XXVII. *An Attempt to compare and connect the Thermometer for strong Fire, described in Vol. LXXII. of the Philosophical Transactions, with the common Mercurial Ones. By Mr.* Josiah Wedgwood, *F. R. S. Potter to Her Majesty.*

Read May 13, 1784.

THIS thermometer, which I had the honour of laying before the Royal Society in May 1782, has now been found, from extensive experience, both in my manufactories and experimental enquiries, to answer the expectations I had conceived of it as a measure of all degrees of common fire above ignition: but at present it stands in a detached state, not connected with any other, as it does not begin to take place till the heat is too great to be measured or supported by mercurial ones.

What is now therefore wanting, to give us clear ideas of the value of its degrees, is, to connect it with one which long use has rendered familiar to us; so that if the scale of the common thermometer be continued indefinitely upwards as a standard, the divisions of mine may be reduced to that scale, and we may thus have the whole range of the degrees of heat brought into one uniform series, expressed in one language, and comparable in every part, from the lowest that have hitherto been produced by any artificial freezing mixtures, up to the highest that can be obtained in our furnaces, or that the materials of our furnaces and vessels can support.

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The hope of attaining this desirable and important object gave rise to the experiments which I have now the honour of communicating. How far I may have succeeded, or whether the means employed were adequate to the end proposed, is, with all deference, submitted to this illustrious Society.

This attempt is founded upon the construction and application of an intermediate measure, which takes in both the heats that are measurable by the mercurial thermometer, and a sufficient number of those that come within the province of mine to connect the two together; the manner of doing which will be apparent from the three first figures (tab. XIV.); wherein F represents FAHRENHEIT'S thermometer, with a continuation of the scale; W my thermometer; and M the intermediate measure divided into any number of equal parts at pleasure.

For if the heat of boiling water, or 212 degrees of FAHRENHEIT, be communicated to M, and its measure upon M marked, as at *a*; and if the heat of boiling mercury, or 600° of FAHRENHEIT, be also communicated to M, and marked as at *b*; it is plain, that the number of degrees upon M between *a* and *b* will be equal to the interval between 212 and 600, that is, to 388° upon FAHRENHEIT.

In like manner, upon exposing M to two different heats above ignition along with my thermometer pieces, if a certain degree of my scale be found to correspond with the point *d*, and another degree of mine with the point *c*; then the interval between those two degrees upon mine must be equal to the interval *dc*; and how many of FAHRENHEIT'S that interval is equivalent to will be known from the preceding comparison. Thus we can find the number of FAHRENHEIT'S degrees contained in any given extent of mine, and the degree of FAHRENHEIT'S with which a given point of mine coincides; whence

whence either ſcale is eaſily reducible to the other through their whole range, whether we ſuppoſe FAHRENHEIT's continued upwards, or mine downwards.

For obtaining the intermediate thermometer different means were thought of; but the only principle which, upon attentive conſideration, afforded any proſpect of ſucceſs, was the *expanſion of metals*. This therefore was adopted, and among different methods of meaſuring that expanſion, which either occurred to myſelf, or which I can find to have been practiſed by others, there is no one which promiſes either ſo great accuracy, or convenience in uſe, as a gage like that by which the thermometer pieces are meaſured: the utility of this gage had now been confirmed to me by experience, and the machines and long rods, which have been employed for meaſuring expanſions on other occaſions, were abſolutely inadmiſſible here, on account of the inſuperable difficulties of performing nice operations of this kind in a red heat, and of communicating a perfectly equal heat through any conſiderable extent.

To give a clearer idea of this ſpecies of gage, which, ſimple as it is, I am informed has been miſunderſtood by ſome of the readers of my former paper, a repreſentation of one uſed on the preſent occaſion is annexed in fig. 4. where ABCD is a ſmooth flat plate; and EF and GH two rulers or flat pieces, a quarter of an inch thick, fixed flat upon the plate, with the ſides that are towards one another made perfectly true, a little further aſunder at one end EG than at the other end FH; thus they include between them a long converging canal, which is divided on one ſide into a number of ſmall equal parts, and which may be conſidered as performing the offices both of the tube and ſcale of the common thermometer. It is obvious, that if a body, ſo adjuſted as to fit exactly at the wider end of this

this canal, be afterwards diminiſhed in its bulk by fire, as the thermometer pieces are, it will then paſs further in the canal, and more and more ſo according as the diminution is greater; and converſely, that if a body, ſo adjuſted as to paſs on to the narrow end, be afterwards expanded by fire, as is the caſe with metals, and applied in that expanded ſtate to the ſcale, it will not paſs ſo far; and that the diviſions on the ſide will be the meaſures of the expanſions of the one, as of the contractions of the other, reckoning in both caſes from that point to which the body was adjuſted at firſt.

I is the body whoſe alteration of bulk is thus to be meaſured, which, in the preſent inſtance, is a piece of fine ſilver: this is to be gently puſhed or ſlid along, towards the end FH, till it is ſtopped by the converging ſides of the canal.

K is a little veſſel formed in the gage for this particular ſeries of experiments, the uſe of which will appear hereafter.

The *contraction*, which the thermometer pieces receive from fire, is a permanent effect, not variable by an abatement of the heat, and which accordingly is meaſured commodiouſly and at leiſure, when the pieces are grown cold. But the *expanſion* of bodies is only temporary, continuing no longer than the heat does that produced it; and therefore its quantity, at any particular degree of heat, muſt be meaſured in the moment while that heat ſubſiſts. And further, if the heated piece was applied to the cold gage, the piece would be deprived of a part of its heat on the firſt contact; and as the gage receives ſome degree of expanſion from heat as well as the piece, it is plain that in this caſe the piece would be diminiſhed in its bulk, and the gage enlarged, before the meaſurement could be taken. It is therefore neceſſary that both of them be heated to an exact equality; and in that ſtate we can meaſure, not indeed the *true expanſion of* *either*, but the *excess* of the expansion of one above that of the other, which is sufficient for the present purpose, as we want only an uniform and graduated effect of fire, and it is totally immaterial whether that effect be the absolute expansion of one or the other body, or the difference of the two, provided only that its *quantity* be sufficient to admit of nice measurement.

Some difficulties occurred with respect to the choice of a proper *matter* for the gage; the essential requisites of which are, to have but little expansibility, and to bear the necessary fires without injury. All the metals, except gold and silver, would calcine in the fire: those two are indeed free from that objection, and accordingly it is of the most expansible of them that the piece is made; but if the gage also was made of the same, the measure itself would expand just as much as the body to be measured, and no expansion at all would be sensible; and though the gage was made of one of those metals, and the piece of the other, the difference between their expansions would be too small to give any satisfactory results, as more than two-thirds of the real expansion of either would be lost or taken off by the other.

