XI. *An Inquiry concerning the Weight ascribed to Heat.* *By* Benjamin *Count of* Rumford, *F. R. S. M. R. I. A. &c.*

Read May 2, 1799.

The various experiments which have hitherto been made with a view to determine the question so long agitated, relative to the weight which has been supposed to be gained, or to be lost, by bodies upon their being heated, are of a nature so very delicate, and are liable to so many errors, not only on account of the imperfections of the instruments made use of, but also, of those, much more difficult to appreciate, arising from the vertical currents in the atmosphere, caused by the hot or the cold body which is placed in the balance, that it is not at all surprising that opinions have been so much divided, relative to a fact so very difficult to ascertain.

It is a considerable time since I first began to meditate on this subject, and I have made many experiments with a view to its investigation; and in these experiments, I have taken all those precautions to avoid errors, which a knowledge of the various sources of them, and an earnest desire to determine a fact which I conceived to be of importance to be known, could inspire; but, though all my researches tended to convince me more and more, that *a body acquires no additional weight upon being heated*, or rather, that heat has no effect whatever upon the weights of bodies, I have been so sensible of the delicacy of

the inquiry, that I was for a long time afraid to form a decided opinion upon the subject.

Being much struck with the experiments recorded in the Transactions of the Royal Society, Vol. LXXV. made by Dr. FORDYCE, upon the weight said to be acquired by water upon being frozen; and being possessed of an excellent balance, belonging to *His most Serene Highness the* ELECTOR PALATINE DUKE *of* BAVARIA; early in the beginning of the winter of the year 1787,—as soon as the cold was sufficiently intense for my purpose,—I set about to repeat those experiments, in order to convince myself whether the very extraordinary fact related, might be depended on; and, with a view to removing, as far as was in my power, every source of error and deception, I proceeded in the following manner.

Having provided a number of glass bottles, of the form and size of what in England is called a Florence flask,—blown as thin as possible,—and of the same shape and dimensions, I chose out from amongst them two, which, after using every method I could imagine of comparing them together, appeared to be so much alike as hardly to be distinguished.

Into one of these bottles, which I shall call A, I put 4107,86 grains Troy of pure distilled water, which filled it about half full; and into the other, B, I put an equal weight of weak spirit of wine; and, sealing both the bottles hermetically, and washing them, and wiping them perfectly clean and dry on the outside, I suspended them to the arms of the balance, and placed the balance in a large room, which for some weeks had been regularly heated every day by a German stove, and in which the air was kept up to the temperature of 61° of FAHRENHEIT's thermometer, with very little variation. Having suffered the bottles,

with their contents, to remain in this situation till I conceived they must have acquired the temperature of the circumambient air, I wiped them afresh, with a very clean dry cambric handkerchief, and brought them into the most exact equilibrium possible, by attaching a small piece of very fine silver wire to the arm of the balance to which the bottle which was the lightest was suspended.

Having suffered the apparatus to remain in this situation about twelve hours longer, and finding no alteration in the relative weights of the bottles,—they continuing all this time to be in the most perfect equilibrium,—I now removed them into a large uninhabited room, fronting the north, in which the air, which was very quiet, was at the temperature of 29°, F; the air without doors being at the same time at 27°; and, going out of the room, and locking the door after me, I suffered the bottles to remain forty-eight hours, undisturbed, in this cold situation, attached to the arms of the balance as before.

At the expiration of that time, I entered the room,—using the utmost caution not to disturb the balance,—when, to my great surprise, I found that the bottle A very sensibly preponderated.

The water which this bottle contained was completely frozen into one solid body of ice; but the spirit of wine, in the bottle B, showed no signs of freezing.

I now very cautiously restored the equilibrium, by adding small pieces of the very fine wire of which gold lace is made, to the arm of the balance to which the bottle B was suspended, when I found that the bottle A had augmented its weight by $\frac{1}{35904}$ part of its whole weight at the beginning of the experiment; the weight of the bottle with its contents having been 4811,23 grains Troy, (the bottle weighing 703,37 grains, and the water

4107,86 grains,) and it requiring now $\frac{134}{1000}$ parts of a grain, added to the opposite arm of the balance, to counterbalance it.

