THE

COMPLETE WORKS

OF

COUNT RUMFORD.

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CONTENTS.

AN INQUIRY

CONCERNING

THE WEIGHT ASCRIBED TO HEAT.

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 upon 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 from each other.

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 exceptionable 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 a file the thickness of 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 an account is given in one of the preceding papers (that on the Moisture absorbed from the Atmosphere by various Substances), that the gilt surfaces of metals do not attract moisture (see Vol. I. p. 232), 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 affect 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 above mentioned, 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 $\frac{1}{700000}$ 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 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 ts 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 phenomena 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 ($32^\circ +$ 140) 172° of Fahrenheit's thermometer, which is only 40° 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 very 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 $9\frac{3}{4}$ 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.

SUPPLEMENT.

The foregoing paper having been originally drawn up for the purpose of being laid before the Royal Society, my respect for that learned body induced me to confine my observations to such points as I conceived to be new; and I took no notice whatever of a considerable number of experiments which I had made in the course of my investigations, because they were very similar to experiments that had before been made by other persons; and because their results did not appear to me to afford sufficient grounds to form any decisive opinion respecting the matter in question. There were, however, among my experiments, two or three of which I shall now give an account, which will probably be thought sufficiently interesting to deserve being mentioned.

Most of the experiments, from the results of which philosophers had been induced to form their opinions respecting the *ponderability of heat*, had been made by weighing the same given body at different temperatures. Thus, solid globes of metal — cannon-balls, for instance — had frequently been weighed when cold, and then, being heated red-hot, had been again weighed at that high temperature, and, from the apparent difference of the weight of the ball when cold and when red-hot, conclusions had been formed respecting the weight or levity of heat. But had the numerous causes of error in these most difficult experiments been less evident than they are, yet the results of the experiments of this kind which have hitherto been made by different persons have been so various and contradictory that no reliance whatever can be placed on them.

When a hot body is suspended in the air to the arm of a balance in order to its being weighed, as it continually gives off heat to the fluid in contact with it, this communication of heat occasions a strong ascending current of air to be formed over and by the sides of the hot body, which current cannot fail to affect the result of the experiment, and render the conclusions drawn from it fallacious. To prevent, if possible, these causes of error, the following experiments were contrived.

The hot body to be weighed, which was a small metallic ball, heated red-hot, was placed in the scale of the balance in a small hemispherical porcelain cup, which had a slender foot, or stand, about one inch high; and this cup, with the hot ball in it, was covered over by a porcelain coffee-cup, turned upside down, which, without touching the hot ball, confined the heated air which surrounded it. This coffee-cup and the porcelain stand were very exactly balanced, by weights in the opposite side, before the ball was introduced.

The following experiment was made at Munich on the 20th of April, 1785. The weather being cloudy, with intervals of sunshine, the thermometer in my room stood at 52° F., and the barometer 26 inches 4 lines, French measure.

At 30 *minutes after noon*, a small bullet, or grapeshot, of cast-iron, very well formed, and apparently solid, having been well washed and cleaned by scouring with sand, and thoroughly dried, was exposed in a clean vessel of porcelain in the midst of a mixture of pounded ice and sea salt, till it had acquired the temperature of 25° F. (7 degrees below the point of freezing water), when it was carefully weighed, and found to weigh very exactly $773\frac{15}{64}$ grains Troy.

At 1 *h.* 30 *m. P. M.*, the same bullet having been exposed 30 minutes in a clean, dry vessel of porcelain, placed in a sand heat, at the temperature of 212° F., or that of boiling water, was again weighed, while yet hot, and found to weigh no more than $773\frac{5}{64}$ grains.

At 2 *h.* 0 *m. P. M.*, the bullet having now been exposed 15 minutes, in a clean new Hessian crucible, well covered, to the heat of a strong charcoal fire, and being thoroughly red-hot, was found to weigh $773\frac{31}{64}$ grains.

The bullet, being yet red-hot, was put again into the crucible, and being once more exposed to the fire, which now burned very bright, *at* 2 *h.* 20 *m.* it had acquired a white heat, and began to show signs of melting, some small bubbles appearing upon its surface. In this state it was taken from the fire, and very carefully weighed sixteen times successively, at different intervals, when it was found to weigh as follows: —

Time when weighed.		Was found to weigh.	
At 2 h.	20 m.	$773\frac{35}{64}$	grains.
2	21	$773\frac{23}{64}$	"
2	23	$773\frac{13}{64}$	"
2	26	$772\frac{61}{64}$	"
2	29	$773\frac{3}{64}$	"
2	32	$773\frac{15}{64}$	"
2	43	$773\frac{63}{64}$	"
2	46	$774\frac{7}{64}$	"
2	49	$774\frac{11}{64}$	"
2	52	$774\frac{13}{64}$	"
2	56	$774\frac{19}{64}$	"
2	58	$774\frac{23}{64}$	"