For these reasons I had recourse to earthy compositions, which expand by heat much less than metallic bodies, and bear the necessary degrees of fire without the least injury. I made choice of tobacco-pipe clay, mixed with charcoal in fine powder, in the proportion of three parts of the charcoal to five of the clay by weight. By a free access of air, in the burning by which the gage is prepared for use, the charcoal is consumed, and leaves the clay extremely light and porous; from which circumstance it bears sudden alternations of cold and heat, often requisite in these operations, much better than the clay alone. Another and more important motive for the use of charcoal

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was, that in confequence of the remarkable porofity which it produces in the clay, it would probably diminifh the expanfibility, by occafioning the mafs to contain, under an equal furface, a much lefs quantity of folid or expanfible matter. It may be objected to this idea, that the expanfions of metals, in Mr. ELLICOTT's * and Mr. SMEATON's † experiments, do not appear to have any connection at all with their denfities: but the cafes are by no means parallel; for there the comparifon lies between different fpecies of matter; but here, between one and the fame matter in different ftates of compactnefs. If a metal could be treated as clay is in this inftance, that is, if a large bulk of any foreign matter could be blended with it, and this matter afterwards burnt out, fo as to leave the metallic particles at the fame diftances to which they had been feparated by the mixture of it, we may prefume that the metal thus enlarged would not expand fo much as an equal volume of the folid metal. Such at leaft were the ideas which determined my choice to a compofition of clay and charcoal powder; and being afterwards defirous of fatisfying myfelf whether they had any foundation in fact, I have, fince the experiments were made, prepared fome pieces of clay with and without charcoal, and having burnt them in the fame fire, I ground them at the fides, to make them both fit exactly to the fame divifion near the narrow end of the gage; then, examining their expanfions by equal heats, I found the piece with charcoal to expand only one-third part fo much as that without; and thus was fully fatisfied with the compofition of the gage.

To afcertain a fixed point on the fcale for the divifions to be counted from, the filver piece and gage were laid together for

* Phil. Tranfact. vol. XLVII. p. 485.

† Ibid. vol. XLVIII. p. 612.

ſome time in ſpring water, of the temperature of 50° of FAHRENHEIT: the point which the piece went to in this cold ſtate is that marked o near the narrow end of the gage. The adjuſtment is re-examined at the beginning and end of every ſucceeding experiment, leſt the repeated attrition, in ſliding the piece backwards and forwards, ſhould wear off ſo much from the ſurface of this ſoft metal as to occaſion an error in the minute quantities here meaſured.

The apparatus is then expoſed ſucceſſively to different degrees of heat, with the piece lying always in a part of the canal at leaſt as wide as it is expected to fill when expanded, otherwiſe the ſides of the gage would be burſt aſunder by its expanſion, as I experienced in ſome of my firſt trials. When the whole has received any particular degree of heat deſired, the piece is cautiouſly and equably puſhed along, till it is ſtopped by the convergency of the ſides, of which I always find notice given me by the gage itſelf (which is ſmall and light) beginning to move upon the continuance of the impulſe. A flat ſlip of iron, a little narrower than the piece, bent down to a right-angle at one end, and fixed in a long handle at the other, makes a convenient inſtrument for puſhing the piece forward, or drawing it back again, whilſt red-hot: this inſtrument, at every time of uſing, is heated to the ſame degree as the piece itſelf.

The heat of boiling water is taken without difficulty, by keeping the apparatus in boiling water itſelf during a ſufficient ſpace of time for the full heat to be communicated to it. The water I made uſe of was a very fine ſpring water, which on chemical trials appeared very nearly equal in purity to that of rain or ſnow; and I had previouſly ſatisfied myſelf, by trials in the cold, that the gage and piece being wet, or under water, made no difference in the meaſurement. The expanſion of the filver by this heat, that is, by an increafe of the heat from 50° to 212°, or a period containing 162° of FAHRENHEIT, was juft 8° of the gage or intermediate thermometer M; whence one of thefe degrees, according to this experiment, contains juft 20°¼ of FAHRENHEIT's. The operation was many times repeated, and the refult was always precifely the fame.

For the boiling heat of mercury, it was neceffary to proceed in a different manner; not to convey the heat from the mercury to the inftrument, but to convey it equally to them both from another body. I made a fmall veffel for holding the mercury in the gage itfelf, feen at K fig. 4. and more diftinctly in fig. 5. which is a tranfverfe fection of the gage through this veffel. The plate CD, which forms the bottom of the canal, ferves alfo for the bottom of the veffel, which is fituated clofe to the fide of the canal, and as near as could be to that part of it, in which both the filver piece, and the divifions required for this particular experiment, are contained. By this arrangement it is prefumed, that all the parts concerned in the operation will receive very nearly an equal heat.

The gage, with fome mercury in the veffel, was laid upon a fmooth and level bed of fand, on the bottom of an iron muffle kept open at one end; the fire increafed very gradually till the mercury boiled, and then continued fteady, fo as juft to keep it boiling, for a confiderable time. The boiling heat of mercury was thus found to be 27°½ of the intermediate thermometer, which anfwering to an interval of 550° of FAHRENHEIT, makes one degree of this equal to juft 20° of his; a refult correfponding even beyond my expectations with that which boiling water had given.

Thefe ftandard heats of FAHRENHEIT's thermometer are obtained with little difficulty on a common fire; but it is far otherwife

otherwife with the higher ones in which mine begins to apply; and all the precautions I could take, by ufing a clofe muffle, furrounding it as equally as poffible with the fuel, varying its pofition with refpect to the draught of air, &c. proved infufficient for fecuring the neceffary equality of heat even through the fmall fpace concerned in thefe experiments. Nor had I any idea, before the difcovery of this thermometer, of the extreme difficulty, not to fay impracticability, of obtaining, in common fires, or in common furnaces, an uniform heat through the extent even of a few inches. Incredible as this may appear at firft fight, whoever will follow me in the operations I have gone through, placing accurate meafures of the heat in different parts of one and the fame veffel, will foon be convinced of its truth, and that he can no otherwife expect to communicate with certainty an equal heat to different pieces, than by ufing a fire of fuch magnitude as to exceed perhaps fome hundreds of times the bulk of the matters required to be heated.

