# 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.

SIR,

HEAT 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

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 ſmoak 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. | Under iron diaphragm. |
|---|---|
| 72° | 67½ |
| 75 | 70 |
| 80 | 76 + |
| 85 | 83 |
| 90 | 88 + |
| 95 | 94 |
| 100 | 100 * |
| 105 | 107 |
| 110 | 115 |
| After 20 minutes, | |
| 110 | 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 refults had been in favour of the iron apparatus acquiring the greateft heat. Every thing, therefore, was the fame, except that the iron and pafteboard were of different weights, of different capacities of heat, and of different degrees of readinefs 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 firft two or three minutes the pafteboard became hotter than the iron, yet as foon as the iron began to be fenfibly heated, it became hot fafter than the pafteboard, and actually became hotter, and even continued to do fo, when the pafteboard no longer could produce more heat than was diffipated from the furface of the apparatus into the air. When they were fet in an air equally cold the apparatus with the iron diaphragm was longer in cooling, although they were both of the fame degree of heat when fet by.

This greater quantity of heat I afcribe to the iron's taking the heat from the black paint fafter than the pafteboard, as being a better conductor. Juft as if a plate of glafs was placed on a plate of fteel, and another, perfectly fimilar, was placed on a plate of clay, and both were placed equally among equal vibrating bodies. In this cafe it is clear, that much greater vibration would take place if the fame means of exciting it were applied to that plate of glafs attached to the plate of fteel than if they were applied to that attached to the clay. I do not mean to fay, that heat is vibration; but merely to illuftrate my idea of heat's being only a quality, and not a fubftance. I am led to this not only by this experiment now related, but by various other confiderations, which I fhall not now infift upon, as they are not fufficiently finifhed to be laid before this Society.

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.