XV. *Rules and principles for determining the dispersive ratio of glass; and for computing the radii of curvature for achromatic object-glasses, submitted to the test of experiment.* By PETER BARLOW, *Esq. F. R. S. Mem. Imp. Ac. Petrop. &c.*

Read May 3, 1827.

1. IT is very remarkable, since the achromatic telescope is altogether of English origin, that in no one of our separate optical treatises are to be found specific rules for its construction, fitted for the use of practical opticians. Some essays of this kind have indeed been attempted; the first of which is found in MARTIN'S "New Elements of Optics," published in 1751; but the principle there adopted is erroneous, and of course the deductions, although possessing a great appearance of simplicity, are wholly useless. Under the article Telescope, in the Encyclopædia Britannica, is another essay of this kind, which is correct in principle, but far from possessing the degree of simplicity which is desirable for practical purposes.

Under the like article in Rees's Cyclopædia is a treatise on the same subject, which may be considered wholly practical; it is founded however upon MARTIN'S method, but corrected by an empyrical multiplier, which answers remarkably well in many instances, but is erroneous in all extraordinary cases.

Lastly, an elaborate and highly scientific investigation relative to these constructions was published by Mr. HERSCHEL, in the Phil. Trans. for 1821, to which I shall refer more at length in a subsequent page. These, I believe, constitute every attempt that has been made in this country to bring the strict laws of optics, applicable to these cases, within the reach of numerical calculation.*

More numerous attempts have been made by foreign mathematicians; but as far as my knowledge of them extends, they have in no instance been attended with the success that might have been expected from the deservedly high reputation of their authors.

I have spoken above principally of the methods of determining the radii of curvature of the lenses; but in order to enter upon this calculation, certain data are necessary, which require previous experiments and tedious numerical computations; so that upon the whole, to take two specimens of glass of unknown indices and dispersions, to form an object glass of them, free from colour and spherical aberration, requires very formidable calculations, involving in them, according to the best methods yet employed, certain principles and operations which we ought hardly to expect practical opticians to be masters of. At all events, every simplification that can be thrown into experiments and calculations of this kind must be desirable; and, I am greatly in hopes it will be found that I have, in the following pages, contributed

* Since this Paper was written, Mr. Herschel has also published in the Encyclopædia Metropolitana, under the article Light, a still more extended investigation relative to this and other optical subjects; to which article it will likewise be necessary for me to refer as we proceed; and if, after all, any reference should be omitted which ought to be made, it must be attributed to this Paper having been written before the publication of the former.

a little towards this object. Probably, also, the immediate comparison of the computed results, with experiments on a large scale, will add a value to this Paper, which it might not otherwise have been thought to possess, and for which I am indebted to Messrs. W. and T. GILBERT, who very liberally engaged to submit to the test of experiments any theoretical deductions I might be led to in an investigation of these subjects.

On the determination of the index of refraction.

2. The following method of determining the index of refraction, by means of a lens, is not given as new; it has, on the contrary, been long practised; but as it forms the foundation of the method for determining the dispersive ratio, and will occupy but a few lines, I shall be excused for introducing it into this Paper.

It is simply this :—since by knowing the radii of curvature of a lens, and its index of refraction, we may compute the focal length; so conversely, by knowing the radii and measuring the focal length, we may compute the index of refraction.

The method which we employed for measuring the focal length of a lens, was as follows: a tube about $2\frac{1}{2}$ inches in diameter, and which exactly measured 10 inches from the back of the lens to its other extremity, was fitted with a draw tube of the same length, graduated to inches and tenths, and which, by means of a vernier, might be read to the hundredth of an inch. This was fitted with a positive eye-piece, which was adjustable to bring the cross wires exactly into its focus, and the graduations above-named commenced from this

XXI. *An Account of the Meteorological Inſtruments uſed at the* Royal Society's *Houſe. By the Hon.* Henry Cavendiſh, *F. R. S.*

R. March 14, 1776.

Of the thermometers, with reflections concerning ſome precautions neceſſary to be uſed in making experiments with thoſe inſtruments, and in adjuſting their fixed points.

THE thermometers are both adjuſted to FAHRENHEIT's ſcale: that without doors is placed out of a two-pair-of-ſtairs window, looking to the North, and ſtands about two or three inches from the wall, that it may be the more expoſed to the air, and the leſs affected by the heat and cold of the houſe. The ſituation is tolerably airy, as neither the buildings oppoſite to it, nor thoſe on each ſide, are elevated above it in an angle of more than 12°; but as the oppoſite building is only twenty-five feet diſtant, perhaps the heat may be a little increaſed at the time of the afternoon obſervation by the reflection from thence. In the middle of ſummer the Sun ſhines on the wall of the houſe, againſt which the thermometer is fixed, for an hour or two before the morning obſervation, but never

never ſhines on the thermometer itſelf, or that part of the wall cloſe to it, except in the afternoon, long after the time of obſerving. On the whole, the ſituation is not altogether ſuch as could be wiſhed, but is the beſt the houſe afforded.

The thermometer within doors is intended chiefly for correcting the heights of the barometer, and is therefore placed cloſe to it. The room in which it is kept looks to the North, and has ſometimes a fire in it, but not often.

It has been too common a cuſtom, both in making experiments with thermometers and in adjuſting their fixed points, to pay no regard to the heat of that part of the quickſilver which is contained in the tube, though this is a circumſtance which ought by no means to be diſregarded; for a thermometer, dipped into a liquor of the heat of boiling water, will ſtand at leaſt 2° higher, if it is immerſed to ſuch a depth that the quickſilver in the tube is heated to the ſame degree as that in the ball, than if it is immerſed no lower than the freezing point, and the reſt of the tube is not much warmer than the air. The only accurate method is, to take care that all parts of the quickſilver ſhould be heated equally. For this reaſon, in trying the heat of liquors much hotter or colder than the air, the thermometer ought, if poſſible, to be immerſed almoſt as far as to the top of the column of quickſilver in the tube. As this, however, would frequently be attended with great inconvenience, the obſerver will often be obliged to content himſelf with immerſing it to a much leſs depth; but then, as the quickſilver in a great part of the

the tube will be of a different heat from that in the ball, it will be neceſſary to apply a correction on that account to the heat ſhewn by the thermometer; to facilitate which the following table is given, in which the upper horizontal line is the length of the column of quickſilver contained in that part of the tube which is not immerſed in the liquor expreſſed in degrees; the firſt perpendicular column is the ſuppoſed difference of heat of the quickſilver in that part of the tube and in the ball; and the correſponding numbers in the table ſhew how much higher or lower the thermometer ſtands than it ought to do. The foundation on which the table is computed is, that quickſilver expands one 11500th part of its bulk by each degree of heat.

