



# V. A brief examination of the received doctrines respecting heat or caloric. Read before the Askesian Society

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A few hours after eating thereof, you will frequently see some of them running about as if drunk, or paralytic, but eventually they generally all retire to their haunts and die. As rats are extremely sagacious, it may be proper, where they have only eaten a small portion, to allow the mixture to remain for forty-eight hours. It will be best to burn what is left after that time, as a fresh mixture may be prepared at a trifling expense, when wanted.

During the time in which the mixture of barytes is exposed to the rats, it is necessary to keep shut the doors of the places where it is laid, to prevent the vermin from being disturbed, or a possibility of accident to any other animal or person; for though it is not so extremely dangerous, if taken internally, as the preparations commonly employed for killing rats, and is even in some cases used in medicine, yet it is fatal if taken improperly.

The oil of aniseeds renders the mixture disagreeable to dogs and many other animals, but is, in small quantities, alluring to rats.

The carbonat of barytes may be procured in large quantities at the lead mines belonging to Sir Frank Standish, Bart. at Anglezark, near Chorley: the proper sort is tasteless, semi-transparent, and effervesces with acids: it is moderately hard and striated. It is frequently called aerated barytes (*terra ponderosa aerata*), and sometimes, by the miners, ponderous spar.

V. *A brief Examination of the received Doctrines respecting Heat or Caloric.* By ALEXANDER TILLOCH. Read before the Askeian Society, December 1799.

[Continued from Page 78.]

A QUANTITY of heat added to that already in water of a known temperature, which shall *sensibly increase* the volume of the compound, heat and water, only *one thousandth* part, is *sensible* heat; caloric not in combination, not in chemical union—it is cognisable by our senses! But if enough be added *sensibly to increase the volume to one thousand times its*

*its original bulk*, then the added heat is latent; it is in chemical union; it is combined caloric! Because now not cognisable by our senses? No. We see the volume of the steam much larger than that of the water; and the more heat we add, the greater is the increase of volume while water is left to receive it. Yet I am not to believe that the heat is cognisable by any organ of sense or external sign! In other words, I am commanded to believe as true, a statement which the evidence of my own senses makes it impossible I can give credit to.

But, say the advocates for this doctrine, a substance so charged with caloric as to become vapour, will not indicate, by the thermometer, any further increase of temperature, though we continue to pour heat into the liquid that produces the vapour; therefore the extra heat must have become *latent* in the steam that has been generated. Nay, we can prove the fact; for this same latent heat may again be made *sensible* in the common process of condensation.

This mode of reasoning appears to me to be more specious than just. It is demanding that the common thermometer should do in this case what it never does in any. This instrument never tells the quantity of heat passing into any body, even in those cases where heat is counted *sensible* or *free*; it only tells the comparative quantity passing into itself from the body in which it is immersed, or with which it is in contact, to bring it into equilibrium with that body as to heat.

Boiling water, steam, the materials of which the thermometer is made, become each charged with heat *in proportion to their capacities*, and this whether the thermometer be in the water or in the steam. The thermometer kept in the steam will never rise higher than  $212^{\circ}$ , "because there the heat is *latent*!" Keep it in boiling water for a year, and it will not rise higher than  $212^{\circ}$ ; yet there the heat is *sensible*! Is this distinction reconcilable with common sense? But the result may even be altered at pleasure. It will not rise higher than  $212^{\circ}$  in boiling water *under the common atmosphere*:—in other words, if we wish to raise the heat higher, we must put a greater pressure upon the water. Confine the water so  
that

that none of it may escape, and the heat will rise in it far above  $212^{\circ}$ . Confine the steam, and in it the heat will rise just as high as in the confined water; yet in the one the heat exists in a different state from what it does in the other!!—The result, we see, may be altered by mechanical contrivances: nay, strictly speaking, What is the effect produced by the atmosphere but mere mechanical pressure? Yet we are to believe that a change has been effected in the *physical properties* of one of the substances subjected to its *mechanical operation*!—If Nature had so constituted the atmosphere as to have only half its present gravity, the point at which heat would become latent, as it is called, in steam would have been far below  $212^{\circ}$ . When water is made to boil in an exhausted receiver at a lower temperature, have we done any thing but removed weight from its surface, and *vice versa*?—Does the heat in the steam in these cases pass into a latent state also? If it does, the effect is mechanical: if it does not, then the mere accident of the atmosphere being of its present weight, has nothing to do with the boiling point happening to fall at  $212^{\circ}$ ! But he that would say so would be counted mad.

