

XXII. *Additional Observations on making a Thermometer for measuring the higher Degrees of Heat. By Mr. Josiah Wedgwood, F. R. S. and Potter to Her Majesty.*

Read June 22, 1786.

**M**Y thermometer for measuring the higher degrees of heat having been honoured with the notice of this illustrious Society, I now request a further indulgence for a few more observations on the same subject.

In my first Paper \* I communicated every thing that experience had then taught me, respecting both the construction and use of this thermometer; but more extensive practice has since convinced me, that other managements and precautions are necessary, in order to bring it to the perfection it is capable of receiving: for pieces made of the same clay, and exactly of the same dimensions, have been found to differ in the degree of their diminution by fire, in consequence of some circumstances in the mode of their formation, at that time unheeded, and very difficult to be developed.

Of the two ways proposed for forming them, the mould and the press, the former was made choice of, as being, for general use, the most commodious. The soft clay was pressed into a square mould with the fingers; and the pieces, when dry, were pared down on two opposite sides, by means of a

\* Philosophical Transactions, Vol. LXXII.

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paring gage made for that purpose, so as to pass exactly to  $0^{\circ}$  at the entrance of the converging canal of the measuring gage.

But the pieces thus formed have been found liable, in passing through strong fire, to receive a little alteration in their figure, which produces an uncertainty with respect to their subsequent measurement: the two sides, instead of continuing flat, become concave; the edges, both at top and bottom, projecting beyond the middle part, sometimes very considerably, as at *a* and *b*, fig. 1, (Tab. XIV.) where AB represents a perpendicular section of an unburnt piece, and *ab* a like section of the same piece after it has undergone a heat of 160 degrees. This irregularity in the form, which is sensible only after passing through the high degrees of fire, was observed in some of the early experiments, but was not then looked upon as being productive of any error.

On more attentively examining this matter, it appeared, that when the clay is pressed into a mould, the surface in contact with the mould acquires a more compact texture than the inner part of the mass;—that this compactness restrains, in some degree, its diminution in the fire;—and therefore, that when this surface, or less diminishable crust, is pared off from the two sides only, the piece may be considered as having its upper and lower strata (AA and BB, fig. 1.) composed of a less diminishable matter than the intermediate part, the necessary consequence of which structure will be such a figure as we find the pieces to assume; for if any stratum in the mass shrinks less than the rest, the extremities of that stratum must be left proportionably prominent. That this was the true cause of the inequality, I was convinced by firing some pieces *unadjusted*, with all their surfaces entire, as they came from the mould; for these pieces, after passing through the same strong

fires with the preceding, continued flat, with the angles regularly sharp, and without the least sensible prominence in any part.

Some of the moulds, employed for this use, were made of plaster, a material more convenient for the workman than metal, as the pieces part more freely from it, but which contributed greatly to increase the above-mentioned irregularity: for the plaster, by absorbing a portion of the water from the clay contiguous to it, renders the surface at the same time, even at the instant of contact, much more *consistent*, and consequently more difficult to press into the angles of the mould; so that the outsides of these pieces were not only more *compressed*, but formed of clay of a different *temper* from the inner parts, being much drier or firmer; a circumstance which, as will appear hereafter, restrains still more their diminution in the fire.

The moulds were therefore laid aside, and the press adopted in their stead; for as the soft clay, pressed in a cylindrical vessel, gives way and escapes through an aperture made for that purpose (by which means it is formed into long rods), the sides of the piece cannot be supposed to receive so great a degree of compressure against the sides of the aperture through which it is *delivered* in this operation, as it does against the sides of the mould, by which it is *confined* till every part has born a pressure sufficient to force the clay into every angle, which is much greater than even a workman would imagine till he comes to try the experiment himself.

But with this change some new difficulties arose; for pieces pressed through the same aperture, and from the same pressful of clay, and adjusted, when dry, to the same point in the gage, were found, after passing together through the same  
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strong fires, to differ in their dimensions from one another, in some instances more than any of the preceding.

Having hitherto paid no particular attention myself to the mere manual labour of pressing the clay, I determined, upon this event, to go through that and every other operation, however simple and seemingly insignificant, with my own hands. In doing this I observed, that the power necessary for forcing the clay through an aperture which bore but a small proportion to the diameter of the mass of clay in the press, was so great as to squeeze out, along with the clay that first passed through, a considerable portion of the water that belonged to the rest. From this over-proportion of water in the composition of the first pieces they were soft and spongy, and the succeeding ones more and more compact, till at length the clay proved so stiff as scarcely to be forced through at all.

