the difference of declination (in time, to be converted into degrees, and multiplied by the cosine of declination) from the angle where that oblique meets the horary; and $\frac{m^2n-n^2m}{m^2+n^2}$ the difference in right ascension from the same angle. It must surely be almost needless to mention, that where the position is true, *half* the interval of time between a star's passing any two corresponding obliques, converted into degrees, and multiplied by the cosine of declination, will give the difference in declination of that star from the angle where those obliques meet, as the *whole* interval does in the rhombus.

But

But it may, perhaps, be of service to astronomy, or at least not unacceptable to those gentlemen who use the Rhombus, that I should subjoin another formula (contrived for me the last summer by my Son, now Mathematical Lecturer at Sidney College, Cambridge) for investigating the comparative right ascensions and declinations of stars observed by it, when the instrument is not placed truly in the plane of the equator. I was led into wishing for some such formula, in consequence of an ingenious Paper, kindly communicated to me by Sir H. C. ENGLEFIELD, Bart. F. R. S. giving an account of his method of doing it by a scale and figure; which, though very easy when one is provided with such a scale, appeared to me to be of less general use than by calculation; and I do not know that any thing of the kind is to be met with in any publication.

Let the angle DLL, fig. 2. (which, by construction, is 63° 26′ 6″) be called - - - - - a

The diagonal LL (whose extent, that is, what portion of a great circle it comprehends, must be known to the observer) be called - - - - - b

The larger interval observed between the passage of a star by an oblique and the horary wire (as bc) - - m

The smaller ditto of the same star (as cd) - n

The larger ditto of another star (as $\beta\gamma$) - - μ

The smaller ditto (as $\gamma\delta$) - - - ν

Then $\frac{2 \cdot \overline{m-n}}{m+n}$ = tangent of the angle which LL makes with a parallel of declination: call this q

The angle q being thus found, then

$\frac{2 \cdot \overline{n\omega} \cdot \times \text{sin.} \overline{a+q}}{R \times \text{sin.}\, a} \times \text{cos.}\, q$. = difference in declination between the two points on the vertical wire where those

those stars pass it. N. B. This being in *time* must be converted into *degrees*, and multiplied by cosine of declination as usual, to give the true difference in declination between the stars.

And the same expression, *viz.*

$\frac{2 \cdot \overline{n \backsim \nu} \times \text{sin.}\, \overline{a+q}}{R \times \text{sin.}\, a} \times \text{sin.}\, q =$ the difference in ÆR between those two points; to be applied as a correction to the observed times.

The same may be done by the larger intervals m and μ, only by substituting $\overline{a-q}$ in the place of $\overline{a+q}$, thus:

$\frac{2 \cdot \overline{m \backsim \mu} \times \text{sin.}\, \overline{a-q}}{R \times \text{sin.}\, a} \times \text{cos.}\, q =$ difference in declination as above;

or . . . . . $\times \text{sin.}\, q =$ ascensional difference.

If the stars differ too much in declination to come within the expression above (as N° 2. and 3.) then the differences of the angles D and E in declination and right ascension may be found thus:

$\frac{2 \cdot b \times \text{cos.}\, q}{R} =$ difference in declination between D and E;

$\frac{2 \cdot b \times \text{sin.}\, q}{R} =$ their ascensional difference;

and the difference of each star from its respectively nearest angle of the rhombus, may be deduced by the former expression, leaving out the consideration of the other star, thus:

$\frac{2 \cdot n \times \text{sin.}\, \overline{a+q}}{R \times \text{sin.}\, a} \times \text{cos.}\, q =$ difference of the star in declination from its nearest angle.

and . . . $\times \text{sin.}\, q =$ its difference in right ascension.

The application of these formulæ is very easy: for having o und q, if you set down its cosine in one column for declination,

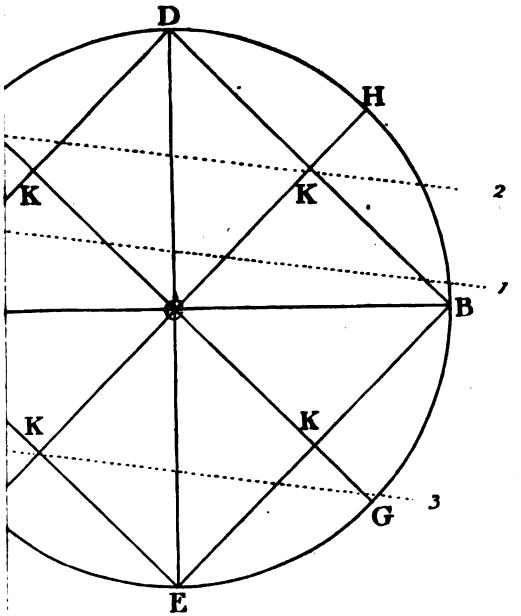
tion, and its fine in another column for right ascension, and under each the constant sin. $\overline{a+q}$, and the arithmetical compl. of sin. a; these being added together will make two sums, for the comparative observations of every star which may pass your field; and, unless your field be very large, and the declination of the stars very great, if to the column for declination you add the cosine of declination of the center of your field, it will adapt itself to all the products.

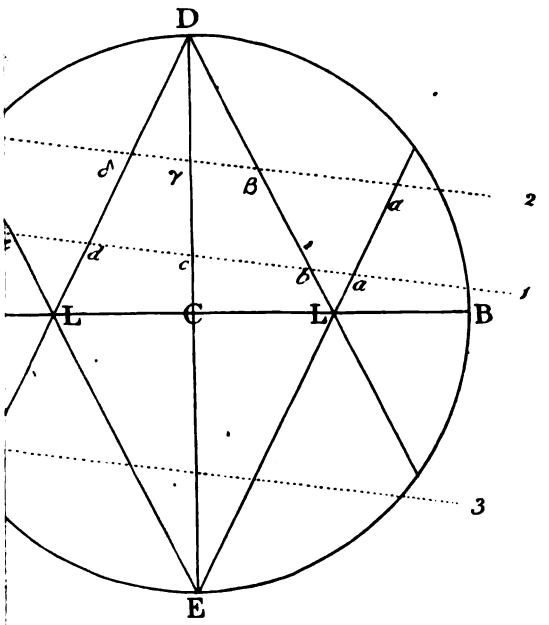
FRANCIS WOLLASTON.

Charter-house-Square,
March 15, 1785.

POSTSCRIPT.

SINCE the delivery of this Paper, it has occurred to me, that it may sometimes be convenient to know the angle of deviation from the true equatorial position in the new system of wires. This is to be deduced nearly in the same manner as in the rhombus; for $\frac{m-n}{m+n}$ = tang. q. By this angle any observed differences in right ascension may be corrected: for the difference in declination between any two stars (or their difference from the angle) multiplied by sin. q, will give the correction required.





XIX. *An Account of a Stag's Head and Horns, found at* Alport, *in the Parish of* Youlgreave, *in the County of* Derby. *In a Letter from the Rev.* Robert Barker, *B.D. to* John Jebb, *M.D. F.R.S.*

Read April 14, 1785.

ABOUT five years ago, some men working in a quarry of that kind of stone which in this part of the country we call tuft *, at about five or six feet below the surface, in a very solid part of the rock, met with several fragments of the horns and bones of one or different animals. Amongst the rest, out of a large piece of the rock, which they got entire, there appeared the tips of three or four horns, projecting a few inches from it, and the scapula of some animal adhering to the outside of it. A friend of mine, to whom the quarry belongs, sent the piece of the rock to me in the state they got it, in which I let it remain for some time. But suspecting that they might be tips of the horns of some head enclosed in the lump, I determined to gratify my curiosity by clearing away the stone from the horns. On doing which I found that the lump contained a very large stag's head, with two antlers upon each horn, in very perfect preservation, inclosed in it.

* Tuft is a stone formed by the deposit left by water passing through beds of sticks, roots, vegetables, &c. of which there is a large stratum at Matlock Bath, in this county.

Though the horns are ſo much larger than thoſe of any ſtag I have ever ſeen, yet, from the ſutures in the ſkull appearing very diſtinct in it, one would ſuppoſe that it was not the head of a very old animal. I have one of the horns nearly entire, and the greateſt part of the other, but ſo broken in the getting out of the rock, that one part will not join to the other, as the parts of the other horn do. The horns are of that ſpecies which park-keepers in this part of the country call throſtle-neſt horns, from the peculiar formation of the upper part of them, which is branched out into a number of ſhort antlers which form an hollow about large enough to contain a thruſh's neſt. I ſend you the dimenſions of the different parts of them, compared with the horns of the ſame ſpecies of a large ſtag, which have probably hung in the place from whence I procured them two or three or perhaps more centuries; and with another pair of horns of a different kind, which are terminated by one ſingle pointed antler, and which were the horns of a ſeven-year-old ſtag.

The river Larkell runs down the valley, and part of it falls into the quarry where theſe horns were found, the water of which has not the property of incruſting any bodies it paſſes through. It is therefore probable, that the animal to which theſe horns belonged was waſhed into the place where they were found, at the time of ſome of thoſe convulſions which contributed to raiſe this part of the iſland out of the ſea. Beſides this complete head, I have ſeveral pieces of horns, bones (particularly the ſcapula I mentioned above), and ſeveral vertebræ of the back, found in the ſame quarry; ſome, if not all, of them probably belonging to the animal whoſe head is in my poſſeſſion.

Dimen-

Dimensions of the horns found at Alport.

	Ft.	In.
Circumference at their insertion into the corona,	0	$9\frac{7}{8}$
Length of the lowest antler,	1	2
Length of second ditto,	0	$11\frac{1}{2}$
Length of third ditto,	1	$1\frac{1}{2}$
Length of the horn,	3	$3\frac{1}{2}$

Dimensions of a large pair of throstle-nest horns.

Circumference at their insertion into the corona,	0	7
Length of the lowest antler,	1	0
Length of second ditto,	0	$10\frac{1}{8}$
Length of third ditto,	0	$11\frac{1}{2}$
Length of the horn,	2	$7\frac{1}{2}$

Dimensions of the horns of a stag seven years old.

Circumference at their insertion into the corona,	0	$5\frac{1}{2}$
Length of the lowest antler,	0	9
Length of second ditto,	0	10
Length of third ditto,	0	10
Length of the horn,	2	$8\frac{3}{4}$

Youlgreave, Jan. 23, 1785.

XX. *An Account of the sensitive Quality of the Tree* Averrhoa Carambola. *In a Letter from* Robert Bruce, *M.D. to Sir* Joseph Banks, *Bart. P.R.S.*

Read April 14, 1785.

THE Averrhoa Carambola of LINNÆUS, a tree called in Bengal the Camruc or Camrunga, is possessed of a power somewhat similar to those species of Mimosa which are termed sensitive plants; its leaves, on being touched, move very perceptibly.

In the Mimosa the moving faculty extends to the branches; but, from the hardness of the wood, this cannot be expected in the Camrunga. The leaves are alternately pinnated, with an odd one; and in their most common position in the day-time are horizontal, or on the same plane with the branch from which they come out. On being touched, they move themselves downwards, frequently in so great a degree that the two opposite almost touch one another by their under sides, and the young ones sometimes either come into contact or even pass each other.

The whole of the leaves of one pinna move by striking the branch with the nail of the finger, or other hard substance; or each leaf can be moved singly, by making an impression that shall not extend beyond that leaf. In this way, the leaves of one side of the pinna may be made to move, one after another, whilst the opposite continue as they were; or you may make them

them move alternately, or, in short, in any order you please, by touching in a proper manner the leaf you wish to put in motion. But if the impression, although made on a single leaf, be strong, all the leaves on that pinna, and sometimes on the neighbouring ones, will be affected by it.

What at first seemed surprising was, that notwithstanding this apparent sensibility of the leaf, I could with a pair of sharp scissars make large incisions in it, without occasioning the smallest motion; nay, even cut it almost entirely off, and the remaining part still continue unmoved; and that then, by touching the wounded leaf with the finger or point of the scissars, motion would take place as if no injury had been offered. But, on further examination, I found, that although the leaf was the ostensible part which moved, it was in fact entirely passive, and that the petiolus was the seat both of sense and action: for although the leaf might be cut in pieces, or squeezed with great force, provided its direction was not changed, without any motion being occasioned; yet, if the impression on the leaf was made in such a way as to affect the petiolus, the motion took place. When, therefore, I wanted to confine the motion to a single leaf, I either touched it so as only to affect its own petiolus, or, without meddling with the leaf, touched the petiolus with any small-pointed body, as a pin or knife.

By compressing the universal petiolus near the place where a partial one comes out, the leaf moves in a few seconds, in the same manner as if you had touched the partial petiolus.

Whether the impression be made by puncture, percussion, or compression, the motion does not instantly follow; generally several seconds intervene, and then it is not by a jirk, but regular

regular and gradual. Afterwards, when the leaves return to their former ſituation, which is commonly in a quarter of an hour or leſs, it is in ſo ſlow a manner as to be almoſt imperceptible.

On ſticking a pin into the univerſal petiolus at its origin, the leaf next it, which is always on the outer ſide, moves firſt; then the firſt leaf on the oppoſite ſide, next the ſecond leaf on the outer, and ſo on. But this regular progreſſion ſeldom continues throughout; for the leaves on the outer ſide of the pinna ſeem to be affected both more quickly, and with more energy, than thoſe of the inner, ſo that the fourth leaf on the outer ſide frequently moves as ſoon as the third on the inner; and ſometimes a leaf, eſpecially on the inner ſide, does not move at all, whilſt thoſe above and below it are affected in their proper time. Sometimes the leaves at the extremity of the petiolus move ſooner than ſeveral others which were nearer the place where the pin was put in.

On making a compreſſion with a pair of pincers on the univerſal petiolus, between any two pair of leaves, thoſe above the compreſſed part, or nearer the extremity of the petiolus, move ſooner than thoſe under it, or nearer the origin; and frequently the motion will extend upwards to the extreme leaf, whilſt below it perhaps does not go farther than the neareſt pair.

If the leaves happen to be blown by the wind againſt one another, or againſt the branches, they are frequently put in motion; but when a branch is moved gently, either by the hand or the wind, without ſtriking againſt any thing, no motion of the leaves takes place.

When left to themſelves in the day-time, ſhaded from the ſun, wind, rain, or any diſturbing cauſe, the appearance of

the leaves is different from that of other pinnated plants. In the last a great uniformity subsists in the respective position of the leaves on the pinna; but here some will be seen on the horizontal plane, some raised above it, and others fallen under it; and in an hour or so, without any order or regularity, which I could observe, all these will have changed their respective positions. I have seen a leaf, which was high up, fall down; this it did as quickly as if a strong impression had been made on it, but there was no cause to be perceived.

Cutting the bark of the branch down to the wood, and even separating it about the space of half an inch all round, so as to stop all communication by the vessels of the bark, does not for the first day affect the leaves, either in their position or their aptitude for motion.

In a branch, which I cut through in such a manner as to leave it suspended only by a little of the bark no thicker than a thread, the leaves next day did not rise so high as the others; but they were green and fresh, and, on being touched, moved, but in a much less degree than formerly.

After sun-set the leaves go to sleep, first moving down so as to touch one another by their under sides; they therefore perform rather more extensive motion at night of themselves than they can be made to do in the day-time by external impressions. With a convex lens I have collected the rays of the sun on a leaf, so as to burn a hole in it, without occasioning any motion. But when the experiment is tried on the petiolus, the motion is as quick as if from strong percussion, although the rays were not so much concentrated as to cause pain when applied in the same degree on the back of the hand; nor had the texture of the petiolus been any ways changed by this; for next day it

could not be diſtinguiſhed, either by its appearance or moving power, from thoſe on which no experiment had been made.

The leaves move very faſt from the electrical ſhock, even although a very gentle one; but the ſtate of the atmoſphere was ſo unfavourable for experiments of this kind, that I could not purſue them ſo far as I wiſhed.

There are two other plants mentioned as ſpecies of this genus by LINNÆUS. The firſt, the Averrhoa Bilimbi, I have not had an opportunity of ſeeing. The other, or Averrhoa Acida, does not ſeem to belong to the ſame claſs; nor do its leaves poſſeſs any of the moving properties of the Carambola. LINNÆUS's generic deſcription of the Averrhoa, as of many other plants in this country which he had not an opportunity of ſeeing freſh, is not altogether accurate. The petals are connected by the lower part of the lamina, and in this way they fall off whilſt the ungues are quite diſtinct. The ſtamina are in five pairs, placed in the angles of the germen. Of each pair only one ſtamen is fertile, or furniſhed with an anthera. The filaments are curved, adapted to the ſhape of the germen. They may be preſſed down gently, ſo as to remain; and then, when moved a little upwards, riſe with a ſpring. The fertile are twice the length of thoſe deſtitute of antheræ.

Calcutta, Nov. 23, 1783.

XXI. *An Account of some Experiments on the Loss of Weight in Bodies on being melted or heated. In a Letter from* George Fordyce, *M.D. F.R.S. to Sir* Joseph Banks, *Bart. P.R.S.*

Read April 28, 1785.

SIR,

ALTHOUGH I have made many experiments on the subject of the loss of weight in bodies on being melted, or heated, I do not think it worth while to lay them all before the Society, as there has not appeared any circumstance of contradiction in them. I shall content myself with relating the following one, which appears to me conclusive in determining the loss of weight in ice when thawed into water, and subject to the least fallacy of any I have hitherto made, in shewing the loss of weight in ice on being heated.

The beam I made use of was so adjusted as that, with a weight between four and five ounces in each scale, $\frac{1}{1000}$ part of a grain made a difference of one division on the index. It was placed in a room, the heat of which was 37 degrees of FAHRENHEIT's thermometer, between one and two in the afternoon, and left till the whole apparatus and the brass weights acquired the same temperature.

A glass globe, of three inches diameter nearly, with an indentation at the bottom, and a tube at the top, weighing about 451 grains, had about 1700 grains of *New-River* water poured into it, and was hermetically sealed, so that the

whole, when perfectly clean, weighed 2150$\frac{3}{12}$ of a grain exactly; the heat being brought to 32 degrees, by placing it in a cooling mixture of salt and ice till it just began to freeze, and shaking the whole together.

After it was weighed it was again put into the freezing mixture, and let stand for about twenty minutes; it was then taken out of the mixture; part of the water was found to be frozen; and it was carefully wiped, first with a dry linen cloth, and afterwards with dry washed leather; and on putting it into the scale it was found to have gained about the $\frac{1}{60}$ part of a grain. This was repeated five times: at each time more of the water was frozen, and more weight gained. In the mean time the heat of the room and apparatus had sunk to the freezing point.

When the whole was frozen, it was carefully wiped and weighed, and found to have gained $\frac{3}{10}$ of a grain and four divisions of the index. Upon standing in the scale for about a minute, I found it began to lose weight, on which I immediately took it out, and placed it at a distance from the beam. I also immediately plunged a thermometer in the freezing mixture, and found the temperature 10 degrees; and on putting the ball of the thermometer in the hollow at the bottom of the glass vessel, it shewed 12 degrees. I left the whole for half an hour, and found the thermometer, applied to the hollow of the glass, at 32°. Every thing now being at the same temperature, I weighed the glass containing the ice, after wiping it carefully, and found it had lost $\frac{1}{3}$ and five divisions; so that it weighed $\frac{1}{10}$, all but one division, more than when the water was fluid.

I now melted the ice, excepting a very small quantity, and left the glass vessel exposed to the air in the temperature of 32 degrees

degrees for a quarter of an hour; the little bit of ice continued nearly the same. I now weighed it, after carefully wiping the glass, and found it heavier than the water was at first one division of the beam. Lastly, I took out the weights, and found the beam exactly balanced as before the experiment.

The acquisition of weight found on water's being converted into ice, may arise from an increase of the attraction of gravitation of the matter of the water; or from some substance imbibed through the glass, which is necessary to render the water solid.

Which of these positions is true may be determined, by forming a pendulum of water, and another of ice, of the same length, and in every other respect similar, and making them swing equal arcs. If they mark equal times, then certainly there is some matter added to the water. If the pendulum of ice is quicker in its vibrations, then the attraction of gravitation is increased. For there is no position more certain, than that a single particle of inanimate matter is perfectly incapable of putting itself in motion, or bringing itself to rest; and therefore, that a certain force applied to any mass of matter, so as to give it a certain velocity, will give half the quantity of matter double the velocity, and twice the quantity, half the velocity; and, generally, a velocity exactly in the inverse proportion to the quantity of matter. Now, if there be the same quantity of matter in water as there is in ice, and if the force of gravity in water be $\frac{1}{28000}$ part less than in ice, and the pendulum of ice swing seconds, the pendulum of water will lose $\frac{1}{28000}$ of a second in each vibration, or one second in 28000, which is almost three seconds a day, a quantity easily measured.

I ſhall juſt take notice of an opinion which has been adopted by ſome, that there is matter abſolutely light, or which repels inſtead of attracting other matter. I confeſs this appears abſurd to me; but the following experiment would prove or diſprove it. Suppoſing, for inſtance, that heat was a body, and abſolutely light, and that ice gained weight by loſing heat; then a pendulum of ice would ſwing through the ſame arc in $\frac{1}{14000}$ leſs time than a ſimilar pendulum of water; for the ſame power would not only act upon a leſs quantity of matter, but a counter-acting force would alſo be taken away.

Till the experiment of the pendulum can be made, or ſome other equally certain be ſuggeſted and made, it would be waſting time to enter into conjecture about the cauſe of the gain of weight in the converſion of water into ice in a glaſs veſſel hermetically ſealed.

I ſhall only obſerve, that heat certainly diminiſhes the attractions of coheſion, chemiſtry, magnetiſm, and electricity; and if it ſhould alſo turn out, that it diminiſhes the attraction of gravitation, I ſhould not heſitate to conſider heat as the quality of diminution of attraction, which would in that caſe account for all its effects.

We come, in the next place, to take notice of the ſecond part of the experiment, *viz.* that the ice gained an eighth part of a grain on being cooled to 12 degrees of FAHRENHEIT'S thermometer. In this caſe, a variation may ariſe from the contraction of the glaſs veſſel, and conſequent increaſe of ſpecific gravity in proportion to the air. But it is unneceſſary to obſerve, that this would be ſo very ſmall a quantity as not to be obſervable upon a beam adjuſted only to the degree of ſenſibility with which this experiment was tried. In the ſecond place, the air cooled by the ice above the ſcale becoming heavier than

the ſurrounding atmoſphere, would preſs upon the ſcale downward with the whole force of the difference. If a little more than half a pint of air was cooled over the ſcale to the heat of the ice and glaſs containing it, that is, twenty degrees below the freezing point, the difference, according to General Roy's table, would have been the eighth part of a grain, which was the weight acquired; but the air within half an inch of the glaſs veſſel being only one degree below the freezing point, I cannot conceive, that even an eighth part of a pint of air could be cooled over the ſcale to twenty degrees below the freezing point; nor that the whole difference of the weight of the air over the ſcale could ever amount to the 32d of a grain. I have, however, contrived an apparatus which is executing, in which this cauſe of fallacy will be totally removed. I ſhall, therefore, reſt at preſent the ſtate of this part of the ſubject; and leave it only proved, that water gains weight on being frozen.

I am, &c.

G. FORDYCE.

XXII. *Sketches and Descriptions of three simple Instruments for drawing Architecture and Machinery in Perspective.* By Mr. James Peacock; *communicated by* Robert Mylne, *Esq. F.R.S.*

Read March 17, 1785.

SOME of the following machines must be placed upon the front edge of the table upon which they are to stand.

The sights may be supported by a three-legged staff.

The stocks of the squares or indexes may have steel springs upon their edges, in order to keep them in any assigned part of the grooves in which they are to slide.

FIG. I. (TAB. XIII.)

ABCD a drawing board, to be fixed on a table or stand, &c. in a vertical position. AB a sliding-piece for the top of the T square, having a rebate therein to form a groove, as expressed by the dotted line. CD, sliding-piece for the bottom of the square, having a rebate therein to form a groove for the reception of the stock as described by the dotted line; this sliding-piece to be of sufficient length to receive and support the said stock when the blade of the square is coincident with the lines KNFH or LNGI. E a hole to receive the arm or slider of the sight-piece, to be constructed in the usual manner. FGHI an opening forming the field of view for the prototype. KLMN a sheet of paper fixed on the upper part of the board for the copy, the four inner lines whereof form and inclose a space of the

the ſame dimenſions as the field of view reſpectively. OP a ſteel ſliding-piece, equal in length to the diſtance KF or IN; at the lower end P is a ſteel arm terminating in a point; and at the upper end, at O, is a ſimilar arm, terminating with a braſs button, in the center of which is a ſharp ſteel pricker; the ſaid pricker and the point P are to be equi-diſtant from the edge of the blade of the ſquare: this arm O is to have the faculty of a ſpring, in order that the pricker may clear itſelf of the ſurface of the paper as ſoon as the finger quits the button, in the ſame manner as is uſual in the apparatus of large protractors. This ſliding ſteel-piece may be drawn out of the dove-tailed or rebated groove at pleaſure, and the T ſquare will then be fit for ordinary uſes.

To uſe the Inſtrument.

Having fixed the board truly level and perpendicular, and placed the point of ſight, or hole for viſion, at ſuch a height and diſtance as ſhall be productive of the beſt effect, move the ſquare with one hand, and the ſteel ſlider with the other, until the point P coincides with the eye and any point or angle in the original object. Preſs the pricker at O, and the puncture will be the true place, or copy, of ſuch original point or angle, &c.