Time when weighed.	Was found to weigh.
At 3 h. 1 m.	$774\frac{25}{64}$ grains.
3 18	$774\frac{39}{64}$ "
* 3 25	$774\frac{34}{64}$ "
6 15	$774\frac{61}{64}$ "

Immediately after this last-mentioned weighing of the bullet, the whole of the apparatus appearing to have acquired the temperature of the air in the room (60° F.), the bullet was taken away, and the porcelain stand and cup were again balanced in the scale, when it appeared that they had lost $\frac{1}{4}$ of a grain in weight during the preceding experiment. This apparent loss of weight I could ascribe to nothing but to the thorough drying of the cups in the experiment with the red-hot bullet, and to the drying of the silk cords by which the scale containing the cup and stand was suspended to the arm of the balance.

This weight $= \frac{1}{4}$ of a grain, which was required to balance the scales at the end of the experiment, being added to the apparent weight of the bullet at 6 h. 15 m. $= 774\frac{61}{64}$, its true weight at that time appears to have been $775\frac{13}{64}$.

The weight of the bullet at 1 h. 30 m. having been no more than $773\frac{5}{46}$ grains, and at 6 h. 15 m. it being found to weigh $775\frac{13}{64}$ grains, it appears that it had gained in weight by being heated red-hot $775\frac{13}{64} - 773\frac{5}{64} = 2\frac{1}{8}$ grains.

This augmentation of weight doubtless arose from the partial oxidation of the iron. It certainly did not arise *from the heat*, for it remained after the bullet had become cold.

* Just before this weighing, the coffee-cup which covered the bullet in the scale had been removed for a moment to look at the bullet, and then immediately replaced. What it was that escaped on this occasion I will not undertake to say, but certain it is that its weight amounted to $\frac{5}{64}$ of a grain, at the least.

An Account of an Experiment made with a Bullet of Fine Gold.

Munich, 23d April, 1785.—Weather cloudy, with intervals of sunshine; thermometer in my room at 65° F; barometer at 26 inches 4 lines.

A small bullet of fine gold, equal in value to 10 German ducats, which I procured from the master of the mint, being weighed in the open air of my room, was found to weigh $477\frac{157}{256}$ grains.

The small open china cup, in which the bullet was weighed, was exactly counterbalanced by a weight = $440\frac{248}{256}$ grains.

At 10 h. 5 m. A. M. the bullet, heated to a clear red heat approaching to whiteness, and weighed in the cup, open to the air, the bullet and the cup together were found to have lost of their weight $\frac{123}{256}$ of a grain.

Removing the bullet immediately, I found that the cup, or rather the cup and the scale in which it was placed, together had lost in weight $\frac{120}{256}$ parts of a grain.

Consequently, the bullet must have lost of its weight by being heated red-hot; or it appeared to be lighter when red-hot than when cold by $\frac{3}{256}$ parts of a grain, or $\frac{1}{40749}$ part of its whole weight.

Upon repeating the experiment I had nearly the same result; but upon varying it, by covering the heated bullet in the scale, in different ways, I found such variations in the results as convinced me that the apparent diminution of weight above mentioned might easily have arisen from currents in the atmosphere, and consequently that no dependence can be placed in experiments of that kind for deciding the fact relative to the weights of heated bodies, or the ponderability of heat.

I afterwards contrived an apparatus for making the experiment in a different and more unexceptionable manner. I provided three hollow globes of brass, very thin, and one larger than the other, and which being made to open in the middle, like a tobacco-box, could be placed one within another. In the centre globe I intended to place a solid bullet of pure gold, red-hot. Between the centre globe and that next it, I proposed to leave a space equal to the diameter of the heated bullet, filled with air; and the space between the second globe and the third I meant to have filled with pounded ice; and I proposed to have made the experiment at a time when the heat of the atmosphere should be just equal to that of freezing water; and in this manner I conceived that I should be able to avoid the currents in the air, whose effects I had found so distressing in my former experiments. But when I considered that the whole of the heat contained by the red-hot bullet would not be sufficient to thaw one half of the ice which surrounded it, and that, when the bullet should be cooled to the temperature of the ice, the whole mass of metal, of ice, and of water, would still remain *at the point of freezing;* and, moreover, that weighing the water produced by the ice would, in fact, be weighing the heat which before existed in the red-hot bullet, it first occurred to me that the point in question might much more readily be determined by simply weighing a quantity of ice at the temperature of freezing, and weighing the same again when changed into water.