Having had occasion just at this time to write to my friend, Sir CHARLES BLAGDEN, upon another subject, I added a postscript to my letter, giving him a short account of this experiment, and telling him how "*very contrary to my expectation*" the result of it had turned out; but I soon after found that I had been too hasty in my communication. Sir CHARLES, in his answer to my letter, expressed doubts respecting the fact; but, before his letter had reached me, I had learned from my own experience, how very dangerous it is, in philosophical investigations, to draw conclusions from single experiments.

Having removed the balance, with the two bottles attached to it, from the cold into the warm room, (which still remained at the temperature of 61°), the ice in the bottle A gradually thawed; and, being at length totally reduced to water, and this water having acquired the temperature of the surrounding air, the two bottles, after being wiped perfectly clean and dry, were found to weigh as at the beginning of the experiment, before the water was frozen.

This experiment being repeated, gave nearly the same result, the water appearing, when frozen, to be heavier than in its fluid state; but, some irregularity in the manner in which the water lost the additional weight which it had appeared to acquire upon being frozen, when it was afterwards thawed, as also a sensible difference in the quantities of weight apparently acquired in the different experiments, led me to suspect, that the experiment could not be depended on for deciding the fact in question; I therefore set about to repeat it, with some variations and improvements; but, before I give an account of my

further investigations relative to this subject, it may not be amiss to mention the method I pursued for discovering whether the appearances mentioned in the foregoing experiments might not arise from the imperfections of my balance; and it may likewise be proper to give an account, in this place, of an intermediate experiment which I made, with a view to discover, by a shorter route, and in a manner less excèptionable than that above mentioned, whether bodies actually lose, or acquire, any weight, upon acquiring an additional quantity of latent heat.

My suspicions respecting the accuracy of the balance arose from a knowledge,—which I acquired from the maker of it,—of the manner in which it was constructed.

The three principal points of the balance having been determined, as nearly as possible, by measurement, the axes of motion were firmly fixed in their places, in a right line, and the beam being afterwards finished, and its two arms brought to be in equilibrio, the balance was proved by suspending weights, which before were known to be exactly equal, to the ends of its arms.

If with these weights the balance remained in equilibrio, it was considered as a proof that the beam was just; but, if one arm was found to preponderate, the other was gradually lengthened, by beating it upon an anvil, until the difference of the lengths of the arms was reduced to nothing, or until equal weights, suspended to the two arms, remained in equilibrio; care being taken, before each trial, to bring the two ends of the beam to be in equilibrio, by reducing, with the file, the arm which had been lengthened.

Though, in this method of constructing balances, the most perfect equality in the lengths of the arms may be obtained,

and consequently the greatest possible accuracy, when used at a time when the temperature of the air is the same as when the balance was made, yet, as it may happen, that in order to bring the arms of the balance to be of the same length, one of them may be much more hammered than the other, I suspected it might be possible that the texture of the metal forming the two arms might be rendered so far different, by this operation, as to occasion a difference in their expansions with heat; and that this difference might occasion a sensible error in the balance, when, being charged with a great weight, it should be exposed to a considerable change of temperature.

To determine whether the apparent augmentation of weight, in the experiments above related, arose in any degree from this cause, I had only to repeat the experiment, causing the two bottles A and B to change places upon the arms of the balance; but, as I had already found a sensible difference in the results of different repetitions of the same experiment, made as nearly as possible under the same circumstances, and as it was above all things of importance to ascertain the accuracy of my balance, I preferred making a particular experiment for that purpose.

My first idea was, to suspend to the arms of the balance, by very fine wires, two equal globes of glass, filled with mercury, and, suffering them to remain in my room till they should have acquired the known temperature of the air in it, to have removed them afterward into the cold, and to have seen if they still remained in equilibrio, under such difference of temperature; but, considering the obstinacy with which moisture adheres to the surface of glass, and being afraid that, somehow or other, notwithstanding all my precautions, one of the globes might acquire or retain more of it than the other, and that by

that means its apparent weight might be increased; and having found by a former experiment, of which I have already had the honour of communicating an account to the Royal Society, that the gilt surfaces of metals do not attract moisture; instead of the glass globes filled with mercury, I made use of two equal solid globes of brass, well gilt and burnished, which I suspended to the arms of the balance, by fine gold wires.