N. B. All perpendicular lines may be drawn at once (in pencil), by bringing the left-hand edge of the ſquare to coincide with the original line and the eye; and their lengths may be very nearly determined by the graduated edge of the ſquare, ſo as to prevent confuſion from unneceſſary lengths of lines. The ſaid graduated edge will alſo give the points in all curved or irregular objects.

FIG. II. (TAB. XIV.)

As the instrument, fig. 1. proceeds chiefly by finding the positions of Points, this is contrived to find the positions of Lines, and to determine their limits by their reciprocal intersections.

ABOCDE is a compound board, to be placed in a vertical position. FGHI is the opening, or field of view. KLMN is a loose board, upon which paper is to be fixed; and the edges of the said board are to be rebated, as described in the plan at *zz*. XYMN and OPQR are grooved recesses, to receive the said loose board, as occasion may require. STUW is a moveable parallelogram, composed of a rebated stock SU, two like-graduated rulers ST and UW, and the regulating piece TW; the whole connected with screws, so as to move freely with a small force; and the distances between the centers of motion SU or TW to be equal to KF or HQ. AE and ED are rebated grooves, in which the stock of the parallelogram is to move.

To use the Instrument.

Having fixed the compound board ABOCDE truly vertical, slip the papered board KLMN into the recess XYMN, or OPQR, as the subject to be drawn may render first necessary, and slide the stock SU of the parallelogram into the groove AE, or ED, to correspond therewith; then, by moving the stock in the groove with one hand, and at the same time regulating the parallelogram with the other, the top edge of the ruler UW may be brought to coincide with any line in the original object, and the figured divisions on the edge of the ruler will at the same time determine the limits thereof, near enough to avoid a confusion of unnecessary lengths of line, &c. The true repre-

ſentation of the place and poſition of the line may be then drawn upon the paper, by the top edge of the ruler ST, a trifle longer at each extremity than it appears to be. This operation may be repeated for as many lines as can be obtained in the firſt poſition of the papered board and parallelogram; when they muſt be ſhifted into the other receſs and groove, to find the reſt, which may be now done without taking any further notice of the diviſions on the rulers.

N. B. A common T ſquare, applied to a board of this kind, will anſwer moſt purpoſes. For example: place the ſtock of ſuch a ſquare in one of the grooves, having a blade not leſs than the length HK or HR; mark the ſpaces HI and QR upon the upper edge thereof, and divide each of them into any convenient number of equal parts, and figure the ſaid parts in the uſual manner, to correſpond with each other, as may be ſeen in fig. 1. Now, ſuppoſe the ſtock of the ſquare to be in ED, it is plain, that all perpendicular lines may be drawn upon the paper KLMN in their proper places, and (by means of the diviſions on the edge of the ſquare) nearly of (though properly a trifle more than) their true length. All the lines of this deſcription being obtained, the ſhifting board muſt be placed in its other receſs, and the ſtock of the ſquare into the other groove; then, beginning with the firſt line, bring the edge of the ſquare to agree with its limits, and mark them off upon the line on the paper, and ſo of all the reſt in ſucceſſion; and join the points, where neceſſary, with a common ruler.

FIG. III. (TAB. XIV.)

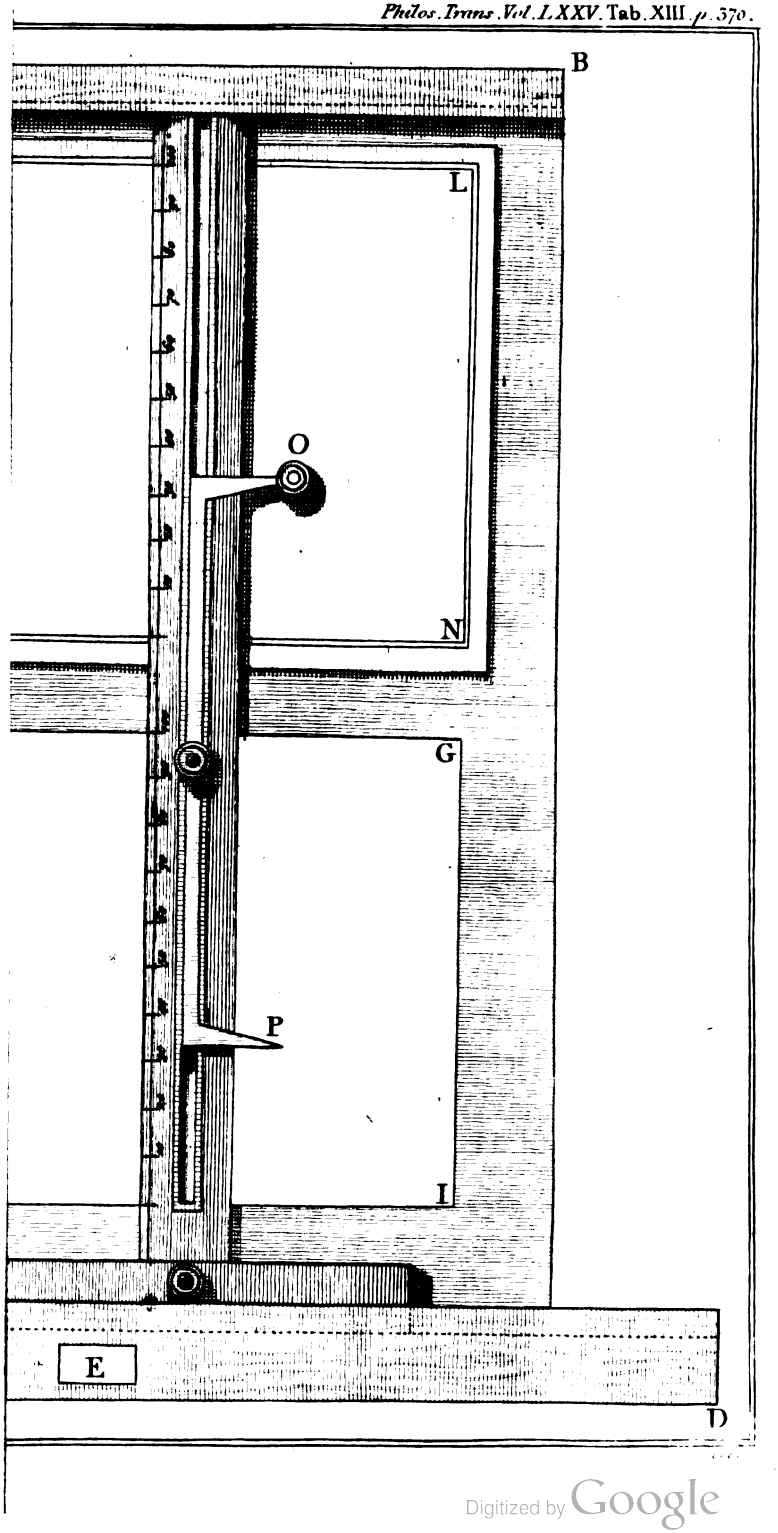
This apparatus is contrived to avoid the trouble of continually working againſt a board in a vertical poſition. In order

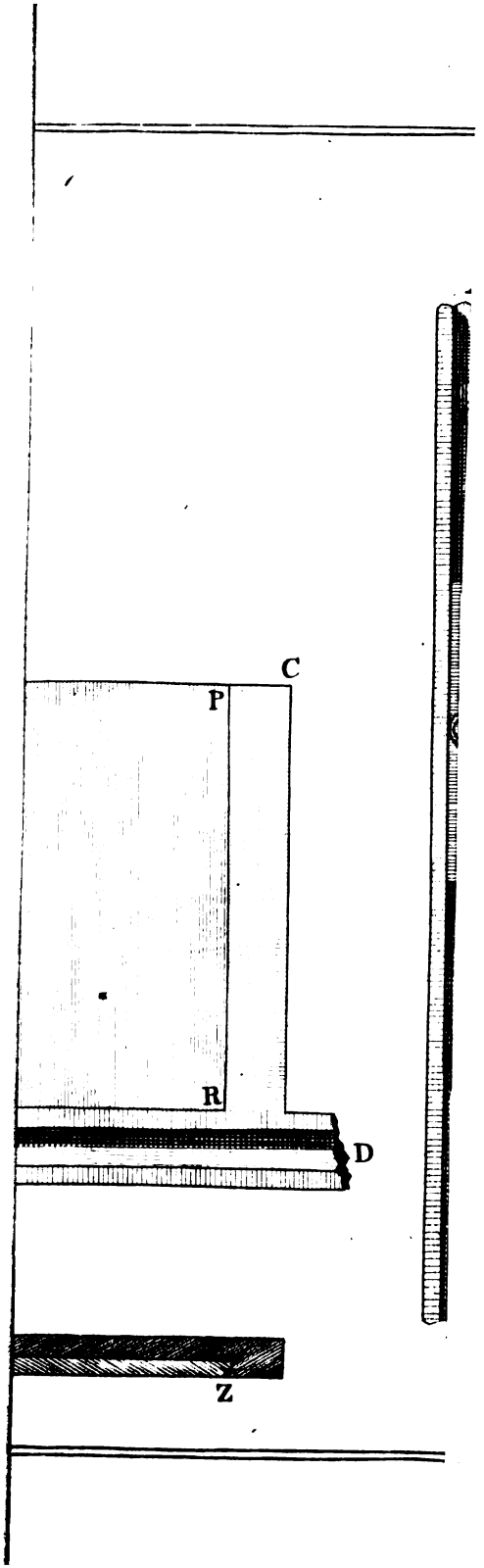
to this, two square boards are to be provided, equal in size, and of similar construction; one is to be fixed in a vertical position, for viewing the original object through a proper aperture; and the other is to be laid flat upon a desk or table, for the greater ease and conveniency of drawing the copy upon paper to be fixed thereon for that purpose.

ABCD is the vertical board; EFGH the opening therein, forming the field of view; IKL the T square, the blade thereof PL being moveable about the center P, with a moderate degree of stiffness; the stock K is to slide in a rebated or dove-tail groove AD, and be fixable to any part thereof by the screw O; the steel points MN are to move with moderate ease in a rebated or dove-tail brass groove in the middle of the blade of the square; upon the back of the groove AD are to be fixed two brass pins QQ, to rest in proper holes, similar to the holes marked RR; and the same kind of holes are to be made in the corner of the board whereon the copy is to be made.

To use the Machine.

Having placed the board ABCD in a truly vertical position, fix the shifting groove AD in the rebate, on the most convenient side of the board, by means of entering the pins Q into the holes R; then loosen the screw O, and move the stock IK, and at the same time turn the blade PL upon its center P, until one of its edges shall be coincident with some original line; then fix the stock by turning the screw O; move the points M and N, till they exactly include the apparent length of the said line; then take off the shifting groove AD, together with the T square or bevil fixed thereto, and apply the same to the





the correſponding ſide of the board on your deſk or table, and draw the line of its preciſe length and poſition.

N. B. If this is thought too operoſe, the braſs groove and ſliding pieces M, N, may be rejected, and the blade of the ſquare may be graduated on one or both of its edges at pleaſure; and all lines in the ſame direction may be drawn thereby exactly as to their poſitions, and nearly, though ſomewhat exceeding, their lengths *; and their preciſe lengths may be determined at the ſame time the lines in the contrary poſitions are drawn, whoſe lengths will be given at the ſame time by the lines firſt drawn.

* This will be effected, by noticing the numbers upon the blade, and taking thoſe next beyond the apparent limits of the line; and by this means the drawing will advance without the leaſt confuſion.

XXIII. *Experiments on Air.*

By Henry Cavendiſh, *Eſq. F.R.S. and A.S.*

Read June 2, 1785.

IN a Paper, printed in the laſt volume of the Philoſophical Tranſactions, in which I gave my reaſons for thinking that the diminution produced in atmoſpheric air by phlogiſtication is not owing to the generation of fixed air, I ſaid it ſeemed moſt likely, that the phlogiſtication of air by the electric ſpark was owing to the burning of ſome inflammable matter in the apparatus; and that the fixed air, ſuppoſed to be produced in that proceſs, was only ſeparated from that inflammable matter by the burning. At that time, having made no experiments on the ſubject myſelf, I was obliged to form my opinion from thoſe already publiſhed; but I now find, that though I was right in ſuppoſing the phlogiſtication of the air does not proceed from phlogiſton communicated to it by the electric ſpark, and that no part of the air is converted into fixed air; yet that the real cauſe of the diminution is very different from what I ſuſpected, and depends upon the converſion of phlogiſticated air into nitrous acid.

The apparatus uſed in making the experiments was as follows. The air through which the ſpark was intended to be paſſed, was confined in a glaſs tube M, bent to an angle, as in fig. 1. (tab. XV.) which, after being filled with quickſilver, was inverted into two glaſſes of the ſame fluid, as in the figure. The air to be

be tried was then introduced by means of a ſmall tube, ſuch as is uſed for thermometers, bent in the manner repreſented by ABC (fig. 2.) the bent end of which, after being previouſly filled with quickſilver, was introduced, as in the figure, under the glaſs DEF, inverted into water, and filled with the proper kind of air, the end C of the tube being kept ſtopped by the finger; then, on removing the finger from C, the quickſilver in the tube deſcended in the leg BC, and its place was ſupplied with air from the glaſs DEF. Having thus got the proper quantity of air into the tube ABC, it was held with the end C uppermoſt, and ſtopped with the finger; and the end A, made ſmaller for that purpoſe, being introduced into one end of the bent tube M, (fig. 1.) the air, on removing the finger from C, was forced into that tube by the preſſure of the quickſilver in the leg BC. By theſe means I was enabled to introduce the exact quantity I pleaſed of any kind of air into the tube M; and, by the ſame means, I could let up any quantity of ſoap-lees, or any other liquor which I wanted to be in contact with the air.

In one caſe, however, in which I wanted to introduce air into the tube many times in the ſame experiment, I uſed the apparatus repreſented in fig. 3. conſiſting of a tube AB of a ſmall bore, a ball C, and a tube DE of a larger bore. This apparatus was firſt filled with quickſilver; and then the ball C, and the tube AB, were filled with air, by introducing the end A under a glaſs inverted into water, which contained the proper kind of air, and drawing out the quickſilver from the leg ED by a ſyphon. After being thus furniſhed with air, the apparatus was weighed, and the end A introduced into one end of the tube M, and kept there during the experiment; the way of forcing air out of this apparatus into the tube being by thruſting

thrusting down the tube ED a wooden cylinder of such a size as almost to fill up the whole bore, and by occasionally pouring quicksilver into the same tube, to supply the place of that pushed into the ball C. After the experiment was finished, the apparatus was weighed again, which shewed exactly how much air had been forced into the tube M during the whole experiment; it being equal in bulk to a quantity of quicksilver, whose weight was equal to the increase of weight of the apparatus.

The bore of the tube M used in most of the following experiments, was about one-tenth of an inch; and the length of the column of air, occupying the upper part of the tube, was in general from 1½ to ¾ of an inch.

It is scarcely necessary to inform any one used to electrical experiments, that in order to force an electrical spark through the tube, it was necessary, not to make a communication between the tube and the conductor, but to place an insulated ball at such a distance from the conductor as to receive a spark from it, and to make a communication between that ball and the quicksilver in one of the glasses, while the quicksilver in the other glass communicated with the ground.

I now proceed to the experiments.

When the electric spark was made to pass through common air, included between short columns of a solution of litmus, the solution acquired a red colour, and the air was diminished, conformably to what was observed by Dr. PRIESTLEY.

When lime-water was used instead of the solution of litmus, and the spark was continued till the air could be no further diminished, not the least cloud could be perceived in the lime-water; but the air was reduced to two-thirds of its original bulk; which is a greater diminution than it could have suffered by mere phlogistication, as that is very little more than one-fifth of the whole. The

The experiment was next repeated with ſome impure dephlogiſticated air. The air was very much diminiſhed, but without the leaſt cloud being produced in the lime-water. Neither was any cloud produced when fixed air was let up to it; but on the further addition of a little cauſtic volatile alkali, a brown ſediment was immediately perceived.

Hence we may conclude, that the lime-water was ſaturated by ſome acid formed during the operation; as in this caſe it is evident, that no earth could be precipitated by the fixed air alone, but that cauſtic volatile alkali, on being added, would abſorb the fixed air, and thus becoming mild, would immediately precipitate the earth; whereas, if the earth in the lime-water had not been ſaturated with an acid, it would have been precipitated by the fixed air. As to the brown colour of the ſediment, it moſt likely proceeded from ſome of the quickſilver having been diſſolved.

It muſt be obſerved, that if any fixed air, as well as acid, had been generated in theſe two experiments with the lime-water, a cloud muſt have been at firſt perceived in it, though that cloud would afterwards diſappear by the earth being re-diſſolved by the acid; for till the acid produced was ſufficient to diſſolve the whole of the earth, ſome of the remainder would be precipitated by the fixed air; ſo that we may ſafely conclude, that no fixed air was generated in the operation.

When the air is confined by ſoap-lees, the diminution proceeds rather faſter than when it is confined by lime-water; for which reaſon, as well as on account of their containing ſo much more alkaline matter in proportion to their bulk, ſoap-lees ſeemed better adapted for experiments deſigned to inveſtigate the nature of this acid, than lime-water. I accordingly made ſome experiments to determine what degree of purity the

air ſhould be of, in order to be diminiſhed moſt readily, and to the greateſt degree; and I found, that, when good dephlogiſticated air was uſed, the diminution was but ſmall; when perfectly phlogiſticated air was uſed, no ſenſible diminution took place; but when five parts of pure dephlogiſticated air were mixed with three parts of common air, almoſt the whole of the air was made to diſappear.

It muſt be conſidered, that common air conſiſts of one part of dephlogiſticated air, mixed with four of phlogiſticated; ſo that a mixture of five parts of pure dephlogiſticated air, and three of common air, is the ſame thing as a mixture of ſeven parts of dephlogiſticated air with three of phlogiſticated.

Having made theſe previous trials, I introduced into the tube a little ſoap-lees, and then let up ſome dephlogiſticated and common air, mixed in the above-mentioned proportions, which riſing to the top of the tube M, divided the ſoap-lees into its two legs. As faſt as the air was diminiſhed by the electric ſpark, I continued adding more of the ſame kind, till no further diminution took place: after which a little pure dephlogiſticated air, and after that a little common air, were added, in order to ſee whether the ceſſation of diminution was not owing to ſome imperfection in the proportion of the two kinds of air to each other; but without effect *. The ſoap-lees being then poured out of the tube, and ſeparated from the quick-

* From what follows it appears, that the reaſon why the air ceaſed to diminiſh, was, that as the ſoap-lees were then become neutralized, no alkali remained to abſorb the acid formed by the operation, and in conſequence ſcarce any air was turned into acid. The ſpark, however, was not continued long enough after the apparent ceſſation of diminution, to determine with certainty, whether it was only that the diminution went on remarkably ſlower than before, or that it was almoſt come to a ſtand, and could not have been carried much further, though I had perſiſted in paſſing the ſparks.

ſilver,

ſilver, ſeemed to be perfectly neutralized, as they did not at all diſcolour paper tinged with the juice of blue flowers. Being evaporated to dryneſs, they left a ſmall quantity of ſalt, which was evidently nitre, as appeared by the manner in which paper, impregnated with a ſolution of it, burned.

For more ſatisfaction, I tried this experiment over again on a larger ſcale. About five times the former quantity of ſoap-lees were now let up into a tube of a larger bore; and a mixture of dephlogiſticated and common air, in the ſame proportions as before, being introduced by the apparatus repreſented in fig. 3. the ſpark was continued till no more air could be made to diſappear. The liquor, when poured out of the tube, ſmelled evidently of phlogiſticated nitrous acid, and being evaporated to dryneſs, yielded $1\frac{4}{10}$ gr. of ſalt, which is pretty exactly equal in weight to the nitre which that quantity of ſoap-lees would have afforded if ſaturated with nitrous acid. This ſalt was found, by the manner in which paper dipped into a ſolution of it burned, to be true nitre. It appeared, by the teſt of *terra ponderoſa ſalita*, to contain not more vitriolic acid than the ſoap-lees themſelves contained, which was exceſſively little; and there is no reaſon to think that any other acid entered into it, except the nitrous.

A circumſtance, however, occurred, which at firſt ſeemed to ſhew, that this ſalt contained ſome marine acid; namely, an evident precipitation took place when a ſolution of ſilver was added to ſome of it diſſolved in water; though the ſoap-lees uſed in its formation were perfectly free from marine acid, and though, to prevent all danger of any precipitate being formed by an exceſs of alkali in it, ſome purified nitrous acid had been added to it, previous to the addition of the ſolution of ſilver. On conſideration, however, I ſuſpected, that this precipitation might ariſe from the nitrous acid in it being phlo-

gisticated; and therefore I tried whether nitre, much phlogisticated, would precipitate silver from its solution. For this purpose I exposed some nitre to the fire, in an earthen retort, till it had yielded a good deal of dephlogisticated air; and then, having dissolved it in water, and added to it some well purified spirit of nitre till it was sensibly acid, in order to be certain that the alkali did not predominate, I dropped into it some solution of silver, which immediately made a very copious precipitate. This solution, however, being deprived of some of its phlogiston by evaporation to dryness, and exposure for a few weeks to the air, lost the property of precipitating silver from its solution; a proof that this property depended only on its phlogistication, and not on its having absorbed sea-salt from the retort, or by any other means.

Hence it is certain, that nitre, when much phlogisticated, is capable of making a precipitate with a solution of silver; and therefore there is no reason to think, that the precipitate, which our salt occasioned with a solution of silver, proceeded from any other cause than that of its being phlogisticated; especially as it appeared by the smell, both on first taking it out of the tube, and on the addition of the spirit of nitre, previous to dropping in the solution of silver, that the acid in it was much phlogisticated. This property of phlogisticated nitre is worth the attention of chemists; as otherwise they may sometimes be led into mistakes, in investigating the presence of marine acid by a solution of silver.

In the above-mentioned Paper I said, that when nitre is detonated with charcoal, the acid is converted into phlogisticated air; that is, into a substance which, as far as I could perceive, possesses all the properties of the phlogisticated air of our atmosphere; from which I concluded, that phlogisticated air is

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nothing elſe than nitrous acid united to phlogiſton. According to this concluſion, phlogiſticated air ought to be reduced to nitrous acid by being deprived of its phlogiſton. But as dephlogiſticated air is only water deprived of phlogiſton, it is plain, that adding dephlogiſticated air to a body, is equivalent to depriving it of phlogiſton, and adding water to it; and therefore, phlogiſticated air ought alſo to be reduced to nitrous acid, by being made to unite to, or form a chemical combination with, dephlogiſticated air; only the acid formed this way will be more dilute, than if the phlogiſticated air was ſimply deprived of phlogiſton.

This being premiſed, we may ſafely conclude, that in the preſent experiments the phlogiſticated air was enabled, by means of the electrical ſpark, to unite to, or form a chemical combination with, the dephlogiſticated air, and was thereby reduced to nitrous acid, which united to the ſoap-lees, and formed a ſolution of nitre; for in theſe experiments thoſe two airs actually diſappeared, and nitrous acid was actually formed in their room; and as, moreover, it has juſt been ſhewn, from other circumſtances, that phlogiſticated air muſt form nitrous acid, when combined with dephlogiſticated air, the above-mentioned opinion ſeems to be ſufficiently eſtabliſhed. A further confirmation of it is, that, as far as I can perceive, no diminution of air is produced when the electric ſpark is paſſed either through pure dephlogiſticated air, or through perfectly phlogiſticated air; which indicates the neceſſity of a combination of theſe two airs to produce the acid. Moreover, it was found in the laſt experiment, that the quantity of nitre procured was the ſame that the ſoap-lees would have produced if ſaturated with nitrous acid; which ſhews, that the production of the nitre was not owing to any decompoſition of the ſoap-lees.

It

It may be worth remarking, that whereas in the detonation of nitre with inflammable ſubſtances, the acid unites to phlogiſton, and forms phlogiſticated air, in theſe experiments the reverſe of this proceſs was carried on; namely, the phlogiſticated air united to the dephlogiſticated air, which is equivalent to being deprived of its phlogiſton, and was reduced to nitrous acid.

In the above-mentioned Paper I alſo gave my reaſons for thinking, that the ſmall quantity of nitrous acid, produced by the exploſion of dephlogiſticated and inflammable air, proceeded from a portion of phlogiſticated air mixed with the dephlogiſticated, which I ſuppoſed was deprived of its phlogiſton, and turned into nitrous acid, by the action of the dephlogiſticated air on it, aſſiſted by the heat of the exploſion. This opinion, as muſt appear to every one, is confirmed in a remarkable manner by the foregoing experiments; as from them it is evident, that dephlogiſticated air is able to deprive phlogiſticated air of its phlogiſton, and reduce it into acid, when aſſiſted by the electric ſpark; and therefore it is not extraordinary that it ſhould do ſo, when aſſiſted by the heat of the exploſion.