To fuch large body of fire, therefore, after many fruitlefs attempts in fmall furnaces, not a little difcouraging by the irregularity of their refults, I at length had recourfe, fitting up for this purpofe an iron oven, ufed for the burning-on of enamel colours upon earthen ware, about four feet long, by two and a half wide, and three feet high, which is heated by the flame of wood conducted all round it. An iron muffle, four inches wide, two inches and three quarters high, and ten inches long, containing the gage and piece, was placed in the middle of this oven, and the vacancy between them filled up with earthen ware, to increafe the quantity of ignited matter, and thereby communicate the heat more equably from the oven to the muffle. In fuch a fituation of the muffle, in the center

center of an oven more than five hundred times its own capacity, it could not well fail of being heated pretty uniformly, at leaſt through the ſmall ſpace which theſe experiments required; nor have I found any reaſon to ſuſpect that it was not ſo.

The gage being laid flat upon the bottom of the muffle, with the ſilver piece in the canal as before, ſome of the clay thermometer pieces were ſet on end upon the ſilver piece, with that end of each downwards which is marked to go foremoſt in meaſuring it; that is, they were in contact with the ſilver in that part of their ſurface by which their meaſure is afterwards aſcertained. I was led to this precaution by an experiment I had made upon another occaſion, in which a number of thermometer pieces having been ſet upright upon an earthen-ware plate, over a ſmall fire, till the plate became red-hot, all the pieces were found diminiſhed, ſome of them more than two degrees, at the lower ends which reſted upon the plate, whilſt the upper ends were as much enlarged, not having yet paſſed the ſtage of extenſion which, as obſerved in the former paper, always precedes the thermometric diminution: thus we ſee how punctually every part of the piece obeys the heat that acts upon it.

The fire about the oven was ſlowly increaſed for ſome hours, and kept as even and ſteady as poſſible, by an experienced fireman, under my own inſpection. Upon opening a ſmall door, which had been made for introducing the apparatus, and looking in from time to time, it was obſerved, that the muffle, with the adjacent parts of the oven and ware, acquired a viſible redneſs at the ſame time; and in the progreſs of the operation, the eye could not diſtinguiſh the leaſt diſſimilarity in the aſpect of the different parts; whereas in ſmall fires, the difference not only between the two ends of the muffle, but in much leſs diſtances, is ſuch as to ſtrike the eye at once.

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When the muffle appeared of a low red heat, ſuch as was judged to come fully within the province of my thermometer, it was drawn forward, towards the door of the oven; and its own door being then nimbly opened by an aſſiſtant, I immediately puſhed the ſilver piece as far as it would go. But as the diviſion which it went to could not be diſtinguiſhed in that ignited ſtate, the muffle was lifted out, by means of an iron rod paſſed through two rings made for that purpoſe, with care to keep it ſteady, and avoid any ſhake that might endanger the diſplacing of the ſilver piece.

When grown ſufficiently cold to be examined, I noted the degree of expanſion which the ſilver piece ſtood at, and the degree of heat ſhewn by the thermometer pieces meaſured in their own gage; then returned the whole into the oven as before, and repeated the operation with a ſtronger heat, to obtain another point of correſpondence on the two ſcales.

The firſt was at $2°\frac{1}{4}$ of my thermometer, which coincided with 66° of the intermediate one; and as each of theſe laſt has been before found to contain 20 of FAHRENHEIT'S, the 66 will contain 1320; to which add 50, the degree of his ſcale to which the 0 of the intermediate thermometer was adjuſted, and the ſum, 1370, will be the degree of FAHRENHEIT'S correſponding to my $2°\frac{1}{4}$.

The ſecond point of coincidence was at $6°\frac{1}{4}$ of mine, and 92° of the intermediate; which 92 being, according to the above proportion, equivalent to 1840 of FAHRENHEIT, add 50 as before to this number, and my $6°\frac{1}{4}$ is found to fall upon the 1890th degree of FAHRENHEIT.

It appears from hence, that an interval of 4 degrees upon mine is equivalent to an interval of 520° upon his; conſequently 1 of mine to 130 degrees of his; and that the 0 of mine correſponds

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to his 1077°½. Several other trials were made, which gave results so nearly alike, that I have little apprehension of any material error.

From these data it is easy to reduce either scale to the other through their whole range; and from such reduction it will appear, that an interval of near 480° remains between them, which the intermediate thermometer serves as a measure for; that mine includes an extent of about 32000 of FAHRENHEIT's degrees, or about 54 times as much as that between the freezing and boiling points of mercury, by which mercurial ones are naturally limited; that if the scale of mine be produced downwards, in the same manner as we have supposed FAHRENHEIT's to be produced upwards, for an ideal standard, the freezing point of water would fall nearly on 8° below 0 of mine, and the freezing point of mercury a little below 8°½; and that, therefore, of the extent of now measurable heat, there are about $\frac{5}{10}$ths of a degree of my scale from the freezing of mercury to the freezing of water; 8° from the freezing of water to full ignition; and 160° above this to the highest degree I have hitherto attained.

As we are now enabled to compare not only the higher degrees among themselves, and the lower among themselves, upon their respective scales, but likewise the higher and lower with each other in every stage, it may be proper to take a general view of the whole range of measurable heat, as expressed both in FAHRENHEIT's denominations and in mine; and for this purpose I have drawn up a little table of a few of the principal points that have been ascertained, to shew their mutual relations or proportions to each other: any other points that have been, or hereafter may be, observed, by these or any other known thermometers, may be inserted at pleasure.

	FAHR.	WEDG.
Extremity of the ſcale of my thermometer	32277°	240°
Greateſt heat of my ſmall air-furnace -	21877	160
Caſt iron melts - - - -	17977	130
Greateſt heat of a common ſmith's forge	17327	125
Welding heat of iron, greateſt -	13427	95
——————— leaſt - -	12777	90
Fine gold melts - - -	5237	32
Fine ſilver melts - - -	4717	28
Swediſh copper melts - -	4587	27
Braſs melts - - -	3807	21
Heat by which my enamel colours are burnt on	1857	6
Red-heat fully viſible in day-light -	1077	0
Red-heat fully viſible in the dark -	947	1
Mercury boils - - -	600	$3\frac{673}{1000}$
Water boils - - -	212	$6\frac{658}{1000}$
Vital heat - - -	97	$7\frac{542}{1000}$
Water freezes - - -	32	$8\frac{42}{1000}$
Proof ſpirit freezes - - -	0	$8\frac{289}{1000}$
The point at which mercury congeals, conſequently the limit of mercurial thermometers, - -	about 40	$8\frac{596}{1000}$

To aſſiſt our conceptions of this ſubject, it may be proper to view it in another light, and endeavour to preſent it to the eye; for *numbers*, on a high ſcale, are with difficulty eſtimated and compared by the mind. I have therefore completed the ſcales of which a part is repreſented in fig. 1. and 3. by continuing the ſame equal diviſions, both upwards and downwards, as far as the utmoſt limits of heat that have hitherto been attained and meaſured *.