These globes, which weighed 4975 grains each, being wiped perfectly clean, and having acquired the temperature (61°) of my room, in which they were exposed more than twenty-four hours, were brought into the most scrupulous equilibrium, and were then removed, attached to the arms of the balance, into a room in which the air was at the temperature of 26°, where they were left all night.

The result of this trial furnished the most satisfactory proof of the accuracy of the balance; for, upon entering the room, I found the equilibrium as perfect as at the beginning of the experiment.

Having thus removed my doubts respecting the accuracy of my balance, I now resumed my investigations relative to the augmentation of weight which fluids have been said to acquire upon being congealed.

In the experiments which I had made, I had, as I then imagined, guarded as much as possible against every source of error and deception. The bottles being of the same size, neither any occasional alteration in the pressure of the atmosphere during the experiment, nor the necessary and unavoidable difference in the densities of the air in the hot and in the cold rooms in which they were weighed, could affect their apparent weights; and their shapes and their quantities of surface being the same, and as they

remained for such a considerable length of time in the heat and cold to which they were exposed, I flattered myself that the quantities of moisture remaining attached to their surfaces, could not be so different as sensibly to effect the results of the experiments.—But, in regard to this last circumstance, I afterwards found reason to conclude that my opinion was erroneous.

Admitting the fact stated by Dr. FORDYCE,—(and which my experiments had hitherto rather tended to corroborate than to contradict,)—I could not conceive any other cause for the augmentation of the apparent weight of water, upon its being frozen, than the loss of so great a proportion of its latent heat as that fluid is known to evolve when it congeals; and I concluded, that if the loss of latent heat added to the weight of one body, it must of necessity produce the same effect on another, and consequently, that the augmentation of the quantity of latent heat must,—in all bodies,—and in all cases,—diminish their apparent weights.

To determine whether this is actually the case or not, I made the following experiment.

Having provided two bottles, as nearly alike as possible, and in all respects similar to those made use of in the experiments abovementioned, into one of them I put 4012,46 grains of water, and into the other an equal weight of mercury; and, sealing them hermetically, and suspending them to the arms of the balance, I suffered them to acquire the temperature of my room, 61°; then, bringing them into a perfect equilibrium with each other, I removed them into a room in which the air was at the temperature of 34°, where they remained twenty-four hours.—But there was not the least appearance of either of them acquiring, or losing, any weight.

Here it is very certain, that the quantity of heat lost by the

water, must have been very considerably greater than that lost by the mercury; the specific quantities of latent heat in water and in mercury, having been determined to be to each other as 1000 to 33; but this difference in the quantities of heat lost, produced no sensible difference on the weights of the fluids in question.

Had any difference of weight really existed, had it been no more than *one millionth* part of the weight of either of the fluids, I should certainly have discovered it;—and, had it amounted to so much as $\frac{1}{7000000}$ part of that weight, I should have been able to have measured it; so sensible, and so very accurate, is the balance which I used in these experiments.

I was now much confirmed in my suspicions, that the apparent augmentation of the weight of the water upon its being frozen, in the experiments before related, arose from some accidental cause; but I was not able to conceive what that cause could possibly be,—unless it were, either a greater quantity of moisture attached to the external surface of the bottle which contained the water, than to the surface of that containing the spirits of wine,—or some vertical current or currents of air, caused by the bottles or one of them not being exactly of the temperature of the surrounding atmosphere.

Though I had foreseen, and, as I thought, guarded sufficiently against, these accidents,—by making use of bottles of the same size and form,—and which were blown of the same kind of glass,—and at the same time,—and by suffering the bottles, in the experiments, to remain for so considerable a length of time exposed to the different degrees of heat and of cold, which alternately they were made to acquire; yet, as I did not know the relative conducting powers of ice and of spirit of wine with respect to

heat; or, in other words,—the degrees of facility or difficulty with which they acquire the temperature of the medium in which they are exposed;—or the time taken up in that operation; and, consequently, was not *absolutely certain* as to the equality of the temperatures of the contents of the bottles at the time when their weights were compared, I determined now to repeat the experiments, with such variations as should put the matter in question out of all doubt.

