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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
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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
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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 *mm* 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 *mm* 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 $2\frac{1}{2}^{\circ}$; 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

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, ΛBC and abc being the brass ring, BA and ba the inner lip, and BC and bc the outer.

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; $ABba$ is the brass frame, the sides AB and ab being parallel; Ee is a circular plate fastened thereto, on which $cDdc$, the box which holds the needle, turns as on a center; Nn is the needle, the pin on which it vibrates, being fixed in the center of the plate Ee ; Bb is the division on the brass frame; and G the index fastened to the box $cDdc$, furnished with a vernier division; the division and vernier being constructed so as to shew the angle which the line Ff makes with AB or ab . The instrument is placed in the meridian by the telescope mm , 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 Ff ; AB is the brass frame; $cDdc$ the box which holds the needle; Ee the circular plate on which it turns: Nn 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 brass plates, on each of which is drawn a line to which the index at the end of the needle is to point; there is also a line parallel to these drawn on the bottom of the box; these three lines form the line Ff in fig. 3. R is a double microscope intended to assist 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 so, that a wire ww in its *focus* appears to coincide with this line; and in observing, the box is moved till the wire appears also 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; so that we may on occasion observe with the under face of the needle uppermost, as is represented in fig. 5. But the regular observations are always made with the needle in its upright position, and by the help of the index p only; the intention of the other index and of inverting the needle is, to shew whether the line joining the indices P and p , or the line pp as I shall call it, is parallel to the direction of magnetism in the needle, and thereby to find whether, in the usual method of observing, the index G shews the true angle which the direction of magnetism makes with the side AB . The way of doing this is as follows; having suffered the needle to settle, the observer moves the box by means of the adjusting screw τ , till the index p coincides with the line F , and reads off the angle shewn 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 suspended is fixed to the brass

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 pp makes with the side AB , supposing the division and vernier to be rightly constructed, even though neither the lines pp nor Ff 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 pp 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

variations 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 caused great trouble to the observer. This inconvenience is intirely removed by the present construction. There is no other material difference, except that of the needle being made to invert, and the addition of the telescope. The contrivance of fixing the pin which carries the needle to the lower frame, is taken from an instrument of LORD CHARLES CAVENDISH; that of making the needle invert I have seen in some compasses made by SISSON.

There is a very common fault in the agate-caps usually 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 consequence of which is, that the point of the pin will not always bear against the same part of the agate, and consequently the needle will not always stand horizontal; but sometimes one end will stand highest, and sometimes the other, which causes a difficulty in observing. There is also another inconvenience attends it when the indices of the needle are on a level with the point of the pin, which is of more consequence; namely, that it causes the two indices not to agree, and consequently makes a sensible error, when only one index is made use of, at least in nice observations: but when the lines, serving by way of index, are drawn on the needle itself, and therefore are nearly on a level with its center of gravity, it can cause very little error. The agate cap, which was first made for this instrument, was of this kind; and was so faulty, that, if no better could have been procured, it would have been necessary either to have

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drawn

drawn the lines serving by way of index on the needle itself, or to have observed by both ends, either of which would have been attended with a considerable increase of trouble to the observer; but Mr. NAIRNE, the artist who made the instrument, has since ground some himself, which are perfectly free from this fault, the concave surface being of an extremely regular shape and well polished, and also of a very small radius of curvature; which is a matter of considerable consequence, as otherwise the point of the pin will not easily slip sufficiently near to the bottom of the hollow.

Care was taken to place the variation compass in a part of the house 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 seemed, 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 instrument was removed into a large garden belonging to a house in Marlborough Street, distant from the Society's House about one mile and a quarter towards the West, where there seemed 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 compass, placed in an adjoining room, and kept fixed constantly in the same situation. It was then removed back to the Society's House, and compared again with the same compass. The observations were as follow:

Observations

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

Time.		Variation by		Diff.	Time.		Variation by		Diff.
		Society's Instrum.	Compass in room.				Society's Instrum.	Compass in room.	
1775 July 21	h / 4 48V	o / 21 31	o / 21 33	' — 2	1775 July 31	h / 11 4M	o / 21 28	o / 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 instrument being removed back to the Society's house.

Time.		Variation by		Diff.	Time.		Variation by		Diff.
		Society's Instrum.	Compass in room.				Society's Instrum.	Compass in room.	
1775	h /	o /	o /	'	1775	h /	o /	o /	'
Aug. 2	1 8V	21 45	21 32	+ 13	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 15'¹/₂ 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 $11\frac{1}{2}'$; 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 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.