At Munich, and other places equally elevated above the level of the sea, that is, having a less weight of atmosphere upon them, water, in open vessels, boils at  $209^{\circ}$ . In a partially exhausted receiver the same effect takes place: and yet the doctrine of latent heat is never considered as inconsistent with the fact; really for no other reason but because a common thermometer cannot measure *specific* heat.

To demand that a thermometer should measure the quantity of heat poured into water to convert it into vapour, and to maintain it in that form; and to insist, because the instrument will not do this, that the heat must have changed its nature, and lost its original character; is about as wise as it would be to demand, that a pint measure dipped into the ocean should determine the quantity of water in the latter, and to insist that, otherwise, the water on the outside of the vessel must have lost its original character, and be different from that within: it is demanding that mercury, which, by its constitution, can at  $212^{\circ}$  only expand a certain quantity, compared with its own bulk in some given lower tempera-

ture, should be able (without our making any comparative experiments to determine the point) to inform us how many times water will be increased its own bulk when we pour a greater quantity of heat into it!

The heat in the steam is as much sensible or free heat as it was before it passed into the steam, if these terms are to be applied to heat cognisable by our senses, and that may be measured comparatively. But *the steam is really its own thermometer*; and it indicates as truly the quantity of heat that has passed into a given quantity of water, as the thermometer does the quantity that passes into itself; aye, and by the same means too—the magnitude of its own volume.

Instead of supposing, in the case of steam, that heat has become latent, or been *changed*, would it not be more correct to ascribe the phenomenon that led to this idea to another cause, *a change in the form of the water*, which, by its constitution, is forced to become vapour, under the *common pressure* of the atmosphere, *whenever a certain number of times its own bulk of heat is poured into it*. The quantity, after proper comparative experiments, could then be expressed in sensible terms, and would turn out to be *the whole bulk of the steam, minus the original volume of the water in the compound*.

If some such method were followed, it appears to me extremely probable that we should soon arrive at many truths respecting the operations of the universally diffused substance *heat*, which otherwise must escape us, though the facts that might lead to them are daily presenting themselves in almost every chemical process. It would surely tend much to the advancement of science, if *the bulk, mass, or volume* of heat necessary to convert different solids into liquids, and liquids into gases, *under a given pressure*, were accurately determined by experiment. The thermometer would then be a more useful instrument than it now is—But we should never look to it to perform impossibilities; we should no more expect it to measure the quantity of heat passing into or out of bodies, than we should attempt to measure the quantity of water delivered from a pump, by placing an hygrometer or any twisted fibrous substance in the stream, and then examining

mining how much its length is diminished or its diameter increased.

Some may say, that the case of the conversion of water into steam is not held by them as one of those that prove the passing of heat from a sensible to a latent state, and that therefore our reasoning, drawn from that example, will not invalidate the doctrine; "for in the case of water they consider the heat as in simple mixture, and it would be an abuse of words to call so weak an union by the name of combination."

I reply, that this case was one of the earliest brought in support of the doctrine, and also thought to be one of the strongest; and if the good understanding of any has led them to give it up, it is the more surprising they should be so blind as to continue upholding a fabric which was built on this as one of its foundation stones; and which does not appear to me to be upheld by any one fact that may not be as satisfactorily explained, without admitting the existence of heat in two distinct states.

But, say these, two different fluids of the same temperature when united will often give out heat—What can we say of this heat, but that it was *latent* or *combined* in one or both of the fluids, and that it is thrown out as sensible or free heat by their union? I would say no such thing without a previous examination of all the accompanying phenomena; and one of these I find to be *a reduction in the volume of the compound*, which is less than that of the sum of the two. The *moleculæ* of the two substances occupy less room united than in their respective fluids. For instance, when sulphuric acid and water are joined, the volume of the mixture is less than that of the two before mixture; there is, therefore, less lodging-room left for the heat: in other words, the capacity of the compound for heat is less than the sum of the capacities of the ingredients: therefore, compared with surrounding bodies, it has now too large a quantity, and, by the law of equilibrium, must give off the surplus to the surrounding bodies in proportion to their capacities, reserving of such surplus only that portion due to itself, and necessary to give it such an increased temperature as the surrounding bodies

will each have acquired, by the diffusion of the dislodged heat, when it has come again to a state of equilibrium; a quantity which must in general be so small as to elude all measurement in the petty processes of the laboratory.

