

XLV. Experiments in an heated Room. By Matthew Dobson, M. D. In a Letter to John Fothergill, M. D. F. R. S.

DEAR SIR,

Liverpool, April 25, 1775.

Redde, June 22, 1775. **I** PERUSED with particular pleasure, your short account of the curious experiment made by Mr. BANKS and Dr. SOLANDER. The same, and some additional experiments, have been made here; the result of which I should sooner have transmitted to you, had I not been prevented by the constant engagements of my profession.

EXPERIMENTS.

I. The sweating-room of our Public Hospital at LIVERPOOL, which is nearly a cube of nine feet, lighted from the top, was heated till the quicksilver stood at 224° on FAHRENHEIT'S scale, nor would the tube of the thermometer indeed admit the heat to be raised higher. The thermometer was suspended by a string fixed to the wooden frame of the sky-light, and hung down about the centre of the room. Myself and several others were at this time inclosed in the stove, without experiencing any oppressive or painful sensation of heat, proportioned to the degree pointed out by the thermometer. Every metallic about us soon became very hot.

VOL. LXV.

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II. My friend Mr. PARK, an ingenious surgeon of this place, went into the stove heated to 202° . After ten minutes, I found the pulse quickened to 120° . And to determine the increase of the animal heat, another thermometer was handed to him, in which the quicksilver already stood at 98° ; but it rose only to $99\frac{1}{2}$, whether the bulb of the thermometer was inclosed in the palms of the hands, or received into the mouth^(a). The natural state of this gentleman's pulse is about 65.

III. Another gentleman went through the same experiment in the same circumstances, and with the same effects.

IV. One of the porters to the Hospital, a healthy young man, and the pulse 75, was inclosed in the stove when the quicksilver stood at 210° ; and he remained there, with little inconvenience, for twenty minutes. The pulse, now 164, and the animal heat, determined by another thermometer as in the former experiments, was $101\frac{1}{2}$.

V. A young gentleman of a delicate and irritable habit, whose natural pulse is about 80, remained in the stove ten minutes when heated to 224° . The pulse rose to 145, and the animal heat to 102° . This gentleman, who had been frequently in the stove during the course of the day, found himself feeble, and disposed to break out into sweats for 24 hours after the experiment.

(a) The scale of the thermometer, which was suspended by the string about the middle of the room, was of metal; this was the only one I could then procure, on which the degrees ran so high as to give any scope to the experiment. The scale of the other thermometer, which was employed for ascertaining the variations in the animal heat, was of ivory.

VI. Two small tin vessels, containing each the white of an egg, were put into the stove heated to 224° . One of them was placed on a wooden seat near the wall, and the other suspended by a string about the middle of the stove. After ten minutes, they began to coagulate; but the coagulation was sensibly quicker and firmer in that which was suspended, than in that which was placed on the wooden seat. The progress of the coagulation was as follows: it was first formed on the sides, and gradually extended itself; the whole of the bottom was next coagulated; and last of all the middle part of the top.

VII. Part of the shell of an egg was peeled away, leaving only the film which surrounds the white; and part of the white being drawn out, the film sunk so as to form a little cup. This cup was filled with some of the *albumen ovi*, which was consequently detached as much as possible from every thing but the contact of the air and of the film which formed the cup. The lower part of the egg stood upon some light tow in a common gallipot, and was placed on the wooden seat in the stove. The quicksilver in the thermometer still continued at 224° . After remaining in the stove for an hour, the lower part of the egg which was covered with the shell, was firmly coagulated; but that which was in the little cup was fluid and transparent. At the end of another hour it was still fluid, except on the edges where it was thinnest; and here it was still transparent; a sufficient proof that it was dried, not coagulated.

VIII. A piece of bees wax, placed in the same situation with the *albumen ovi* of the preceding experiment, and exposed to the same degree of heat in the stove, began to melt in five minutes: another piece suspended by a string, and a third piece put into the tin vessel and suspended, began likewise to liquify in five minutes.

OBSERVATIONS.