Diff. of Heat	Degrees not immerſed in the liquors.														
	50	100	150	200	250	300	350	400	450	500	550	600	650	700	750
50	,2	,4	,7	,9	1,1	1,3	1,5	1,7	2.	2,2	2,4	2,6	2,8	3,1	3,3
100	4,	,9	1,3	1,8	2,2	2,6	3,0	3,5	3,9	4,4	4,8	5,2	5,7	6,1	6,6
150	7,	1,3	2,0	2,6	3,3	3,8	4,6	5,2	5,9	6,5	7,2	7,9	8,4	9,2	9,8
200	9,	1,8	2,6	3,5	4,4	5,1	6,1	7,0	7,8	8,7	9,6	10	11	12	13
250	1,1	2,2	3,3	4,4	5,5	6,4	7,6	8,7	9,8	11	12	13	14	15	16
300	1,3	2,6	3,8	5,1	6,4	7,7	9,1	10	12	13	14	16	17	18	20
350	1,5	3,0	4,6	6,1	7,6	9,1	11	12	14	15	17	18	20	21	23
400	1,7	3,5	5,2	7,0	8,7	10	12	14	16	17	19	21	23	24	26
450	2.	3,9	5,9	7,8	9,8	12	14	16	18	20	22	24	25	27	29
500	2,2	4,4	6,5	8,7	11	13	15	17	20	22	24	26	28	31	33
550	2,4	4,8	7,2	9,6	12	14	17	19	22	24	26	29	31	34	36

But as the generality of obſervers will be apt to neglect this correction, it would be proper to form

two

two ſets of diviſions on ſuch thermometers as are intended for trying the heat of liquors; one of which ſhould be uſed when the tube is immerſed almoſt to the top of the column of quickſilver; and the other, when not much more than the ball is immerſed; in which laſt caſe the obſerver ſhould be careful, that the tube ſhould be as little heated by the ſteam of the liquor as poſſible. It muſt be obſerved, however, that the heat of the liquor may be eſtimated with much more accuracy by the firſt ſet of diviſions, with the help of the correction, than it can by the ſecond ſet, as the latter method is juſt only in one particular heat of the atmoſphere, namely, that to which the diviſions are adapted; but, if they are adapted to the mean heat of the climate for which the thermometer is intended, the error can never be very great, and, when the liquor is much hotter or colder than the air of that climate ever is, will be much leſs than if the firſt ſet of diviſions were uſed without any correction; but, when the liquor is within the limits of the heat of the atmoſphere, greater accuracy will ſometimes be obtained by uſing the firſt ſet of diviſions than the ſecond, for which reaſon the latter ſet ſhould not be continued within thoſe limits. I would willingly have given rules for the conſtruction of this ſecond ſet of diviſions, but am obliged to omit it, as it cannot be done properly without firſt determining, by experiment, how much the quickſilver in the tube is heated by immerſing the ball in hot liquors.

In a ſpirit thermometer, the error proceeding from the fluid in the tube being not of the ſame heat as that in the

the ball, is much greater; as ſpirits of wine expand much more by heat than quickſilver: for which reaſon ſpirit thermometers are not ſo proper for trying the heat of liquors as thoſe of quickſilver.

Another circumſtance which ought to be attended to in adjuſting the boiling point of a thermometer is, that the ball ſhould not be immerſed deep in the water; for, if it is, the fluid which ſurrounds it will be compreſſed by conſiderably more than the weight of the atmoſphere, and will therefore acquire a ſenſibly greater heat than it would otherwiſe do. The moſt convenient veſſel I know for adjuſting the boiling point is repreſented in fig. 1. ABCD is the veſſel; AB the cover, made to take on and off readily; E a chimney to carry off the ſteam; FG the thermometer, paſſed through a hole M*m* in the cover, and reſting in a little bag faſtened to the wire HK, intended to prevent the ball from being broken by accidentally falling to the bottom. This wire is made ſo as to be raiſed higher or lower at pleaſure, and muſt be placed at ſuch a height that the boiling point ſhall riſe very little above the cover. The hole M*m* is ſtopped with bits of cork or tow. By this means, as the tube is incloſed in a veſſel intirely filled with the ſteam of boiling water, the quickſilver in it is heated to the ſame degree as that in the ball; and beſides, that part of the tube, on which the boiling point is to be placed, is defended from the vapour, ſo that it is eaſy making a mark on the glaſs with ink. If ſuch a veſſel as this is uſed, the thermometer will be found to ſtand not ſenſibly higher

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when the water boils vehemently than when it boils gently; and if the mouth of the chimney is covered by any light body, in ſuch manner as to leave no more paſſage for the ſteam than what is neceſſary to prevent the body from being blown off by the preſſure of the included vapour, the thermometer will ſtand only half or three quarters of a degree higher, if the ball is immerſed a little way in the water, than if it is expoſed only to the ſteam. But if the covering of the chimney is removed, the thermometer will immediately ſink ſeveral degrees, when the ball is expoſed only to the ſteam, at leaſt if the cover does not fit cloſe; whereas when the ball is immerſed in the water, the removal of the covering has ſcarce any effect upon it. Whence it appears, that the ſteam of water boiling in a veſſel, from which the air is perfectly excluded, is a little but not much cooler than the water itſelf, but is conſiderably ſo if the air has the leaſt admiſſion to the veſſel. Perhaps a ſtill more convenient method of adjuſting the boiling point would be not to immerſe the ball in the water at all, but to expoſe it only to the ſteam, as thereby the trouble of keeping the water in the veſſel to the right depth would be avoided; and beſides, ſeveral thermometers might be adjuſted at the ſame time, which cannot be done with proper accuracy when they are immerſed in the water, unleſs the diſtance of the boiling point from the ball is nearly the ſame in all of them. At preſent there is ſo little uniformity obſerved in the manner of adjuſting thermometers, that the boiling point, in inſtruments made by our beſt artiſts, differ from one another by not leſs than $2\frac{1}{2}°$; owing partly to a difference in the height of the barometer at which they were adjuſted, and partly to the quickſilver in the tube being more heated in the method uſed by ſome perſons than in that uſed by others. It is very much to be wiſhed, therefore, that ſome means were uſed to eſtabliſh an uniform method of proceeding; and there are none which ſeem more proper, or more likely to be effectual, than that the Royal Society ſhould take it into conſideration, and recommend that method of proceeding which ſhall appear to them to be moſt expedient.