* Mr. WEDGWOOD preſented this, in the form of a very long roll, to the Society.

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In a ſcale of heat drawn up in this manner, the comparative extents of the different departments of this grand and univerſal agent are rendered conſpicuous at a ſingle glance of the eye. We ſee at once, for inſtance, how ſmall a portion of it is concerned in animal and vegetable life, and in the ordinary operations of nature. From freezing to vital heat is barely a five-hundredth part of the ſcale; a quantity ſo inconſiderable, relatively to the whole, that in the higher ſtages of ignition, ten times as much might be added or taken away, without the leaſt difference being diſcernible in any of the appearances from which the intenſity of fire has hitherto been judged of. From hence, at the ſame time, we may be convinced of the utility and importance of a phyſical meaſure for theſe higher degrees of heat, and the utter inſufficiency of the common means of diſcriminating and eſtimating their force. I have too often found differences, aſtoniſhing when conſidered as a part of this ſcale, in the heats of my own kilns and ovens, without being perceivable by the workmen at the time, or till the ware was taken out of the kiln.

SINCE the foregoing experiments were made, I have ſeen a very curious Memoir by Meſſ. LAVOISIER and DE LA PLACE, containing a method of meaſuring heat by the quantity of ice which the heated body is capable of liquefying. The application of this important diſcovery, as an intermediate ſtandard meaſure between FAHRENHEIT's thermometer and mine, could not eſcape me, and I immediately ſet about preparing an apparatus, and making the experiments neceſſary for that pur-

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pose; in hopes either of attaining by this method a greater degree of accuracy than I could expect from any other means, or of having what I had already done confirmed by a series of experiments upon a different principle.

But in the prosecution of these experiments I have, to my great mortification, hitherto failed of success; and I should have contented myself for the present with saying little more than this, if some phænomena had not occurred, which appear to me not unworthy of farther investigation.

The authors observe, that if ice, cooled to whatever degree below the freezing point, be exposed to a warmer atmosphere, it will be brought up to the freezing point through its whole mass before any part of its surface begins to liquefy; and that consequently ice, beginning to melt on the surface, will be always exactly of the same temperature, *viz.* at the freezing point; and that if a heated body be inclosed in a hollow sphere of such ice, the whole of its heat will be taken up in liquefying the ice; so that if the ice be defended from external warmth, by surrounding it with other ice in a separate vessel, the weight of the water produced from it will be exactly proportional to the heat which the heated body has lost; or, in other words, will be a true physical measure of the heat.

For applying these principles in practice, they employ a tin vessel, divided, by upright concentric partitions, into three compartments, one within another. The innermost compartment is a wire cage, for receiving the heated body. The second, surrounding this cage, is filled with pounded ice, to be melted by the heat; and the outermost is filled also with pounded ice, to defend the former from the warmth of the atmosphere. The first of these ice compartments terminates at bottom in a stem like a funnel, through which the water is conveyed off; and the other ice compartment terminates in a separate canal, for difcharging the water into which *that* ice is reduced. As foon as the heated body is dropped into the cage, a cover is put on, which goes over both that and the firft ice compartment; which cover is itfelf a kind of fhallow veffel, filled with pounded ice, with holes in the bottom for permitting the water from this ice to pafs into the fecond compartment, all the liquefaction that happens here, as well as there, being the effect of the heated body only. Over the whole is placed another cover with pounded ice, as a defence from external warmth.

As foon as this difcovery came to my knowledge, on the 23d of February, a thaw having begun three days before, after a froft which had continued with very little intermiffion from the 24th of December, I collected a quantity of ice, and ftored it up in a large cafk in a cellar.

I thought it neceffary to fatisfy myfelf in the firft place, by actual experiment, that ice, how cold foever it may be, comes up to the freezing point through its whole mafs before it begins to liquefy on the furface. For this purpofe I cooled a large fragment of ice, by a freezing mixture, to 17° of FAHRENHEIT's thermometer, and then hung it up in a room whofe temperature was 50°. When it began to drop, it was broken, and fome of the internal part nimbly pounded and applied to the bulb of a thermometer that was cooled by a freezing mixture below 30°. The thermometer rofe to, and continued at, 32°; being then taken out, and raifed by warmth to 40°, fome more of the fame ice, applied as before to the bulb, funk it again to 32°; fo that no doubt could remain on this fubject.

Apprehenfive that pounded ice, directed by the authors, might imbibe and retain more or lefs of the water by capillary attraction, according to circumftances, and thereby occafion fome error in the refults, I thought it neceffary to fatisfy myfelf in this refpect alfo alſo by experiment. I therefore pounded ſome ice, and laid it in a conical heap on a plate; and having at hand ſome water, coloured with cochineal, I poured it gently into the plate, at ſome diſtance from the heap: as ſoon as it came in contact with the ice, it roſe haſtily up to the top; and on lifting up the lump, I found that it held the water, ſo taken up, as a ſponge does, and did not drop any part of it till the heat of my hand, as I ſuppoſe, began to liquefy the maſs. On further trials I found, that in pounded ice preſſed into a conical heap, the coloured water roſe, in the ſpace of three minutes, to the height of two inches and a half; and by weighing the water employed, and what remained upon the plate unabſorbed, it appeared, that four ounces of ice had thus taken up, and retained, one ounce of water.

To further aſcertain this abſorbing power, in different circumſtances, more analogous to thoſe of the proceſs itſelf, I preſſed ſix ounces of pounded ice pretty hard into the funnel, having firſt introduced a wooden core in order to leave a proper cavity in the middle: then, taking out the core, and pouring an ounce of water upon the ice, I left the whole for half an hour; at the end of which time the quantity that ran off was only 12 pennyweights and 4 grains, ſo that the ice had retained 7 pennyweights and 20 grains, which is nearly one-twelfth of its own weight, and two-fifths of the weight of the water.

Theſe previous trials determined me, inſtead of uſing pounded ice, to fill a proper veſſel with a ſolid maſs of ice, by means of a freezing mixture, as the froſt was now gone, and then expoſe it to the atmoſphere till the ſurface began to liquefy. The apparatus I fitted up for this purpoſe was made of earthen ware well glazed, and is repreſented in fig. 6. (tab. XV.).