The ſoap-lees uſed in the foregoing experiments were made from ſalt of tartar, prepared without nitre; and were of ſuch a ſtrength as to yield one-tenth of their weight of nitre when ſaturated with nitrous acid. The dephlogiſticated air alſo was prepared without nitre, that uſed in the firſt experiment with the ſoap-lees being procured from the black powder formed by the agitation of quickſilver mixed with lead*, and that uſed

* This air was as pure as any that can be procured by moſt proceſſes. I propoſe giving an account of the experiment, in which it was prepared, in a future Paper.

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in the latter from turbith mineral. In the first experiment, the quantity of soap-lees used was 35 measures, each of which was equal in bulk to one grain of quicksilver; and that of the air absorbed was 416 such measures of phlogisticated air, and 914 of dephlogisticated. In the second experiment, 178 measures of soap-lees were used, and they absorbed 1920 of phlogisticated air, and 4860 of dephlogisticated. It must be observed, however, that in both experiments some air remained in the tube uncondensed, whose degree of purity I had no way of trying; so that the proportion of each species of air absorbed is not known with much exactness.

As far as the experiments hitherto published extend, we scarcely know more of the nature of the phlogisticated part of our atmosphere, than that it is not diminished by lime-water, caustic alkalies, or nitrous air; that it is unfit to support fire, or maintain life in animals; and that its specific gravity is not much less than that of common air: so that, though the nitrous acid, by being united to phlogiston, is converted into air possessed of these properties, and consequently, though it was reasonable to suppose, that part at least of the phlogisticated air of the atmosphere consists of this acid united to phlogiston, yet it might fairly be doubted whether the whole is of this kind; or whether there are not in reality many different substances confounded together by us under the name of phlogisticated air. I therefore made an experiment to determine, whether the whole of a given portion of the phlogisticated air of the atmosphere could be reduced to nitrous acid, or whether there was not a part of a different nature from the rest, which would refuse to undergo that change. The foregoing experiments indeed in some measure decided this point, as much the greatest part of the air let up into the tube lost its elasticity; yet,

yet, as ſome remained unabſorbed, it did not appear for certain whether that was of the ſame nature as the reſt or not. For this purpoſe I diminiſhed a ſimilar mixture of dephlogiſticated and common air, in the ſame manner as before, till it was reduced to a ſmall part of its original bulk. I then, in order to decompound as much as I could of the phlogiſticated air which remained in the tube, added ſome dephlogiſticated air to it, and continued the ſpark till no further diminution took place. Having by theſe means condenſed as much as I could of the phlogiſticated air, I let up ſome ſolution of liver of ſulphur to abſorb the dephlogiſticated air; after which only a ſmall bubble of air remained unabſorbed, which certainly was not more than $\frac{1}{120}$ of the bulk of the phlogiſticated air let up into the tube; ſo that if there is any part of the phlogiſticated air of our atmoſphere which differs from the reſt, and cannot be reduced to nitrous acid, we may ſafely conclude, that it is not more than $\frac{1}{120}$ part of the whole.

The foregoing experiments ſhew, that the chief cauſe of the diminution which common air, or a mixture of common and dephlogiſticated air, ſuffers by the electric ſpark, is the converſion of the air into nitrous acid; but yet it ſeemed not unlikely, that when any liquor, containing inflammable matter, was in contact with the air in the tube, ſome of this matter might be burnt by the ſpark, and thereby diminiſh the air, as I ſuppoſed in the above-mentioned Paper to be the caſe. The beſt way which occurred to me of diſcovering whether this happened or not, was to paſs the ſpark through dephlogiſticated air, included between different liquors: for then, if the diminution proceeded ſolely from the converſion of air into nitrous acid, it is plain that, when the dephlogiſticated air was perfectly pure, no diminution would take place; but when it contained

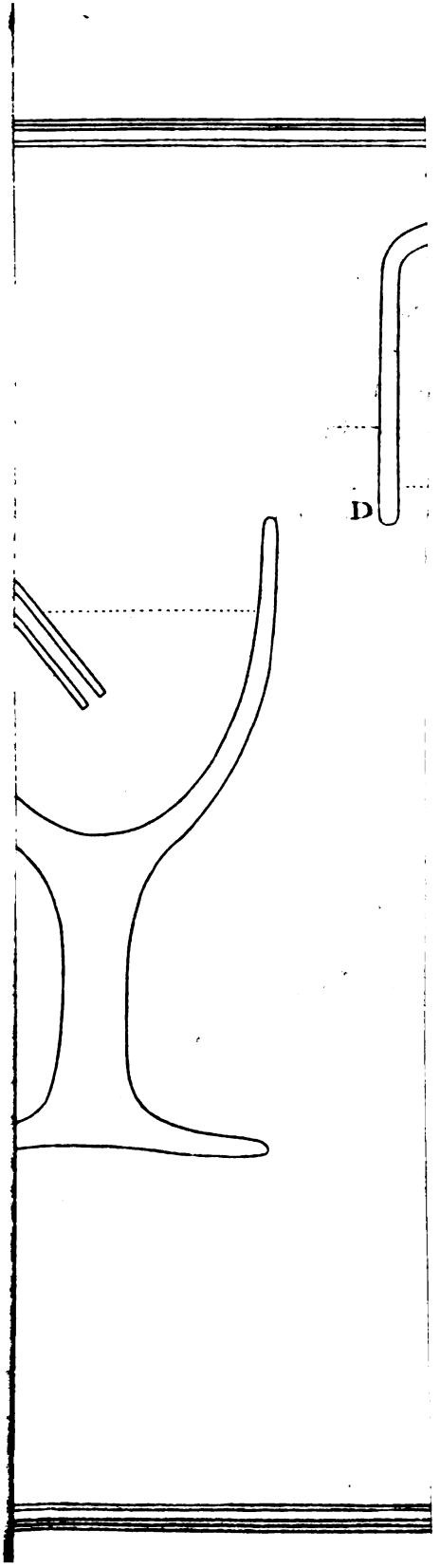
contained any phlogisticated air, all this phlogisticated air, joined to as much of the dephlogisticated air as must unite to it in order to reduce it into acid, that is, two or three times its bulk, would disappear, and no more; so that the whole diminution could not exceed three or four times the bulk of the phlogisticated air: whereas, if the diminution proceeded from the burning of the inflammable matter, the purer the dephlogisticated air was, the greater and quicker would be the diminution.

The result of the experiments was, that when dephlogisticated air, containing only $\frac{1}{20}$ of its bulk of phlogisticated air (that being the purest air I then had), was confined between short columns of soap-lees, and the spark passed through it till no further diminution could be perceived, the air lost $\frac{43}{200}$ of its bulk; which is not a greater diminution than might very likely proceed from the first-mentioned cause; as the dephlogisticated air might easily be mixed with a little common air while introducing into the tube.

When the same dephlogisticated air was confined between columns of distilled water, the diminution was rather greater than before, and a white powder was formed on the surface of the quicksilver beneath; the reason of which, in all probability, was, that the acid produced in the operation corroded the quicksilver, and formed the white powder; and that the nitrous air, produced by that corrosion, united to the dephlogisticated air, and caused a greater diminution than would otherwise have taken place.

When a solution of litmus was used, instead of distilled water, the solution soon acquired a red colour, which grew paler and paler as the spark was continued, till at last it became quite colourless and transparent. The air was diminished

by almoſt half, and I believe might have been ſtill further diminiſhed, had the ſpark been continued. When lime-water was let up into the tube, a cloud was formed, and the air was further diminiſhed by about one-fifth. The remaining air was good dephlogiſticated air. In this experiment, therefore, the litmus was, if not burnt, at leaſt decompounded, ſo as to loſe entirely its purple colour, and to yield fixed air; ſo that, though ſoap-lees cannot be decompounded by this proceſs, yet the ſolution of litmus can, and ſo very likely might the ſolutions of many other combuſtible ſubſtances. But there is nothing, in any of theſe experiments, which favours the opinion of the air being at all diminiſhed by means of phlogiſton communicated to it by the electric ſpark.



XXIV. *An Account of the Measurement of a Base on* Hounslow-Heath. *By Major-General* William Roy, *F.R.S. and A.S.*

Read from April 21 to June 16, 1785.

INTRODUCTION.

ACCURATE surveys of a country are universally admitted to be works of great public utility, as affording the surest foundation for almost every kind of internal improvement in time of peace, and the best means of forming judicious plans of defence against the invasions of an enemy in time of war, in which last circumstances their importance usually becomes the most apparent. Hence it happens, that if a country has not actually been surveyed, or is but little known, a state of warfare generally produces the first improvements in its geography: for in the various movements of armies in the field, especially if the theatre of war be extensive, each individual officer has repeated opportunities of contributing, according to his situation, more or less towards its perfection; and these observations being ultimately collected, a map is sent forth into the world, considerably improved indeed, but which, being still defective, points out the necessity of something more accurate being undertaken, when times and circumstances may favour the design.

The rise and progress of the rebellion which broke out in the Highlands of Scotland in 1745, and which was finally suppressed,

preſſed, by his Royal Highneſs the late Duke of Cumberland, at the battle of Culloden in the following year, convinced Government of what infinite importance it would be to the State, that a country, ſo very inacceſſible by nature, ſhould be thoroughly explored and laid open, by eſtabliſhing military poſts in its inmoſt receſſes, and carrying roads of communication to its remoteſt parts. With a view to the commencement of arrangements of this ſort, a body of infantry was encamped at Fort Auguſtus in 1747, under the command of the late Lord Blakeney, at that time a Major-General; at which camp my much reſpected friend, the late Lieutenant-General Watson, then Deputy Quarter-Maſter-General in North Britain, was officially employed. This officer, being himſelf an engineer, active and indefatigable, a zealous promoter of every uſeful undertaking, and the warm and ſteady friend of the induſtrious, firſt conceived the idea of making a map of the Highlands. As aſſiſtant Quarter-Maſter, it fell to my lot to begin, and afterwards to have a conſiderable ſhare in, the execution of that map; which being undertaken under the auſpices of the Duke of Cumberland, and meant at firſt to be confined to the Highlands only, was nevertheleſs at laſt extended to the Lowlands; and thus made general in what related to the mainland of Scotland, the iſlands (excepting ſome leſſer ones near the coaſt) not having been ſurveyed.

Although this work, which is ſtill in manuſcript, and in an unfiniſhed ſtate, poſſeſſes conſiderable merit, and perfectly anſwered the purpoſe for which it was originally intended; yet, having been carried on with inſtruments of the common, or even inferior kind, and the ſum annually allowed for it being inadequate to the execution of ſo great a deſign in the beſt manner, it is rather to be conſidered as a magnificent military

sketch, than a very accurate map of a country. It would, however, have been completed, and many of its imperfections no doubt remedied; but the breaking out of the war of 1755 prevented both, by furnishing service of other kinds for those who had been employed upon it.

On the conclusion of the peace of 1763, it came for the first time under the consideration of Government, to make a general survey of the whole island at the public cost. Towards the execution of this work, whereof the direction was to have been committed to my charge, the map of Scotland was to have been made subservient, by extending the great triangles quite to the northern extremity of the island, and filling them in from the original map. Thus that imperfect work would have been effectually completed, and the nation would have reaped the benefit of what had been already done, at a very moderate extra-expence.

It will not be expected, that I should here attempt to assign causes for the long delay that has taken place in carrying a work of so laudable a nature into execution: suffice it to say, that a period of twelve years having elapsed, since the scheme had been first proposed, as a work that could be best executed in time of profound peace, without any thing being done in it, previous to the nation's being unfortunately involved in the American war; it was sufficiently obvious, that peace must be once more restored, before any new effort could be made for that purpose. In the mean while, as I still entertained hopes that a work which seemed to merit the attention of the public, would, at some future period, be begun, and, by gradual perseverance, ultimately brought to perfection; therefore, in the course of my ordinary military employments, wherein the very best opportunities have offered of acquiring a thorough

thorough knowledge of the country, I have not failed to observe, at least in a general way, such situations as seemed to be the best adapted for the measurement of the bases that would be necessary for the formation of the great triangles, and connecting the different serieses of them together.

The peace of 1783 being concluded, and official business having detained me in or near town during the whole of that summer, I embraced the opportunity, for my own private amusement, to measure a base of 7744.3 feet, across the fields between the Jews-Harp, near Marybone, and Black-Lane, near Pancras; as a foundation for a series of triangles, carried on at the same time, for determining the relative situations of the most remarkable steeples, and other places, in and about the Capital, with regard to each other, and the Royal Observatory at Greenwich. The principal object I had here in view (besides that it might possibly serve as a hint to the public, for the revival of the now almost forgotten scheme of 1763) was, to facilitate the comparison of the observations, made by the lovers of astronomy, within the limits of the projected survey; namely, Richmond and Harrow, on the west; and Shooter's-Hill and Wansted, on the east: and thinking, that a Paper, containing the result of these trigonometrical operations, might not prove unacceptable to the Royal Society, I was engaged in making the computations for that purpose, when, very unexpectedly, I found, that an operation of the same nature, but much more important in its object, was really in agitation. This I saw would supersede, at least for the present, my own private observations, and perhaps render them wholly useless, unless it were as a matter of mere curiosity hereafter, to see how far such as depended on a short base, and a small instrument (a quadrant of a foot radius) would agree with

 those

those founded on a much longer base, and angles determined by a large circular instrument, being that proposed, as the best that could be made use of in the operation now to be mentioned.

In the beginning of October, 1783, Comte D'ADHEMAR, the French Ambassador, transmitted to Mr. FOX, then one of his Majesty's principal Secretaries of State, a Memoir of M. CASSINI DE THURY, in which he sets forth the great advantage that would accrue to astronomy, by carrying a series of triangles from the neighbourhood of London to Dover, there to be connected with those already executed in France, by which combined operations the relative situations of the two most famous observatories in Europe, Greenwich and Paris, would be more accurately ascertained than they are at present *.

This Memoir the Secretary of State, by his Majesty's command, transmitted to Sir JOSEPH BANKS, the very respectable and worthy President of the Royal Society; who, about the middle of November, was pleased to communicate it to me, proposing at the same time, that I should, on the part of the Society, charge myself with the execution of the operation. To this proposition I readily assented, on being soon afterwards assured, through the proper official channels, that my undertaking it met with his Majesty's most gracious approbation.

A generous and beneficent Monarch, whose knowledge and love of the sciences are sufficiently evinced by the protection which HE constantly affords them, and under whose auspices they are seen daily to flourish, soon supplied the funds that were judged necessary. What his Majesty has been pleased to

* M. CASSINI's Memoir, with the Astronomer Royal's remarks on what is therein alledged, concerning the uncertainty of the relative situations of the two Observatories, will be given in the sequel.

give

give so liberally, it is our duty to manage with proper and becoming frugality, consistent with the best possible execution of the business to be done, so as to make it redound to the credit of the Nation in general, and of this Society in particular.

The operation, whereof we are now to give some account, being the first of the kind, on any extensive scale, ever undertaken in this country, naturally enough sub-divides itself into two parts. First, the choice and measurement of the base, with every possible care and attention, as the foundation of the work; secondly, the disposition of the triangles, whereby the base is to be connected with such parts of the coast of this island as are nearest to the coast of France, and the determination of their angles, by means of the best instrument that can be obtained for the purpose, from which the result or conclusion will be drawn. It is the first part only, as a subject of itself sufficiently distinct, that we are now to lay before the Society; it having been judged more advisable, to shew that no time has been lost in making reasonable progress, than to defer the account till the whole operation should be ultimately completed.

Choice of the Base. Tab. XVI.

Hounslow-Heath having always appeared to be one of the most eligible situations, for any general purpose of the sort now under consideration, because of its vicinity to the Capital and Royal Observatory at Greenwich, its great extent, and the extraordinary levelness of its surface, without any local obstructions whatever to render the measurement difficult; being likewise commodiously situated for any future operations of a similar nature, which his Majesty may please to order to be extended

extended from thence, in different directions, to the more remote parts of the island, it was proposed to Sir JOSEPH BANKS, that the local circumstances should be actually examined; so far, at least, as to enable us to form some judgement, of the best position of the line to be measured.

The 16th day of April, 1784, being accordingly fixed on for the purpose, and Mr. CAVENDISH and Dr. BLAGDEN accompanying the President on this occasion, we began our observations at a place called King's Arbour, at the north-west extremity of the Heath, between Cranford-Bridge and Longford; and having proceeded from thence through the narrow gorge, formed by Hanworth-Park and Hanworth-Farm, we finished at Hampton Poor-house, near the side of Bushy-Park, at the south-east extremity; the total distance, from the survey of Middlesex, being upwards of five miles.

On this inspection it was immediately perceived, that the first part of the operation, in order to facilitate the measurement, would be, the clearing from furze-bushes and ant-hills, a narrow tract along the heath, as soon as the ground should be sufficiently dry to permit the base to be accurately traced out thereon.

First tracing of the Base, and clearing of the Ground. Tab. XVI.

Chiefly with a view to the more effectual execution of the work, it was judged to be a right measure to obtain and employ soldiers, instead of country labourers, in tracing the base, clearing the ground, and assisting in the subsequent operations. For, at the same time that this was obviously the most frugal method, it was evident, that soldiers would be more attentive to orders than country labourers; and by encamping on the

ſpot would furniſh the neceſſary centinels, particularly during the night, for guarding ſuch parts of the apparatus, as it was foreſeen muſt remain carefully untouched, in the frequent interims of diſcontinuing and reſuming the work. Accordingly, a party of the 12th regiment of foot, conſiſting of a ſerjeant, corporal, and 10 men, were ordered to march from Windſor to Hounſlow-Heath, where they encamped on the 26th of May, cloſe by Hanworth Summer-houſe, to which ſpot the neceſſary tents, camp equipage, and entrenching tools, &c. had been previouſly ſent.

Whatever might have been the particular direction given to the baſe conſidered by its extremities, from conſulting the plan it will eaſily appear, that it muſt always neceſſarily lead through the narrow gorge of the Heath formed by Hanworth-Park and Hanworth-Farm. The firſt point therefore to be attended to, in tracing it out, was, that it might lead through this paſs, without interfering with certain ponds, or gravel-pits full of water, which are in it. Theſe were eaſily avoided by carrying the line pretty near to Hanworth Summer-houſe; and in directing the teleſcope from thence towards the ſouth-eaſt, it was accidentally found, that by leaving Hampton Poor-houſe a very little to the weſtward, or right, the line would coincide with a remarkable high ſpire, ſeen at the diſtance of eleven or twelve miles, and known afterwards to be Banſted-Church. As there could not be a better ſituated, or more conſpicuous object than this, therefore the firſt or ſouth-eaſt ſection of the baſe, comprehended between the Summer-houſe and the angle of the ſmall field adjoining to Hampton Poor-houſe, was immediately directed upon it; and the ſoldiers were the ſame day ſet to work to clear the tract, which, at a medium, was made from two to three yards in breadth. This operation

operation continued eight or ten days, owing to the lower part of the heath, between Wolsey-River and the Poor-house, being encumbered with brush-wood.

When the clearing of the first section was completed, the second, comprehended between the Summer-house and the great road leading from Staines to London, was traced out in the following manner. One of the pyramidal bell-tents (whereof two had been provided, one of twenty-five, and the other of fifteen feet in height) being placed at the station near the Summer-house, camp colours were then arranged from distance to distance, so as to be in a line with the bell-tent and Bansted spire. In like manner, the third section, comprehended between the Staines Road and King's Arbour, was traced out.

This first tracing of the base was done by means of a common telescope held in the hand only, that no time might be lost in employing the soldiers to smooth the tract which was to be measured; because the transit instrument (my own property, for which a portable stand had been for some time preparing) was not yet ready to be applied, as it afterwards was, in tracing out the base more accurately.

The camp still remained, where it was originally pitched, at the angle of Hanworth-Park, this being a very convenient position, with regard to the first and second sections; but being too remote from the third, that time might not be lost, and the men unnecessarily fatigued in marching backwards and forwards; therefore, one half of the party, under the command of the corporal, was detached to the northward, and quartered in the neighbouring villages, to clear the third section, while the serjeant, with the remainder, were occupied in smoothing the second. Owing to the extraordinary wetness of the season, this operation required more time than had been at first

imagined; not having been entirely finished before the first week of July. We shall therefore leave it going on, and in the mean time proceed to describe the instruments that were subsequently made use of in the first and second measurements.

Steel Chain. Tab. XVII.

One of the first instruments, which that able artist Mr. RAMSDEN had orders to prepare, was a steel chain, one hundred feet in length, the best that he could make. Not that it was intended, nor could it be supposed, that we should absolutely abide by the result that this chain should furnish us with, for the length of the base; but it was hoped, that an instrument of this sort might be made, which would measure distances much more accurately than any thing of that kind had ever done before: and it was considered as an object of some consequence, to endeavour to simplify, and render as easy as possible, the measurement of bases in future: an operation which, hitherto, has always been found to be tedious and troublesome, to which we may now further add, uncertain likewise, when done with rods of deal, as will appear from the account hereafter to be given.

The construction of the chain, which is on the principles of that of a watch, will be understood from the representation of some of its chief parts, to the full size, in tab. XVII. where the first, or zero-end link, is shewn both in plan and elevation, in the state in which it was originally applied to measurement on the surface of the ground. Each link consists of three principal parts; namely, a long plate; two short ones, half the thickness of the former, with circular holes near the

extremities of each; and two cast-steel pins, or axes, suited to the diameters of the holes, which serve to connect the adjoining links together. The holes in the short plates are made rough or jagged with a file; so that when they have embraced the ends of two adjoining long ones, and the pins have passed through all the holes, in rivetting their extremities, they are made perfectly fast, and as it were united to the short plates; while the embraced ends of the long ones turn freely round on the middle part of the pins.

At every tenth link the joint, just now described, has a position at right-angles to the former; that is to say, the short plates lie here horizontally, and the pins passing through them stand vertically. Thus, there being in the whole chain two hundred cast-steel pins, one hundred and eighty lie horizontally; and twenty, including the two by which the handles are attached, stand vertically. These cross-joints, which were chiefly intended that the chain might fold up in a smaller compass, by returning upon itself at every tenth link, are likewise useful in presenting a horizontal surface, to which small circular pieces of brass are screwed, with figures 1, 2, 3, &c. to 9, engraved on them, denoting the decimal parts of the length. Thus the middle cross-joint, or that which separates the 50th from the 51st link, is shewn in the Plate with the figure 5 upon it.

The chain, in its first construction (for we are now to point out some alterations that were afterwards made in it), was one hundred feet in length, including the two brass handles; in the extremity of each of which there was a semi-circular hole, of the same diameter with the steel arrows successively fixed in the ground, and serving to keep the account of the number of chains, when applied to common measurement. In this its

first

first mode of application it was soon discovered, as we shall have occasion to mention hereafter, how admirably the chain performed; and that, with some farther precautions, a still greater degree of exactness might be attained, by supporting it on stands, or even on planks, laid on, or but little removed from, the common surface of the earth. For this purpose, the two end-links were altered, each being now made equal to one foot, exclusive of the handles. By referring to tab. XVII. the nature of this alteration will be easily conceived. It consisted in screwing to the under side of the handles, very near the joints, two feather-edged pieces of brass *; the one denoting zero, and the other 100 feet. Over the dart at the first, a plummet with a fine silver wire being suspended, that wire, by a very simple apparatus, hereafter to be described, may be brought accurately to coincide with any point whatever of commencement: and at the second, a fine line with a knife, or other sharp instrument, being drawn on a piece of card placed there for the purpose, and changed as often as needful; or, as was likewise practised, and found to answer better, a line on a moveable slide of brass, attached to the top of the stand or plank, being brought to coincide with the feather-edge, and then fastened underneath; the extremity of the 100 feet is readily ascertained: and thus the measurement may be continued on with great accuracy to any distance at pleasure.

That the chain, in this its altered state, may still be advantageously applied to ordinary measurement on the surface of the earth, the pieces above described, having steady pins, and being fastened with screws, can be easily removed, and others,

* They were originally of brass, but are now of steel, that the edges by being harder might run less risk of being damaged.

exactly of the same length, substituted in their stead, with semi-circular holes (as represented in the Plate by dotted lines near the joint of the handle) to receive the steel arrows, then to be made use of in the manner already mentioned.

This most excellent chain seems not to have suffered any perceptible extension from the use that has hitherto been made of it. It is so accurately constructed, that when stretched out on the ground, as in common use, all the long plates lying vertically or edge-wise, if a person, laying hold of either end with both hands, gives it a flip or jerk, the motion is, in a few seconds, communicated to the other end, in a beautiful vertical serpentine line; when the person, holding that handle, receives a sudden shock, by the weight of the chain pulling him forcibly. The chain weighs about eighteen pounds, and when folded up is easily contained in a deal box, about fourteen inches long, eight inches broad, and the same in depth.

Deal Rods. Tab. XVIII.

The bases which have hitherto been measured in different countries, with the greatest appearance of care and exactness, have all, or for the most part, been done with deal rods of one kind or other, whose lengths being originally ascertained by means of some metal standard, were, in the subsequent applications of them, corrected by the same standard. Having thus had so many precedents, serving as examples to guide us in our choice, it was natural enough that we should pursue the same method in the measurement to be executed on Hounslow-Heath; taking, however, all imaginable care, that our rods should be made of the very best materials that could be procured;

cured; with this farther precaution, that by trussing them, they should be rendered perfectly inflexible, a circumstance not before attended to.