Clay, containing different proportions of water, is well known to diminish differently in drying; but it was not imagined that, when dry, there would be any difference in its subsequent diminutions by fire. Experiments however, multiplied in a variety of circumstances, shewed decisively, what the pieces formed in the mould had given grounds to suspect, that those formed of the softest clay, and which had undergone the least pressure, diminished most in burning; and that the diminution is uniformly less and less, in proportion to the greater degree of pressure or compactness.

The knowledge of the cause of the irregularity suggested a remedy. I lessened the width of the press very much, so as to bring the diameter of the mass of clay, and that of the aperture through which it is delivered, to a nearer proportion with one another. A much less degree of force being now sufficient, the pieces, or rods, were proportionably more uniform, though

though there was still a sensible difference, in consistence, between those which were first and last pressed out from the same mass of clay. The intermediate ones, within a certain distance from the two extremes, corresponded very nearly with one another; so that by rejecting a sufficient number of the first and last, and using the *intermediate ones only*, the inequality may be considered as almost annihilated.

I nevertheless still found that, in strong fire, the edges became a little prominent, though not so much as before. I was aware that these pieces must partake, in some degree, of the imperfection of those made in the mould; having their surfaces rendered, by their friction against the sides of the aperture, more compact than the inner part. But I suspected that something might depend also upon the *form*, and accordingly made many trials for ascertaining the form that might be least liable to this irregularity: the angles only were bevelled off, the sides were rounded, the pieces were rounded all over, made of ovals and other curves, and both the longest and shortest dimensions were used as the extent to be measured: the general result was, that the nearer they came to a circular figure, the less inequality they contracted in the fire, and by making them entirely circular, the imperfection appeared to be obviated altogether; cylindric pieces bearing the strongest fires without the least appearance of prominence or inequality in any part of their surface. I have therefore chosen this last form, leaving only one narrow flat side (*ab*, fig. 2.) as a bottom for the pieces to rest upon, and to distinguish the position in which they are to be measured in the gage.

I have endeavoured at the same time to obviate whatever inaccuracy the inequality of compactness may be capable of producing, by so adjusting the aperture through which the

rods are pressed, and on which their figure and dimensions depend, as to supersede the use of the paring gage altogether; that the whole surface may remain of the same uniform compactness which it received in the press. And as it is scarcely practicable, in any mode of forming soft clay, to have all the pieces precisely of the same dimensions after drying, I do not reject those which come within two or three degrees of the standard, but, instead of injuring the surface by paring or rubbing, I mark on the ends the degrees which they respectively exceed or fall short; which degrees are accordingly to be subtracted, or added, in all observations of heat made with those pieces. Strictly speaking, an allowance ought to be made also for the proportional diminution upon this excess or deficiency itself; but the allowance for three degrees would not, at the melting heats of copper, silver, or gold, amount to more than a seventh part of a degree; and at the extreme point of heat that I have been able to attain, when the piece has diminished  $\frac{1}{600}$ , or nearly one-fourth of its whole thickness, it comes only to four-fifths of a degree.

It may be proper to take notice of an irregularity in the *apparent* diminutions of the pieces, which was sometimes observed to happen from another cause, their bending a little in strong fire, so as to be prevented from going so far in the gage as they would have done if they had continued perfectly straight. But as this takes place only in pieces of considerable length, and as they derive no advantage of any kind from that length, the remedy is too obvious to need being here mentioned.

Another fallacious appearance arose, not from any imperfection in the pieces themselves, but from a deception with respect to the heat in which the comparison of them had been

made. In one period of the course of my experiments, I employed, for firing them, a small, shallow, cylindrical vessel (fig. 3.) setting the pieces on end, close together, on its bottom, and placing it in the middle of the fuel, in a common air-furnace, with care to keep the fire as equal all round it as possible. It was expected, that all the pieces would receive an equal heat; and as they were found, after the operation, to differ in their dimensions, sometimes considerably, from one another, these differences proved a source of much perplexity, till it was discovered that the pieces had really undergone unequal degrees of heat.