A, is

A, is a large funnel, filled with a ſolid maſs of ice. B, a cavity in the middle of this ice, formed, part of the way, by ſcraping with a knife, and for the remaining part, by boring with a hot iron wire. C, one of my thermometer pieces, which ſerves for the heated body, and reſts upon a coil of braſs wire: it had previouſly been burnt with ſtrong fire, that there might be no danger of its ſuffering any further diminution of its bulk by being heated again for theſe experiments. D, a cork ſtopper in the orifice of the funnel. E, the exterior veſſel, having the ſpace between its ſides and the included funnel A, filled with pounded ice, as a defence to the ice in the funnel. F, a cover for this exterior veſſel, filled with pounded ice for the ſame purpoſe. G, a cover for the funnel, filled alſo with pounded ice, with perforations in the bottom for allowing the water from this ice to paſs down into the funnel.

The thermometer piece was heated in boiling water, taken up with a pair of ſmall tongs equally heated, dropped inſtantly into the cavity B, and the covers put on as expeditiouſly as poſſible; the bottom of the funnel being previouſly corked, that the water might be detained till it ſhould part with all its heat, and likewiſe to prevent the water from the other ice, which ran down on the outſide of the funnel, from mingling with it.

After ſtanding about ten minutes, the funnel was taken out, wiped dry, and uncorked over a weighed cup: the water that ran out weighed 22 grains. Thinking this quantity too ſmall, as the piece weighed 72 grains, I repeated the experiment, and kept the piece longer in the funnel; but the water this time weighed only 12 grains. Being much diſſatisfied with this reſult, I made a third trial, continuing the piece much longer in the cavity; but the quantity of water was now ſtill leſs, not amounting to quite three drops; and, to my great ſurpriſe, I found the piece frozen to the ice, ſo as not to be eaſily got off, though all the ice employed was, at the beginning of the experiment, in a thawing ſtate.

I had prepared the apparatus for taking the boiling heat of mercury; but being entirely diſcouraged by theſe very unequal reſults, I gave that up, for the preſent at leaſt, and heating the piece to 6° of my thermometer, turned it nimbly out of the caſe in which it was heated into the cavity, throwing ſome fragments of ice over it. In about half an hour, I drew off the water, which amounted to 11 pennyweights; then ſtopping the funnel again, and replacing the covers, I left the whole about ſeven hours.

At the end of that time, I found a conſiderable quantity of water in the funnel: the melting of the ice had produced a cavity between it and the ſides, great part of the way down, which, as well as that in the middle, was nearly full. The water nevertheleſs ran out ſo ſlowly, that I apprehended ſomething had ſtopped the narrow end of the funnel, but the true cauſe became afterwards apparent upon examining the ſtate of the ice. The fragments which I had thrown over the thermometer piece were frozen entirely together, and in ſuch a form as they could not have aſſumed without freſh water ſuperadded and frozen upon them, for the cavities between them were partly filled with new ice. I endeavoured to take the ice out with my fingers, but in vain; and it was with ſome difficulty I could force it aſunder even with a pointed knife, to get at the thermometer piece. When that was got out, great part of the coiled wire was found enveloped in new ice. The paſſage through the ice to the ſtem of the funnel, which I had made pretty wide with a thick iron wire red-hot, was ſo nearly cloſed

up,

up, that the ſlow draining off of the water was now ſufficiently accounted for, and indeed this draining was the only apparent mark of any paſſage at all. On taking the ice out of the funnel, and breaking it to examine this canal, I found it almoſt entirely filled up with ice projecting from the ſolid maſs in cryſtalline forms, ſimilar in appearance to the cryſtals we often meet with in the cavities of flints and quarzoſe ſtones.

If, after all theſe circumſtances, any doubt could have remained of the ice in queſtion being a new production, a fact which I now obſerved muſt have removed all ſuſpicion. I found a coating of ice, of conſiderable extent and perfectly tranſparent, about a tenth of an inch in thickneſs, upon the outſide of the funnel, and on a part of it which was not in contact with the ſurrounding ice, for that was melted to the diſtance of an inch from it.

Some of the ice being ſcraped off from the inſide of the funnel, and applied to the bulb of the thermometer, the mercury ſunk from 50° to 32°, and continued at that point till the ice was melted; after which, the water being poured off, it roſe in a little time to 47°.

Aſtoniſhed at theſe appearances, of the water freezing after it had been melted, though ſurrounded with ice in a melting ſtate, and in an atmoſphere about 50°, where no part of the apparatus or materials could be ſuppoſed to be lower than the freezing point, I ſuſpected at firſt that ſome of the ſalt of the freezing mixture might have got into the water, and that this, in diſſolving, might perhaps abſorb, from the parts contiguous to it, a greater proportion of heat than the ice of pure water does. But the water betrayed nothing ſaline to the taſte, and I had applied the freezing mixture with my own hands with great care, to prevent any of it being mixed with the water.

 To

To remove all doubts, however, upon this point, I purposed repeating the experiment with some pieces of the ice I had stored up in the cellar, to see if this would congeal, after thawing, in the same manner. But going to fetch the ice, and examining it in the cask in which it was kept, I was perfectly satisfied with the appearances I found there; for though much of it was melted, yet the fragments were frozen together, so that it was with difficulty I could break or get out any pieces of it with an iron spade; and, when so broken, it had the appearance of *breccia* marble or plum-pudding stone, for the fragments had been broken and rammed into the cask with an iron mall.

A porcelain cup being laid upon some of this ice about half an hour, in a room whose temperature was 50°, it was found pretty firmly adhering, and when pulled off, the ice exhibited an exact impression of the fluted part of the cup which it had been in contact with; so that the ice must necessarily have liquefied first, and afterwards congealed again. This was repeated several times, with the same event. Fragments of the ice were likewise applied to one another, to sponges, to pieces of flannel and of linen cloth, both moist and dry: all these, in a few seconds, began to cohere, and in about a minute were frozen so as to require some force to separate them. After standing an hour, the cohesion was so firm, that on pulling away the fragments of ice from the woollen and sponge, they tore off with them that part of the surface which they were in contact with, though at the same time both the sponge and flannel were filled with water which that very ice had produced.

To make some estimate of the force of the congelation, which was stronger on the two bodies last mentioned than on linen, I applied a piece of ice to a piece of dry flannel which weighed two pennyweights and a half, and furrounded them with other ice. After lying together three quarters of an hour, taking the piece of ice in my hand and hooking the flannel to a fcale, I found a weight of five ounces to be neceffary for pulling it off, and yet fo much of the ice had liquefied as to increafe the weight of the flannel above 12 pennyweights. I then weighed the piece of ice, put them together again, and four hours after found them frozen fo firmly as to require 78 ounces for their feparation, although, from 42 pennyweights of the ice, 15 more had melted off: the furface of contact was at this time nearly a fquare inch. I continued them again together for feven hours; but they now bore only 62 ounces, the ice being diminifhed to 14 pennyweights, and the furface of contact reduced to about fix-tenths of a fquare inch.