Of the barometer, rain-gage, wind, and hygrometer.

THE barometer is of the ciſtern kind, and the height of the quickſilver is eſtimated by the top of its convex ſurface, and not by the edge where it touches the glaſs, the index being properly adapted for that purpoſe. This manner of obſerving appears to me more accurate than the other; becauſe if the quickſilver ſhould adhere leſs to the tube, or be leſs convex at one time than another, the edge will, in all probability, be more affected by this inequality than the ſurface. I prefer the ciſtern to the ſyphon barometer, becauſe both the trouble of obſerving and error of obſervation are leſs; as in the latter we are liable to an error in obſerving both legs. Moreover, the quickſilver can hardly fail of ſettling truer in the former

 than

than in the latter; for the error in the ſettling of the quickſilver can proceed only from the adheſion of its edge to the ſides of the tube; now the latter is affected by the adheſion in two legs, and the former by that in only one: and, beſides, as the air has neceſſarily acceſs to the lower leg of the ſyphon barometer, the adheſion of the quickſilver in it to the tube will moſt likely be different, according to the degree of dryneſs or cleanneſs of the glaſs. It is true, as Mr. DE LUC obſerves, that the ciſtern barometer does not give the true preſſure of the atmoſphere; the quickſilver in it being a little depreſſed on the ſame principle as in capillary tubes. But this does not appear to me a ſufficient reaſon for rejecting the uſe of them. It is better, I think, where ſo much nicety is required, to determine, by experiment, how much the quickſilver is depreſſed in tubes of a given bore, and to allow accordingly.

By ſome experiments which have been made on this ſubject by my father Lord CHARLES CAVENDISH, the depreſſion appears to be as in the following table:

Inſide diameter of tube.	Grains of quickſilver in one inch of tube.	Depreſſ. of ſurface of quickſilver.	Inſide diameter.	Grains of quickſilver.	Depreſſ. of ſurface.	Inſide diameter.	Grains of quickſilver.	Depreſſ. of ſurface.
,6	972	,005	,35	331	,025	,20	108	,067
,5	675	,007	,30	243	,036	,15	61	,092
,4	432	,015	,25	169	,050	,10	27	1,40

The

The first column is the inside diameter of the tube, expressed in decimals of an inch; the second is the weight of a quantity of quicksilver sufficient to fill one inch in length of it; and the third is the corresponding depression of the convex surface of the quicksilver in a cistern barometer, whose tube is of that size. The reason of giving the second column is, because the easiest way of ascertaining the inside diameter of the tube is, by finding the quantity of quicksilver sufficient to fill a given length of it. It is needless saying, that the part of the tube, whose diameter is to be measured, is that answering to the upper part of the column of quicksilver; and that the table can be of no use but to those only who observe by the convex surface.

In this barometer, the inside diameter of the tube is about ,25 of an inch, and consequently the depression is ,05; the area of the cistern is near 120 times as great as that of the bore of the tube; so that as the quantity of quicksilver was adjusted when the barometer stood at $29\frac{3}{4}$, the error arising from the alteration of the height of the quicksilver in the cistern can scarce ever amount to so much as $\frac{1}{100}$th of an inch. As the tube appeared to be well filled, it was thought unnecessary to have the quicksilver boiled in it; but that is certainly the surest way of filling a barometer well.

The principal reason of setting down the mean heat of the thermometer within doors, during each month, in the journal of the weather, is this: suppose that any one desires to find the mean height of the barometer in any

any month, corrected on account of the heat of the quickſilver in the tube; that is, to find what would have been the mean height, if the quickſilver in the tube had been conſtantly of a certain given heat. To do this it is ſufficient to take the mean height of the barometer, and correct that according to the mean heat of the thermometer; the reſult will be exactly the ſame as if each obſervation had been corrected ſeparately, and a mean of the corrected obſervations taken. For example, ſuppoſe it is deſired to find what would have been the mean height of the barometer in the month of Auguſt 1775, if the quickſilver during that time had been always at 50 degrees of heat: the mean of the obſerved heights is 29,86 inches, and the mean heat of the thermometer is 65° or 50+15. The alteration of the height of the barometer by 15° of heat, according to M. DE LUC's rule, is ,047 inches; conſequently, the corrected mean height is 29,813.

The veſſel which receives the rain is a conical funnel, ſtrengthened at the top by a braſs ring, twelve inches in diameter. The ſides of the funnel and inner lip of the braſs ring are inclined to the horizon, in an angle of above 65°; and the outer lip in an angle of above 50°[(a)]; which are ſuch degrees of ſteepneſs, that there ſeems no probability either that any rain which falls within the funnel, or on the inner lip of the ring, ſhould daſh out,

(*a*) To make what is here ſaid the more intelligible, there is, in fig. 2. given a vertical ſection of the funnel, ABC and *abc* being the braſs ring, BA and *ba* the inner lip, and BC and *bc* the outer.

or

or that any which falls on the outer lip ſhould daſh into the funnel. This veſſel is placed on ſome flat leads on the top of the Society's Houſe. It can hardly be ſcreened from any rain by the chimnies, as none of them are elevated above it in an angle of more than 25°; and as it is raiſed $3\frac{1}{2}$ feet above the roof, there ſeems no danger of any rain daſhing into it by rebounding from the lead.

The ſtrength of the wind is divided in the journal into three degrees; namely, gentle, briſk, and violent or ſtormy, which are diſtinguiſhed by the figures 1, 2, and 3. When there is no ſenſible wind it is diſtinguiſhed by a cypher.

In the future journals of the weather will be given obſervations of the hygrometer. The inſtrument intended to be uſed is of Mr. SMEATON's conſtruction, and is deſcribed in Phil. Tranſ. vol. LXI. p. 198. It is kept in a wooden caſe, made ſo as to exclude the rain, but to leave a free paſſage for the wind, and placed in the open air, where the Sun ſcarce ever ſhines on it. The inſtrument and caſe are both a preſent to the Society from Mr. SMEATON. The hygrometer was laſt adjuſted in Dec. 1775, and as the ſtring has now been in uſe upwards of five years, it is not likely to want re-adjuſting ſoon.