In the paper on the comparison of this thermometer with FAHRENHEIT'S\*, I have taken notice of the great difficulty of obtaining, in small furnaces, a perfectly equal heat, even through the extent occupied by a few of these little pieces: and how different the heat may be in different parts of one vessel, we may be satisfied by an easy experiment, *viz.* setting a cylindrical rod of clay, of the length of eight or ten inches, upright in the middle of a crucible, and urging it with strong fire in a common small furnace; the rod will be found very differently diminished at different parts of its height; and if its height be sufficient to reach some way above the fuel, nearly the whole range of the thermometric scale may be produced in one rod; an ocular proof, not only of the diversity of heat within a small compass, but likewise of the *peculiar* sensibility of this thermometer, every *part* of the mass expressing distinctly the degree of heat which it has itself felt. It will be proper, in this experiment, to have a tube fixed in the bottom of the crucible, for keeping the rod steady, as at fig. 4. By this means the heat of my air-furnace renders a

\* Philosophical Transactions, vol. LXXIV.

rod of the thermometric clay tapering, from about four parts in diameter at top to three at bottom, which are nearly the proportions between the width of the piece when unburnt, or but just ignited, and when it has suffered a heat of 160 degrees.

To the foregoing sources of inequality in the pieces, one more may be added, small cavities, or air-bubbles accidentally inclosed, which sometimes happened in the earlier experiments. In order to prevent these, particular attention is now paid by the workmen to what we call *banding* or *slapping* the clay, an operation by which its different parts are intermixed, and the mass rendered of an uniform temper throughout. The workman takes a lump of the clay in his hands, perhaps of two pounds weight, and, breaking it in two in the middle, lays one part upon the other, and presses them flat again, repeating this forty or fifty times, or perhaps oftener. Now, considering the pieces at first as two dissimilar masses, with any number of air-bubbles inclosed; each of these pieces being by the first doubling divided into two, by the next into four, by the third into eight, and so on in geometrical progression, each of the original masses will be divided by the fiftieth repetition into upwards of eleven thousand millions of millions of invisible laminæ:—invisible, because the lump of clay would, long before the last doubling, be of one uniform colour, though at first one-half of it had been black, and the other white. If therefore no air be inclosed between the pieces at the times of their being put together in this process, all the air which might have been in the mass before would certainly be driven out; and, to avoid as much as possible the introduction of any fresh portions of air, the two separated pieces are each time made smooth, and a little convex, on the surfaces that are to be brought together.



By due attention to the circumstances above stated, any single quantity of clay may be made up into thermometer-pieces, that shall differ very little, if any thing at all, from one another.

But a new difficulty now arose, more embarrassing than any of the former; that of procuring fresh supplies of clay, of the same thermometric quality with the first. The quantity of the clay which, after trial of many others, I had made choice of, was small; but the particular spot it was taken from being known, and having purchased the little estate in which it was raised, I had not a doubt of obtaining more of the same when it should be wanted: for clays in general, when raised from an equal depth, in the same stratum, and near the same place, are found to possess the same properties, with respect to ductility in the hands of the workman, a disposition to assume by fire a porcelain or vitreous texture, singly or in composition, and all other qualities relative to their use in pottery. In this, however, I was deceived; for when a fresh supply was wanted, to complete my experiments, though I had some taken from a pit joining to the first, and at the same depth, it was found to diminish differently from the former parcel. I then had some raised from different parts of the same field and bed, and at different depths; and in various other places in Cornwall, from the spot where this species of clay is first met with to the Land's-End; but all these clays differed so much from the first in the quantity of their diminution by fire, and most of them likewise from each other, that I despaired of being ever able to find one that would correspond with it, or any natural clays that could be obtained twice of exactly the same *thermometric* properties, how similar soever in other respects.

Upon a review of the numerous comparisons which I have made of these new clays, in different degrees of heat, from the

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commencement of redness up to intense fire, the most striking differences of the greatest part of them from the old seemed to originate in the lower stages of heat; and of those which were got from the neighbourhood of the old, the variations from it in the higher stages seemed, for the most part, to be only consequences of those differences in the lower ones.

I have mentioned, in the first Paper, that the original thermometer-pieces had their bulk enlarged a little on the approach of ignition; but that by the time they became visibly red-hot throughout, they are reduced to their former dimensions again; and at this moment the thermometric diminution begins. The new clays had their bulk enlarged in a much greater proportion, and the enlargement was of much longer continuance: some of them required a heat of 15 degrees to destroy the increase which ignition had produced in their bulk, and bring them back to their original dimensions: after this period, most of them diminished pretty regularly, and uniformly with the old, being nearly so many degrees behind it, in all the succeeding stages of heat, as they required to bring them back from the enlarged state.