That heated air should have such a speedy and powerful effect in quickening the pulse, while the animal heat is little altered from its natural standard; that the human body should so easily bear to be surrounded with air heated to 224° ; that the *albumen ovi*, which begins to coagulate in water at 150° , should remain fluid in 224° ; and that the same *albumen ovi*, still placed in air heated to 224° , should coagulate if in contact either with tin or its own shell, are facts as singular as they are difficult of explanation. From the different effects of heated air on the pulse and the heat of the body, do we not discover the fallacy of that theory of animal heat which has been adopted by BOERHAAVE and other celebrated physiologists? They suppose that animal heat is produced by the attrition of the *globules* of the circulating fluids against the sides of the containing vessels; but in several of the preceding experiments, the circulation was amazingly quickened with little increase of the animal heat. But whence is it that the human body can bear without immediate injury, to be surrounded with air heated to 224° ? And whence is it, that the *albumen ovi* does not coagulate in

in this degree of heat? Is it that fire as it passes into some bodies becomes latent, agreeable to a doctrine which has for some time been taught at Edinburgh by Professor BLACK? Or does fire become fixed and *quiescent*, according to a similar system adopted by Dr. FRANKLIN^(b)? Air we know exists either in a fixed or elastic state; and fire may in like manner exist in bodies, either in a latent, fixed, and *quiescent*; or in a sensible, fluid, and active state. Agreeable to this idea, the bees wax receives the fire in an active state, and dissolves; while the human body and the *albumen ovi*, receiving the fire in a latent state, are little altered in their temperature. Let each of these, however, be put in contact with a different body, tin for instance; and though the heat of the air continues the same, yet the fire no longer enters in a latent state, but with all its sensible and active powers; for the *albumen ovi* suspended in a tin vessel soon coagulates; and the human body, covered with the same metal, would quickly experience an intolerable and destructive degree of heat. Or are the above phenomena more satisfactorily explained, by considering different bodies as possessing different conducting powers; some being strong, others weak conductors of fire? All those bodies then which are weak conductors of fire from air, may be placed in air, without receiving the heat of this medium. Hence the *albumen ovi* remains fluid in air heated to 224°. Hence likewise the frog, the lizard, the camelion, &c. retain their natural temperature, and feel cold

(b) Exper. and Observ. p. 346. and 412.

to the touch, though perpetually furrounded with air hotter than their own bodies. Hence also, the human body keeps nearly its own temperature, in a stove heated to 224° : or may even pass without injury into air heated to a much greater degree, according to the observations of DU HAMEL and TILLET, published in the Memoirs of the Academy of Sciences^(c). On the other hand, all those bodies which are powerful conductors of fire from air, are influenced in proportion when furrounded with this medium. The bees wax melted from the mere contact of the air in experiment VIII; and in experiment VI, the *albumen ovi* was coagulated on the intervention of another body, which is a strong conductor of fire from air. But whether this method of reasoning on the natural cause of these effects be just or not, the final cause is obvious, and is to be resolved into the wise and benevolent appointment of the Almighty. Man is happily so framed, as to possess a power of keeping nearly the same tenor of heat, in all the variations of the temperature of the air in summer and in winter, in hot and cold climates; and consequently changes his situation on the surface of the globe, with much less inconvenience or injury, than he could otherwise have done. The same power likewise happily adapts different animals to their respective destinations. The lizard and the camelion remain cool under the Equator, while the whale and porpoise retain a degree of heat above that of the human body, though furrounded with the waters of the coldest

(c) Memoires pour 1761.

Northern seas, and amidst mountains of ice in the neighbourhood of the Pole.

Should you think these experiments and observations on heated air of sufficient importance to be communicated to the Royal Society, they are at your disposal.

I have the pleasure to find, that Dr. PRIESTLEY is prosecuting his very ingenious inquiries on air. In a letter I lately received from him, he informs me, that he has discovered a species of air, which will preserve animal life six times longer than atmospheric air.

I remain, with great esteem, &c.