I was the more anxious to assure myself of the real temperatures of the bottles and of their contents, as any difference in their temperatures might vitiate the experiment, not only by causing unequal currents in the air, but also, by causing, at the same time, a greater or less quantity of moisture to remain attached to the glass.

To remedy these evils, and also to render the experiment more striking and satisfactory in other respects, I proceeded in the following manner.

Having provided three bottles, A, B, and C, as nearly alike as possible, and resembling in all respects those already described; into the first, A, I put 4214,28 grains of water, and a small thermometer, made on purpose for the experiment, and suspended in the bottle in such a manner that its bulb remained in the middle of the mass of water; into the second bottle, B, I put a like weight of spirit of wine, with a like thermometer; and, into the bottle C, I put an equal weight of mercury.

These bottles, being all hermetically sealed, were placed in a large room,—in a corner far removed from the doors and windows, and where the air appeared to be perfectly quiet; and, being suffered to remain in this situation more than twenty-four hours, the heat of the room (61°) being kept up all that time

with as little variation as possible, and the contents of the bottles A and B appearing, by their inclosed thermometers, to be exactly at the same temperature, the bottles were all wiped with a very clean dry cambric handkerchief; and, being afterwards suffered to remain exposed to the free air of the room a couple of hours longer, in order that any inequalities in the quantities of heat,—or of the moisture attached to their surfaces,—which might have been occasioned by the wiping, might be corrected by the operation of the atmosphere by which they were surrounded, they were all weighed, and were brought into the most exact equilibrium with each other, by means of small pieces of very fine silver wire, attached to the necks of those of the bottles which were the lightest.

This being done, the bottles were all removed into a room in which the air was at 30°, where they were suffered to remain, perfectly at rest and undisturbed, forty-eight hours; the bottles A and B being suspended to the arms of the balance, and the bottle C suspended, at an equal height, to the arm of a stand constructed for that purpose, and placed as near the balance as possible, and a very sensible thermometer suspended by the side of it.

At the end of forty-eight hours,—during which time the apparatus was left in this situation,—I entered the room, opening the door very gently, for fear of disturbing the balance; when I had the pleasure to find the three thermometers,—*viz.* that in the bottle A, (which was now inclosed in a solid cake of ice,)—that in the bottle B,—and that suspended in the open air of the room, all standing at the same point, 29° F, and the bottles A and B *remaining in the most perfect equilibrium.*

To assure myself that the play of the balance was free, I

now approached it very gently, and caused it to vibrate; and I had the satisfaction to find,—not only that it moved with the utmost freedom,—but also,—when its vibration ceased,—that it rested precisely at the point from which it had set out.

I now removed the bottle B from the balance, and put the bottle C in its place; and I found that *that* likewise remained of the same apparent weight as at the beginning of the experiment, being in the same perfect equilibrium with the bottle A as at first.

I afterwards removed the whole apparatus into a warm room, and, causing the ice in the bottle A to thaw, and suffering the three bottles to remain till they and their contents had acquired the exact temperature of the surrounding air, I wiped them very clean, and, comparing them together, I found their weights remained unaltered.

This experiment I afterwards repeated several times, and always with precisely the same result; the water, *in no instance*, appearing to gain, or to lose, the least weight, upon being frozen, or upon being thawed; neither were the relative weights of the fluids in either of the other bottles in the least changed, by the various degrees of heat, and of cold, to which they were exposed.

If the bottles were weighed at a time when their contents were not *precisely of the same temperature*, they would frequently appear to have gained, or to have lost, something of their weights;—but this doubtless arose from the vertical currents which they caused in the atmosphere, upon being heated or cooled in it; or to unequal quantities of moisture attached to the surfaces of the bottles;—or to both these causes operating together.

As I knew that the conducting power of mercury, with respect to heat, was considerably greater than either that of water, or that of spirit of wine, while its capacity for receiving heat is much less than that of either of them, I did not think it necessary to inclose a thermometer in the bottle C, which contained the mercury; for it was evident, that when the contents of the other two bottles should appear, by their thermometers, to have arrived at the temperature of the medium in which they were exposed, the contents of the bottle C could not fail to have acquired it also, and even to have arrived at it before them; for, the time taken up in the heating or in the cooling of any body, is, *cæteris paribus*, as the capacity of the body to receive and retain heat, *directly*, and as its conducting power, *inversely*.

