

XXXII. *An Account of an Experiment on Heat.* By George Fordyce, M. D. F. R. S. *In a Letter to Sir Joseph Banks, Bart. P. R. S.*

Read May 24, 1787.

S I R,

H EAT changes the qualities and appearances of matter in various ways. It is also a powerful agent in many of the operations which mankind employ to fit matter for their use. Although the ancients performed many of these operations with a considerable degree of accuracy, yet there are many which they were totally unacquainted with, and others they brought to little perfection. One principal cause was their having no means of measuring heat accurately. VAN HELMONT was the first who found the mode of measuring heat by expansion. His measure was an air thermometer, which is described in his Dissertation, named "*Aer*", cap. 12. Since his time, various improvements have been made on thermometers; many are still wanted. This instrument is, however, the foundation of modern discoveries on this subject. The ancients were acquainted with the manner of heating bodies by communication, by friction, by burning fuel, by the sun, by fermentation, and the taking place of chemical combinations in other cases. BOYLE found, that melting a solid body produced cold (*Experimental History of Cold*, title I. chap. 18.):

Dr. CULLEN, that cold was also produced by converting bodies into vapour. It has been since that time found, that the opposite condensations, *viz.* of vapours into fluids, or fluids into solids, generate heat. CRAMER was the first who took notice of the different conducting powers of different bodies, in his "*Ars Docimastica*," P. I. § 274. Scholium.

The power of animal bodies, of resisting the cold of the medium they are in, has been long known. Mr. ELLIS took notice of their being also able to resist the heat. Dr. CULLEN ascribed this power to a peculiar quality in animals different from the powers of inanimate matter. You, Sir, saw a confirmation of this power being very great when we kept a dog of no large size (he might weigh, as far as I can recollect, about twenty-five pounds, not more) in air heated to 160 degrees of FAHRENHEIT's thermometer for half an hour. We took him out with only the addition of a few degrees of heat; not from any uneasiness of the animal, but from being satisfied with the experiment. This power has been shewn by Mr. HUNTER to extend to vegetables. The degree of heat one body is capable of impregnating another with, was hardly touched upon by any author before Dr. CRAWFORD, who has done a great deal in this branch, and is still pursuing it.

The subject of the present enquiry is different from all these. The proposition is, supposing we can make an application to a cold body, so as to produce heat in it, and this application be made with the same force to the same body, whether by this means an equal quantity of heat will always be produced in an equal quantity of matter? That is, for instance, whether an equal quantity of the rays of the sun being thrown on an equal surface of the same matter, so that they shall be equally lost, bent, or reflected, an equal mass of matter below shall be

equally heated according to its capacity ; whether equal vibrations excited shall always produce the same quantity of heat ; whether a chemical attraction taking place between an equal quantity of two substances shall always produce an equal quantity of heat ?

The importance of this enquiry is sufficiently evident, since if the same quantity of fuel being burnt the same quantity of heat be always produced, our whole attention will be to take care that no part of the heat shall be lost ; but if burning the fuel under one set of circumstances will actually produce a greater quantity of heat than burning it in other circumstances ; or if burning it, will produce a great heat in one place, which cannot be carried to another place, but will be again annihilated, a very different attention must be paid. I was first led into this train of thinking by observing reverberatory furnaces. Formerly I had no doubt but that it was obvious, that the same quantity of fuel burnt would produce the same quantity of heat ; but having occasion to try some experiments in reverberatory furnaces, where great heat and cleanness were required, I tried to heat the furnace with charcoal and coak, or pit-coal charred, that is, burnt till no smoke arises, but could never produce the heat required, although I could do it easily with coal. I insulated my furnace, so that after twenty-four hours strongest fire, it did not feel in the least warm on the outside. I heightened the chimney ; but all to no effect : in the fire-place the heat was sufficient to melt malleable iron, but in the laboratory, in the horizontal part of the chimney, the heat was trifling. Since that time I have made various experiments to ascertain the proposition laid down. The following one, which has been varied and repeated with the same result, may, perhaps, draw the attention of chemists to this point.

I formed a cylinder of thin pasteboard, six inches diameter and sixteen inches long. The inside I lined with rabbit skin, laying the fur smooth; a thin ring of pasteboard was placed in the middle. One end was closed with a bottom of the same pasteboard; the other was open. This cylinder was placed in the center of another wider cylinder, also of pasteboard, which had likewise a bottom of pasteboard. It was so placed, as that the outer cylinder was distant from the inner one inch and a half; at the bottom and sides the space between was filled with Eider down, suffered to rise to as great a bulk as it would from its own elasticity. The two cylinders were even at top, and the space between them shut by a cover of pasteboard. In the side of the machine, a little below the middle of the inner cylinder, a pasteboard tube was made to pass through the outer and open into the inner, half an inch wide, for the insertion of a thermometer. A similar tube was placed a little from the middle, towards the other end of the smallest cylinder. A circular plate, of pasteboard, six inches diameter, and about one-eighth thick, weighing 1 oz. 102 grs. was pushed down the inner cylinder, until it was stopped by the ring. A circle of flint glass, ground flat and parallel on both sides, was fixed over the mouth of the inner cylinder so as not to obstruct any part of it.