Having feen before that pounded ice abforbs water in very confiderable quantity, I fufpected that fomething of the fame kind might take place even with entire maffes; and experiment foon convinced me, that even apparently folid pieces of ice will imbibe water, flower or quicker according to its ftage of decay. I have repeatedly heated fome of my thermometer pieces, and laid them upon ice, in which they made cavities of confiderable depth, but the water was always abforbed, fometimes as faft as it was produced, leaving both the piece and the cavity dry.

Thus, though I cannot fufficiently exprefs how much I admire the difcovery that gave rife to thefe experiments, I have neverthelefs to lament my not being able to avail myfelf of it at prefent for the purpofe I wifhed to apply it to.

That in my experiments the two feemingly oppofite proceffes of nature, congelation and liquefaction, went on together, at

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the ſame inſtant, in the ſame veſſel, and even in the ſame fragment of ice, is a fact of which I have the fulleſt evidence that my ſenſes can give me; and I ſhall take the liberty of ſuggeſting a few hints, which may tend perhaps to elucidate their cauſe, and to ſhew that they are not ſo incompatible as at firſt ſight they appear to be.

It occurred to me at firſt, that water highly attenuated and divided, as when reduced into vapour, may freeze with a leſs degree of cold than water in its aggregate or groſſer form; hence hoar-froſt is obſerved upon graſs, trees, &c. at times when there is no appearance of ice upon water, and when the thermometer is above the freezing point*. BOERHAAVE, I find, in his elaborate theory of fire, aſſigns 33° as the freezing point of vapour, and even of water when divided only by being imbibed in a linen cloth.

* I am aware, that experiments and obſervations of this kind are not fully deciſive; that the atmoſphere may, in certain circumſtances, be much warmer or colder than the earth and waters, which, in virtue of their denſity, are far more retentive of the temperature they have once received, and leſs ſuſceptible of tranſient impreſſions; that even inſenſible undulations of water, from the ſlighteſt motion of the air, by bringing up warmer ſurfaces from below, may prove a further impediment to the freezing; and, therefore, that the degree of cold, which is ſufficient to produce hoar-froſt, may poſſibly, if continued long enough, be ſufficient alſo to produce ice. I am not acquainted with any ſatisfactory experiments or obſervations yet made upon the ſubject; nor do I advance the principle as a certain, but as a probable one, which occurred to me at the moment, which is countenanced by general obſervation, and conſentaneous to many known facts; for there are numerous inſtances of bodies, in an extreme ſtate of diviſion, yielding eaſily to chemical agents which, before ſuch diviſion, they entirely reſiſt: thus ſome precipitates, in the very ſubtile ſtate in which they are at firſt extricated from their diſſolvents, are re-diſſolved by other menſtrua, which, after their concretion into ſenſible moleculæ, have no action upon them at all.

 Now,

Now, as the atmoſphere abounds with watery vapour, or water diſſolved and chemically combined, and muſt be particularly loaded with it in the neighbourhood of melting ice; as the heated body introduced into the funnel muſt neceſſarily convert a portion of the ice or water there into vapour; and as ice is known to melt as ſoon as the heat begins to exceed 32°, or nearly one degree lower than the freezing point of vapour; I think we may from hence deduce, pretty ſatisfactorily, all the phænomena I have obſerved. For it naturally follows from theſe principles, that vapour may freeze where ice is melting; that the vapour may congeal even upon the ſurface of the melting ice itſelf; and that the heat which (agreeably to the ingenious theory of Dr. BLACK) it emits in freezing, may contribute to the further liquefaction of that very ice upon which the new congelation is formed.

I would further obſerve, that the freezing of water is attended with plentiful evaporation in a cloſe as well as an open veſſel, the vapour in the former condenſing into drops on the under ſide of the cover, which either continue in the form of water, or aſſume that of ice or a kind of ſnow, according to circumſtances *; which evaporation may perhaps be attributed to the heat that was combined with the water, at this moment rapidly making its eſcape, and carrying part of the aqueous fluid off with it. We are hence furniſhed with a freſh and continual ſource of vapour as well as of heat; ſo that the proceſſes of liquefaction and congelation may go on uninterruptedly together, and even neceſſarily accompany one another, although, as the freezing muſt be in an under proportion to the melting, the whole of the ice muſt ultimately be conſumed.

* See Mr. BARON's paper on this ſubject, in the Memoires of the Academy of Sciences at Paris for the year 1753.

In

In the remarkable inſtance of the coating of ice on the outſide of the throat of the funnel, there are ſome other circumſtances which it may be proper to take notice of. Neither the cover of the outer veſſel, nor the aperture in its bottom which the ſtem of the funnel paſſed through, were air-tight, and the melting of the ſurrounding ice had left a vacancy of about an inch round that part of the funnel on which the cruſt had formed. As there was, therefore, a paſſage for air through the veſſel, a circulation of it would probably take place: the cold and denſe air in the veſſel would deſcend into the rarer air of the room then about 50°, and be replaced by air from above. The effect of this circulation and ſudden refrigeration of the air will be a condenſation of part of the moiſture it contains upon the bodies it is in contact with: the throat of the funnel, being one of thoſe bodies, muſt receive its ſhare; and the degree of cold in which the ice thaws being ſuppoſed ſufficient for the freezing of this moiſt vapour, the contact, condenſation, and freezing, may happen at the ſame inſtant.

The ſame principles apply to every inſtance of congelation that took place in theſe experiments; and a recollection of particulars which paſſed under my own eye convinces me, that the congelation was ſtrongeſt in thoſe circumſtances where vapour was moſt abundant, and on thoſe bodies which, from their natural or mechanic ſtructure, were capacious of the greateſt quantity of it; ſtronger, for inſtance, on ſponge than on woollen, ſtronger on this than on the cloſer texture of linen, and far ſtronger on all theſe than on the compact ſurface of porcelain.