As some difficulty had been found in procuring well-seasoned Pine-wood of sufficient length, and perfectly free from knots, for the intended purpose; therefore Sir JOSEPH BANKS had early applied to the Admiralty for assistance in this respect; and forthwith obtained an order to be furnished with what we might have occasion for, from his Majesty's yard at Deptford, where an old New-England mast, and also one of Riga wood, were speedily cut up for our use.

New-England white Pine is lighter, less liable to warp, and less affected by moisture, than Riga red wood. But the New-England mast, when it came to be very minutely examined, was found to be too much wounded by shot-holes in some parts, or too much decayed or knotty in others, to afford us a sufficiency. This being the case we had recourse to the Riga wood, which was indeed extremely smooth and beautiful; and so perfectly straight-grained, that a fibre of it, when lifted up, might be drawn, like a thread, almost from one end to the other.

It had been in contemplation, to make the rods of twenty-five or thirty feet in length; and one of the former dimensions was actually constructed: but this being found to be rather too unwieldy, it was judged best to content ourselves with those of about twenty feet.

Different opinions have been entertained with regard to the best mode of applying rods in measurement; some contending that contacts, or that of butting the end of one rod against the end of the other, is the best; while others (with more probability of being

being right) are of opinion, that the adjustment by the coincidences of lines should have the preference. The first is undoubtedly the most expeditious method; but seems at the same time to be liable to this very objectionable circumstance, that the probable errors fall all one way: whereas, in the second method, although by far the most tedious, the errors of coincidence falling sometimes on one side, and sometimes on the other, they compensate for, or destroy, each other; and therefore no error is committed.

With the view of satisfying both parties, and in order to put the matter, if possible, out of doubt, it was judged proper to construct the rods in such a manner as to admit of both methods being tried, that we might adhere to that which should be found by experience to be the best.

Three measuring rods were accordingly ordered to be made, and also a standard rod, with which the former were from time to time to be compared. Their general construction will be better conceived from the plan and elevation, and other representations of their principal parts, in tab. XVIII. than by any description, however particular, conveyed in words. It will be sufficient to say, that the stems of the three measuring rods are each twenty feet three inches in length, reckoning from the extremities of the bell-metal tippings; very near two inches deep; and about 1¼ inch broad. Being trussed laterally and vertically, they are thereby rendered perfectly, or at least as to sense, inflexible. The standard rod could only be trussed laterally; and it is justly represented by the plan of the other rods, excepting that its stem is something stronger, and that it has two or three inches at each end of extra-length, the reasons for which differences will appear hereafter.

By referring to the Plate it will be obſerved, that two narrow pieces of ivory, each faſtened with two ſmall ſcrews, are inlaid into the upper ſurface of the rods, within one inch and a half of the extremities of the tippings. Theſe ivory pieces received the fine black lines cut into them when the lengths of the rods were laid off, in the manner hereafter to be mentioned, and accurately determined the intermediate diſtance of 20 feet, or 240 inches, the meaſure to be uſed in the application by coincidences: whereas, in that by contacts, the ſpace comprehended between the extremities of the projecting lips of the tippings, is 243 inches.

Immediately behind each ivory piece, a cavity is formed underneath, in the middle of the ſtem. This receives a braſs wheel, about eight-tenths of an inch in diameter, whoſe axis turns in the fork of a braſs ſpring, five inches long, faſtened by a ſcrew to the under ſurface juſt before the croſs feet. Theſe ſprings are only of ſuch ſtrength as to permit the wheels to be forced up into the cavities by the weight of the rod, which, in its adjuſted ſtate, always reſts entirely on the ſurfaces of the two ſtands that ſupport its extremities. But when the rod is to be raiſed from the ſtands, then the milled-headed ſcrews, projecting above the upper ſurface, and ſtanding over the middle of the ſprings, being brought to act, the wheels are thereby preſſed downwards, and receive the full weight of the rod, which is then eaſily moved backwards or forwards to its true poſition, either of contact or coincidence.

The croſs-feet, placed about 5½ inches from the ends of the rods, and 1¾ inch from the inſertion of the truſſings, are each about nine inches long, 1¼ broad, and nearly an inch in depth, having their lower ſurfaces level with that of the ſtem. By

means of these, the rods are not only kept more steady on the stands, against the common action of the wind upon the trussings; but they likewise serve as holds for the vertical and horizontal brass clamps, whereby the rods are made fast to the stands on one side or other, and in both modes of application, contacts and coincidences; as will be more fully explained hereafter, in describing the tops of the stands.

Brass Standard Scale, and method of laying off the lengths of the Deal Rods.

At the sale of the instruments of the late ingenious optician Mr. JAMES SHORT, I purchased a finely divided brass scale, of the length of 42 inches, with a VERNIER's division of 100 at one end, and one of 50 at the other, whereby the 1000th part of an inch is very perceptible. It was originally the property of the late Mr. GRAHAM, the celebrated Watch-maker; has the name of JONATHAN SISSON engraved upon it; but is known to have been divided by the late Mr. BIRD, who then worked with SISSON.

It is sufficiently well known to this Society, that their brass standard scale, about 42 inches long, which contains on it the length of the standard yard from the Tower, that from the Exchequer, and also the French half-toise, together with the duplicate of the said scale, sent to Paris for the use of the Royal Academy of Sciences, were both made by Mr. JONATHAN SISSON, under Mr. GRAHAM's immediate direction. Now, although there seemed to be every reason to suppose, that the scale at present in my possession, originally Mr. GRA-

HAM's property, would correſpond with thoſe above-mentioned, which he had been directed by the Royal Sociey, with ſo much care and pains, to provide; yet, that nothing of this ſort might remain doubtful, it was judged right, in ſettling the abſolute length of the baſe, which I meaſured near London in 1783, as has been mentioned in the introduction to this Paper, that the two ſcales ſhould be actually compared. Having accordingly obtained an order from the Preſident, for admiſſion into the Society's Apartments, I went there in the afternoon of the 13th of Auguſt, and laid both ſcales taken out of their caſes on the table of the meeting-room, with thermometers along-ſide of them, that they might acquire the ſame temperature. On the forenoon of the 15th of Auguſt the compariſon was made, with the aſſiſtance of Mr. RAMSDEN, who for that purpoſe carried along with him his curious beam-compaſſes, whoſe micrometer-ſcrew ſhews very perceptibly a motion of $\frac{1}{5000}$th part of an inch. Thus the extent of three feet, being carefully taken from the Society's ſtandard, and applied to my ſcale, it was found to reach exactly to 36 inches, the temperature being 65°. In like manner, the beam-compaſſes being applied to the length of the Exchequer yard, the extent was now found by the micrometer to overreach that yard by $\frac{69}{10000}$th, or nearly $\frac{7}{1000}$th parts of an inch.

Having thus ſhewn that my ſcale is accurately of the ſame length with the Society's ſtandard, it remains to point out the uſe that was made of it, for aſcertaining the lengths of the deal rods, intended for the operation on Hounſlow-Heath. In the firſt place, Mr. RAMSDEN prepared a beam-compaſs, ſufficient to take in twenty feet, truſſed in all reſpects like the mea-

measuring rods, but something deeper, and fitted as usual with proper points and micrometer. The standard rod being now constructed was laid on the shop-board, strongly framed for the purpose, and nearly level. To one side of it, at the distance of about twenty feet two inches from center to center, two strong bell-metal cocks were firmly screwed. These cocks were about 2¼ inches in length, three-eighths in thickness, and rose above the stem nearly two inches, so as to be on the same plane with the surface of the measuring rods, when placed upon it.

A large plank, cut from the New-England mast, upwards of thirty feet long, nine or ten inches broad, and about three inches thick, being set edge-wise in the same room, on part of the stands now ready for the operation, was, in that position, planed perfectly smooth and straight. A silver wire being then stretched very tight, along the middle of the plank, from one end to the other, six spaces of forty inches each were marked off by the side of the wire, at which points seven brass pins, about one-tenth of an inch in diameter, were driven into the wood, and their tops polished with the stone. During the whole of this operation, and that which followed, the thermometer, lying by the side of the brass scale, continued steadily at or very near 63°.

A fine dot being now made on one of the extreme pins, and the silver wire being stretched over the dot, and as near as possible over the middle of the other pins, in which position it was made fast; the extent of forty inches, taken with the utmost care from the brass scale, was then marked off, by placing one point of the beam-compasses in the dot, and with the other describing a short faint arc on the surface of the se-

cond

cond pin. The beam being then removed, and one point placed in the interſection of the arc and wire, with the other point a dot was made on the third pin, under the middle of the wire. Upon this dot, as a center, a faint arc was next deſcribed on the ſame pin where the firſt had been traced. In this manner the ſix times forty inches were marked off, alternately with dots and arcs; a method found by Mr. RAMSDEN, in his practice, to be more accurate, than when dots only are made uſe of.

The exact length of twenty feet, thus obtained, was next taken between the points of the long beam-compaſſes, and transferred to the tops of the bell-metal cocks, placed, as has been already mentioned, on the ſide of the ſtandard rod, in ſuch manner as to leave more than one inch and a half of the ſaid cocks beyond or without the lines denoting the extent of the twenty feet. This being done, the meaſuring rods were ſucceſſively placed on the ſtandard, and their ſides applying cloſe to the cocks, the diſtance of twenty feet was readily transferred from them to the inlaid ivory pieces, on which fine lines were afterwards cut, by marks accurately made for that purpoſe.

With regard to the adjuſtment of the lips of the bell-metal tippings, which extend exactly one inch and a half beyond the ivory lines, ſo as to make the total length of the rod 243 inches, it is to be obſerved, that they terminate in flat curves of $3\frac{1}{2}$ inches radius, paſſing through the inch and half points, to which they were cautiouſly ground down, that at firſt they might rather exceed than be defective in length. Any two of the rods, lying in the ſame plane, and alſo in the ſame ſtraight line, being brought into contact with each other; if of

of the true length, the space in that position, comprehended between the two lines on the inlaid ivory pieces, must be exactly three inches. For the purpose of this adjustment, the extent of three inches was therefore taken from the brass scale and cut upon the side of a detached piece of ivory; which being readily applied to the aforesaid intermediate space, the same was gradually reduced, by grinding the lips equally, till it exactly corresponded with that taken from the scale.

The three rods are numbered by a cypher on the surface of the metal at each end, 1.2; 3.4; 5.6; and that being the order in which they were to be applied in actual measurement, so it was likewise the order in which they were adjusted; that is to say, the rod 1.2 was adjusted with 3.4, and with 5.6; and the rod 3.4 was, in like manner, adjusted with 1.2 and 5.6.

One of these deal rods, when finished, was found to weigh twenty-four pounds. They were intended to be contained in two chests, one large and the other smaller. The large chest, which is about 2½ feet deep, may be called a double one, because it has two lids that lift quite off, which, in turning upside down, become alternately top and bottom, having between them, but much nearer to the one than the other, a bottom that is common to both. The shallow side holds the standard rod; and the other, two of the measuring rods; which last is rendered practicable by having one of the side braces of each fixed only with screws, so as to be removed and replaced at pleasure. Thus one of the rods being laid in its place, the other is put over it in an inverted position; and both having the proper fastenings to keep them in their positions, the lid is then put on, and fixed by screws. The chest being now turned upside down, and the other lid removed, the standard is thereby

thereby discovered resting on the common bottom, which has bands laid across it for the purpose, a few inches below what has now become the surface of the chest. It was necessary that the standard should rest thus high, both that the light might come freely upon it, and that, being supported by the deep sides of the chest, it might be prevented from twisting, for it will be remembered that it is only trussed laterally. By means of a small brass spring fixed to each end of the standard, a fine silk thread, as being less liable to accident than silver wire, is stretched along its stem, which by small wedges prepared for the purpose, and slipped in between it and the bands on which it rests, is always brought into the same position. This being done, the silk thread is turned off, so as to permit the measuring rods to be laid on the standard for comparison. With regard to the smaller chest, such a one was actually made, and sent down to the heath, towards the close of the operation with the deal rods; but from some mistake in its dimensions, it would not admit the third rod.

Stands for the Measuring Rods. Tab. XVIII. and XIX.

From the extraordinary levelness of Hounslow-Heath, the ascent from the south-east towards the north-west being little more than one foot in a thousand in the distance of five miles, it was easily seen, that the computed base-line, or that actually forming a curve parallel to the surface of the sea, at that height above it, would fall so little short of the hypothenusal distance, measured on, or parallel to, the surface of the Heath, as scarcely to deserve notice, had it not been thought necessary to

shew, how much one end of the base was really higher than the other; and to convince the world, that in an operation of this sort, where so much accuracy was expected, no pains were spared, nor the most trivial circumstances neglected.

From the trouble and uncertainty attending the frequent use of plummets, especially in windy weather, instead of measuring level or base lines, as has hitherto been customary (in which case it would have been necessary to make use of the plummet, or some such contrivance, at every step of ascent or descent) it was judged to be a better method to measure hypothenuses, and, having obtained the relative heights of the stations by the accurate application of the telescopic spirit-level, to compute the base lines. Thus it was proposed, that the length of the base on Hounslow-Heath should be obtained by measuring a line through the air, drawn parallel to the common surface from station to station, in equal distances of 200 yards or 600 feet each, as represented in the figure at the top of tab. XVIII.

For this purpose, two kinds of stands were used; one whose height was fixed, to be placed at the beginning and end of each 200 yards; and the others, whose heights were moveable, that their surfaces might be brought more easily to coincide with the line passing through the air from one fixed stand to the other. The fixed stands in their first state, represented by that towards the left-hand in the plate for the deal rods, were only two feet seven inches in height; but when the glass rods were afterwards used, they had an additional piece of ten inches fastened to the top (as in the left-hand stand of tab. XIX.) which made their total height above the Heath, including the platform on which they stood, three feet and a half. They are tripods of white deal, whose legs extend about

three feet from each other; and being braced diagonally, are mortoised at top into circles of the same sort of wood. Over this circle, a square table of about 11½ inches is fixed, composed of oak, and mahogany at top; but both taken together do not exceed 1¼ inch in thickness.

The nature of the moveable stands, whereof there were at last no fewer than seventeen provided, will be comprehended from the representations of them towards the right-hand in tab. XVIII. and XIX. Their general construction, in what regards the part of them which is fixed, differs not from that of the others, excepting that they were of different heights, from two feet to about two feet eight inches, so as better to suit the irregularities of the ground where it might be necessary to place them. In the middle of each of these, an hexagonal wooden pipe descends, from the top to within two or three inches of the bottom, where it is joined by a brace reaching from each leg. This pipe receives the common cheese press wooden screw (having three sides screwed and three plane), to the top of which the square table is attached. It is embraced by the circular nut, or winch with four handles, whereby the table is elevated or depressed at pleasure; and being brought to its proper height, is there made perfectly fast by means of the flat-headed iron screw, which passing through one of the legs, presses an iron plate, fixed in the inside of the pipe, against one of the plane sides of the screw.

In describing the deal rods, there has already been occasion to make mention of the vertical and horizontal clamps, whereby the cross-feet are fastened to the table on the top of the stand. The nature of these tables will be best understood by consulting the two plans of them towards the right hand in tab. XVIII.; whereof one represents the two grooves fitted for the alternate reception

of the horizontal clamp, according to the side on which the rod lies that is to be moved on into coincidence; and the other shews it actually in its place, with the clamp itself detached in elevation along-side of it. Thus from the plan it may be perceived, that the first, or adjusted rod, lies towards the farther side of the table, and is there secured by the vertical clamp. The second, or moveable rod, lies on the hither side, and therefore the horizontal clamp is placed in the farther groove, where it is firmly pinched by the nut underneath. The rod has been brought to coincidence by working with the two milled-headed screws against the opposite sides of the cross-foot. This apparatus, although perfectly good in theory, was found to be much too confined in its nature to answer well in practice, requiring the stands to be placed with a degree of precision, which could not be effected in the field without great loss of time; and this was the real cause, as will be seen hereafter, that the measurement by coincidences with the deal rods was given up, and that by contacts adhered to.

Towards the left-hand of tab. XVIII. the plan of one of the square tables is represented with the ends of the second and third rods upon it in contact. In this operation it will be perceived, that only one cross-foot of each rod could now rest on and be clamped to the stand, the tables having been inadvertently cut too small to admit of both; and although this has the appearance of imperfection, yet no inconveniency whatever was found to result from it in practice, experience having shewn, that the clamping of either end sufficed to keep the rod steady. Along-side of the table, the vertical clamp, being that now solely made use of, is likewise represented in elevation.

On the face or exterior side of each leg of all the stands, fixed as well as moveable, a plate of brass is screwed near the bottom, with two holes in each, over a groove purposely made in the wood underneath. By means of these plates, parallelopid leaden weights, about fourteen pounds each, having brass pins with heads suited to enter the holes, and fall down in the grooves, into a narrow-pointed part of them, are readily slipped on or off each leg. Thus every stand, exclusive of its own weight, which is about thirty-one pounds, being loaded with forty-two pounds of lead, is thereby rendered perfectly firm and steady.

A number of wedges were also prepared, and always ready to be placed under the legs; by means of which, and a spirit level laid on the table, its plane is brought to the proper position.

Notwithstanding all these precautions, it having been found, in the measurement with the deal rods, that time was lost in levelling the stands, particularly in situations where the surface happened to be more than usually uneven, or where it was of a loose or spungy nature; therefore Mr. SMEATON advised (and no man's advice is more deserving of attention), that deal platforms, standing on pickets driven into the ground, and properly levelled, should be used to receive the legs of the stands. Accordingly, for the operation with the glass rods (table XIX.) twenty such triangular platforms made of inch deal, whose sides were each three feet two inches in length, and void in the middle, were provided; as also a number of beech-pickets, about an inch and a half square, and of different lengths, from seven to twelve or fourteen inches. Three of these pickets, short or long as the situation required, being driven into the ground, till their heads (by the carpenter's

levels were brought to the proper height, the platform was laid upon them; and on that the stand itself being placed, its position was ultimately corrected by the spirit level laid on the top of the table. Each of the beech pickets had a hole bored through its top, fit to receive a piece of strong tent-line, by which, and the help of one of the camp mallets, the pickets were easily pulled up again, when the platform was to be removed to a new situation.

Boning Telescope and Rods. Tab. XVIII.

In order to trace the line of 200 yards or 600 feet through the air, from one fixed stand to the other, it was usual, in the first place, to stretch a cord extremely tight along the ground, and to divide the space into rod lengths, by small wooden pins placed close by the cord, which remained there, and accordingly marked, very nearly, the points over which the centers of the intermediate stands were to come. A piece of wood, about fourteen inches in length, and one and a half in breadth, painted white, with a narrow black line along the middle of it, being prepared for the purpose, was laid on the surface of the farther stand. The boning telescope, fourteen inches long and one and a half in diameter, with a small magnifying power, and moveable object-glass, so as to fit it for very short distances, was then laid on the surface of the nearest stand; which, by means of wedges placed under the legs, had that side towards the farther stand so elevated or depressed, as to bring the cross wires to coincide with the black line on the painted board. Twenty-four boning rods had been originally provided; but it rarely happened, that more than eight or ten of

of that number were used in any one station. They are of clean deal, upwards of five feet in length, one inch square, and pointed with plate iron at the bottom, so as to be easily fixed into the ground. Each rod carries a cross vane, six or seven inches in length, and three-quarters of an inch in breadth. This cross vane, being moved upwards or downwards along the rod, till its upper surface coincided with the cross wires of the telescope and black line on the painted board, its under surface then marked the height to which the surface of the stand was to be brought at that particular place. In this manner, a certain number of points, in the line passing through the air from one fixed stand to the other, being accurately obtained, it was very easy, at all the intermediate places, by the application of the eye alone to the surface of any one stand or rod, to bring the surfaces of the other stands near it into the same plane.

Cap and Tripod for preserving the point upon the ground, where the measurement was discontinued at night, and resumed next morning. Tab. XVIII.

It has been already mentioned, and, in giving the account of the rough measurement with the chain, there will be farther occasion to remark, that the base was divided into hypothenuses of 200 yards or 600 feet each, where square pickets were driven into the ground, and regularly numbered, so as to be easily referred to on any occasion. In the measurement with the rods, it was customary to finish the day's work at or near one of these stations. When the rods of twenty feet were used, the termination of a rod was, of course, always found

found to be within a few inches of the picket corresponding with the hypothenuse, as determined by the chain. But with the rods of twenty feet three inches, the day's work was always ended with a fractional rod, by suspending a plummet from some convenient part of the stem, marked for the purpose, and which consequently became the point of commencement next morning.

The brass cup, made use of on these occasions, is of the figure of an inverted truncated cone, whose mean diameter is four inches, and its depth about five, with a very small inclination in the sides. It was placed in a hole dug for it in the earth, immediately under the point of suspension of the plummet, serving only to hold the water in which it vibrated.

The nature of the tripod will be best conceived from the plan and elevation of it in tab. XVIII. It consists of two strong pieces of beech wood, mortoised into each other, so as to resemble a half cross, or the letter T inverted, having three strong iron prongs, about twelve inches in length, which pass through the ends of the wood, and are fastened to it by square nuts at top. On the surface of the tripod lies a similar half cross of mahogany, moveable by means of grooves in the direction of the longest side, and fixable by its proper screws, when brought to the desired position. This mahogany half-cross carries on its surface a brass ruler, moveable at right-angles to the former direction, fixable also by means of its own screws, and on whose end is cut a very fine intersection. Thus any day's operation having been finished, the tripod was placed near the cup, with it longest side parallel to the line of measurement, and its prongs driven into the ground, so as to be rendered perfectly immoveable without great violence. The plummet being then suspended by a fine gilt wire, at any part of

of the ſtem of the deal rods indifferently, but always at the fixed* or hindermoſt end of the glaſs rods, the braſs ruler was advanced ſo near as almoſt to touch the wire, and there made faſt. This being done, the mahogany half-croſs was laſtly moved backwards or forwards, in the direction of the line of meaſurement, until the interſection, as ſeen by a perſon lying down on the ground for the purpoſe, accurately coincided with the gilt wire, where it was likewiſe faſtened by its proper ſcrews. A tent was then pitched very near the apparatus, for the ſoldiers who furniſhed the centinel for its ſecurity, till the meaſurement was reſumed; and particularly to guard it from being diſturbed by cattle during the night.

Wheels for terminating, in a permanent manner, the extremities of the Baſe. Tab. XVIII.

Before any accurate meaſurement could ultimately be made of the baſe by means of rods, in order that we might with certainty refer to the ſame point, on any occaſion that might ariſe of correction or repetition of the work, it had all along been foreſeen, that it would be abſolutely neceſſary to ſink deep into the ground wooden pipes, or ſuch like things, at the extremities of the baſe, which could not be removed, or even diſturbed, by idle or ignorant people, without very conſiderable labour. Mr. Mylne, F.R.S. was accordingly requeſted to

* That this might be conveniently done, a moveable ſtand was placed, under the glaſs rod, about four feet from the fixed end, and its table elevated till, by bearing againſt the lower part of the caſe, it received its weight. This permitted the ſtand under the fixed end to be lowered and removed, to make room for the apparatus.

order

order two such pipes to be provided, about six feet in length each, and one foot in diameter, with a bore of four inches in the uppermost end, for the depth of two feet, and cross-arms near the lowermost end, in the stile of the common warping posts. As an improvement on this idea, Mr. MYLNE very judiciously proposed that, instead of the cross-arms, the lower ends of the pipes should pass through the nave of an old coach-wheel, and then be secured by a bolt underneath. This alteration was approved of; and the machines, thus executed, were sent soon after by water to Hampton.

The plan and section of one of these wheels, with the dished side downwards, are represented towards the left-hand in tab. XVIII. where it will be perceived, that by means of four knee-pieces, made of crooked oak, the pipe is firmly bolted to the wheel, and thereby kept at right-angles to its plane. The top of the pipe is also secured exteriorly by an iron hoop, and has a cast-iron box driven into it, whose inner diameter is four inches, answering to that of the bore. Four oak piles for each wheel were prepared to be driven into the bottoms of the pits dug for their reception, which were six feet in diameter, and the same in depth. The soil near Hampton Poor-house being of a loose sandy nature, there the piles were easily driven into the bottom, until their tops were on the same level. The flat of the fellies of the wheel being then laid on the piles, the earth was filled in and well rammed around the pipe, quite up to the surface, with which its mouth is even. But the soil at King's Arbour, being a hard-bound gravel, the piles could not be driven into the bottom of that pit; wherefore, the flat of the wheel rests there on the gravel only.

The brass cup, formerly described, was from the first intended to be placed in the pipes, for which purpose it has two

lids; one a ſemi-circle, with the central point marked by a line cut on its diameter, brought into the direction of the baſe; with which line the gilt wire, ſuſpended at the extremity of the firſt rod, was made to coincide on the commencement of the meaſurement. The other lid has a very ſmall hole made in its center, through which the plummet wire is to paſs, when ſuſpended from the center of the inſtrument, hereafter to be made uſe of for the determination of the angles at the baſe, or in any other ſtation whatever, where it may be neceſſary to bring it very accurately over a point on the ſurface of the ground underneath.

Rough meaſurement of the Baſe with the Chain, and determination of the relative heights of the Stations by means of the Teleſcopic Spirit Level. Tab. XVI. and XVII.