A similar apparatus, as exactly as possible, was formed, excepting that the circular plate in the middle of the inner cylinder was iron, of the same dimensions with the pasteboard one, and weighing 12 oz. 62 grs. These apparatus's were set in a warm exposure for several months, to dry.

The circular plates, which were destined to receive the direct rays of the sun, were placed as nearly perpendicular to the inner cylinder as possible. They were both covered with a

black paint, sufficient to prevent the rays of the sun from penetrating either to the iron or pasteboard.

On the 28th day of July, 1786, the sun shining on a room facing about S.W. the air not cloudy, but not very bright; the air in the room 71° ; at a quarter after twelve, thermometers being passed through the tubes below the plates of iron and pasteboard, after standing a quarter of an hour, shewed the heat 67° in both apparatus's. Both were now exposed to the sun, so that the rays fell perpendicular on the paint covering the plates, in equal quantity on each as nearly as possible. If there was any difference, rather more were thrown on the pasteboard diaphragm. In five minutes the thermometer below the pasteboard diaphragm shewed 72 degrees; the thermometer under the iron had hardly risen half a degree.

Progress of the rising of the thermometers.

Under pasteboard diaphragm.

72°
75
80
85
90
95
100
105
110

After 20 minutes,

110

Under iron diaphragm.

67½
70
76 +
83
88 +
94
100 *
107
115

121

* At this time thermometers were put through tubes into the chambers of the apparatus, between the glasses and diaphragms. The apparatus with the iron diaphragm raised this thermometer to 121° ; that with the pasteboard to 120° .

The

The apparatus with the pasteboard diaphragm was exposed still to the sun; that with the iron was removed, and suffered to cool till its thermometer shewed 107° ; it was then exposed again to the sun till it had acquired the heat of 110° , to which degree the apparatus with the pasteboard hardly reached. The windows were now shut. The heat of the room had arisen to 80° . Both the apparatus's were placed on a table; the doors were shut, so that there was no current of air.

Pasteboard apparatus, after 30 minutes.	Iron apparatus.
96	104
After 75 minutes, or 1 h. 45' from the beginning.	
83	89
After 2 h. or 3 h. 45' from removal from the sun.	
78	room 75
	80.

A similar result arose when there were no glasses to exclude the external air. Likewise when the diaphragms were changed from one apparatus to the other.

If any one wishes to repeat these experiments, he must take care that the size of both apparatus's be the same; the weight the same, excepting the difference of the iron and pasteboard; that they be equally stuffed, and perfectly dry: for if there be the least moisture, the evaporation will not only make a fallacy in the experiment, but it will soon obscure the glasses, so as to prevent the rays of the sun from passing through them.

The first thing to be noted in this experiment is, that the rays of the sun acted on the same black paint only: for it was so thick, that the rays could not penetrate to the iron or pasteboard below. The colour was the same, and there was the same gloss; if any thing, that on the iron, in the experiment related, was rather more glossy, in order that it might not be favoured,

favoured, as in former experiments the results had been in favour of the iron apparatus acquiring the greatest heat. Every thing, therefore, was the same, except that the iron and pasteboard were of different weights, of different capacities of heat, and of different degrees of readiness to acquire heat, and communicate it.

It is evident, that a greater quantity of heat was actually produced in the apparatus with the iron diaphragm: for although in the first two or three minutes the pasteboard became hotter than the iron, yet as soon as the iron began to be sensibly heated, it became hot faster than the pasteboard, and actually became hotter, and even continued to do so, when the pasteboard no longer could produce more heat than was dissipated from the surface of the apparatus into the air. When they were set in an air equally cold the apparatus with the iron diaphragm was longer in cooling, although they were both of the same degree of heat when set by.

This greater quantity of heat I ascribe to the iron's taking the heat from the black paint faster than the pasteboard, as being a better conductor. Just as if a plate of glass was placed on a plate of steel, and another, perfectly similar, was placed on a plate of clay, and both were placed equally among equal vibrating bodies. In this case it is clear, that much greater vibration would take place if the same means of exciting it were applied to that plate of glass attached to the plate of steel than if they were applied to that attached to the clay. I do not mean to say, that heat is vibration; but merely to illustrate my idea of heat's being only a quality, and not a substance. I am led to this not only by this experiment now related, but by various other considerations, which I shall not now insist upon, as they are not sufficiently finished to be laid before this Society.

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I shall only add that, among other things which may be illustrated by it, one is, that all the planets may possibly be of the same heat; since, if the matter of which Mercury consists was averse to the generation of heat in proportion to the greater number of the rays of the sun it receives more than the Georgium Sidus, they would be both of the same heat, notwithstanding their different distances from the sun.

I have already said, that I was led to an enquiry into the subject by the effect it has on chemical operations.

I remain, &c.

G. FORDYCE.