Of the Variation Compaſs.

IN this inſtrument, the box which holds the needle is not fixed, but turns horizontally on a center, and has an index faſtened to it, pointing to a divided arch on the braſs

braſs frame on which it turns; and the method of obſerving is to move the box, till a line drawn on it points exactly to the end of the needle; which being done, the angle that the needle makes with the ſide of the frame is ſhewn by the index. Fig. 3. is the plan of the inſtrument; AB*ba* is the braſs frame, the ſides AB and *ab* being parallel; E*e* is a circular plate faſtened thereto, on which CD*dc*, the box which holds the needle, turns as on a center; N*n* is the needle, the pin on which it vibrates, being fixed in the center of the plate E*e*; B*b* is the diviſion on the braſs frame; and G the index faſtened to the box CD*dc*, furniſhed with a vernier diviſion; the diviſion and vernier being conſtructed ſo as to ſhew the angle which the line F*f* makes with AB or *ab*. The inſtrument is placed in the meridian by the teleſcope M*m*, the line of collimation of which is parallel to AB, and is pointed to a mark fixed due North of it.

Fig. 4. is a vertical ſection of the inſtrument paſſing along the line F*f*; AB is the braſs frame; CD*dc* the box which holds the needle; E*e* the circular plate on which it turns: N*n* is the needle; P and *p* are ſmall plates of braſs fixed to the ends of it, on each of which is drawn a line ſerving by way of index. Theſe pieces of braſs are raiſed to ſuch a height that their tops are on a level with the point of the pin on which the needle turns. The uſe of them is, that it is much eaſier obſerving this way than when the lines, ſerving by way of index, are drawn on the needle itſelf, as by this means the inconvenience proceeding from one kind of vibration in the needle is avoided.

avoided. S and *s* are two brafs plates, on each of which is drawn a line to which the index at the end of the needle is to point; there is alfo a line parallel to thefe drawn on the bottom of the box; thefe three lines form the line F*f* in fig. 3. R is a double microfcope intended to affift us in judging when the index P points exactly to the line F, that is, to the line drawn on the plate S. It is placed fo, that a wire W*w* in its *focus* appears to coincide with this line; and in obferving, the box is moved till the wire appears alfo to coincide with the index P.

The cap in the center of the needle is made to take on and off readily, and to fit on upon either face; fo that we may on occafion obferve with the under face of the needle uppermoft, as is reprefented in fig. 5. But the regular obfervations are always made with the needle in its upright pofition, and by the help of the index P only; the intention of the other index and of inverting the needle is, to fhew whether the line joining the indices P and *p*, or the line P*p* as I fhall call it, is parallel to the direction of magnetifm in the needle, and thereby to find whether, in the ufual method of obferving, the index G fhews the true angle which the direction of magnetifm makes with the fide AB. The way of doing this is as follows; having fuffered the needle to fettle, the obferver moves the box by means of the adjufting fcrew T, till the index P coincides with the line F, and reads off the angle fhewn by the vernier. He then moves the box till the other index *p* coincides with the line *f*, which, as the pin on which the needle is fufpended is fixed to the brafs

 frame,

frame, may be done without any danger of altering the poſition of the needle or making it vibrate, and reads off the angle as before. The mean of theſe two is the true angle which the line P*p* makes with the ſide AB, ſuppoſing the diviſion and vernier to be rightly conſtructed, even though neither the lines P*p* nor F*f* ſhould paſs through the center of the pin. Having done this, he takes off the cap and inverts the needle, and obſerves by both indices as before. It is plain, that if the line P*p* is parallel to the direction of magnetiſm in the needle, this mean will agree with the former, ſuppoſing that the magnetic variation has not altered between the obſervations. On the other hand, if it is not parallel to the direction of magnetiſm, but makes the variation appear greater than it ought to do when the needle is upright, it will make it appear as much leſs when the needle is inverted; ſo that the mean of the two abovementioned means is the true angle which the direction of magnetiſm in the needle makes with the ſide AB; that is, the true variation of the needle at that time and place, ſuppoſing AB to be placed accurately in the meridian. Having thus found the true angle which the direction of magnetiſm makes with AB, he ſubtracts that ſhewn by the index P in the upright poſition of the needle; the difference is the error of the inſtrument in the uſual manner of obſerving.

It was by this method that the error of the inſtrument, at the time of the obſervations in 1774, was found to be 10′. For example, by a mean of the obſervations

vations made on Sept. 5. the variation with the needle, in its upright poſition, was 21.36 by the South end, and 21.27 by the North; with the needle inverted it was 21.19 by the South end, and 21.29 by the North. The mean of all four is 21.28, which is the true variation at that time and place(*b*), and is 8′ leſs than that ſhewn in the upright poſition of the needle by the South end, which is the end always uſed in obſerving; ſo that by this day's experiment the error of the inſtrument appeared to be 8′; but by a mean of the obſervations of this and two other days it came out 10′. Since that time the needle has been altered; and, at the time of the obſervations in 1775, the error was ſo ſmall as to be ſcarcely ſenſible.

Great care was taken that the metal, of which this variation compaſs is compoſed, ſhould be perfectly free from magnetiſm. There is a contrivance in it for lifting the needle from off the point, and letting it down gently, to prevent injury in carrying from one room to another. The inſtrument is conſtructed nearly on the ſame plan as ſome made by the late Dr. KNIGHT. The principal difference is, that in his the pin which carried the needle was not fixed to the lower frame as in this, but to the box; the conſequence of which was, that when the needle had ſettled, and the box was moved to make the index on the needle point to the proper mark, it was again put

(*b*) The quantity found by taking a mean of all the four numbers is evidently the ſame as that got by taking a mean of the two firſt and of the two laſt, and taking a mean of thoſe two means.

into vibration, which caufed great trouble to the obferver. This inconvenience is intirely removed by the prefent conftruction. There is no other material difference, except that of the needle being made to invert, and the addition of the telefcope. The contrivance of fixing the pin which carries the needle to the lower frame, is taken from an inftrument of Lord CHARLES CAVENDISH; that of making the needle invert I have feen in fome compaffes made by SISSON.