Having in the preceding deſcription of the various inſtruments, originally provided for the meaſurement of the baſe, fully explained their conſtructions, uſes, and modes of application; and having thereby anticipated, in a great degree, what muſt otherwiſe have been ſaid to make them underſtood in any account, blended with that of the execution; little more now remains to be given than the journal of our proceedings from day to day, and the ultimate reſult of the operation.

After a very tedious delay, Mr. Ramsden having at laſt produced his hundred-feet chain, with the portable tranſit inſtrument; and having lent us an excellent teleſcopic ſpirit level, for determining the relative heights; two ſections of the baſe being likewiſe cleared by the ſoldiers, and ſome progreſs made in

the third, we found ourselves, on the 16th of June, in readiness to begin the rough measurement.

Lieut. Colonel CALDERWOOD, of his Majesty's Horse-Guards, F.R.S. had, from the beginning, been so good as to promise his assistance in the operation. Lieut. Colonel PRINGLE too, of the Corps of Engineers, obligingly became a volunteer on the occasion; as did also Mr. LLOYD, F.R.S. a few days afterwards; while Ensign REYNOLDS, of the 34th regiment, who had for some time past been employed in surveying the environs of the Heath, continued that work with such spare hands as could be afforded him for that purpose; and it is to the plan (tab. XVI.) done by that officer, that it will be necessary to refer in any thing regarding locality, in what has hitherto been said, as well as in the subsequent relation.

The lower end of the base had for some time past been distinguished by a St. George's flag fixed to the top of a fir spar, thirty-five feet in height; and one of the signal bell-tents still remained at the station near the summer-house. A rope of 200 yards being made very fast by a strong iron picket, driven into the ground at the bottom of the flag-staff, the other end was carried on along the base, and placed at the bottom of a camp-colour, in a line with the bell-tent. The rope being wound around a strong iron reel, prepared for the purpose, was thereby stretched extremely tight, a person occasionally lifting it up in the middle, or at other places, and letting it drop again, so as to bring the whole into the same straight line. Five persons were necessary for the proper management of the chain; two at each end for its adjustment there, and one towards the middle, to lay it close to the rope, or to bear it up in any particular place, where the circumstances of the ground rendered such precautions useful. The zero or rear end of the chain

 being

being ſtrained back ſo as to coincide with the point of commencement, a ſteel arrow was placed as erect as poſſible in the ſemi-circular cavity of the braſs handle at the other end. The chain being then drawn on, till the cavity in the rear handle could be applied to the firſt arrow, a ſecond was then placed in that of the front handle, and ſo on until ſix chain lengths were thus meaſured off; which terminating the firſt hypothenuſe, a beech picket, ſomething more than an inch ſquare, and about ſeven in length, with N° 1. cut upon it, was driven into the ground, till its head was nearly level with the ſurface. It is however to be remarked, that the ſixth arrow of each hypothenuſe was conſtantly left in the ground till the firſt of the ſucceeding one was placed, to avoid the error that would have otherwiſe ariſen in applying the rear end of the chain to the picket inſtead of the arrow.

In this manner we proceeded on the 16th of June, and in the ſpace of about three hours and a half, completed the firſt meaſurement of the ſouth-eaſt ſection of the baſe, comprehending the thirteen hypothenuſes between the flag-ſtaff and ſtation near Hanworth Summer-houſe, the diſtance being 78 chains or 7800 feet, making 2600 yards; and the mean temperature of the air being 63°.

On the ſubſequent day this ſection was re-meaſured with equal care, when the total extent fell ſhort of the thirteenth picket only five inches. And here it is to be obſerved, that a conſiderable part of this difference probably aroſe from the ſtretching of the chain acroſs Wolſey River, at the ſame time that the irregularities of the ground are greater in this than in either of the other two ſections. The mean heat of this day was 65°.

The

The operation with the chain was suspended during the 18th and 19th of June, those days having been employed in settling certain matters with Mr. RAMSDEN relative to the deal rods, as well as to give time for the making of a holdfast for the rear end of the chain, invented by Lieut. Colonel PRINGLE. This machine, whereof the plan at large is represented by dotted lines at the handle of the chain, as it is in small by the two elevations adjoining in tab. XVII. consists of a semi-circular iron plate, from the bottom of which projects two double and one single prong. In the middle, between the two double prongs, a semi-circular cavity is formed, fitted to receive the steel arrow on one side, while that in the brass receives it on the other. In a socket in the middle, a strong wooden handle, resembling that of a spade, is placed. Thus the rear handle of the chain being applied to the arrow, the holdfast embraces with its double prongs the straight part of the brass, and in that position, being forced into the ground by the action of a man at the handle, the rear end of the chain is thereby kept so firm as to be immoveable by the efforts of the two men at the other end, in stretching it to its true position, for the front arrow.

On Monday, the 21st of June, the operations were resumed, by measuring twice with the chain (forwards and again backwards) the thirteen hypothenuses comprehended in the second section of the base, between Hanworth Summer-house and the north-west bank of the great road (an old Roman way) leading from Staines to London. This being the smoothest part of the Heath, and the holdfast being now applied, the two measurements differed only one inch and a half in the distance of 7800 feet. This instance of accuracy is alone suf-

ficient

ficient to prove the great excellence of the chain, although another will be given hereafter ſtill more ſurpriſing.

On the ſame day that the ſecond ſection of the baſe was meaſured, the levels of that and the firſt were taken. The operation of levelling is ſo univerſally known, as to render any detail of it unneceſſary. It will be ſufficient to ſay, that the ſpirit level made uſe of on this occaſion was a very good one, about eighteen inches in length, and could at all times be very readily and accurately adjuſted by inverſion in its Y's. The tops of the pickets, marking the hypothenuſal diſtances, were the points on which the levelling rods were placed on each ſide of the level; which being inverted at the intermediate picket, points equi-diſtant from the center of the earth were thereby obtained, at the croſs vanes of the levelling rods, and no correction for curvature or refraction neceſſary. It will be readily underſtood, that the relative heights of the pickets were found by meaſuring their diſtances from the centers of the croſs vanes and axis of the teleſcope reſpectively.

The ſix firſt columns towards the left-hand of the firſt or general table ſubjoined to this Paper, ſhew diſtinctly every thing relating to the levels of the whole baſe, thoſe of the third ſection having been determined on the 22d of June. By examining the table it will be ſeen, that the aſcent on the firſt ſection is 10.555 feet,

on the ſecond	.	8.580
and on the third	.	12.130
Total	. .	31.265 feet,

between the lower extremity at Hampton Poor-houſe, and the higher near King's-Arbour.

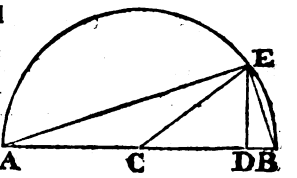
The

The computed numbers in the seventh column are the reductions * depending on the aforesaid heights, or the differences between the hypothenusal distances of 600 feet each and the reduced base distances. With regard to the remaining columns of the table, or those towards the right-hand, they will be severally spoken to hereafter, in taking into consideration the expansion of metals, as determined with great accuracy by the experiments with the pyrometer.

Hitherto no use had been made of the transit instrument: for, in order that it might be applied to advantage, there was a necessity for laying the wheel into the ground at the lower end of the base, and so to modify the St. George's flag-staff that, being placed in the pipe, it might be steadily supported by braces in a true vertical position; which we found, from experience, could not be effected by ropes only.

The wheel being accordingly laid in its place, and the other precautions taken for securing the flag-staff, which was likewise painted white, that it might be more distinctly seen from

* The reduction in the seventh column, I have computed by the difference between the square of the hypothenuse, actually measured, and the square of the height found by the level; and Lieut. Colonel CALDERWOOD has done the same thing by a much shorter method. Thus, in the annexed figure, CE being the hypothenuse of 600 feet, DE the perpendicular height obtained by levelling, DB the reduction required, or the difference between the hypothenuse and true base; then, substituting the chord BE instead of DE, the following analogy is obtained; AB : BE :: BE : DB; consequently, $\frac{BE^2}{AB} = DB$: that is, the square of the perpendicular height being divided by double the distance, or 1200 feet, the quotient is equal to DB the reduction, without sensible error. For if DE were four feet, the greatest perpendicular height in the base, BE the chord would only exceed it $\frac{1}{00000}$, which would not be more than $\frac{1}{4000}$ part of an inch. The difference between the results, by the two modes of computation, is so trifling as not to deserve notice.



the

the farther extremity; on the 22d of June, the tranſit inſtrument was adjuſted over the thirteenth picket at Hanworth Summer-houſe, while directed upon the flag-ſtaff. But it being now found, that the vertical plane paſſing through the flag-ſtaff fell to the eaſtward of the center of Banſted Spire, therefore the tranſit was gradually moved to the eaſtward, until by repeated trials the three points were perceived to be in the ſame vertical plane, when the picket was moved, and re-placed exactly under the axis of the teleſcope, a few inches from its firſt poſition. The ſame operation was repeated at the twenty-ſixth ſtation, on the farther bank of the Staines Road; and, laſtly, at the forty-ſixth, forming the north-weſt extremity of the baſe; where a pit was immediately dug for the wheel, which was placed therein, without however filling in the earth for the preſent, that being deferred till near the completion of the meaſurement with the deal rods. Thus the two extremities, and two intermediate points of the baſe, being accurately placed, by the help of the tranſit inſtrument, in the ſame vertical plane with Banſted Spire, it was eaſily ſeen, that by arranging camp colours in the intervals at any time, all the other points might be brought ſo nearly to coincide with theſe firſt, as not to occaſion, by deviation, any ſenſible error in the meaſurement afterwards to be made. This application of the tranſit ſhewed us, however, that ſome labour had been loſt by not uſing it ſooner: for at the Staines Road, the tract cleared by the ſoldiers deviated about two feet and a half too much to the weſtward for the true line; and at King's Arbour it was twice as much; ſo that we were now obliged to widen the cleared tract, by adding to the eaſtern ſide of it.

On the ſame day that the chief points in the baſe were fixed by means of the tranſit, and the levels of the third ſection

taken

taken as before-mentioned, the rough measurement of that section with the chain was completed, and found to contain nineteen hypothenusal distances of 600 feet each, and one of 404.55, making in the whole 11804.55 feet, between the twenty-sixth station at the Staines Road and the center of the pipe near King's Arbour, the mean temperature being 62°½. Here it is to be observed, that this last section was only measured once with the chain, the tract not being yet sufficiently cleared to admit of its being done to the best advantage; and, when completed, it was judged to be better to proceed directly in the operation with the rods, than to lose time in the usual repetition, since the merits of the chain, in this way of applying it, were already sufficiently well established; and any future tests to which it was to be put were proposed to be of a more rigid nature.

When the length of the chain, in its original state, was ascertained by the dots on the brass pins in the New-England plank, it was found, in the then temperature of 74°, to exceed the 100 feet by near one quarter of an inch, or 0.245 inch. Therefore, in the temperature of 63°, being that in which the lengths of the deal rods were laid off, and differing very little from what was likewise the mean heat of the air, when applied upon the Heath, the chain, according to the experiments on the expansion of the very same steel, would exceed the 100 feet by 0.161 inch, or 0.0134 foot. Hence the sum of the three sections of the base, 274 chains, being multiplied by 0.0134 foot, we shall have 3.67 feet for the equation of the chain + 4.55 feet, to be added to its length, which will then become 27408.22 feet from the center of one pipe to the center of the other: and this would have been the true length of the base, as given by the rough measurement with the

chain, if the ſurface had been one uniform inclined plane throughout its whole extent. But, although the aſcent of Hounſlow-Heath is ſo ſmall, and ſo gradual, as to occaſion little more than half an inch of reduction, from the 46 hypothenuſal to the 46 baſe diſtances, into which it is divided, as may be ſeen by referring to the table; yet each of theſe hypothenuſes containing again many other ſmall irregularities, all of which affect the meaſurement by the chain, in proportion to their number and height, in every ſpace of 600 feet, their united effects, including the lateral deviations from the true line in meaſuring, do ſomewhat more than compenſate for the extra-length of the chain, as will be ſeen hereafter in comparing the length of the baſe juſt now obtained with that given by the rods.

The weather, which during the greater part of June had been wet, became ſtill worſe towards the end of the month and firſt week of July; ſo much ſo, that even if the deal rods had been ready they could not have been uſed with advantage. The ſoldiers, nevertheleſs, were not idle, being, when the weather would permit, partly employed in clearing the Heath, and partly in aſſiſting Mr. Reynolds in the ſurvey, towards the perfecting of which many chief points were fixed by means of my aſtronomical quadrant, placed for that purpoſe at ſeveral different ſtations of the baſe. At this time too (July 8th) I levelled from the lower end of the baſe to the ſurface of the Thames at Hampton, and found the deſcent to be 36.1 feet.

Meaſurement:

Measurement of the Base with the Deal Rods. Tab. XVI. and XVIII.

Such extraordinary care and pains had been bestowed in the construction of the deal rods, in order to render them the best which had ever been made, that, although begun early in June, they were not completely finished before the 15th of July. They were brought that afternoon by Mr. RAMSDEN, together with the various parts of the apparatus necessary for their application in the field, to the camp now moved from Hanworth Summer-house to the intersection of the base with Wolsey River; whence they were transported, early next morning, to the pipe near Hampton Poor-house, where we were met by Sir JOSEPH BANKS, accompanied by Mess. BLAGDEN, CAVENDISH, LLOYD, and SMEATON, all ready to lend their assistance in the subsequent mensuration.

Before I proceed farther, I think it here incumbent upon me very gratefully to remark, that the respectable and very worthy President of the Royal Society, ever zealous in the cause of science, and who had repeatedly visited the heath, to offer aid, if such had been necessary, while the first and rougher part of the operations were going on; now, that others of a more delicate nature were to commence, and where it was of importance, that those entrusted with the execution should meet with as few, and as short, interruptions as possible, not only gave his attendance from morning to night in the field, during the whole progress of the work; but also, with that liberality of mind which distinguishes all his actions, ordered his tents to be continually pitched near at hand, where his immediate

guests, and the numerous visitors whom curiosity drew to the spot, met with the most hospitable supply of every necessary and even elegant refreshment. It will easily be imagined, how greatly this tended to expedite the work, and how much more comfortable and pleasant it rendered the labour to all who obligingly took part in it; but more especially to him, who, being a volunteer in it at first, considered himself as bound to persevere in his best endeavours to bring it to a successful conclusion.

From the description that has been given of the deal rods, it will be remembered, that they are fitted to be applied in measuring, either by the coincidences of lines, inlaid one inch and a half from each extremity, or by the contacts of the spherical lips of the bell-metal with which they are tipped. The first, seeming to be the most accurate, although the most tedious method, was that by which we proposed to set out.

The flag-staff having been previously removed from the pipe, and the brass cup filled with water put in its stead, all the necessary precautions being likewise taken for preserving the line of direction, horizontally, by the rope stretched along the first hypothenuse, and vertically, by means of the boning rods; the first ivory line on the first rod was brought by the plummet to coincide with the center of the cup, in which position, being clamped, it accurately marked the commencement of the base. The second rod being now applied to the first, and moved up by the apparatus formerly described (tab. XVIII.) till its line coincided with that on the first; and, in like manner, the third rod being applied on the alternate side of the second, moved up and clamped as the rest; thus the exact distance of sixty feet was ascertained, care being always taken, that the first adjustments were not disturbed, while the

subsequent.

subsequent ones were forming. The clamps fastening the first rod to its stands being then detached, it was carried by two men and laid on the alternate side of the third; and so on in succession, until fifteen rod lengths were measured off, being the half of the first hypothenuse.

The time consumed in measuring this short distance of 300 feet was not less than five hours; owing, as has been formerly mentioned, to the confined nature of the apparatus for moving the rods on into coincidence, which required such nicety in placing the stands, as could not be effected until after several repeated unsuccessful trials. All the executive people were therefore of opinion, that it would be proper to discontinue this mode of measurement, at least until a more convenient apparatus could be thought of for the purpose; and that, in the mean time, we should proceed by the method of contacts, as the only alternative we could for the present adopt*.

The rods being accordingly placed in contact with each other, we soon made greater progress, finishing the operations of the day at the middle of the fourth hypothenuse, where the tripod, with its guard, was placed, to preserve the point of commencement for the ensuing morning.

* Although I acquiesced in the change thus become necessary, yet it was with much reluctance, because it left undecided the contested point, with regard to coincidences and contacts. If we could have proceeded with the coincident rods till eighty-one lengths were measured off, and then measured back the same space by placing eighty rods in contact, the point would have been clearly settled. For if the termination of the eightieth rod agreed exactly with the point of departure, contacts being the most expeditious would have been judged the best method. On the contrary, if the eightieth rod fell short of reaching the point of departure, there could have been no doubt, that the difference must have arisen from butting one rod against the other, whereby a certain small proportion of each rod came to be lost in the account, by being measured twice over.

The

The measuring rods, when put into the chest in London, had been compared and found to agree with the standard. The comparison was not repeated on the 16th; but this being done on the 17th, at 7 h. A.M. under the oil-cloth canopy at the camp, they were found at a medium to exceed the standard by one-fiftieth of an inch, the temperature then being 62°. After the comparison they were carried to the place of the tripod, when the operation was resumed by bringing, with the help of the plummet, the same point of the rod with which we had left off work, to coincide with the intersection on the brass ruler. The measurement of this day was closed at the end of the tenth hypothenuse, when the rods being carried back to camp, were compared, and found accurately to agree with the standard.

A considerable fall in the barometer, between the evening of the 17th and the morning of the 19th, portended rain. Nevertheless, all parties repaired to the place of rendezvous, which was appointed at the lower end of the base, in order to re-measure the two first hypothenuses, by placing all the rods in contact, which on the 16th had been done partly one way and partly the other. The operation being according repeated with great care, the point of the sixtieth rod, which formerly corresponded to the center of the second picket, was now found to be pushed forward exactly forty-five inches, answerable to the deficiency on the fifteen coincident rods, with which the mensuration was begun. It now began to rain, therefore the rods were carried back to camp, and being severally compared, they were found to exceed the standard each by one-thirtieth of an inch, occasioned by the extraordinary humidity of the air. A heavy rain ensued; and what made this much more regretted by all was, that in the forenoon their Majesties graciously condescended to honour the camp with their presence, and continued

tinued there some time; but the weather becoming rather worse, it was utterly impossible to shew their Majesties the nature of the operation, by any progress that could at that time be made in the work.

After a continuance of unfavourable weather for several days, the operations were resumed at 9 h. A.M. of the 23d, when the rods being compared were found still to exceed the standard by one-thirtieth of an inch, and the temperature now was 61°. Here it is to be observed, that in our progress forward, an accurate register had been all along kept of that point of each rod corresponding to the center of the hypothenusal pickets, by noting its distance from either end, whereby the error of the chain at each station was readily discovered, at the same time that the revolutions of the three rods served to keep the account of the total measurement. In order, therefore, that this method might be distinctly adhered to, it was judged proper to push on the rod that lay over the tripod at N° 10. exactly forty-five inches, to make good the deficiency of the first fifteen coincident rods, and that the account might be kept from the lower end of the base in entire rods of 243, and complete revolutions of 729 inches each. This being done, the rest were placed in the ordinary succession; and we finished the business of the day at the eighteenth station, where the rods being compared at 6 h. P.M. their mean length was found to exceed that of the standard $\frac{1}{67}$th part of an inch, the temperature then being 54°.

On Saturday the 24th of July, the rods were three times compared; at 7 h. 30′ A.M., 11 h. 15′ A.M., and 5 h. 45′ P.M. Their mean excess above the standard was found to be one-thirtieth of an inch, and the mean heat 64°. In the course of the day, the measurement was continued from the eighteenth to the

the twenty-ſeventh ſtation, or firſt of the third ſection of the baſe, where the tripod was placed as uſual; and there it remained untouched, on account of bad weather, till Monday the 2d of Auguſt.

Conſidering how much time and labour had been beſtowed in obtaining what we certainly had every reaſon to conclude were the beſt deal rods that ever were made, it was no ſmall diſappointment now to find, that they were ſo liable to lengthen and ſhorten by the humid and dry ſtates of the atmoſphere, as to leave us no hopes of being able, by their means, to determine the length of the baſe to that degree of preciſion we had all along aimed at. But ſince more than one-half of it was already meaſured, it was judged proper to proceed with them in their preſent ſtate, and then to have them carefully painted or varniſhed, before they ſhould be farther uſed.

The unfavourableneſs of the ſeaſon, and delays in obtaining the inſtruments, had already been the cauſes of protracting the operations on Hounſlow-Heath greatly beyond what was at firſt expected; and the failure of the deal rods gave no immediate proſpect of their being ſpeedily brought to a concluſion. On revolving in my own mind the different alternatives we might ultimately be obliged to have recourſe to, metal rods of ſome kind or other, whoſe expanſion could always be determined by experiment, ſeemed to promiſe a reſult that might be ſafely relied on. Caſt iron was what I had thoughts of propoſing, knowing from an experiment which I had made myſelf, that it expanded leſs than ſteel. The cumberſomeneſs of its weight appeared indeed objectionable; but that inconvenience was either to be ſubmitted to, or one of another kind, namely, the reduction of the length, which was always, if poſſible, to be avoided.

5 At

At this time Lieut. Colonel CALDERWOOD could not conveniently lend us his assistance in the field; but he visited us occasionally, and on one of these visits proposed to me, that glass rods should be made use of instead of deal; putting me in mind of another experiment * that I had made, which seemed to shew that solid glass rods expanded less than tubes. This proposition the Lieutenant Colonel, before he came to the heath, had made to Mr. RAMSDEN, who appeared averse from making the trial, because of the great length of the rods, and the brittleness of the material. Nevertheless, it being sufficiently obvious, that glass rods or tubes of the full length, or something approaching towards it, would be much sooner provided than any metal rods whatever, and the saving of time being a point of consequence; Lieut. Colonel CALDERWOOD was accordingly requested to make the trial at the glass-house, as soon as possible after his return to town. Next day he succeeded in getting a fine tube drawn, eighteen feet long, and about one inch in diameter; and there seemed to be no longer any doubt, that those of the proper length might be obtained. It was found, that solid glass rods of such extraordinary dimensions could not be had, it being impossible to take at once a sufficient

* The experiment here alluded to was made with Mr. CUMMING's pyrometer, which from its construction did not admit of a very accurate estimation of the heat communicated to the standard bar, the rod, and tube respectively. Either, therefore, the natures of the glass rod and tube, made use of at that time, must have been very different, to cause the difference of expansion; or some circumstance in the instrument unattended to had occasioned the fallacious appearance: for it will be found, from the experiments hereafter to be given in detail, that a solid glass pendulum rod expands fully as much as, nay in this particular instance even more than a tube; but different glass, having different specific gravities, will no doubt be susceptible of different degrees of expansibility.

quantity of the melted metal on the irons, made use of for drawing them at the glass-house.

The week of rainy weather, which ended the month of July, occasioning, as has been said, a total suspension of the operations on the heath, was employed in procuring a sufficient number of glass tubes (one whereof was not less than twenty-six feet long) and regulating with Mr. RAMSDEN every thing concerning their construction into measuring rods. The description of them we shall however defer until the time of their application in the field, after having finished the operation with those of deal.

On Monday the 2d of August, the operations on the heath were resumed at 8 h. 30′ A.M. by comparing the rods with the standard, which they were found to exceed by one-fortieth of an inch, the temperature then being 66°. The forward end of the rod now placed over the tripod at N° 27, completing the 800th length, reckoned from the lower end of the base by rods of 243 inches each; and these being equal to 810 rods of 240 inches; it was judged proper to mark a point upon the ground corresponding to this forward end, that it might be referred to in returning back with the measurement by the glass rods. This was done by sinking two small pickets into the ground, about a foot asunder, one on each side of the base, and at right angles to it. A silk thread being then stretched over the tops of the pickets, and gently moved on till it touched the silver wire suspended from the end of the rod, fine notches were then made with a pen-knife in the tops of the pickets, whereby the thread could be replaced in the same situation; which being done, the pickets were covered over with earth. In the course of this day nine hypothenuses were measured; and at 7 h. P.M. the tripod was placed at the thirty-

sixth

sixth station. The rods, being now compared, were found to agree with the standard; and the temperature was 67°½.

On Tuesday the 3d of August, the rods were compared at 7 h. A.M. and found only to exceed the standard by one-sixtieth of an inch. Being arrived at the middle of the forty-first hypothenuse, a point corresponding to the forward end of the 1215th rod was transferred to the ground by the double pickets and silk-thread, as had been done at the twenty-seventh station. The measurement was then continued to the north-west extremity of the base, which was found in the whole to contain 1353 complete long rods of 243 inches each +21 inches, where the tripod was placed, in the point which of course corresponded to the 1370th short rod of 240 inches each, equal to 328800 inches, or 27400 feet. To which distance we have yet to add 4.31 feet, being the space intercepted between the intersection on the tripod and the center of the pipe marking the north-west extremity of the base; whose total length, as given by the deal rods, without regard to expansion, or reduction of the hypothenusal line, becomes 27404.31 feet. And here it is to be observed, that the intersection on the tripod terminating the 27400 feet only over-shot the picket answering to the 274th chain by two inches and nine-tenths. But this nice agreement between the result by the deal rods, and that furnished by the rough measurement with the chain, arises from the extra-length of this last, which so nearly compensated for all the irregularities of the surface.

The measurement with the deal rods being finished, they were compared at 5 h. PM. and found to agree with the standard, the temperature then being 75°.