I have mentioned also, in my former paper, that a quantity of air is extricated from the clay, most rapidly at the period in which the augmentation of bulk takes place; and that the augmentation was probably owing to this air forcing the particles of the clay a little asunder, previous to the instant of its escape. It was therefore presumed, that the greater extension of these new clays might be owing, either to a greater quantity, or stronger adhesion, of this combined air: and as clay, kept moist for a length of time, in certain circumstances, undergoes a process seemingly analogous to fermentation,

tion, it was hoped that, by such a process, part of its combined air might be detached.

But experiments made on this idea have proved, that these clays, kept moist for a twelvemonth,—kept for a considerable length of time in a heat just below visible redness,—boiled in water for many hours,—alternately, and repeatedly, moistened and dried,—suffer no alteration in their thermometric properties, and continue to differ from the standard clay just as much as they did at first.

Some of these new clays differed from the old in a property still more essential, and by which I was much more disconcerted; for though they continued diminishing with tolerable regularity, keeping only some degrees behind it, up to a certain period of heat, about that in which cast iron melts; yet many of the pieces, urged with a heat known to be greater than that, were found not to be diminished so much as those which had suffered only that lower heat. Further experiments shewed, that, after diminishing to a certain point, they begin, upon an increase of the heat beyond that point, to swell again: and as this effect is constant in certain clays, and begins earliest in those which are most vitrescible, and as clays are found to swell upon the approach of vitrification, I look upon this second enlargement of bulk, however inconsiderable, as a sure indication of the clay or composition having gone beyond the true porcelain state, and of a disposition taking place towards vitrification; which stage is always, so far as my experience reaches, attended with a new extrication of air; and in some instances, this air being unable to make its escape from the tenacious mass that envelopes it, the burnt clay is thereby so much increased in bulk as to swim on water like very light wood. The degree of heat therefore, at which this enlargement

ment begins, may be considered as a criterion of the degree of vitrescibility of the composition; which points out a new use of this thermometer, enabling us to ascertain the *degree of vitrescibility* of bodies that cannot actually be vitrified by any fires which our furnaces are capable of producing.

All my researches among the natural clays proving fruitless, and the experiments having shewn that all those, which could sufficiently resist vitrification, diminished *too little* in the fire, I endeavoured to find a body possessed of the opposite property, that is, diminishing *too much*, and, by a mixture of these two, to produce the *medium* diminution required. As I could not find any natural substance possessed of that property, which would not at the same time render the compound too vitrescible, I was obliged to have recourse to some artificial preparation; and as the earth of alum is the pure argillaceous earth, to which all clays owe their property of diminution in the fire, possessing that property in a greater or less degree according to the quantity of alum earth in their composition, I mixed some of this earth with the clay, and found it to answer my wishes completely, both in procuring the necessary degree of diminution, and increasing its unvitrescibility. So little is this compound disposed to vitrification, that the greatest heat I could give it, that of  $160^{\circ}$ , did not even bring it to a porcelain texture, but left it still bibulous; and as it does not arrive at the *porcelain* state in this fire, there can be no danger of its approaching too near to the *vitrescent* in any heat that we can produce in a furnace.

In order to obtain the exact medium required, I took one of the best of the clays I had procured from Cornwall, and mixed it with different proportions of the alum earth, till the composition was found, on repeated trials, to agree with the original

in all degrees of heat. This coincidence was not indeed essential; but as many degrees of heat were already before the public, measured by thermometer-pieces made of the first clay, and as the correspondence of the first with FAHRENHEIT'S scale had likewise been in some measure ascertained, it was desirable that the same degrees of heat should continue to be expressed by the same numbers.

The alum earth is prepared for this purpose by dissolving the alum in water, precipitating with a solution of fixed alkali, and washing the earth repeatedly with large quantities of boiling water: when the earth has settled, the water above it is let off by cocks in the sides of the hogheads; and when the vessels are filled up with fresh water, care is taken to stir up the earth from the bottom, and mix it thoroughly with the liquor. I find it most convenient to use the earth undried, in its gelatinous state, as in this state it unites easily and perfectly with the clay; whereas, when the alum earth has concremented into dry masses, great labour is necessary to mix them uniformly together.