There is a very common fault in the agate-caps ufually made for needles, which is, that they are not hollowed to a regular concave, but have a little projecting part in the center of the hollow; the confequence of which is, that the point of the pin will not always bear againft the fame part of the agate, and confequently the needle will not always ftand horizontal; but fometimes one end will ftand higheft, and fometimes the other, which caufes a difficulty in obferving. There is alfo another inconvenience attends it when the indices of the needle are on a level with the point of the pin, which is of more confequence; namely, that it caufes the two indices not to agree, and confequently makes a fenfible error, when only one index is made ufe of, at leaft in nice obfervations: but when the lines, ferving by way of index, are drawn on the needle itfelf, and therefore are nearly on a level with its center of gravity, it can caufe very little error. The agate cap, which was firft made for this inftrument, was of this kind; and was fo faulty, that, if no better could have been procured, it would have been neceffary either to have drawn the lines ferving by way of index on the needle itfelf, or to have obferved by both ends, either of which would have been attended with a confiderable increafe of trouble to the obferver; but Mr. NAIRNE, the artift who made the inftrument, has fince ground fome himfelf, which are perfectly free from this fault, the concave furface being of an extremely regular fhape and well polifhed, and alfo of a very fmall radius of curvature; which is a matter of confiderable confequence, as otherwife the point of the pin will not eafily flip fufficiently near to the bottom of the hollow.

Care was taken to place the variation compafs in a part of the houfe where it is as little likely to be affected by the attraction of the iron work as in any that could be found. As it feemed, however, to be not intirely out of the reach of the influence of that metal, I took the following method to examine how much it was influenced thereby. The inftrument was removed into a large garden belonging to a houfe in Marlborough Street, diftant from the Society's Houfe about one mile and a quarter towards the Weft, where there feemed no danger of its being affected by any iron-work. Here it was placed exactly in the meridian, and compared for a few days with a very exact compafs, placed in an adjoining room, and kept fixed conftantly in the fame fituation. It was then removed back to the Society's Houfe, and compared again with the fame compafs. The obfervations were as follow:

Obfervations

Obſervations made with the Society's inſtrument in the garden.

Time.		Variation by Society's Inſtrum.	Variation by Compaſs in room.	Diff.	Time.		Variation by Society's Inſtrum.	Variation by Compaſs in room.	Diff.
	h ′	° ′	° ′	′		h ′	° ′	° ′	′
1775 July 21	4 48V	21 31	21 33	— 2	1775 July 31	11 4M	21 28	21 32	— 4
	5 0	32	35	— 3		11 20	28	30	— 2
	5 26	30	28	+ 2		11 38	30	30	0
	5 43	31	32	— 1		11 57	29	32	— 3
	5 48	30	30	0		0 13V	29	33	— 4
22	10 45M	33	33	0		0 32	30	31	— 1
	11 2	29	30	— 1		2 24	32	35	— 3
	11 18	31	29	+ 2		2 54	32	31	+ 1
	11 37	31	31	0	Aug. 1	10 34M	26	28	— 2
	11 55	31	33	— 2		3 13V	32	33	— 1
	4 36V	31	32	— 1		4 33	29	29	0
	4 53	27	30	— 3		4 46	29	31	— 2
	5 22	24	26	— 2		5 12	27	29	— 2
	5 54	26	26	0		5 35	27	28	— 1
						5 57	28	30	— 2

The

The inſtrument being removed back to the Society's houſe.

Time.		Variation by Society's Inſtrum.	Variation by Compaſs in room.	Diff.	Time.		Variation by Society's Inſtrum.	Variation by Compaſs in room.	Diff.
	h ′	° ′	° ′	′		h ′	° ′	° ′	′
1775 Aug. 2	1 8V	21 45	21 32	+13	1775 Aug. 4	10 50M	21 47	21 33	+14
	1 10	44	30	+14		11 0	47	34	+13
	1 20	46	29	+17		11 10	47	35	+12
	1 30	47	29	+18		11 20	47	35	+12
	1 40	47	32	+15		11 30	46	35	+11
	1 50	47	31	+16		11 40	47	34	+13
	2 0	47	31	+16					

By a mean of the obſervations, the variation ſhewn by the compaſs in the room is 1′,3 greater than by the Society's inſtrument in the garden, and 14′,1 leſs than by the ſame inſtrument placed in its proper ſituation; ſo that the variation appears to be 15′,4 greater in that part of the Society's Houſe where the compaſs is placed, than in the abovementioned garden; and therefore, as there is no likelihood of its being affected by any iron in the latter place, the needle ſeems to be drawn aſide 15′½ towards the N.W. by the iron work of the houſe and adjacent buildings.

On comparing the obſervations of the two laſt years together, the variation appears, after allowing for the error of the inſtrument, to have been 27′ greater in 1775 than in 1774; though I have been informed by Dr.

HEBERDEN,

HEBERDEN, who has made obſervations of this kind for ſeveral years paſt, that the annual alteration of the variation has, in general, been not more than 10′; and in particular, that the alteration in the laſt year appears to be only 11½′; ſo that the great difference obſerved at the Society's Houſe ſeems to be owing, not ſolely to the real alteration in the variation, but partly to ſome other cauſe; though what that ſhould be I cannot conceive, unleſs ſome change was made in the iron work either of this or the adjoining houſes between the two periods; but I do not find that any ſuch change has been made. During the laſt year, indeed, there have been two large magnets in the houſe, each conſiſting of ſeveral great bars joined together, being what the late Dr. KNIGHT uſed for making artificial magnets, and at the time of the obſervations in 1774 there was only one; but their diſtance from the compaſs is above fifty feet: and I am well aſſured, that in the ſituation in which they are actually placed, they cannot draw the needle aſide more than 3′, and not more than 15′, when the line joining their poles is placed in ſuch a direction as to act with moſt force (c). The ſingle

(c) The principle by which this was determined is, that if a magnet is placed near a variation compaſs, with its poles equi-diſtant from it, and ſituated ſo that each ſhall act equally oblique to the length of the needle, it can have no tendency to alter the variation; and that the ſituation in which it alters it moſt, except when placed nearly North or South of the compaſs, is when the line joining its poles points almoſt directly towards the needle. This experiment I tried purpoſely on the occaſion, and found it anſwer; but, I believe, any one ſkilled in magnetiſm would have granted the truth of the poſition without that precaution.

magnet

magnet in the year 1774 was placed nearly in the ſame ſituation and direction that the two were in 1775, ſo that the difference of their effect in theſe two years can hardly have been ſo much as 3′; and therefore, the great apparent alteration of the variation between the two periods cannot have been owing to them. Neither can it have been owing to the fault of the agate cap uſed in the year 1774, as the error proceeding from thence could hardly be more than 2 or 3′. It is intended that, for the future, the abovementioned magnets ſhall be kept always in the ſame ſituation and direction that they are in at preſent, and in which they were in 1775.