Expanſion of the Deal Rods.

It has been an opinion generally enough, although, as we have ſeen, erroneouſly received, that very ſtraight-fibred deal was not at all, or but little, affected longitudinally by the humidity of the air. That we might not be led aſtray by truſting to fallacies of this ſort, the ſtandard rod had been provided; which being always cloſely ſhut up in its cheſt, except during the ſhort interim of compariſon, could feel but a ſmall proportion of the effects which the meaſuring rods ſuffered, theſe being conſtantly expoſed to the open air throughout the day, as well as to the moiſture of the night, when lying under the oil-cloth canopy. The ſtandard rod, it is true, could not be accurately compared with the braſs ſcale: for although when conſtructed, braſs pins, forty inches aſunder, had been driven into its ſtem, for the purpoſe of ſuch compariſon, yet theſe had afterwards been diſplaced, or at leaſt the points upon them defaced, by the planing over of the upper ſurface. This circumſtance, which was unattended to when the operations commenced, is now of no conſequence; becauſe, from an experiment hereafter to be mentioned, the lengthening of the ſtandard may be pretty nearly aſcertained. But ſince there are ſome contradictory circumſtances, ſoon to be mentioned, in the operation with the deal rods, which would have made a repetition of it abſolutely neceſſary, if we had not now obtained thoſe of a different kind, ſo very unexceptionable in their nature and mode of application, as, in the preſent caſe, to admit of no competition between the two reſults, and to render it improper on our part ever to have farther recourſe to the firſt; ſo there can

can be little doubt, that deal rods will be universally rejected by other countries, in any measurements they may have occasion to make in future.

About the 10th of July, two rods, one of New-England and the other of Riga deal, being measured by the fixed points in the great plank in Mr. RAMSDEN's shop, and having each two brass pins driven into them at the distance of twenty feet, were laid on the top of the house, where they remained until the 26th, the weather, for the greater part of the time, having been very wet. They were then taken down, and being, by means of the long beam compasses, compared with the measures on the plank, the New-England rod was found to have lengthened 0.031 inch, and the Riga rod 0.041 inch. By which experiment the fact seems to be established, that Riga red wood, notwithstanding the quantity of turpentine which it contains, is more susceptible of the effects of moisture than New-England white wood. Mr. RAMSDEN likewise finds, that the great plank so often mentioned, suffers, in ordinary summer weather, an alternate expansion and contraction, amounting at a medium to 0.0041 of an inch every day: that is to say, if the distance between the twenty-feet brass points be measured from the scale, by means of the beam compasses, in the evening, it is found to have lengthened next morning 0.0041 of an inch, by the humidity of the intervening night. In the course of the following day it contracts again to its former length, and so on. Mr. RAMSDEN has often observed this alternate change in the deal plank; but it was particularly on the 11th and 12th of August, that the quantity was actually measured. It will readily be understood, that any difference of temperature which might have happened in the brass scale, at the

the times of comparison, was always carefully taken into the account.

Now, from this last experiment, it seems probable, that we shall not be very wide of the truth in supposing, that the standard deal rod, which lay closed up in its chest, under the canopy on Hounslow-Heath, would suffer the same sort of alternate expansion and contraction with the above-mentioned plank; that is to say, being of Riga wood, its mean expansion about the middle of the day would be $\frac{25}{10000}$ of an inch. By this quantity then we must augment the actual observed expansion of the measuring rods, in order to obtain within certain probable limits (since we cannot determine it accurately) the equation for the expansion; or that space by which the apparent measurement, given by the 1370 deal rods, should be augmented in order to obtain the true length of the base; or that which would have been given by unalterable rods, of the same original length with those of deal, as expressed in the following table.

Table

Table of the Expansion of the Deal Rods.

Days.	N° of rods meas.	Hour of comparison.	Temp. of the air.	Observed expansion.	Decimal mean.	Equation for the meas. rods.	Equation for the standard.	Total expansion.
		h. ′	°	In.		In.	In.	In.
July 16	105	4 0A.M. 6 0P.M.	48 62	$\frac{1}{30}$th 0	0.010	1.050	0.2625	1.3125
17	195	7 0A.M. 6 0P.M.	62 —	$\frac{1}{30}$th 0	0.010	1.950	0.4875	2.4375
23	240	9 0A.M. 6 0P.M.	61 54	$\frac{1}{30}$ $\frac{1}{67}$	0.021	5.040	0.6000	5.6400
24	270	7 30A.M. 11 15A.M. 5 45P.M.	61 66 64	$\frac{1}{26}$ $\frac{1}{31}$ $\frac{1}{33}$	0.033	8.910	0.6650	9.5750
Aug. 2	270	8 30A.M. 7 0P.M.	66 67½	$\frac{1}{30}$ 0	0.0125	3.375	0.6650	4.0400
3	290	7 0A.M. 5 0P.M.	56 75	$\frac{1}{60}$ 0	0.017	0.493	0.7250	1.2180
Total	1370	— — — — — — — —				20.818	3.405	24.223

N. B. Although the rods were not compared with the standard on the 16th of July, yet the expansion probably was, and therefore has been estimated, at the same rate as it was found on the following day.

By examining the preceding table, it will appear, that the total expansion on the 1370 deal rods, including the small equation for the lengthening of the standard, amounts to 24.223 inches, or 2.02 feet; which being added to the apparent length of the base 27404.31 feet formerly obtained, we shall have, for the hypothenusal length, 27406.33 feet: and from this deducting 0.07 foot, the excess of the hypothenusal above the base line, or the reduction contained in the seventh column of the general table of the base, there will remain 27406.26 for the distance given, by the deal rods, between the

 centers

centers of the pipes terminating the base, reduced to the level of the lowest, or that at Hampton Poor-house, in the temperature of 63°, being that of the brass scale when the lengths of the deal rods were laid off. All this, however, supposes three things to be absolutely certain: first, that the expansion of the rods has been accurately estimated; secondly, that no error has arisen from the butting of the rods against each other, in order to bring them into contact; and, thirdly, that no mistake of any kind has been committed in the execution. When we come to give the true length of the base, as ultimately ascertained by means of the glass rods, it will appear, that one or more of these three have actually taken place; although it is most probable, that only the two first sources of error have contributed their share of the total difference between the two results. But the discussion of this point must be deferred for the present; and I shall now finish the subject of the expansion of the deal rods, by mentioning two other comparisons of them, which serve to shew still more obviously, how improper they are for very accurate measurement!

It has already been remarked, that the last week of July was so wet as to occasion a total suspension of the operations on Hounslow-Heath. On the 26th of that month, at 8 h. A.M. the temperature being then 63°, the rods were compared with the standard, and found to exceed it, at a medium, one-fifteenth part of an inch. Now, if we suppose the whole base to have been measured with the rods in that state, the difference would have amounted to more than 7½ feet, exclusive of what the standard itself might have altered from its original length.

The other comparison was made at Spring-Grove, in the beginning of September, after our operations on the heath had been finished, and the deal rods with their apparatus deposited

under the roof of Sir JOSEPH BANKS's Barn. The object here in view was the measurement of such a space as the garden would conveniently admit of, when the rods were in their dry or contracted state; and to re-measure the same space next morning, when the rods, being left out for the purpose, had imbibed all the humidity they could from the moisture of the intervening night. Accordingly, the fourth being a fine dry day, the sun shining bright, and the thermometer about 68°, seventeen stands were arranged in the long walk, with so much nicety in the same inclined plane as to appear but like one. The first or lowermost stand had a brass cock screwed to its top. The two uppermost, that is to say, the sixteenth and seventeenth, were of the fixed kind, each with a brass slide, and placed only forty-five inches asunder. The first deal rod was made to butt against the brass cock, and the rest successively against each other, until fifteen rod lengths were measured off, and a fine line drawn on the slide marking the extremity of the fifteenth. That rod being removed, forty-five inches, taken from the brass scale, were then laid off backwards from the line on the slide of the seventeenth to the slide of the sixteenth stand, where another fine line was drawn. Thus the space comprehended between this last line and the cock on the first stand, was just 300 feet, or fifteen coincident rods. During the night of the 4th, which was very fine, the rods lay on the smooth grass. About sun-rising of the 5th there came on a thick fog, which entirely dispelled about 8 o'clock. At 7 h. A.M. the rods being lifted from the grass, it was perceived, that the under sides were perfectly dry, while all the rest was quite wet with the dew that had fallen. The fourteen stands, comprehended between the first and sixteenth, having their distances gradually reduced from twenty feet three

inches to twenty feet, the operation of re-measurement was then begun, by placing the rods in coincidence with each other (which was now found to be easily and accurately effected by a few repeated strokes with a wooden wedge only) until the fifteen rod lengths were measured off, and a fine line, corresponding with the ivory on the fifteenth, was drawn on the brass slide. This line was found to be 0. $\frac{498}{1000}$, or near half an inch beyond that which terminated the 300 feet the preceding evening. Hence it is evident, that the dew imbibed only in one night, or a space of time not exceeding fourteen hours, occasioned such an expansion in the deal rods, as in the whole base would have amounted to 45.484 inches.

It is sufficiently obvious, that this last mentioned experiment was more accurate, in the proportion of about fifteen to one, than any comparison we could at that time have made with the standard. But since immediately after it was finished, the sun shone out very bright, it is by no means certain, how soon the rods would again have contracted to their former length, or near it, had they been exposed to his rays. Repeated comparisons for ascertaining facts of this sort, at very short interims, are absolutely incompatible with the nature of such tedious and troublesome operations as the measurement of long bases: and here, indeed, lies the great objection to the use of deal rods, that at no time can we be certain how soon, after a comparison has been made, they may alter their length in a proportion, and sometimes too even in a sense, different from what was expected.

Description of the Glass Rods, ultimately made use of to determine the length of the Base. Tab. XIX.

It has been already mentioned, that the week of rainy weather in the end of July was employed in providing the glass tubes, and in concerting matters with Mr. RAMSDEN, relative to their construction as measuring rods. Notwithstanding their great length, they were found to be so straight that, when laid on a table, the eye, placed at one end looking through them, could see any small object in the axis of the bore at the other end.

The nature and construction of the glass rods, whereof three were finished for the operation, will be best conceived by considering, with care and attention, the plans and elevations of them, in whole or in part, to different scales in tab. XIX.; where likewise may be seen, plans and sections of the ends of the tubes, in their real dimensions, for the better understanding the several parts of the apparatus placed therein.

The case containing the tube, and which serves to keep it from bending in its original straight position, is every where of the depth of eight inches, of the same width in the middle, and tapers from thence, in a curvilinear manner, towards each end, where it is only two inches and a quarter broad. It is made of clean white deal, the two sides being half an inch, and the top and bottom three-eighths in thickness. These last are placed in grooves fitted to receive them, about half an inch from the upper and lower edges of the sides, which bending easily, and applying closely, are then firmly fastened by two rows of wood screws on each side, to the top and bottom

respectively. Thus, the depth of the sides in one sense, and the spring which they have by bending in the other, act as trusses, prevent the case from warping, and render it sufficiently strong, although at the same time, considering its great length, very light.

The plan of the middle rod represents the case with the top off, that the tube may be seen placed therein: the right and left-hand rods have the tops on, whereby may be seen the oval opening in the middle of each, shut by a mahogany lid; and also the positions of the two thermometers, with tubes bent at right-angles, so as to place the ball about two inches downwards within the case, for the better ascertaining the temperature of the glass, as will easily be conceived, by considering the representation of the tube and ball in the section across the middle of the rod.

It is to be observed, that the middle of the tube is made fast to the middle of the case in the following manner. First, around the middle of the tube, a quantity of pack-thread, immersed in liquid glue, was wound by several returns on itself, for the space of about two inches in length; and upon this mass of pack-thread, while the glue was warm, a strong mahogany collar was forced; whereby the three substances became so perfectly united to each other, that they might be considered as one only. Across the bottom of the case in the inside, three mahogany braces or girders, one in the middle, and one half-way between it and each end, are fastened, by means of screws, to the bottom and sides. These rise about 1¼ inch above the bottom, so as to place the axis of the tube, when in use, about 2½ inches above the surface of the stands on which it rests. The end-pieces of the case are likewise of mahogany, about 1¼ inch thick. Each consists of two parts, a lower and an

upper.

upper. In the lower parts, as well as in the cross braces, there are semi-circular cavities lined with broad-cloth, fitted to receive the diameter of the tube, which rests in them, and is consequently supported at five different points. The upper end-pieces, having likewise semi-circular cavities fitted to embrace the upper part of the tube, slip down upon it, when it has been, by repeated trials, brought to its true position; that is to say, the axis of the bore into the same straight line, the case being all the while supported by its extremities on two stands only, in the manner in which the rods are applied in actual measurement. The braces within the case have also their upper pieces, which, in like manner, apply closely to the tube, and are fixed to the lower ones by means of screws. The whole together serve only as stays to keep the tube in its true place from shaking; but without binding it however too closely. Lastly, the mahogany collar glued to the pack-thread on the middle of the tube, being strongly fixed by four screws to the middle brace, as may be seen in the section, is that by which the tube is kept perfectly immoveable with respect to the middle of the case; while it is unconfined longitudinally in the cavities lined with broad-cloth every where else.

Both ends of the tube are ground perfectly smooth, and truly at right-angles to the axis of the bore. That end, which in measuring usually lies towards the left-hand (since most people will work the screw with the right) projects about seven-tenths of an inch without the case, and is called the fixed end, because the apparatus belonging to it is fixed. The other end towards the right-hand projects about nine-tenths of an inch, and, having a moveable apparatus, is called the moveable end.

The

The fixed apparatus consists of a cork about three inches in length, made of the very best material, and so nicely fitted to the bore as just to admit of being forced into without bursting it. In the middle of the cork a cylindrical brass tube is placed, whose sides are thin, the inward end thick, and the outward end open. It receives a steel pin, whose inward end being formed into a screw, is thereby fixed into the thick metal of the tube. The steel pin carries outwardly a button and neck of bell-metal. The neck fits so very closely the open end of the brass tube as to prevent any shake there; at the same time that the inside of the button applies very justly to the ground end of the glass tube, to which the outward surface (being a true plane) is exactly parallel.

The moveable apparatus consists, like the other, of a cork and brass tube of the same length. Before the insertion of this cork, an oblong piece seven-tenths of an inch long, and two-tenths broad, was cut from it, in that part of its cylinder answering to the upper part of the outward end of the glass tube, on the inward surface of which, about half an inch from the end, a fine line had been previously cut by a diamond point. The brass tube in this cork contains within it a loose steel worm, or helical spring, something less than the interior diameter of the tube. Along the cavity formed by the spiral, there passes a steel pin, like that in the fixed end; but it is longer, and has no screw at the inward end, that being nicely ground, so as to fit a circular hole in the inward end of the brass tube, while a triangular bell-metal neck fits one of that figure in the outward end. Thus the pin moves freely backwards or forwards without any shake, and presses upon the steel spring, by means of a circular brass collar, placed for the purpose, at the inward end of the neck; while the outward

end is attached to a bell-metal button. The outward surface of this moveable button is spherical, described on a radius of about two inches; while the inward surface, like that at the fixed end, would apply closely to the ground end of the glass tube, but should not be pushed so far forward as to touch it. A circle and narrow slide, cut from a solid cylinder of ivory, fitted originally to enter easily the glass tube, is attached to the inside of the button by small screws, and permits the neck to pass through a hole made on purpose in the circle. The slide is about eight-tenths of an inch long, and has a fine intersection cut upon it near the inward end, made black to render it more conspicuous. Thus, two rods being brought into contact, and the fixed button of one being pressed against the moveable button of the other, the intersection is thereby pushed forwards until it coincides with the diamond line on the interior surface of the tube; whose length is so adjusted, as that, when the coincidence is perfect, the distance between the plane surface of one button, and the spherical surface of the other, is exactly twenty feet. The left-hand side of the plate represents the relative positions of the extremities of the first and second rods, when the ivory is in coincidence with the diamond line. And the right-hand side shews the relative situations of the extremities of the second and third rods, before the ivory is brought to coincidence with the diamond line, the slide being then pushed out by the action of the spiral spring within the cork.

Every rod has four wheels, two at each end. They are two inches in diameter, and connected by a common steel axis, which rises and falls in a vacuity prepared for its admission in the mahogany end-pieces, the under part of which vacuity is afterwards filled up.

A brass

A brafs ftrap or bridle, about eight-tenths of an inch broad, paffes over the top of the cafe, and defcending down each fide, bends outwards, fo as to form a projection for the reception of the wheels, whofe pivots turn in, but near to the lower end of the bridle, which is kept in its place by means of the two fide fcrews working in grooves, and the milled-headed fcrew at top. This laft ferves likewife to raife or depreffs the wheels at pleafure.

Each rod has two crofs feet, placed immediately behind their refpective pair of wheels, extending outwards about 4¾ inches from the center on each fide. Under their outward extremities, fmall pieces of hardened fteel, formed into the teeth of a file, are fixed by means of fcrews. When the firft rod has been laid in its true place, by unfcrewing the milled heads, the wheels are fuffered to rife; whereby the whole weight is removed from them, and thrown upon the teeth of the files, which then indent themfelves into the furface of the ftand, and become as it were united to it. But when the fixed button of the fecond rod is brought to prefs againft the moveable button of the firft, the weight being then thrown upon the wheels by fcrewing the milled heads at top, the rod is eafily moved on by the following apparatus.

The three rods are numbered, as were thofe of deal, 1.2; 3.4; 5.6. On the firft or odd end of each rod 1. 3. and 5. there ftands a brafs fork, about two inches high, fixed by four fcrews and an oblong plate to the top of the cafe. On the fecond, or even end of each, 2. 4. and 6. there ftands a brafs pillar of the fame height with the fork, likewife fixed to the top of the cafe by four fcrews and a circular plate. Two fteels rods or hooks were indifferently ufed for bringing up the moveable rod (the weight then lying on the wheels) into its true place. They are

are both represented in the plate, and only differ from each other in the shape of the brass milled-headed nuts that work upon the screw, of about 2¼ inches in length, into which the right-hand end of each hook is formed. Thus, while the nut enters very freely into, and rests upon, the fork, the left-hand end of the hook has a circular hole in it, whereby it slips easily off and on of the brass pillar. By referring to the plate, it will appear sufficiently obvious, from the nature of the nut on the left-hand hook, that it could only move the rod on to coincidence, and could not bring it back again, if the business happened at any time to be overdone; in which case it was necessary to move the rod a little backwards by the hand, and then to work anew with the nut, until the coincidence was accurate: whereas the nut on the right-hand hook, having two shoulders, could either push or pull the rod forwards or backwards: and although this appeared to be an advantage, yet it was found from experience, that it rather bound the hook too much, and occasioned a kind of spring in the parts, which sometimes disturbed the coincidence on the removal of the hook; wherefore it was often applied, like the other, by placing the screw itself in the fork, and working with both shoulders of the nut behind it.

The positions of the thermometers, and mahogany oval lid on the top of the case, have already been mentioned. This last, being unlocked and removed, permits the case to be looked into, or the hand to be admitted, in order to be certain that the fastenings remain safe and entire in the inside. Brass caps, with the respective number of the rods engraved on them, are likewise screwed on the male-screws in the ends of the case, through which the extremities of the tubes project, to preserve them from accidents when not in use. And, lastly, to

ſtrengthen the caſes, but more particularly to prevent them from being rent when long expoſed to the ſun's rays in the field, the ſides are covered with brown linen laid on very ſmoothly, and carefully glued with thin glue, uſed as a ſtronger kind of paſte, to which it may yet be neceſſary to add a coat of oil paint.

Each of the glaſs rods, completed in the manner above-mentioned, weighs about ſixty-one pounds. Their lengths were aſcertained by means of new braſs points placed in the great plank, the ſpaces of forty inches being laid off, with the utmoſt care, from the braſs ſcale, when the temperature of all had remained for the greater part of two days (Auguſt 15th and 16th) at or very near 68°. For this purpoſe two braſs rectangular cocks, whoſe alternate ſurfaces had been previouſly ground together, were placed upon the plank, ſo as to biſect the extreme dots; in which ſituation they preſented to each other ſurfaces that were truly parallel. The rods being then ſeverally placed between the cocks * (or, as was found to be a better method,

* The firſt of theſe cocks, or that to which the fixed button was applied, had a hole in it exactly of the height of the center of the button, and large enough to permit the point of the micrometer ſcrew to paſs through it, the ſaid ſcrew being fixed on the farther ſide, or beyond the cock. Thus, while the temperature continued accurately at 68°, the fixed button, or any other plane ſurface, being brought up to the hole in the cock, and the micrometer point ſcrewed ſo far as juſt to touch it, the coincidence continuing in the interim perfect, the exact diſtance of twenty feet was obtained between the point of the ſcrew and the ſecond cock; at which time the diviſion anſwering to the index on the head of the micrometer was carefully noted. This being done, the cock with the hole was removed from the plank, and the rods were ſeverally adjuſted by being placed between the point of the ſcrew and the ſecond cock. This ſubſtitution of the micrometer point, inſtead of the firſt cock, was found neceſſary; becauſe, during the operation of adjuſtment, the temperature would ſometimes change a degree, generally

method, between the point of a micrometer screw, supplying the place of the first cock, and the second) the ivory intersection was at first necessarily carried beyond the diamond line, so as to make the intermediate space less than it should be, until by the gradual grinding down of the moveable bell-metal button, it was enlarged to twenty feet, as then shewn by the accurate coincidence of the intersection with the diamond line.

It was by these distances in the great plank, prolonged to twenty-five feet, that the new length of the steel chain was now settled, so as to obtain the full one hundred feet at four measurements. At this time too, brass points were introduced into the chain at every twenty-five feet, whereby its extent may be compared on any future occasion; but the temperature had now fallen to 66°½.

Disposition of the Stands for the double measurement with the Chain and Glass Rods; description of the apparatus then applied to the ends of the Chain; and ultimate continuation of the measurement with the Glass Rods alone. Tab. XVII. and XIX.

From the various circumstances already mentioned, in the course of this tedious, yet necessary recital, it had been for a considerable space of time foreseen, that the result given by the measurement with the deal rods must be entirely rejected,

generally in excess, from handling the instruments. One degree of alteration, producing a difference of about $\frac{1}{1000}$th part of an inch in the twenty feet, was very easily and accurately allowed for by such a micrometer as this, which shewed the coincidence of the ivory intersection with the diamond line to be more or less perfect, when the head of the screw was moved two divisions, that is to say, $\frac{2}{10000}$ths or $\frac{1}{5000}$th part of an inch.

and that by the glass rods adhered to, as every way deserving of the preference; because of the obvious impropriety there would be, in taking a mean between one indisputably good and another less perfect, however small or trifling in reality the difference of the two might ultimately be found, on a minute and scrupulous comparison.

In order, therefore, to avoid any repetition of the operation with the glass rods, and at the same time to give something like a fair trial to the chain, it was proposed, that a double measurement should be carried on with both at once; that is to say, that the number of stands, and several other parts of the apparatus, should be so far augmented, as to admit the chain to be placed twice in advance, and then the rods to follow in succession on the same stands. Accordingly, the various articles having been sent to the north-west end of the base on the evening of the 17th of August, the operation of the double measurement commenced next morning the 18th.

By referring to tab. XVII. it will be seen, that seventeen stands were necessary for supporting the chain, the apparatus attached to each end of it, and ten coffers, whereof every five made about ninety-eight feet, in order that one length of the chain being measured off in the first five, it might be drawn forward into the last five, and so on. These seventeen stands were disposed of in three groups of three each, and four intermediate, between the central and extreme groups. The middle or slide stand of each group (so distinguished because some of them had brass slides on their tops) supported the handle of the chain, and of course received the traces made at the feather-edged pieces of brass, terminating the beginning and ending of the hundred feet. Thus, there were in all six stands, intermediate to those in the center of each group that supported

the ninety-eight feet of coffering, which was kept so much short of the hundred feet, that its extreme parts might not rest upon, or even touch, the central stands. To that on the left of the center was attached the apparatus for the first or zero end of the chain; and to that on the right of the center was attached the apparatus for the last end of the chain. When the second chain length had been measured off, the first and sixth of the coffer stands of the first chain were moved forward to prepare for the third chain; and the four remaining coffer stands were raised, until their surfaces came into the same plane with the slide stands, for the reception of the glass rods. The space by which these stands were raised was about three inches; for so much higher was the surface of the intersole or flooring of the coffers than the stands which supported them.

The apparatus attached to the first end of the chain, or that which served to pull it back to the point of commencement, while a weight continued suspended at the farther end, consists of two parts, as may be seen by referring to the left-hand side of tab. XVII. First, a small wooden frame, fitted to slip on to the top of any one of the ordinary stands, placed immediately to the left of that which supports the handle. Secondly, a flat steel rod, about two feet in length, wherein a number of holes are pierced, about an inch asunder, for the reception of a steel pin placed in one of the holes, as best suits the distance of the stand from the handle. That end of the steel rod nearest to the end of the chain is formed into a screw about four inches in length, and it receives upon it a forked hook fitted to lay hold of the straight part of the handle of the chain. Within the forked hook there works a strong milled-headed brass nut, which acting upon the bottom of the fork, the chain is thereby pulled back, until the wire suspending the plummet

from

from the dart on the feather-edge coincides with the point of commencement on the ground underneath; for which purpose there is a hole in the top of the stand through which the wire passes. The apparatus stand, thus serving to pull back the chain, was commonly loaded with double weights, placed on the two hindermost legs.