I have tried several different parcels of English alum, from the same and from different manufactories, and found no material difference in the quantity of earth it contains\*. Nor indeed would it be of any consequence if there was a difference in this respect, as the proportion of alum earth necessary for

\* A difference in the quantity of earth *may* arise from different proportions of GLAUBER'S salt and vitriolated tartar, of which I have found quantities very considerable, but nearly alike, in all the English alum I have examined. These salts are doubtless formed by the kelp ashes employed in the preparation of the alum. They are discovered by calcining the dried alum with charcoal powder, which decomposes the alum only, leaving the other two salts intermixed with the alum earth, from whence they may be extracted by water.

different clays, and even for different parcels of the same clay, can only be ascertained by repeated trials, adding successive quantities of the earth till the desired effect is found to be produced. Ten hundred weight of the Cornwall porcelain clay which I have now in use required all the earth that was afforded by five hundred weight of alum.

It is material in this place to observe, that the earth of alum is extremely tenacious of water, inasmuch that, though apparently dry, the water and air amount to near as much as the pure earth, and are not to be completely driven out without a full red heat. When divided by the admixture of other earthy bodies, it parts with its water easier indeed than before; but a mixture containing so much of it as the thermometric composition does, is far more retentive of water than common clay, and requires to be kept for some time in a heat equal to that of boiling water, before it is to be considered as dry, that is, before the adjustment of the pieces in the gage. If they are adjusted when only apparently dry, or of such a degree of dryness as they can be brought to by a heat that the hand can bear, the heat of boiling water will diminish them two or three degrees; and the greatest part of what they have thus been deprived of, they gradually recover again on being exposed to the atmosphere, so that the adjustment must be made immediately after the boiling heat.

By the same expedient to which I have thus been obliged to have recourse for procuring to the porcelain clay of Cornwall the standard degree of diminution, and resistance to fire, the same qualities may probably be communicated to any other clay that is tolerably pure from calcareous earth and iron; so that the thermometer clay is no longer to be considered as the produce of any particular spot (which was the principal incon-

venience originally imagined to attend it), but may be procured and prepared in all parts of the world where good common clay, and alum, are to be found; and *corresponding* thermometers may, consequently, be constructed, without any standard to copy from. For, if a converging canal be formed, of any convenient length, with the widths at the two ends in the proportion of 5 to 3, with the sides perfectly straight, and divided into 240 equal parts, numbering the divisions from the wider end\*;—and if a clay be obtained of such quality, that when formed, in the manner already mentioned, into pieces of such size as to enter to 0 in the gage or canal, these pieces shall just begin to diminish, or go a little further in the canal, by a heat visibly red;—go to 27, by the heat in which copper melts;—about 90 by the welding heat of iron; about 160, by the greatest heat that can be produced with coaked pit-coal in a well constructed common air-furnace, about eight inches square, still continuing bibulous, so as to stick to the tongue: such gages, and pieces of such clay, so adjusted, will always compose correspondent thermometers.

Having mentioned occasionally several alternate periods of dilatation and contraction in clay, it may be proper to state, and bring into one view, the whole succession of changes which I have observed in this curious material; as otherwise they might create some confusion in the minds of those who have not had occasion to think attentively on this subject, and lead them to ask how a body so variable, and liable to such opposite changes from different degrees of heat, can yet be a just measure of those degrees.

\* Or the divisions on the side may be continued to 300; and in that case, instead of the widths of the two ends being in proportion of the odd numbers 5 and 3, the one will be just double to the other.

The changes which take place in all the natural clays that have come under my examination are six.

1. The first is, the *shrinking* of the moist clay in drying, from the mere loss of its water. The purer the clay is, the more water it requires to soften it, and the more it diminishes in bulk by the loss of that water.

2. The dry clay, gradually heated, preserves its bulk unvaried up to the approach of ignition. At this period it is *enlarged* a little; probably, as already observed, from its combined air endeavouring to escape.

3. When this air has made its escape, the clay begins to diminish, or to *lose the bulk it had before acquired*; and returns back, sooner or later, to the same dimensions which it was of when dry. It is at this point that the thermometric diminution commences.

4. From this point the clay continues to *diminish* more and more in proportion as the heat is increased. This I call the *thermometric stage* of diminution: it is of greater or less extent, terminating at different periods of heat, according to the nature of the clay: in the standard thermometer clay, it commences with visible ignition, and continues to (doubtless far beyond) the extreme heats of our furnaces, an interval consisting of 160 degrees of the scale: in others, it begins 4, 6, and in some even 15 of those degrees later, and terminates also much sooner: and in some its whole extent is not above 20 of the same degrees. Throughout the greatest part of this stage, the clays are found to retain their property of sticking to the tongue and imbibing water: between this *bibulous* state and the *vitrescent* there is an intermediate one, distinguished by the name of *porcelain*; and to the higher term of this porcelain state the stage of thermometric diminution seems to continue.