Of the Dipping-needle.

IN this inſtrument the ends of the axis of the needle roll on horizontal agate planes, a contrivance being applied, by which the needle is at pleaſure lifted off from the planes, and let down on them again, in ſuch manner as to be ſupported always by the ſame points of the axis reſting on the ſame parts of the agate planes; and the motion with which it is let down is very gradual and without ſhake. The general form of the inſtrument, the ſize and ſhape of the needle, and the croſs uſed for balancing it, are the ſame as in the dipping-needle deſcribed in Phil. Tranſ. vol. LXII. p. 476. It is alſo made by the ſame artiſt Mr. NAIRNE.

It may be ſeen in the Meteorological Journal, that the dip was obſerved firſt with the front of the inſtrument to to the West, and then to the East; after which the poles of the needle were reversed, and the dip observed both ways as before. The reason of this is, that the mean of the observed dips, in these four situations, differs very little from the truth, though the needle is not well balanced, and even though a great many other errors are committed in the construction of the instrument; provided the needle is made equally magnetical after the poles are reversed as before[(d)]; and that the difference of the observed dip, in these four situations, is not very great, as will appear from the following considerations.

First, let fig. 7. be a front view of the needle; AB a line parallel to the direction of magnetism therein; and CD a perpendicular thereto, meeting it in the line joining the centers of the cylindrical ends of the axis, or in the axis of motion as we may call it. If the needle was truly balanced, its center of gravity would be in *d*, the intersection of AB and CD. Suppose now, that the needle is not truly balanced, but that its center of gravity is in *g*; draw *gn* perpendicular to AB, cutting it in *m*; and let the parts *mn* and *mg* be equal. When the instrument is turned half-way round, so that the contrary face of the needle is presented towards us, the edge ADB, which is now lowest, will become uppermost, and the center of gravity will be in that situation in which the point *n* now is; therefore, the mean between the forces with which the

(*d*) It is easy to see whether the needle is made equally magnetical after the poles are reversed as before, by counting the number of vibrations which it makes in a minute.

needle is drawn out of its true poſition in theſe two ſituations, in conſequence of its not being truly balanced, is accurately the ſame; and the mean between the two obſerved dips is very nearly the ſame, as if the center of gravity was at *m*. But if the center of gravity is at *m*, the dip will be very nearly as much too great in the preſent ſtate of the needle, as it will be too little when the poles are reverſed. Therefore, the mean of the obſerved dips in theſe four ſituations will be very nearly the ſame as if the needle was truly balanced.

Secondly, if the planes on which the axis rolls are not horizontal, the dip will be very nearly as much greater than it would otherwiſe be, when one face is turned to the Weſt, as it is leſs when the other is; for if theſe planes dip towards the South in one caſe, they will dip as much towards the North in the other, ſuppoſing the levels by which the inſtrument is ſet to remain unaltered. Conſequently, the mean of the two obſervations will be very nearly the ſame as if they were placed truly horizontal.

Thirdly, by the ſame method of reaſoning it appears, that the mean of the two abovementioned obſervations will be not at all altered, though the line, joining the mark on that end of the needle by which we obſerve, with the axis of motion, is not parallel to the direction of magnetiſm in the needle; that is, though the mark does not coincide with the point A or B, or though the line joining the two diviſions of 90° is not perpendicular to the horizon, or though the axis of motion does not paſs through the center of the divided circle, provided it is in

 the

the ſame horizontal plane with it. If, indeed, the axis of motion is not in the ſame horizontal plane with the center of the divided circle, the error proceeding from thence will not be compenſated by this method of obſerving, unleſs both ends of the needle are made uſe of. This, however, is of no conſequence as, it is eaſy to examine whether they are in the ſame horizontal plane or not.

But the error which is moſt difficult to be avoided is, that which proceeds from the ends of the axis being not truly cylindrical. I before ſaid, that the parts of them which reſt on the agate planes are always exactly the ſame. The inſtrument is ſo contrived, however, that we may on occaſion, by giving the axis a little liberty in the notches by which it is lifted up and down, make thoſe planes bear againſt a part of the axis diſtant about $\frac{1}{100}$ or $\frac{1}{50}$th of an inch from their uſual point of bearing. Now, I find, that when the axis is confined ſo as to have none of this liberty, and when care is taken, by previouſly making the needle ſtand at nearly the right dip, that it ſhall vibrate in very ſmall arches when let down on the planes; that then, if the needle is lifted up and down any number of times, it will commonly ſettle exactly at the ſame point each time, at leaſt the difference is ſo ſmall as to be ſcarcely ſenſible; but if it is not ſo confined, there will often be a difference of 20 in the dip, according as different parts of the axis reſt on the planes, and that though care is taken to free the axis and planes from duſt as perfectly as poſſible, which can be owing only to ſome irregularity in the axis. Moreover, if