The apparatus for the last end of the chain consists, like the former, of a small wooden frame that can be readily slipped upon any of the common stands, as may be seen by referring to the right-hand side of tab. XVII. This frame carries a pulley, over which a rope passes having fourteen pounds weight suspended at one end of it, while a forked iron hook at the other end lays hold of the straight part of the brass handle. By means of these two apparatuses the chain is always kept to the same degree of tension in its coffers, in each of which a thermometer was placed to indicate the temperature; the whole being covered up from the direct rays of the sun by a narrow piece of linen cloth, stretched along it from one end to the other.

Each coffer consisted of three boards about half an inch thick. The sides were about five inches deep, nailed at the middle to an intersole bottom of four inches, in such manner as to be represented in section by the letter H. They were ill made, being by their parallelogram shape apt to warp, which might have been prevented by giving them the figure of the cases of the glass rods, that is to say, making them wide in the middle and narrow at each end.

We are now to proceed to give some account of the double measurement with the chain and glass rods; wherein it must be remembered, as also in continuing the operation with the glass rods alone, that in referring to the map for the daily progress in the work, we are going from the forty-sixth towards the first station;

station; and in having recourse to the general table of the base, for altitude, temperature, or correction for expansion, we are ascending from the bottom towards the top, contrarily to the order in which the operation with the deal rods was conducted.

On the morning of the 18th of August, the stands with the various parts of the apparatus being placed in the manner just now described, the operation was begun by bringing the first end of the chain to coincide with the intersection on the tripod, answering to the end of the 1370th deal rod, and 4.31 feet distant from the center of the pipe terminating the north-west extremity of the base. The chain being stretched along its five coffers by the fourteen pounds weight suspended over the pulley at the farther end, and the temperatures of the five thermometers being registered in a book kept for that purpose, a fine trace was made on a piece of card fastened under the feather-edge at the farther handle, denoting the end of the first hundred feet. The chain being then moved on into the next five coffers, those that had been thus vacated were carried forward to prepare for the third chain length, and thereby permit the first set of stands to be elevated for the reception of the glass rods; and so in succession with the others.

In this manner we proceeded, and in the course of the day were only able to measure the length of ten chains, or 1000 feet, being the forty-sixth and forty-fifth hypothenuses of the base, the first of 400 and the last of 600 feet. Being arrived at this point it was found, that the fine line on the brass slide, marking the extremity of the tenth chain, fell short of another fine line on the same slide, denoting the end of the fiftieth glass rod, just two-tenths of an inch. Now it will appear hereafter, when we come to shew, by the experiments with the pyrometer, what the real contractions of the chain and

glass

glass rods were, for the degrees of difference of temperature * below that in which their respective lengths were laid off, that this small apparent difference of two-tenths of an inch, between the two modes of measuring the thousand feet, should have been 0.17938 in. to have made the two results exactly agree, which is a real difference of only 0.02062 of an inch. Supposing then every thousand feet of the base to have been measured by the chain with the same attention, and consequently with the same, or nearly the same success (and there surely cannot be any reason to doubt of the practicability) we shall have 27.404 × 0.02062 in. = 0.565 in. or a defect of something more than half an inch on the whole length of the base.

* When the length of the chain was laid off, the heat was 66°½, and that of the glass rods 68°. They will, therefore, only agree with each other accurately in these respective temperatures. The mean of twenty thermometers for the four chain lengths of the forty-sixth hypothenuse gave a heat of 61°.6; and for the six chain lengths of the forty-fifth, the mean of thirty thermometers gave 59°.75. The temperature of the 400 feet of glass by the mean of sixty thermometers was 65°.3; and of the 600 feet, by the mean of sixty thermometers, it was 60°.8. Now, from these data, and the expansions of steel and glass, as determined by the pyrometer, the computation will stand as follows:

				In.	In.	In.	
Steel	400	66.5° − 61.6°	= 4.9°	× 0.03052	= 0.14955	= 0.45856	contract. of 1000 feet.
	600	66.5° − 59.75°	= 6.75°	× 0.04578	= 0.30901		
Glass	400	68.0° − 65.3°	= 2.7°	× 0.02068	= 0.05584	= 0.27918	contract. of 1000 feet.
	600	68.0° − 60.8°	= 7.2°	× 0.03102	= 0.22334		

The 1000 feet of steel should have contracted more than the 1000 feet of glass, - - -	= 0.17938
But the difference was found to be - -	= 0.20000
Therefore the error of the chain in defect was -	0.02062 × 27.404 = 0.565 in. or little more than half an inch on the whole base.

So

So nice an agreement between two results, with instruments so very different, could not fail to be considered as astonishing; and as it rarely happens, that the graduation of thermometers will so nearly correspond with each other, as not to occasion a much greater error, all were very desirous that it could have been farther confirmed by continuing the operation in the same way through a more considerable proportion of the whole length. But besides the tedious nature of the double measurement, owing to the multiplicity of stands, platforms, coffers, and other articles, that were now successively to be moved forward, and for which purpose it had been found necessary to re-inforce the party of soldiers with six additional men; the operation had already trained out to a much more considerable length than had been expected; the summer was now far advanced, and the continuance of good weather uncertain; the coffers likewise for the chain, having been constructed in a hurry, were found to be defective: in short, all these reasons contributed to induce us to give up, for the present, any farther experiment with the chain, and to proceed with the glass rods alone in the completion of the measurement.

Accordingly, on Thursday the 19th of August, the operation with the glass rods was continued for the five hypothenuses, from the forty-fourth to the fortieth inclusive. It will be remembered, that in proceeding with the deal rods, double pickets had been placed in the ground, at the middle of the forty-first hypothenuse, or that point which terminated the 1215th rod, reckoning from the south-east, or the 155th from the north-west end of the base. Now, in returning to this point with the glass rods, the extremity of the 155th fell short of the silk thread stretched from picket to picket, just one-tenth of an inch. The expansion of the brass standard scale, and

that of glass being taken into the account, it appears, that the small expansion * of the deal rods from the humidity of the air, must, at this point, have exceeded what it was estimated at in the general table by 0.931 of an inch, supposing no error of any kind whatever to have arisen in the execution, from bringing the rods into contact, or otherwise.

On Saturday the 21st of August, the measurement was resumed at the thirty-ninth station, and continued for five hypothenuses to the thirty-fifth inclusive.

This day, about noon, HIS MAJESTY deigned to honour the operation by HIS presence, for the space of two hours, entering very minutely into the mode of conducting it, which met with HIS gracious approbation.

On Monday the 23d, the mensuration was farther continued for five hypothenuses, that is, to the thirtieth inclusive.

On Tuesday the 24th, we proceeded with the measurement for the space of seven hypothenuses, finishing the business of the day at the twenty-second station.

	In.	
* 155 deal rods = 3100 feet	+0.383	for 1° excess of temperature of the brass scale from 62° to 63°.
	+0.651	proportionable part of the estimated expansion from humidity.
	+1.034	equation of the deal rods on 3100 feet.
155 glass rods = 3100 feet	+2.301	for 6° excess of the heat of the brass scale from 62° to 68°.
	−0.436	observed contraction of the glass from the 11th and 12th columns of the table.
	+0.100	by which the 155th rod fell short of the thread.
	+1.965	equation of the glass rods on 3100 feet.
	0.931	Difference of the two equations, under-rated in the expansion of the deal rods.

It

It will be remembered, that in carrying on the operation with the deal rods, double pickets were left in the ground at the twenty-seventh station, answering to the extremity of the 810th rod from the first, or the 560th from the last end of the base. Now, on arrival at this point, the 560th glass rod overshot the silk thread, stretched from one picket to the other, 2.525 inches. Here again we find, that the lengthening * of the deal rods from the moisture of the atmosphere differs but little from what it has been estimated at by comparison with the standard, being over-rated only two-tenths of an inch on the 560 rods. In this day's operation, in passing the bridge laid over the old river, the measurement, instead of being made in the hypothenusal, was carried on in the level line, for the space of twenty rods, namely, fifteen rods of the twenty-seventh, and five of the twenty-sixth hypothenuse; which occasions the alteration in the reduction of these two spaces, marked with asterisks in the general table.

As some trouble had been found to attend the crossing of the great road, in the first measurement, owing to the number of carriages that were continually passing, the depth of

	In.	
* 560 deal rods = 11200 ft.	+ 1.390	for 1° excess of heat of the brass scale from 62° to 63°.
	+ 5.258	estimated expansion from moisture,
	+ 6.648	equation of the 560 deal rods.
560 glass rods = 11200 ft.	+ 8.343	for 6° excess of heat of the brass scale from 62° to 68°.
	+ 1.821	observed expansion of glass } from columns 11th and 12th.
	− 1.191	observed contraction of ditto } from columns 11th and 12th.
	− 2.525	over-shot the silk-thread.
	+ 6.448	equation of the 560 glass rods.
	0.200	Difference over-rated in the expansion of the 560 deal rods.

the ditches, and height of the banks of the old Roman way; therefore trestels, suited for the purpose, had been now prepared: and lest any accident might have happened in conducting this part of the operation, so as to oblige us to a repetition, double pickets were placed in the usual manner in the ground two rod lengths from the twenty-sixth station, to which we could have referred, without going back as far as the tripod left at the twenty-ninth station, the point from which we had departed in the morning.

Bad weather prevented any progress being made on the 25th; and, on the 26th, all that could be done was to measure the twenty-second and twenty-first hypothenuses.

On Friday the 27th, the work went on more expeditiously, having in the course of that day measured six hypothenuses, and placed the tripod at the fourteenth station.

On Saturday the 28th, eight hypothenuses were measured, and the tripod was placed at the sixth station. In this day's operation, being arrived near the bridge laid over Wolsey River, double pickets were placed in the ground in the point answering to the extremity of the 1172d rod, reckoning from the north-west, or the 198th rod from the south-east end of the base, that we might recur to them in case of accident; and the eighteen rod lengths, between this point and the sixth station, were measured on the level, instead of the hypothenusal line, which required the alteration of the reduction as distinguished by the asterisk in the general table.

On Monday the 30th of August, the measurement with the glass rods was completed *; when the extremity of the 1370th rod

* The gentlemen who were present at, and assisting in, the last day's operation were Captain Bisset, Mr. Greville, Sir William Hamilton, Mr. Lloyd, and

rod over-shot the center of the pipe terminating the base towards the south-east by 17.875 inches, or 1.49 foot. Hence, when the several equations for expansions are respectively taken into the account, we find, that the alteration of the deal rods from the humidity of the air, which, by comparison with the standard, was apparently most considerable in the first and second sections of the base, has now wholly vanished; that is to say, the total amount of it has been over-rated by 20.964 inches*; and this is the contradictory circumstance that has been formerly alluded to.

I have already suggested what appear to me to have been the only three possible causes of this difference, found between the estimated and real expansion of the deal rods; and as we are to abandon that measurement entirely, it is of little or no importance now to endeavour to discover, were it possible, whence it may have arisen. If any error was actually com-

and Dr. Usher, Professor of Astronomy in the College of Dublin. This last gentleman was so obliging as to observe, with the most scrupulous attention, throughout the whole operation with the glass rods, that the coincidence of the second with the first remained undisturbed, while that of the third with the second was completing.

	In.	
* 1370 deal rods = 27400 ft.	+ 3.389	for 1° of the brass scale from 62° to 63°.
	+ 24.223	estimated expansion from humidity.
	+ 27.612	equation of the 1370 deal rods.
1370 glass rods = 27400 ft.	+ 20.336	for 6° of the brass scale from 62° to 68°.
	+ 5.989	observed expansion of glass } from columns 11th and 12th.
	− 1.802	observed contraction of ditto } from columns 11th and 12th.
	− 17.875	space by which the 1370th rod over-shot the pipe.
	+ 6.648	equation of the 1370 glass rods.
	20.964	over-rated in the total expansion of the deal rods.

mitted,

mitted, which is the leaſt of all probable, it could only have happened at the place of the tripod, by bringing a wrong point of the ſtem over it when the operation was reſumed. But it is well known, how much care and pains were taken to prevent any thing of that ſort. Indeed the hypothenuſal diſtances, as given by the chain, agreed ſo nearly among themſelves, that even a foot or ten inches would have made ſo remarkable a difference in the ſituation of the next picket as could not have paſſed unobſerved. Beſides, in returning with the glaſs rods, after paſſing the Staines Road, the meaſurement was gradually found (without any leap whatever) to over-ſhoot the pickets, and at laſt over-reached the ſouth-eaſt pipe by 17.875 inches. I am therefore inclined to believe, that the difference ariſes partly from what may have been loſt by conſtantly butting one rod againſt the other, whereby the end of the 1370th did not reach ſo near to the north-weſt pipe as it ought to, and would have done, if the rods had been applied to each other by coincident lines. It muſt, however, be confeſſed, that the near agreement between the glaſs and deal rods in the upper part of the heath ſeems not perfectly reconcileable to this ſuppoſition. Nevertheleſs, the deſcent being quickeſt, and the irregularities of the ſurface much more conſiderable in the lower than the upper part, might produce ſome effect in one which did not take place in the other. But the chief part of the difference I take to have proceeded from over-rated expanſion; that is to ſay, the rods, when brought into uſe, contracted ſooner than we imagined, and thereby gave a ſhorter meaſure than what was aſſignable to them from the mean of any two or more compariſons.

The

The last day of August was employed in discharging the party, and removing the various parts of the apparatus to Spring-Grove House.

Description of the Microscopic Pyrometer, made use of for determining by experiment the expansion of the metals concerned in the measurement of the Base. Tab. XX.

Having, in the preceding part of this Paper, given a very minute account of the actual operations in the field, that the Public, being thus informed of every circumstance, might be the better enabled to judge of the accuracy of the result, it remains yet to point out, in what manner the equations for the expansions of the standard scale, steel chain, and glass rods, applied to the apparent measurement of the base, in several of the preceding notes, have been obtained by means of experiments with the pyrometer.

It is sufficiently well known, that many years ago, a very ingenious and valuable Member of this Society did publish in the Philosophical Transactions (vol. XLVIII. 1754, N° 79.) an account of experiments made with a pyrometer of his invention. No doubt was entertained of the accuracy of the experiments here alluded to; on the contrary, they will be confirmed by the account now to be given of these recently made, with which they very nearly agree. But as different pieces of metal of the same kind are certainly susceptible of different degrees of expansion, it was judged best, on the present occasion, to put rods to the test of those very metals that had been made use of in the actual measurement of the base.

For, ſuppoſing both ſets of experiments to have been made with inſtruments equally perfect, and to have been in other reſpects equally well conducted, this muſt always be conſidered as the moſt unexceptionable method. Beſides, the expanſion of rods of the length of five feet being aſcertained, the unavoidable error of obſervations of this delicate nature, becomes leſſened in proportion to the exceſs of their length above ſhorter rods. In theſe new experiments too, another ſort of pyrometer, invented by Mr. Ramsden, has been applied, of ſuch accurate conſtruction that it ſeems not eaſy to improve it.

The microſcopic pyrometer, ſo named becauſe, by means of two microſcopes attached to it, the expanſion is meaſured, conſiſts of a ſtrong deal frame five feet in length, nearly twenty-eight inches broad, and about forty-two inches in height. The elevation of the eye-piece ſide, or that which preſents itſelf to the obſerver, and alſo of the micrometer end, or that which is towards his right-hand, as well as the general plan of the top, are repreſented by a ſcale of one inch to a foot, or one-twelfth part of the real dimenſions, in tab. XX. where likewiſe may be ſeen the angular view of the fixed end, together with plans, ſections, and elevations, of ſeveral of the principal parts, done to larger ſcales. From theſe, it is hoped, the conſtruction of the machine will be eaſily underſtood, without entering into a minute deſcription of the almoſt numberleſs ſmaller parts whereof it is compoſed.

On the top of the frame, two deal troughs, upwards of five feet in length, are firmly ſcrewed. That towards the obſerver overhangs the frame ſomething more than an inch: that on the farther ſide is even with the back part. Each of theſe troughs, which are about three inches ſquare in the inſide, contains a caſt-iron ſtandard priſm, whoſe ſides are 1¼ inch.

The

The manner in which the prisms are fastened to the bottoms of their respective troughs, and the nature of the apparatuses they carry on their extremities, will be readily conceived, by referring to the particular plans and elevations of them, comprehended in the group of eight small figures towards the right-hand of the general plan. Four of these appertain to the left-hand or fixed microscope; and the other four to the right-hand or micrometer microscope, so distinguished because it has a micrometer attached to it. By means of the brass collars which embrace the prisms, their left-hand or fixed ends are screwed down extremely fast to the brass pieces whereon they rest, so as to be perfectly immoveable there with regard to their troughs; whereas their right-hand ends are kept easy, yet without shake, in their collars, that they may contract or lengthen freely as the temperature may require, without occasioning any strain upon the parts. The prism in the nearest trough may be called the eye-piece prism, because it carries the eye-pieces of the microscopes; and that in the farther trough, the mark prism, because it carries the marks or cross wires at which the microscopes respectively point. The troughs are covered with pitch in the inside, to make them hold water; and each has a cock in the left-hand end for discharging it.

Between the two deal troughs, one of copper, as a boiler, is placed, somewhat shorter than the former, but still upwards of five feet in length. It is about 2¾ inches broad, and 3½ in depth. The center of the boiler, or rather the center of the object lens which stands in it, as we shall have occasion soon to point out, is distant from the cross wires of the mark 5.81 inches; and from the wires of the micrometer attached to the corresponding eye-piece 20.33 inches. The boiler rests on five small rollers, one being fixed to each end of the frame, and

the other three to the braces which run acrofs it. This copper trough has likewife a cock in the left-hand end; and in the general plan a caft iron prifm is reprefented in it; but this laft carries no apparatus, as thofe in the wooden troughs do, being exactly of the length of five feet, and only placed there as one of the rods whofe expanfion was tried, and to fhew that the machine was capable of receiving a rod of that weight and magnitude.

By referring to the general plan it will be feen, that twelve lamps are made ufe of to bring the water in the copper to boil. They ftand on four fhelves, three in each compartment formed by the crofs braces of the frame. They can readily be pufhed forwards or drawn backwards, and when actually in ufe, their handles are only feen, projecting from under the copper. It was found, by burning oil in the lamps, the heat of the water could not be raifed above 209° or 210°; but with fpirits of wine it was brought into violent ebullition. The plan of the frame likewife fhews, that the tubes of the microfcopes are fub-divided into feveral diftinct parts; and that one of thefe parts is attached by a collar to a mahogany prifm, which reaches from one end to the other. But the ufe of thefe contrivances it will be beft to defer fpeaking of, till after having defcribed the apparatufes that are placed within the copper boiler.

At the bottom of the plate the boiler is reprefented, both in plan and longitudinal fection, to a fcale of one-fourth part of its real dimenfions. It contains within it two brafs flides, the one long and the other fhort; which, from the braces that bind the cheeks together, very much refemble the form of a ladder. The long flide, whofe cheeks are 1¾ inch deep, reaches almoft the whole length of the copper, although every where unconnected with it except at the points A and B. At

the first of these, two strong pieces of brass, fixed to the cheeks, and notched underneath, embrace the ends of a brass cylindrical bar fastened to the bottom. At the last, the cheeks of the slide rest on a roller. Whence it follows, that the copper and slide remain immoveable with regard to each other at A; but from thence, towards either end, they have full liberty to change place; that is to say, to expand by heat, or contract by cold, in any proportion their different natures may require. The left-hand end of the slide is shut up by a strong perpendicular piece of brass, connected with the two side rings which support the object lens of the fixed microscope, whose center corresponds accurately with its inward face. This piece being firmly screwed to the cheeks of the slide, and counter-arched outwardly, forms a strong butt for the fixed end of the expanding rod (supposed here to be the steel bar) to act against. Within the right-hand end of the long slide, rests a short one of about 14½ inches in length, whose cheeks are 1¼ inch deep. Its outward end, at C, rests on the cylindrical surface of the last brace of the long slide, fitted purposely to receive it; while a narrow longitudinal bar fixed in its inward end, at DE in the section, moves freely in the notch of a bridge F, framed for it in the long slide. The outward end of this short slide is shut up in a similar manner with the opposite end of the long one.

This end-piece is also connected with the two side rings which support the tube containing the object lens of the micrometer microscope, whose center is perpendicularly over its inward face, and being fortified outwardly by an edge bar, it forms a butt for the expanding end of the rod that is in experiment to push against. By attending to the plate it will be perceived, that to this end of the boiler a brass tube (R) is fixed, which contains within it a brass rod, surrounded by a

 helical

helical ſteel ſpring; which acting upon a broad ſhoulder of the rod prepared for the purpoſe, thereby preſſes its inward end, which enters the boiler, againſt the perpendicular ſurface of the end-piece of the ſhort ſlide. Thus, the farther end of the rod in experiment, ſuppoſed now to be in its contracted ſtate, is conſtantly made to bear againſt the ſurface that is under the fixed microſcope. But on the application of heat, the irreſiſtible force of expanſion in the rod obliges the ſpring to give way; the ſhort ſlide changes its place, and with it the object lens of the micrometer microſcope moves on a ſpace proportionable to the degree of heat that is applied; and it is this diſtance, meaſured by means of the micrometer, as hereafter will be ſhewn, that determines the quantity of expanſion, or the ſpace by which the rod has lengthened. From the plate it will be further obſerved, that the rod in experiment reſts on the ſurfaces of three rollers, about an inch in diameter; and by means of three pair of milled-headed nuts 1½ inch in diameter, which move on axes that are formed into ſcrews, until they almoſt touch the ſides of the rod, this is kept in its true centrical poſition, whatever may be its form or lateral dimenſions.

The microſcope towards the left-hand has been denominated fixed, becauſe it correſponds with the firſt or fixed end of the rod in experiment, and never changes its place while theſe are of the length of five feet. But it appearing to be of conſequence, that the expanſion of the ſtandard braſs ſcale, which is not quite forty-three inches long, ſhould be determined, the pyrometer has therefore been adapted for the reception of any rods leſs than five feet, whereby it is made more univerſally uſeful. For this purpoſe it becomes neceſſary to move the marks and eye-pieces of the fixed microſcope, along their reſpective priſms,

to

to the proper position for the rod that is to be tried. Nevertheless the object lens remains in its original place; and in its stead another lens, of the same focal distance, is fixed on a similar end-piece, that can be firmly clamped to any corresponding place whatever of the cheeks of the long slide. Hence will appear the reason for breaking the screening tubes of the microscopes into several parts, and the use of the mahogany prism, along which the thick part of the tube moves from one end to the other.

The pyrometer, since it was first made and tried, has undergone several small alterations, by way of improvements, which it is now unnecessary to describe particularly. One of these was the application of cross levels to the parts of the tube (SS in the general plan) connected with the object glasses. The manner in which they are fixed on will appear from the representations of them in the lowermost left-hand angle of the plate. And the section at the right-hand angle shews the appearance of the double brass hook, universal joint, and milled-headed nut, applied across the middle of the boiler (at TU) whereby the levels are brought to be consistent, when the water is boiling, with the position they had been adjusted to when the temperature was at freezing; that is to say, they are kept parallel to themselves in both states. This was thought necessary, because the application of the boiling water sunk the middle of the slide a small matter, and thereby made the levels run outwards.

The micrometer so often mentioned, being a very essential part of the machine, is represented both in elevation and horizontal section to the full size. Its chief parts consist of a micrometer steel screw, which works in the square nut of a brass slide, while the plane part of it enters into a long brass socket, nicely

nicely ground to receive it, and thereby preventing all ſhake. To the ſquare nut, one end of a watch chain is attached; the other end having paſſed around is fixed to a barrel, which contains a watch ſpring coiled up in the uſual manner. By this contrivance, any loſs of time in the motion of the moveable wire, fixed to the ſquare ſlide, is effectually prevented, whether the ſcrew be turned backwards or forwards. The fixed wire, ſo called becauſe it is only made uſe of occaſionally, appears in the elevation to the left-hand of the former, and is farther removed from the obſerver, being attached to the oval ſlide which bounds the field of the micrometer. This wire is moved by the inſertion of a milled-headed key (although not repreſented in the plate) fitted to ſlip upon the ſquare end of its proper ſcrew, which may be ſeen, in the elevation, projecting above the micrometer head. It has but little motion, being only intended for the meaſurement of ſmall differences of expanſion, or any ſmall ſpace, by leaving it there, while the other wire is repeatedly brought to coincide with, and again depart from it. For particular purpoſes this wire may be uſeful; nevertheleſs, the inſtrument would have performed very well without it.