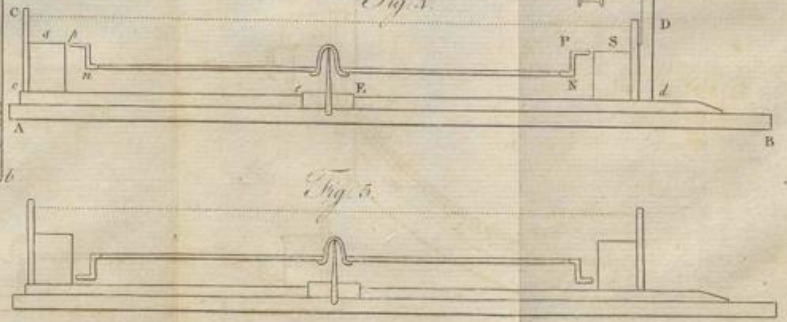
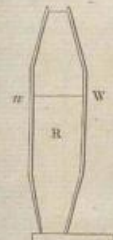
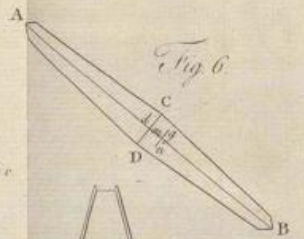
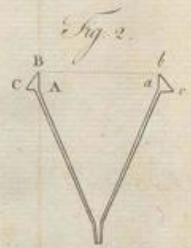
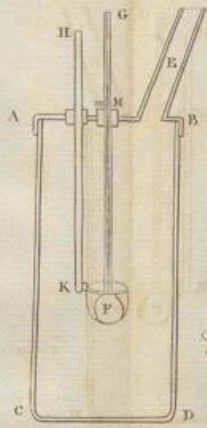
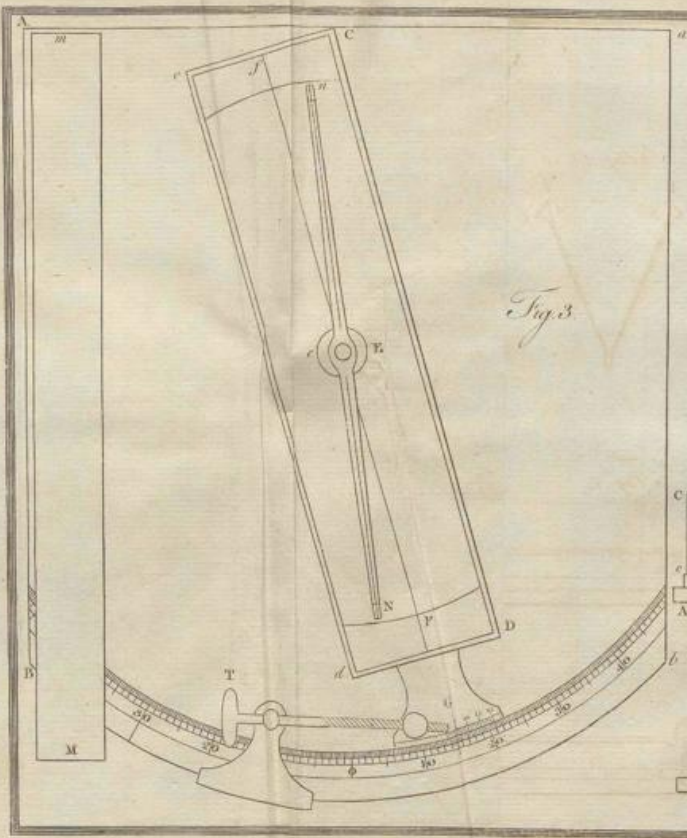
needle is drawn out of its true position in these two situations, in consequence of its not being truly balanced, is accurately the same; and the mean between the two observed dips is very nearly the same, as if the center of gravity was at *m*. But if the center of gravity is at *m*, the dip will be 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.