The bottles were suspended to the balance by silver wires, about two inches long, with hooks at the ends of them; and, in removing and changing the bottles, I took care not to touch the glass. I likewise avoided, upon all occasions, and particularly in the cold room, coming near the balance with my breath, or touching it, or any part of the apparatus, with my naked hands.

Having determined that water does not acquire or lose any weight, upon being changed from a state of *fluidity* to that of *ice*, and *vice versâ*, I shall now take my final leave of a subject which has long occupied me, and which has cost me much pains and trouble; being fully convinced, (from the results of the above mentioned experiments,) that if heat be in fact a *substance*, or matter,—a fluid *sui generis*, as has been supposed,—which, passing from one body to another, and being accumulated, is the immediate cause of the phænomena we observe in

heated bodies, (of which, however, I cannot help entertaining doubts,) it must be something so infinitely rare, even in its most condensed state, as to baffle all our attempts to discover its gravity. And, if the opinion which has been adopted by many of our ablest philosophers, that heat is nothing more than an intestine vibratory motion of the constituent parts of heated bodies, should be well founded, it is clear that the weights of bodies can in no wise be affected by such motion.

It is, no doubt, upon the supposition that heat is a substance distinct from the heated body, and which is accumulated in it, that all the experiments which have been undertaken, with a view to determine the weight which bodies have been supposed to gain, or to lose, upon being heated or cooled, have been made; and, upon this supposition (but without, however, adopting it entirely, as I do not conceive it to be sufficiently proved,) all my researches have been directed.

The experiments with *water*, and with *ice*, were made in a manner which I take to be perfectly unexceptionable;—in which no foreign cause whatever could affect the results of them;—and the quantity of heat which water is known to part with, upon being frozen, is so considerable, that if this loss has no effect upon its apparent weight, it may be presumed that we shall never be able to contrive an experiment by which we can render the weight of heat sensible.

Water, upon being frozen, has been found to lose a quantity of heat amounting to 140 degrees of FAHRENHEIT'S thermometer; or,—which is the same thing,—the heat which a given quantity of water, previously cooled to the temperature of freezing, actually loses, upon being changed to ice, if it were to be imbibed and retained by an equal quantity of water, at the

given temperature, (that of freezing,) would heat it 140 degrees, or would raise it to the temperature of ($32° + 140$) 162° of FAHRENHEIT'S thermometer, which is only 60° short of that of boiling water; consequently, any given quantity of water, at the temperature of freezing, upon being actually frozen, loses almost as much heat as, added to it, would be sufficient to make it boil.

It is clear, therefore, that the difference in the quantities of heat contained by the water in its fluid state, and heated to the temperature of 61° F, and by the ice, in the experiments before mentioned, was *at least* nearly equal to that between water in a state of boiling, and the same at the temperature of freezing.

But this quantity of heat will appear much more considerable, when we consider the great capacity of water to contain heat, and the great apparent effect which the heat that water loses upon being frozen would produce, were it to be imbibed by, or communicated to, any body whose power of receiving and retaining heat is much less.

The capacity of water to receive and retain heat,—or what has been called its specific quantity of latent heat,—has been found to be to that of gold as 1000 to 50,—or as 20 to 1; consequently, the heat which any given quantity of water loses upon being frozen,—were it to be communicated to an equal weight of gold, at the temperature of freezing, the gold, instead of being heated 162 degrees, would be heated $140 \times 20 = 2800$ degrees, or, would be raised to a *bright red heat.*

It appears therefore to be clearly proved, by my experiments, that a quantity of heat equal to that which 4214 grains (or about 9¾ oz.) of gold would require to heat it from the temperature of freezing water to be *red hot,* has no sensible effect

upon a balance capable of indicating so small a variation of weight as that of $\frac{1}{1000000}$ part of the body in question; and, if the weight of gold is neither augmented nor lessened by *one millionth part*, upon being heated from the point of *freezing water* to that of a *bright red heat*, I think we may very safely conclude, that ALL ATTEMPTS TO DISCOVER ANY EFFECT OF HEAT UPON THE APPARENT WEIGHTS OF BODIES, WILL BE FRUITLESS.