The conſtruction of the microſcopes will be readily underſtood, by referring to the figures under that head on the right-hand ſide of the plate; where the relative ſituations of the different eye-glaſſes, with regard to the wires or place of the magnified image, as well as to the eye, are truly repreſented in their real dimenſions; but the diſtances from theſe to the object lenſes and marks reſpectively, are contracted or broken off, from want of ſufficient room to delineate them otherwiſe. To increaſe the angle of viſion in microſcopes, it is always neceſſary that they ſhould have at leaſt two eye-glaſſes, and the fixed microſcope in the plate ſhews them in their

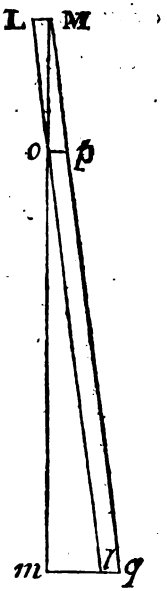
their usual position, the image from the object lens there being formed between the two, that the dispersion of rays in the first may be corrected by that of the second. But although this construction serves perfectly well every purpose of the fixed microscope, yet it could not answer in the moveable one, to which the micrometer is attached, where equal parts of an image, or their motion, are to be measured by the equable motion of the object lens, as shewn by the micrometer: for in that case, the interposition of an eye-glass before the image was formed, would not only have diminished its size, and thereby rendered the measure less accurate; but likewise, by refracting the oblique pencils more than those nearer the center, it would have destroyed the equality of the scale, and made equal parts of the object itself to have been represented unequally in the magnified image, and consequently erroneously measured by unequal parts of the micrometer. It was to remedy a defect of this sort that Mr. RAMSDEN proposed his new system of eye-glasses, described in the Philosophical Transactions, vol. LXXIII. 1783, N° 5. And he has here applied that system in the construction of the micrometer microscope; where it will be perceived, that both glasses stand between the eye and the image, whereby the greater magnitude of this last is obviously preserved, as well as the just similarity of all its parts to those of the object itself.

With regard to the scale of the pyrometer, it is, in the first place, to be observed, that the head of the micrometer screw, which is nine-tenths of an inch in diameter, is divided into fifty equal parts, each of which being reckoned two, it is therefore numbered to 100. Fifty-five revolutions of the head, being equal to 0.77175 of an inch, as measured with great accuracy by Mr. RAMSDEN's straight-line engine, it follows,

follows, that there are 71.27 threads of the ſcrew in an inch; that ſeven revolutions and nearly $\frac{13}{100}$th parts move the wire of the micrometer one-tenth of an inch; and that $\frac{1}{100}$th part of a revolution, or half a diviſion, anſwers to a motion of ſomething more than 0.00014 of an inch.

Having thus obtained the number of revolutions and parts of the micrometer (7.13) correſponding to one-tenth of an inch at the wires, it is ſufficiently obvious, that the number anſwering to one-tenth LM at the mark being likewiſe obtained, and added to the former, their ſum will give the meaſure of one-tenth at the object lens, or the ſpace by which the expanding rod has lengthened, as ſhewn by the motion of the lens from *o* to *p*. This meaſure of one-tenth of an inch at the mark was aſcertained in two different ways, and the reſults exactly agreed with each other. In the firſt place, a very thin ivory ſlide, whereon ſeveral twentieths of an inch were nicely divided by exceeding fine lines, was prepared, and made to move in the mark where the braſs ſlide now exiſts. A candle being then placed behind it at night, while the pyrometer ſtood within doors, and the micrometer wire being repeatedly moved by the ſcrew, its coincidence with the lines was diſtinctly ſeen through the ivory; whereby two of the ſpaces were found to be meaſured by 24.93 revolutions of the head. The ſecond method was, by means of two exceeding fine wires placed parallel to each other on the braſs ſlide, where they now remain, at the diſtance of one-twentieth of an inch on each ſide of the interſection wires, as may be ſeen by obſerving the real mark, or rather its magnified image, as ſhewn in the oval field of the micrometer, in the central figure of conſtruction. The revolutions of the micrometer anſwering to the diſtance between theſe parallel wires was, as before, found to be 24.93; which being

being added to 7.13, we have 32.06 for the number of revolutions measuring a motion of one-tenth at the object lens, or the expansion of one-tenth. In this manner Mr. RAMSDEN obtains the scale of his pyrometer in the easiest and most simple way imaginable, without any necessity for knowing the absolute distances of the object lens from the wires of the mark on one hand, and those of the micrometer on the other; distances not easily ascertained by actual measurement, on account of the position of that glass in its cell, which cannot conveniently be come at. Thus, in tab. XX. as well as in the annexed figure, LM being the object at the distance of the mark, equal to one-tenth of an inch; then *ml* will be its magnified image, in proportion to the former as *mo* is to *o*M. And, if through the point *p*, the place to which the object lens *o* has been carried by the motion of the expanding rod, a line M*q* be drawn parallel to L*l*, we shall have $ml = 24.93 + lq = 7.13 = mq = 32.06$, the number of revolutions of the micrometer measuring *op* the expansion. Having thus obtained the total number of revolutions corresponding to *mq*; and having likewise measured the total distance *m*M = 26.144 inches, a space easily ascertained between the wires of the micrometer and those of the mark, the partial distances *mo* and *o*M may then be readily found by computation: for $mq : ml :: mM : mo = 20.33$ inches; and $mq : mM :: op : oM = 5.814$ inches.



In order to finish the description of the pyrometer, it is only necessary to observe farther, that the circular scale, seen in the elevation of the micrometer, whose zero appears

pears to coincide with the dart on the plane part of the brass, is that which serves by its motion to register the turns of the head. A forked key, fitted to enter the holes near the circumference of the circle, is made use of for the adjustment of this zero. The circle should never be turned backwards or towards the left, lest the watch chain should thereby be thrown off the barrel, but always forwards or towards the right, even if it should be necessary to move it almost an entire revolution. The zero of the head is that which should be first brought to correspond with its proper dart. They may be seen to coincide in the horizontal section of the micrometer; and the departure of zero from this dart, indicates, by the number of divisions that are intercepted, the value of any fractional part of a revolution.

Account of the experiments with the Pyrometer.

Although the instrument which I have here endeavoured to describe was begun early in the winter of 1784, yet it was not finished till the beginning of last April; at which time it was brought to Argyll-Street, and being placed truly level on the stone pavement of the yard, was covered with an oil-cloth canopy, that the experiments might not be interrupted by rainy weather.

To fill the three troughs completely it required from twenty-five to thirty pounds of pounded ice, which was always put in with great care, so as to apply as compactly as possible to the standard prisms and rod respectively, with but little common water *

* When common water was used, although not in any very considerable proportion, the thermometer kept always half, and sometimes three quarters of a degree above 32°.

at

at first added; it having been found in these experiments, that ice water only, such as drains from the ice itself, is that which should properly be made use of to mix with the pounded ice, in order to bring the whole mass to the true freezing temperature. Being at the commencement uncertain what time might be necessary for the rods, especially when of so large a size as the standard prisms, to acquire the just temperature of freezing, at first the ice was put into the troughs over night, to prepare for the continuation of the experiment next morning. But after many repeated trials, this precaution was found to be needless; a quarter of an hour being more than sufficient to give to all the freezing temperature, as well as to render the lens on the expanding rod stationary, after the water supplying the place of the ice had been brought fairly to boil.

The instrument, in its first state, having in some cases made the expansion appear to be progressive, and not equable; therefore its rate was attempted to be ascertained by noting the progression answering to 60°, 120°, and 180° above freezing. But when the instrument was rendered perfect, and that no sensible difference was found between the expansion at the lower and that at the upper part of the scale, a fair mean being taken between its ascending and descending rates, and allowing for the difficulty of keeping the water, for any length of time, precisely to the same intermediate heat; then this tedious mode of conducting the experiments was given up, and the expansion for 180° was at once determined by bringing the water to boil around that rod, which but a little before had been lying in melting ice, and which the standard prisms still continued to do throughout each experiment, care being taken to have a supply of pounded ice always ready to keep these two troughs quite full.

Two obſervers are neceſſary for the effectual application of the pyrometer. He who obſerves with the fixed microſcope, takes care that its object lens is kept in its true place, that is to ſay, that the wire in the eye-piece accurately biſects the interſection wires of the mark. This he is enabled to do by means of the apparatus attached to the fixed end of the boiler, as will be beſt conceived by obſerving the plan (at WX) along with the elevation of that end placed near it. The apparatus conſiſts of two milled-headed ſcrews, working in braſs plates faſtened to the end of the frame, and acting againſt a ſmall cock which projects from the lower part of the boiler, whereby this laſt receives ſuch longitudinal motion to and fro on its rollers, as is ſufficient for the adjuſtment of the lens. He who obſerves with the micrometer microſcope, having brought the zero of the micrometer head to its dart, as ſhewn in the horizontal ſection, and alſo the revolution zero to its dart, as repreſented in the elevation, takes care, when the rod has acquired the freezing temperature, that the micrometer wire biſects the interſection wires of its proper mark. This he effects by working with the milled-headed ſcrew, repreſented in the plan and elevation of that mark, whereby the mark itſelf is moved until the biſection is accurate; and during the whole of this time, the firſt obſerver muſt be extremely attentive to keep his lens adjuſted.

One aſſiſtant at leaſt is neceſſary, who takes his ſtation on the oppoſite ſide of the pyrometer, to obſerve the levels, and keep them adjuſted, by means of the double hook applied near the middle of the boiler, and repreſented in the ſection on the line TU, at the lowermoſt right-hand angle of the plate.

The pyrometer having been adjuſted in the manner here deſcribed, by giving ſufficient time for the ſtandard priſms and

rod to contract to the true freezing temperature, as was easily known by the wires becoming perfectly fixed and stationary with regard to the marks; the ice was then removed from the copper trough; and the same being filled with water nearly on the boil, the ebullition was completed, and kept up, by means of the lamps now lighted for the purpose, and slipped in underneath.

The expansion, answering to the 180° between freezing and boiling, was now measured by working with the micrometer screw until the bisection * of its wire with those of the mark was again complete; the observer at the fixed microscope taking also especial care all the while to keep his bisection perfectly accurate. The number of revolutions, registered by the number of entire divisions that the zero of the circular scale had departed from its dart or index, and also the value of any fractional revolution, registered by the divisions on the head intercepted between zero and its proper dart, were then noted, as expressed in the first column of the subjoined table of experiments; which requires no other explanation than what is therein inserted, and which has been extended purposely to shew at one view, from inspection only, how much the length of our base would have been affected, if measured by these metals respectively, in temperatures between 32° and 62°.

All the experiments were repeated at least twice, and some of them three times, except the standard scale and glass pen-

* This bisection of the wires may always be made to a great degree of precision, by one with a tolerably good eye, and accustomed to observations of this sort. I have myself repeatedly adjusted the wires eight or ten times running, allowing another person to read off and unadjust each time, without the mean difference exceeding one-fourth of a division of the head, which is only $\frac{1}{64000}$th part of an inch.

dulum

dulum rod, whose expansions were only tried once. The difference of a few divisions between the mean and extremes on the heat of 180° being, in things of this sort, of no importance, it was judged wholly unnecessary to aim at a greater degree of precision in repeating them oftener. By referring to the table, particularly that column containing the expansions on one foot by 180°, it will be perceived, that they are uniformly a small matter less than what has been assigned to the same metals respectively, in the experiments formerly alluded to.

Ultimate determination of the length of the Base on Hounslow-Heath.

In the former part of this paper, we have had occasion to speak of the seven first columns of the general table of the base; and the titles at the tops of the others respectively serve sufficiently to explain those towards the right-hand; the expansion of glass above, and its contraction below 62°, contained in the eleventh and twelfth columns, being deduced from the recent experiments with the pyrometer.

	Feet.
The hypothenusal length of the base, as measured by 1369.925521 glass rods of twenty feet each + 4.31 feet, being the distance between the last rod and the center of the north-west pipe, has been shewn to be - - -	27402.8204
The reduction contained in the seventh column of the general table to be deducted is : .	0.0714
Hence the apparent length of the base, reduced to the level of the south-east extremity, becomes	27402.7490

The

	Feet.
The apparent length is to be augmented by the excess of the expansion above the contraction of the glass rods, contained in the thirteenth column of the general table = 4.1867 inches, reduced to the heat of 62°, as has been usually done in former operations of this nature	0.3489
The apparent length is farther to be augmented by the equation for 6° difference of temperature of the standard brass scale between 62° and 68°, this last being the heat in which the lengths of the glass rods were laid off = 20.3352 inches, as deduced from the experiments with the pyrometer	1.6946
Hence we have the correct length of the base in the temperature of 62° reduced to the level of the lowermost extremity near Hampton Poor-house,	27404.7925
This last length requires yet a small reduction for the height of this lowermost end above the mean level of the sea, supposed to be fifty-four feet, or nine fathoms, ...	0.0706
Hence the true or ultimate length of the base, reduced to the level of the sea, and making a portion of the mean circumference of the earth, becomes	27404.7219

As some small degree of uncertainty remains with regard to this last reduction, it may not be improper to say yet a few words on the principles that have been adhered to in making the computation. It will be remembered, that the measurement was made 3½ feet above the surface of the heath, that being the height of the stands whereon the rods were placed; and

and that the telescopic spirit level gave a descent of 36.1 feet from the lowermost pipe to the surface of summer water in the Thames at Hampton. The accurate section of the river lately published, gives a fall of 13.33 feet from Hampton to the level of low water spring tides at Isleworth. Now these three being added together, we have nearly fifty-three feet for the height of the base above Isleworth. Having had no immediate means of determining what real difference there may be between Isleworth and low water spring tides at the mouth of the Thames (for instance at the Hope or the Nore), I have supposed that fall to be about seven feet, so as to make the total descent sixty feet. Now, supposing the spring tides at the Nore to rise eighteen feet, if, according to M. DE LA LANDE's method, we deduct one-third of eighteen, *viz.* six feet from sixty, we shall have fifty-four feet, or nine fathoms, that the mean surface of the sea is below the measured base. Whether this conclusion be perfectly accurate or not is of no moment, since a whole fathom of difference (and I apprehend we are not farther from the truth) does not vary the reduction quite one-tenth of an inch. The reduced base has therefore been found by the following analogy: as the mean semi-diameter of the earth (supposed here to be 3492915 fathoms) augmented by nine fathoms, is to the mean semi-diameter, so is the measured base 27404.7925 to the reduced base 27404.7219 at the level of the sea. It will doubtless be allowed, that infinite pains have been taken in the field and otherwise, throughout the whole of this operation, to obtain a just conclusion; but as the most accurate measurement imaginable is still more liable to err in excess than in defect, we will throw away some useless decimals, and establish the ultimate length of the base at 27404 feet and seven-tenths.

General

General Table of the Base, shewing the relative heights of the Stations above the Poor-house, the Reduction of the Hypothenuses, and the Correction for the whence the true length is obtained in the heat of 62°.

1	2	3	4	5	6	7	8	9	10	
Sections.	Hypothenuses, each.	Relative heights.				Reduction	Number of rods.	Temperature.		
				Total ascent.						
Entire part of the side of the Coinb	43	1.505	[illegible]	[illegible]		0.002049	30	56.2	− 3.8	
	44	1.485	− −	29.765		0.001845	30	57.3	− 4.7	
	45	0.24	− −	30.005		0.000055	30	60.8	− 1.2	
	46	1.26	− −	31.265	12.130	0.001973	20	65.3	+ 3.3	+0
	400 ft.									
		40.44	9.175	31.265	31.265	0.071401	1369+ .925521			+5

Hypothenusal length of the base containing 1369.925521 glass rods of twenty feet each + 4.31 feet,
Reduction contained in the seventh column to be subtracted, - - - -

Total apparent length of the base reduced to the level of the south-east extremity, - -
Add to the apparent length the difference between the expansion of glass above, and the contrac contained in the thirteenth column = 4.1867 inches = - - - -
Add further to the apparent length the equation for 6° difference of temperature of the standard brass f 68°, the heat in which the glass rods were laid off = 20.3352 inches, = - - - -

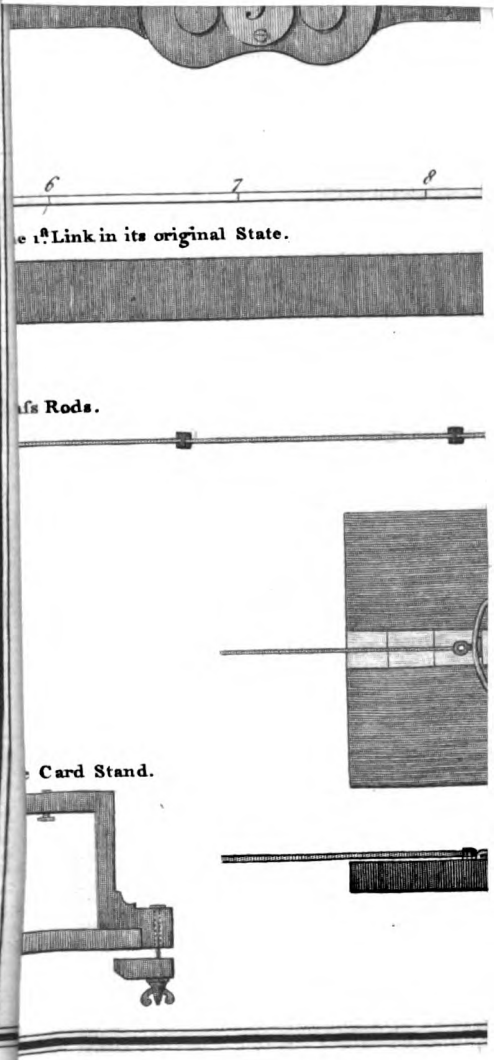
Correct length of the base in the temperature of 62°, reduced to the level of the lowermost extremity,
Reduction for the height of the lower end of the base above the mean level of the sea, supposed to be

True length of the base reduced to the mean level of the sea, - - -

and that the telescopic spirit level gave a descent of 36.1 feet from the lowermost pipe to the surface of summer water in the

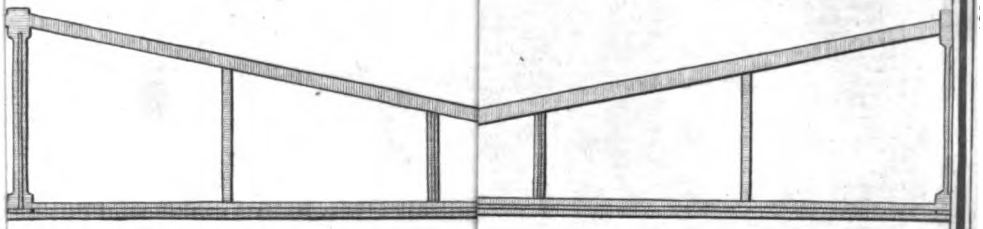
meter in April 1785.

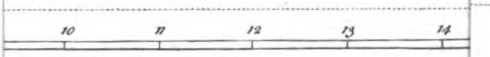
Description of the M		panſion is			Baſes of 27400 feet of theſe metals would expand.			
		et.	600 feet.	1000 ft.	By 1°.	By 10°.	By 20°.	By 30°.
			In.	In.	In.	In.	In.	In.
Standard braſs ſcale.	Suppoſed 42.187 incl inch; thick 10½ oz. It revolutions on five feet	8	0.07422	0.1237	3.38938	33.8938	67.7876	101.6814
Engliſh plate braſs, in form of a rod.	Length fi neſs 0.15 Difficult fro warping,	8	0.07572	0.1262	3.45788	34.5788	69.1576	103.7364
Engliſh plate braſs, in form of a trough.	Length fi inch; weigh ſtraight,	52	0.07578	0.1263	3.46062	34.6062	69.2124	103.8168
Steel rod.	Length fi 0.3 inch; very ſame b	52	0.04578	0.0763	2.09062	20.9062	41.8124	62.7186
Caſt iron priſm.	Length fi and weight rod with th	50	0.04440	0.0740	2.02760	20.2760	40.5520	60.8280
Glaſs tube.	Length weight 1 lb pot of meta	58	0.03102	0.0517	1.41658	14.1658	28.3316	42.4974
Solid glaſs rod.	Length 4 meter ſix-te 2 oz. It ha clock. Its revolutions on five feet	56	0.03234	0.0539	1.47686	14.7686	29.5372	44.3058



ICATION of the DEAL RODS

Lengths, and method of tracing the Line





middle Rod.

XXV. *Abstract of a Register of the Barometer, Thermometer, and Rain, at* Lyndon, *in* Rutland, 1784. *By* Thomas Barker, *Esq. Also of the Rain at* South Lambeth, Surrey; *and at* Selbourn *and* Fyfield, Hampshire. *Communicated by* Thomas White, *Esq. F.R.S.*

Read June 16, 1785.

		Barometer.			Thermometer.						Rain.			
					In the House.			Abroad.			Lyndon	S. Lambeth, Surrey.	Selbourn, Hants.	Fyfield.
		Highest	Lowest	Mean.	High.	Low.	Mean	High.	Low.	Mean				
		Inches.	Inches.	Inches.	°	°	°	°	°	°	Inch.	Inch.	Inch.	Inch.
Jan.	Morn.	29,96	28,49	29,34	40½	28	33½	40	15½	27	1,877	2,54	3,18	2,44
	Aftern.				42½	29	34	48½	24½	32½				
Feb.	Morn.	30,00	28,50	29,23	47	30	36	45	9	29	1,225	1,49	0,77	1, 7
	Aftern.				47½	31	37	52½	23	36				
Mar.	Morn.	29,63	28,59	29,23	47½	36	39½	45	21	32½	1,096	2,63	3,82	2,24
	Aftern.				48½	36	40½	52	33½	40½				
Apr.	Morn.	29,74	28,44	29,26	50½	35½	43½	51	29	38½	1,741	2,56	3,92	2,10
	Aftern.				53	36	45	60½	35	48				
May	Morn.	29,92	29,17	29,62	66½	47	57½	63	41	52	2,890	1,36	1,52	1,57
	Aftern.				69½	48	59½	78	48½	65				
June	Morn.	29,92	28,98	29,43	62	55½	58	61½	48	54	3,810	3,45	3,65	2,45
	Aftern.				63½	56½	59½	71	53	63½				
July	Morn.	29,85	28,74	29,48	69	56	61	66	51	56	5,080	2,26	2,40	2,80
	Aftern.				72	57	63	79½	57½	67				
Aug.	Morn.	29,92	29,04	29,56	65	54	59	60½	42½	52	2,814	2,84	3,88	2,79
	Aftern.				67	55	60	71½	51	63				
Sept.	Morn.	29,90	29,01	29,55	66½	53	61	57	39	52	1,740	1,65	2,51	2, 7
	Aftern.				71½	54	63	73½	51½	64				
Oct.	Morn.	30,00	28,98	29,62	52½	42	48½	45	27½	39½	0,223	0,83	0,39	0,17
	Aftern.				53½	43	49½	55½	40	49				
Nov.	Morn.	29,85	28,75	29,38	51	39½	45	51½	23½	38	2,376	5,60	4,70	3,14
	Aftern.				51	41	45½	53	33½	44				
Dec.	Morn.	29,75	28,15	29,26	43½	31½	36½	41	13½	29	2,335		3,06	1,72
	Aftern.				43	32½	37½	46½	19	32½				
											Inches 27,207	27,21	33,80	24,56

The froſt, which began at Chriſtmas laſt year, continued, except a few thawing days, till February 21, and was very ſevere, and with frequent ſnow. Some thought it the hardeſt winter ſince 1740; but that may be hard to determine. January 1776 ſeems to have been rather colder than this; but the froſt laſted only a month. In 1780 the froſt was not out of the ground for nine or ten weeks; but it was not ſo ſteady as this. It was certainly one of the mildeſt winters before Chriſtmas, and one of the ſevereſt after it; yet the corn and other plants did not ſuffer ſo much as might be feared. After the froſt there was ſome warm, windy, ſhowery weather; but moſt part of March was froſty mornings, and was often ſo in the ſhade all day; and at the latter end ſtrong cold winds, with ſnow and perfect winter; and it did not much mend till near the middle of April.

While in January and February moſt parts of Europe had ſevere froſt, the ſouthern parts of it ſeem to have had great ſtorms and floods; and, at the breaking of the froſt, the flat countries by the ſides of the great rivers of Europe ſuffered much by floods and ice.

The latter half of April the weather mended, and things came on gradually, yet with frequent froſty mornings till the firſt week in May; then, for three weeks, one of the fineſt and hotteſt Mays ever known; every thing before was exceeding backward, but now came on at a vaſt rate; the graſs and leaves were remarkably green, a great bloſſom year, and plenty of fruit. This hot weather brought up thunder, which turned the weather wet near the end of May, and it was wet or ſhowery and cool all June; this brought on the corn again, which was made rather thin by ſo much heat too early. Near the firſt two thirds of July was again fine and hot, and being in

in the height of hay time, a good deal of it was well made; but ſome of the firſt cut, and the latter was caught in the wet; for after the 19th it was ſhowery or wet, and the 30th and 31ſt were, all over Leiceſterſhire, Rutland, and part of Lincolnſhire, the greateſt flood ſince July 1736; and it continued wet and cool all Auguſt, ſo that the ſummer was in general cold, wet, and backward, yet with ſome very fine fits in it. The harveſt began but indifferently; but being late this year, and the weather wet, not much was carried before September, when, in about three weeks calm, hot, and dry weather, yet with vaſt dews, moſt of the white corn was well got in this country; but ſome of the peaſe, and, where it was earlier and later, ſome of the white corn was carried damp, for the end of September was again wet.

The autumn was various; a dry and fine October, toward the end of it ſharp froſty mornings; a ſhowery November, with a ſharp froſt in the middle, yet often pleaſant; and after December 5th, a conſiderable ſnow (in ſome countries it was very great) and a ſeverer froſt than is uſual before Chriſtmas laſted till into January.

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