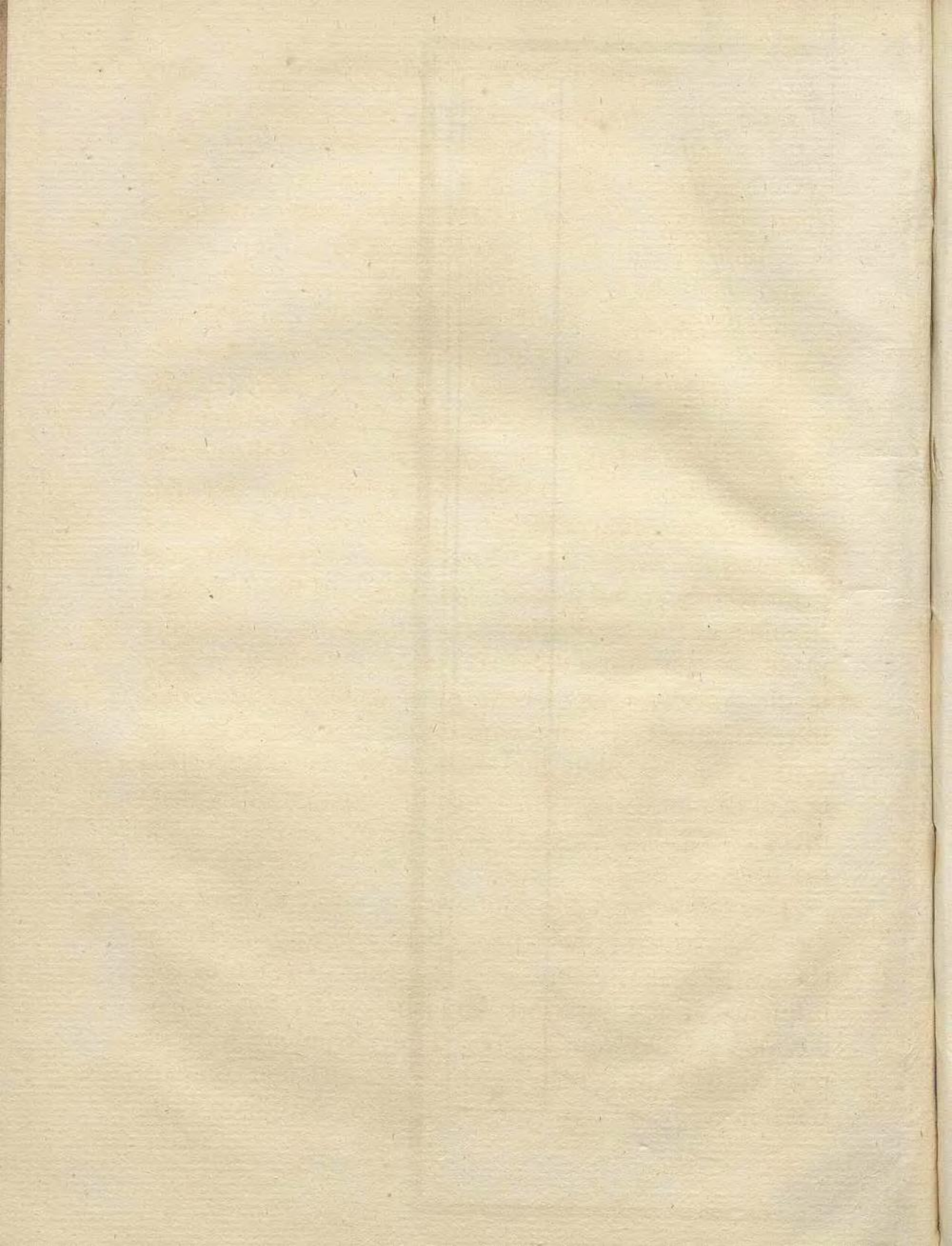
Secondly, if the planes on which the axis rolls are not horizontal, the dip will be very nearly as much greater than it would otherwise be, when one face is turned to the West, as it is less when the other is; for if these planes dip towards the South in one case, they will dip as much towards the North 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 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





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 ^(e) ,	*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 $72^{\circ} 30'$, the different needles all agreeing

(e) See Phil. Transf. 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 $71^{\circ} 50'$ ^(e); in 1676 it was $73^{\circ} 47'$, according to Mr. BOND ^(f); Mr. WHISTON in 1720 made it $75^{\circ} 10'$ ^(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 $72^{\circ} 30'$. 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^o 389. p. 332.

XXII. *The Method taken for preserving the Health of the Crew of His Majesty's Ship the Resolution during her late Voyage round the World. By Captain James Cook, F. R. S. Addressed to Sir John Pringle, Bart. P. R. S.*

TO SIR JOHN PRINGLE, BART. P. R. S.

S I R,

Mile-end,
March 5, 1776.

R. Mar. 7,
1776.

AS many gentlemen have expressed some surprize at the uncommon good state of health which the crew of the *Resolution*, under my command, experienced during her late voyage; I take the liberty to communicate to you the methods that were taken to obtain that end. Much was owing to the extraordinary attention given by the Admiralty, in causing such articles to be put on board, as either by experience or conjecture were judged to tend most to preserve the health of seamen. I shall not trespass upon your time in mentioning all those articles, but confine myself to such as were found the most useful.

We had on board a large quantity of Malt, of which was made sweet-wort, and given (not only to those men who had manifest symptoms of the scurvy, but to such also as were, from circumstances, judged to be most liable to
that