If, nevertheleſs, the principle I have aſſumed (that water highly attenuated will congeal with a leſs degree of cold than water in the maſs) ſhould not be admitted; another has above

been

been hinted at, which experiments have decidedly eſtabliſhed, from which the phænomena may perhaps be equally accounted for, and which, even though the other alſo is received, muſt be ſuppoſed to concur for ſome part of the effect; I mean, that *evaporation produces cold*; both vapour and ſteam carrying off ſome proportion of heat from the body which produces them. If, therefore, evaporation be made to take place upon the ſurface of ice, the contiguous ice will thereby be rendered colder; and as it is already at the freezing point, the ſmalleſt increaſe of cold will be ſufficient for freſh congelation. It ſeems to be on this principle that the formation of ice is effected in the Eaſt Indies, by expoſing water to a ſerene air, at the coldeſt ſeaſon of the year, in ſhallow porous earthen veſſels: part of the water tranſudes through the veſſel, and evaporating from the outſide, the remainder in the veſſel becomes cold enough to freeze; the warmth of the earth being at the ſame time intercepted by the veſſels being placed upon bodies little diſpoſed to conduct heat *. If ice is thus producible in a climate where natural ice is never ſeen, we need not wonder that congelation ſhould take place where the ſame principle operates amidſt actual ice.

It has been obſerved above, that the heat emitted by the congealing vapour probably unites with and liquefies contiguous portions of ice; but whether the whole, either of the heat ſo emitted, or of that originally introduced into the funnel, is thus taken up; how often it may unite with other portions of ice, and be driven out from other new congelations; whether there exiſts any difference in its chemical affinity or

* See a deſcription of this proceſs in the Philoſophical Tranſactions, vol. LXV. p. 253.

elective

elective attraction to water in different ſtates and the contiguous bodies; whether part of it may not ultimately eſcape, without performing the office expected from it upon the ice; and to what diſtance from the evaporating ſurface the refrigerating effect of the evaporation may extend; muſt be left for further experiments to determine.

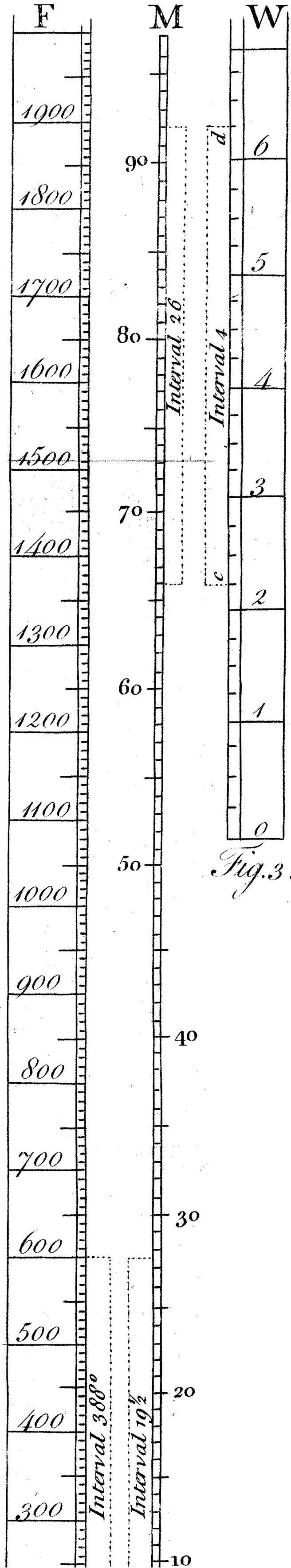


Fig. 3.

Fig. 4.

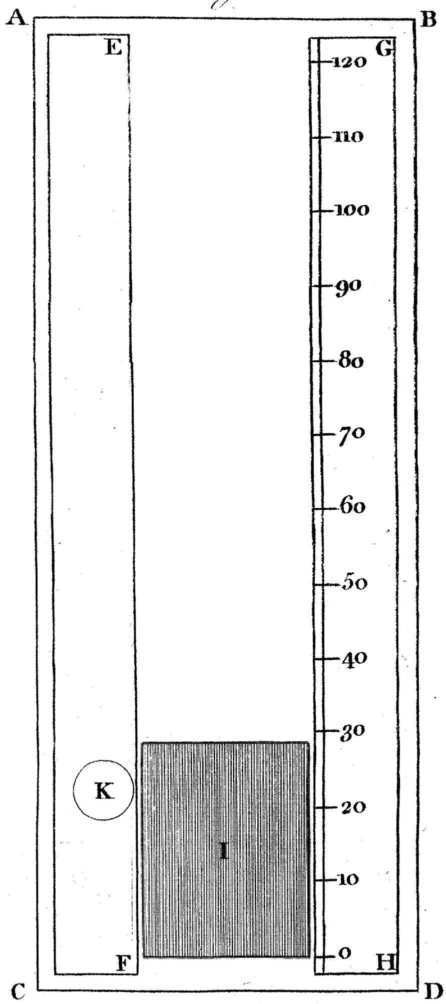
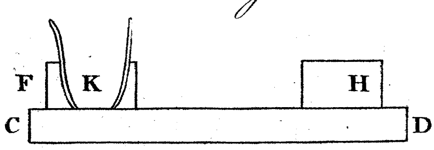


Fig. 5.