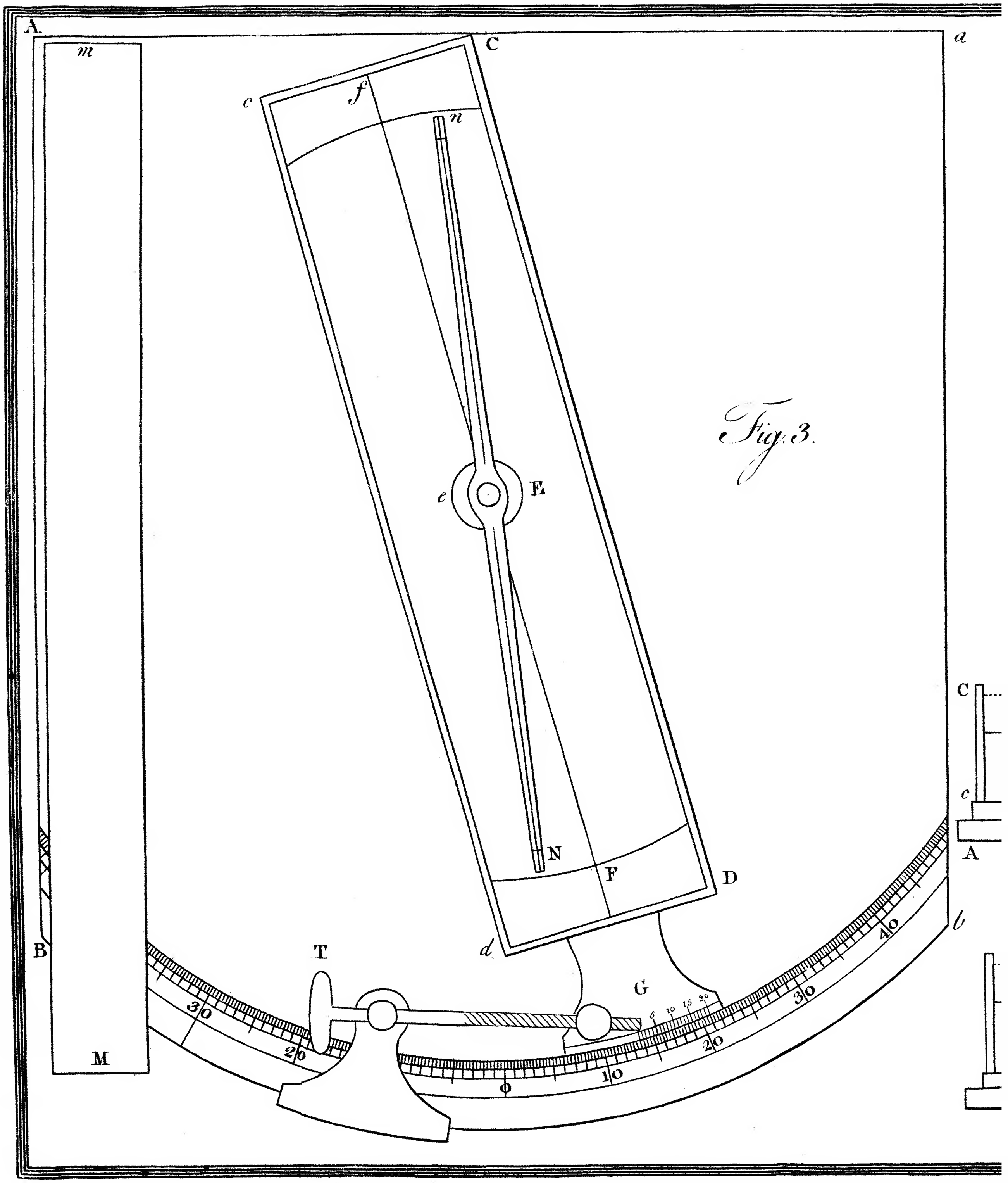
Fig. 3.
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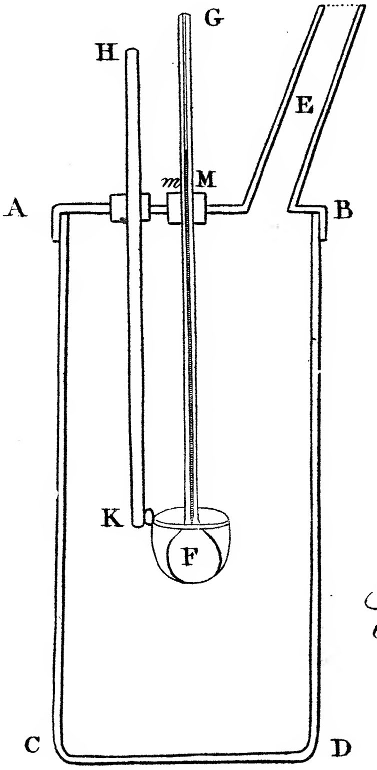
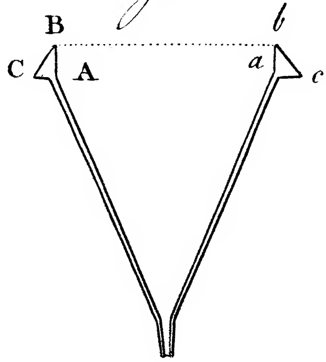


Fig. 1.

Fig. 2.



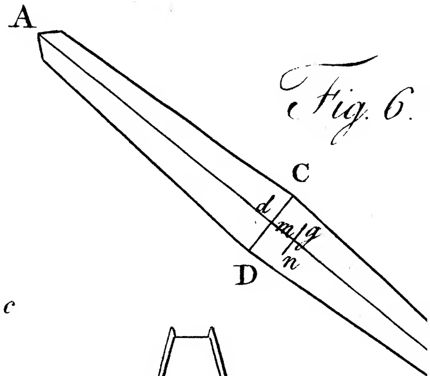


Fig. 6.

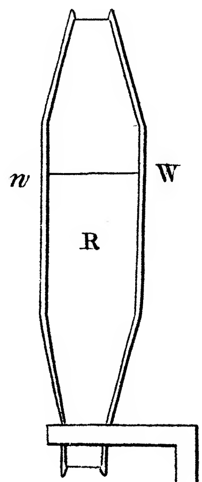


Fig. 4.