Are there any cases in which heat is dislodged by the union of two liquids, and where, at the same time, the volume of the mixture is not reduced below that of the sum of the volumes of the ingredients? I do not recollect any. There may, however, be some, and it will be time enough to attempt to explain them when they are adduced. In the mean time, when a diminution of volume follows, or rather accompanies, the extrication of heat from any body, instead of running to the doctrine of latent heat being then made sensible (that is, heat being changed in its character), we ought to content ourselves with stating an obvious fact, namely, that the molecularæ of the two liquids are so constructed and formed as to admit of their coming closer together when mixed than they could when respectively alone; and, of course, now fill reciprocally spaces that, before their mixture, were filled with heat: the latter substance, in consequence of being thus dislodged (for two substances cannot, at one and the same time, occupy the same space), diffuses itself among the surrounding bodies in proportion to their capacities, constituting in them, what it did in those it has quitted, *bulk or volume*.

What takes place in such cases may be illustrated by one of a different kind. If a pint of small shot and a pint of dry sand be mixed, they will occupy a less volume than two pints; air is thrown out that was before lodged in the interstices of both of them. Was it *latent air* then, and is it *sensible air* now? Did it differ in its properties before and after being ejected? Weighing the ingredients before and after mixture, will not tell how much air has been ejected; but we know, notwithstanding, that its volume may be measured; and so may that of the heat driven out in the case which this was brought to illustrate.

How are fluids in general measured but by their bulk? or their weight, when circumstances will allow it? Water, for instance, in common cases, is directly measured by any vessel

vessel whose capacity is known or can be come at; in others, as in a wet piece of wood, a brick, or other substance, the quantity must be found out by other means: but in every case where water is added to another substance, which is made thereby to expand exactly in the direct ratio of the quantity of water added (if there be any such), the quantity may be determined by measuring the compound, and deducting therefrom the original volume of the other substance. *Would it be absurd to talk of measuring the matter, fluid, or substance, called heat, in a similar manner?*

When a thermometer is applied to any substance of a higher temperature than itself, it is, by the operation of the general law, soon brought into equilibrium with that substance as to heat; and we say, "it has risen so many degrees." We are habituated to this mode of speaking, and satisfy ourselves, without any more inquiry, that the phenomenon requires no further investigation; and as to the accompanying phenomena, we generally overlook them altogether. When we find that the mercury has increased in volume, would it be absurd to ask this simple question? Is the increase to be attributed not merely to the addition of heat, but to the addition of a quantity *equal in bulk to the increase of volume acquired by the mercury?* I think it extremely probable that the amount of increase or diminution of the volume of any substance, when heat is added or abstracted, is the real bulk of the heat so added or abstracted. That I may be clearly understood, I shall illustrate my meaning by a comparison:

If to a cubic inch of a compact piece of gum and water new unknown quantities of water be added, who would ever think of wire-drawing the mixture through a tube, and expressing the result in degrees of no known quantity, nor referable to any determinate measure? The cases to me appear perfectly parallel.

To a mixture of gum and water we add water, and the volume of the mixture is increased; and to a mixture of mercury and heat we add heat, and the volume of that mixture is increased.

When

When the volume of a substance is increased by heat; can any thing be conceived more easily practicable, in many cases, than to determine, by actual measurement, the proportion that the increase bears to the volume of the mass at a given lower temperature? Is not this already done in many cases? The experiments on this point should be multiplied, so as to embrace, if possible, every known substance and every degree of heat. Most substances would then become their own thermometers: nay, all are so at present, but we have not examined the relations of all their different scales.

[To be continued.]

VI. *Experiments on the Solar and on the Terrestrial Rays that occasion Heat; with a comparative View of the Laws to which Light and Heat, or rather the Rays which occasion them, are subject, in order to determine whether they are the same, or different.* By WILLIAM HERSCHEL, LL.D. F.R.S.

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*5th Experiment. Reflection of the Heat of a Coal Fire by a plain Mirror.*

I PLACED a small speculum, such as I use with my 7-feet reflectors, upon a stand, and so as to make an angle of 45 degrees with the front of it\*. This was afterwards to face the fire in my parlour chimney, and would make the same angle with the bars of the grate. At a distance of  $3\frac{1}{2}$  inches from the speculum, on the reflecting side of it, was placed the thermometer No. 1; and close by it, but out of the reach of the reflected rays, the thermometer No. 4. The whole was guarded in front, against the influence of the fire, by an oaken board  $1\frac{1}{2}$  inch thick, which had a circular opening of  $1\frac{1}{4}$  inch diameter, opposite the situation of the plain mirror, in order to permit the fire to shine upon it. The thermometers were divided from the mirror by a wooden partition, which also had an opening in it, that the reflected

\* See Plate II. fig. 2.