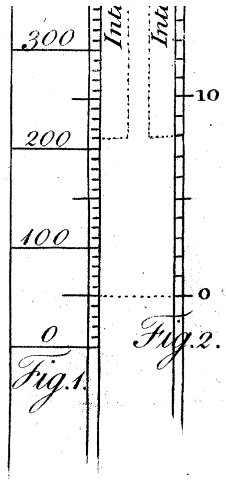
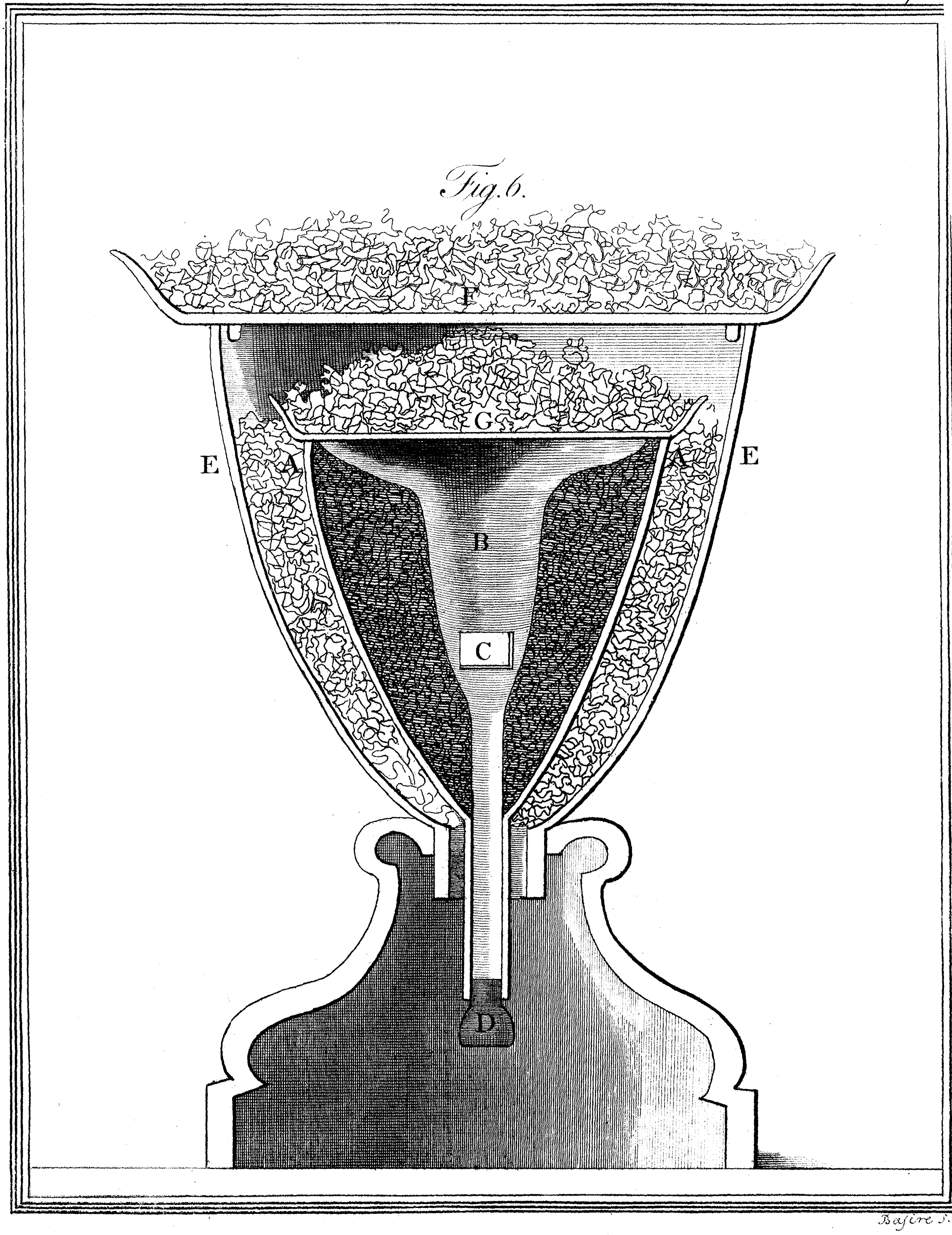


Fig.1. *Fig.2.*

Basire S.



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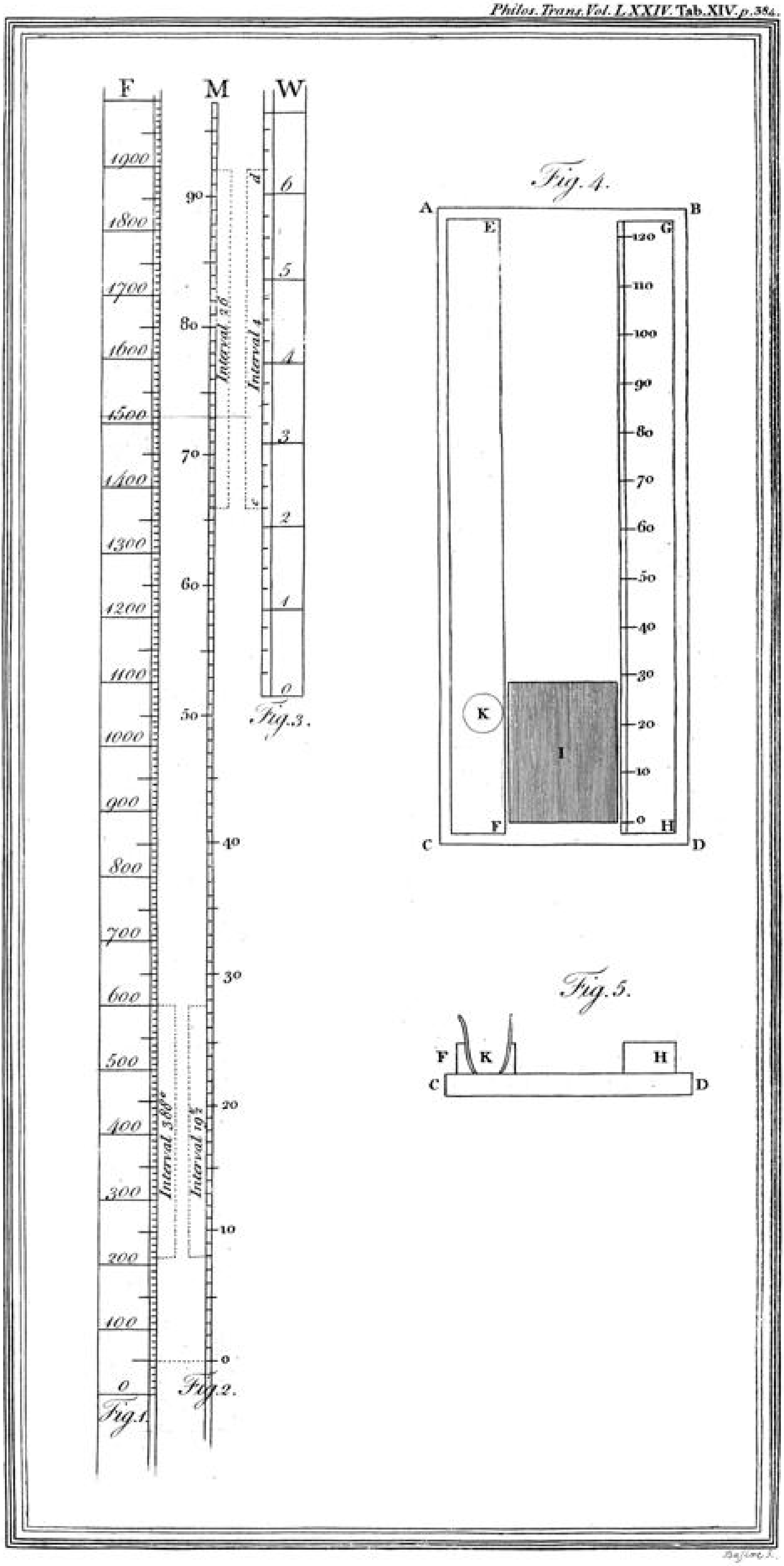
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Fig. 1.
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Fig. 2.
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Interval 4
Interval 388°
Interval 19°
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Fig. 3.
Fig. 4.
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Fig. 5.
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Basire sc.