5. When the clay has passed the porcelain state, it begins to be *enlarged* again, a symptom of the vitrescent stage being commenced; and in this period it swells more or less, according to the nature of its composition.

6. By further heat the swelled mass, becoming fluid, subsides, is converted into glass or slag, and *contracted into less volume* than the clay occupied in any of its preceding states.

It is plain, therefore, that clay can be a measure of heat no further than from ignition, or that point beyond ignition where the third stage terminates, to the beginning of the vitrescent stage; and that, as the three first changes are completely passed before the clay is applied to thermometric purposes, being strictly no other than preparatory processes, the thermometer-pieces, whatever *clay* they may be made of (provided it is sufficiently unvitrescible), are to be considered as possessing only the fourth stage. But a singular property of the *composition* of clay and alum earth remains to be mentioned, *viz.* that it has really no other than this one stage: it suffers no enlargement of its bulk at ignition, or in any other period; but proceeds in one uninterrupted course of diminution, from the soft state in which the pieces are formed, up to the extreme fires of our furnaces. Though the diminution, however, is uninterrupted, it is at the same time so inconsiderable at the beginning, from the heat of boiling water (at which the pieces are adjusted) up to ignition, that the same point of visible redness is taken for the commencement of the scale, in this as in the original clay, without any sensible error or variation in their progress.

I am inclined to believe, though experiments have not yet enabled me to speak with certainty on this point, that the same cause which enlarges the *natural clays* on their first exposure to the fire, operates also in this *composition*, but in a much lower degree;

degree; that while the natural *clays* have their whole mass distended by the efforts of the air in forcing its passage, the *composition* is only restrained in its diminution, or prevented from diminishing so fast as it otherwise would do, and as it is found to do in the subsequent part of its course, after the air has escaped from it.

As the composition of clay and alum earth is far more tenacious of water than the clay itself, and was found, after being dried by the heat of boiling water, to yield, by distillation in a retort, above three times as much aqueous fluid as the original thermometric clay did; it seems probable, that a part of this water, retained to the approach of ignition, and in a state of chemical combination, may facilitate the passage of the air, serving as a vehicle to convey it off through interstices not permeable to air alone, and consequently enabling it to escape without doing that violence to the mass, which the natural clays sustain from the expulsion of their air after the water has been detached from it; for the experiments of Dr. PRIESTLEY have shewn, that vessels even of burnt clay are permeable to air when they have imbibed water into their substance, though not at all so in a dry state.

I have now communicated the result of a series of experiments which have taken considerable time, attention, and labour to complete. Whether the importance of the object will justify me in troubling this illustrious Society with so minute a detail of the most material operations, and their results, is not for me to determine. If the thermometer should not yet be brought to the perfection that may be wished, I flatter myself that some abler hand may now take up the subject to more advantage; and that philosophers and artists will

not be less successful in supplying what may still be deficient, and in ascertaining, by the *contraction of argillaceous matter*, the measurements and effects of the various degrees through the immense extent of luminous fire, than they have been with respect to the limited and narrow compass of low heat, which is measurable by the *expansion of fluids*.



Fig. 4.

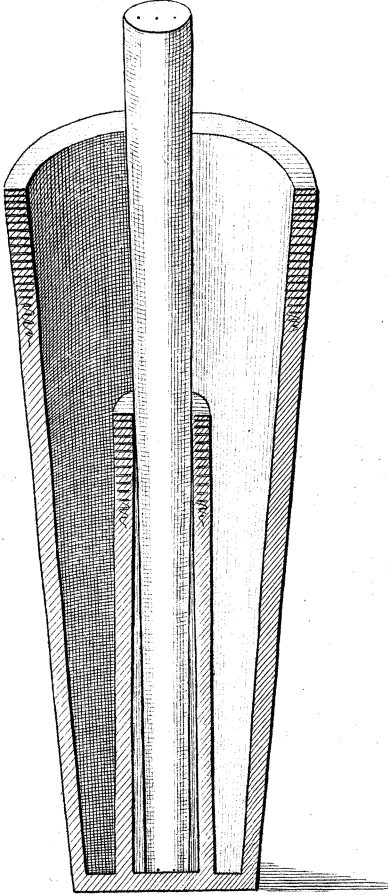


Fig. 1.

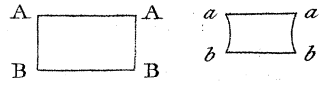


Fig. 2.

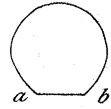


Fig. 3.