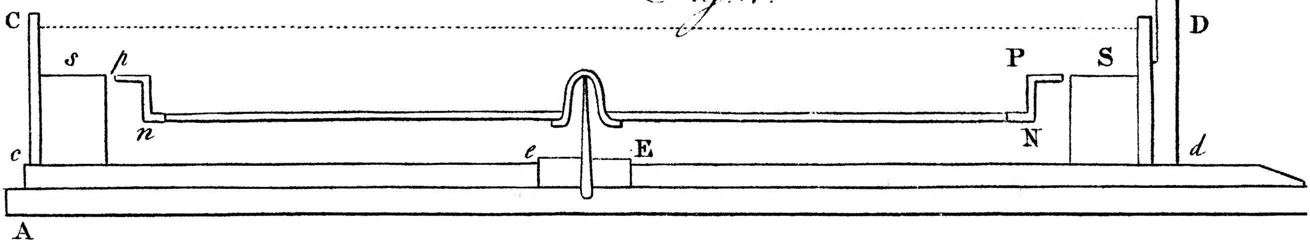
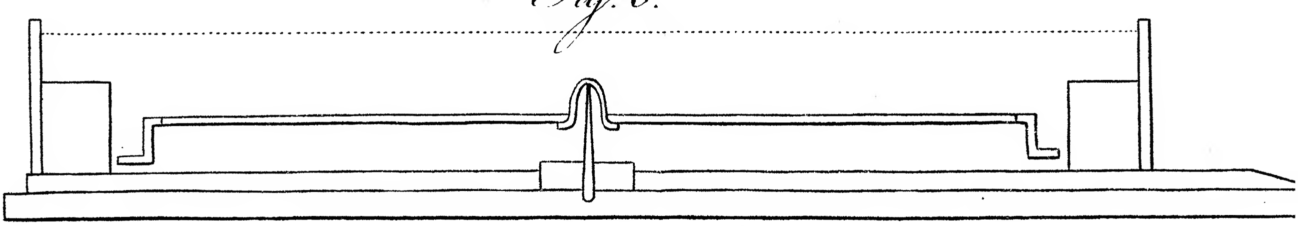
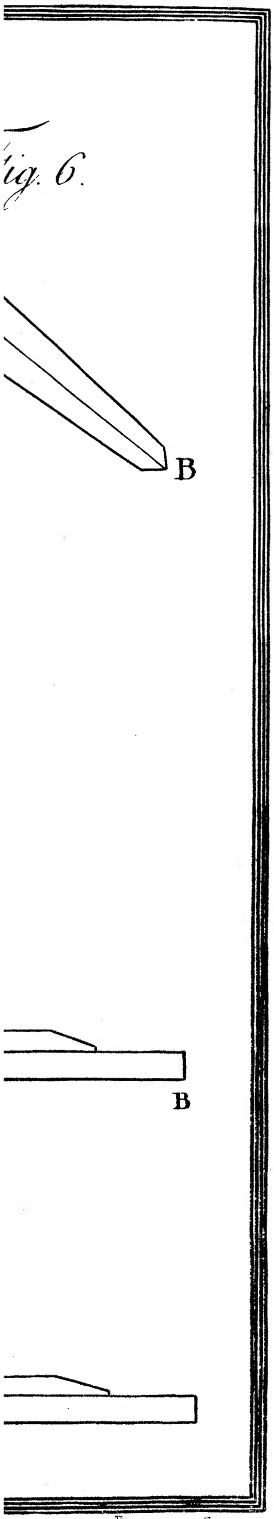


Fig. 5.





Basire, Sc.

if the needle vibrates in arches of five or more degrees, when let down on the planes, there will frequently be as great an error in the dip. It is true, that the part of the agate planes, which the axis reſts on when the vibrations are ſtopped, will be a little different according to the point which the needle ſtood at before it was let down; which will make a ſmall difference in the dip as ſhewn by the divided circles, when only one end of the needle is obſerved, though the real dip or inclination of the needle to the horizon is not altered: but this difference is by much too ſmall to be perceived; ſo that the above-mentioned error cannot be owing to this cauſe. Neither does it ſeem owing to any irregularity in the ſurface of the agate planes, for they were ground and poliſhed with great accuracy; but it moſt likely proceeds from the axis ſlipping in the large vibrations, ſo as to make the agate planes bear againſt a different part of it from what they would otherwiſe do. I have great reaſon to think, that this irregularity is not owing either to want of care or ſkill in the execution, but to the unavoidable imperfection of this kind of work. I imagine too, that this inſtrument is at leaſt as exact, if not more ſo, than any which has been yet made.

The following table contains the reſult of ſome obſervations which I made, partly with a view to determine the true dip at this time in a place out of reach of the influence of any iron work, and partly to ſee how nearly different needles would agree. The inſtruments were all tried in the ſame garden in which the variation compaſs was obſerved, and all on the 10th, 11th, 13th, and 14th

days

days of October, 1775, except that marked *, which was tried on the 15th of the preceding April.

			Poles reversed.		Poles restored to their first situation.		True Dip.
	East	West	East	West	East	West	
The Society's needle,	72 32	72 8	72 9	72 40	72 59	71 50	72 23
Another of the same construction, belonging to Mr. NAIRNE,	72 56	72 29	71 45	73 21	72 51	72 27	72 37
One of mine on nearly the same construction,	72 33	72 22	71 41	73 23	72 34	72 18	72 30
Another needle in the same frame,	72 22	72 7	71 40	73 53	72 16	72 30	72 33
A needle of mine, made by SISSON, partly on the same construction as Mr. LORIMER's (c),	*73 1	71 49	71 57	73 0			72 27
Another of Mr. NAIRNE's on the same construction,	73 8	72 0	73 15	71 57			72 35

Each of the numbers set down in the above table is the mean of two observations, the instruments being observed first with the front to the East, then to the West; then a second time to the East, and then again to the West; and in all the observations, except those with the two last instruments, which are of a different construction, care was taken that the needle should vibrate in very small arches when let down on the agate planes. By a mean of all, the true dip at London, at this time, comes out 72° 30′, the different needles all agreeing

(c) See Phil. Trans. vol. LXV. p. 79.

within

within 14′, which is a difference conſiderably leſs than I ſhould have expected. It appears alſo, that the dipping-needle, in the ſituation in which it is placed at the Society's Houſe, is not much affected by any iron work, as the dip ſhewn by it in the garden differs only 7′ from that ſet down in the journal of the weather.

According to NORMAN, the inventor of the dipping-needle, the dip at London in the year 1576 was 71° 50′(e); in 1676 it was 73° 47′, according to Mr. BOND(f); Mr. WHISTON in 1720 made it 75° 10′(g); Mr. GRAHAM in 1723 made it between $73\frac{1}{2}$ or 75°(h), his different trials varying ſo much; and at preſent it appears to be 72° 30′. I do not know how much Mr. BOND's determination is to be depended on, as he does not ſay by what means he arrived at it; but, I believe, Mr. WHISTON's is pretty accurate, for he obſerved the dip in many parts of the kingdom, and the obſervations agree well together; ſo that it is reaſonable to ſuppoſe, that his inſtrument was a good one, and that he obſerved in places where the needle was not much influenced by iron work. The dip, therefore, ſeems to have been conſiderably greater about the year 1720, than it was in NORMAN's time, or is at preſent: it appears, however, to alter very ſlowly in compariſon of the variation.

(e) New Attractive, c. 4. (f) Longitude found, p. 65.
(g) Longitude and Latitude found by Dipping-needle, p. 7, 49, and 94.
(h) Phil. Tranſ. N° 389. p. 332.

XXII. *An*

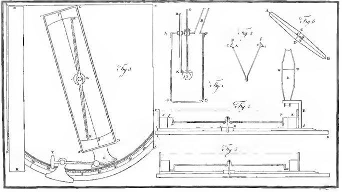


Fig. 3
Fig. 1
Fig. 2
Fig. 6
Fig. 4
Fig. 5