I therefore left my apparatus unfinished, and turned my whole attention to the experiments of which an account has been given in the former part of this paper.

[This paper is printed from Rumford's Philosophical Papers, pp. 366 – 383.]

AN INQUIRY

CONCERNING THE

NATURE OF HEAT, AND THE MODE OF ITS COMMUNICATION.

HEAT is employed in such a vast variety of different processes, in the affairs of life, that every new discovery relative to it must necessarily be of real importance to mankind; for, by obtaining a more intimate knowledge of its nature and mode of action, we shall no doubt be enabled not only to excite it with greater economy, but also to confine it with greater facility, and direct its operations with more precision and effect.

Having many years ago found reason to conclude that a careful observation of the phenomena which attend the heating and cooling of bodies, or the communication of heat from one body to another, would afford the best chance of acquiring a farther insight into the nature of heat, my view, in all my researches on this subject, has been principally directed to that point; and the experiments of which I am now to give an account may be considered as a continuation of those I have already, at different times, had the honour of laying before the Royal Society, and of presenting to the public in my Essays.

In order that the attention of the Society may not be interrupted unnecessarily by description of instruments in the midst of the accounts of interesting experiments, I shall begin by describing the apparatus which was provided for these researches; and, as a perfect knowledge of the instruments made use of is indispensably necessary in order to form distinct ideas of the experiments, I shall take the liberty to be very particular in these descriptions.

The thermometers, four in number, which were used in these experiments, were constructed under my own eye, and with the greatest possible care; and, after every trial I have been able to make with them, in order to ascertain their accuracy, they appear to be very perfect.

They are mercurial thermometers, graduated according to Fahrenheit; their bulbs are cylindrical, 4 inches long, and $\frac{4}{10}$ of an inch in diameter; and their tubes are from 15 to 16 inches long. The mercury with which they are filled is quite pure, and they are freed from air. Their scales were divided with the greatest care; and, by means of a nonius, they show eighth parts of a degree very distinctly; they are graduated from about 10 degrees below the freezing point to 5 or 6 degrees above the point of boiling water. Their bulbs are quite naked; their scales ending about 1 inch above the junction of the bulb with its tube. The freezing point is situated about 5 inches above the upper end of the bulb. The reason for placing it so high will be evident from the details of the experiments in which these instruments were used.

The instrument I contrived for ascertaining the warmth of clothing is extremely simple; it is merely a hollow cylindrical vessel made of thin sheet brass. It is closed at both ends, and has a narrow cylindrical neck, by which it is occasionally filled with hot water.

This vessel, being covered with a garment made to fit it, composed of any kind of cloth or stuff, or other warm covering, is supported in a vertical position on a wooden stand, which is placed on a table in a large quiet room; and one of the thermometers above described being placed in the axis of the vessel, the time employed in cooling the water, through the clothing with which the instrument is covered, is observed and noted down.

Now, as the time of cooling through any given interval of the scale of the thermometer (or from any given degree above the temperature of the air of the room to any other given lower degree, but still above the temperature of the air of the room) will be longer or shorter as the covering of the instrument is more or less adapted for confining heat, it is evident that the relative warmth of clothing of different kinds may be very accurately determined by experiments of this sort.

I provided four instruments of this kind, all very nearly of the same dimensions. Their cylindrical bodies are each 4 inches in diameter and 4 inches long; and their cylindrical necks are about $\frac{8}{10}$ of an inch in diameter, and 4 inches in length. This neck is placed in the centre of the circular flat top, or upper end, of the vertical cylindrical body; and opposite to it, in the centre of the flat bottom of the body, there is a hollow cylinder, $\frac{8}{10}$ of an inch in diameter and 3 inches long, projecting downwards, into which a vertical cylinder of wood is fitted, on the top of which the instrument is supported, in such a manner that the air has free access to every part of it. This cylinder of wood constitutes a part of the wooden stand above mentioned.

As the thermometer is placed in the axis of the cylindrical vessel, and as its bulb is just as long as the body of this vessel, it is evident that it must ever indicate the *mean temperature* of the water in the vessel, however different the temperature of that water may be at different depths.

The thermometer is firmly supported in its place by causing a part of the lower end of its scale to enter the neck of the cylindrical vessel, and to fit it with some degree of accuracy, but not so nicely as to be in danger of sticking fast in it.

The lower end of the bulb of the thermometer does not absolutely touch the bottom of the vessel, but it is very near touching it.

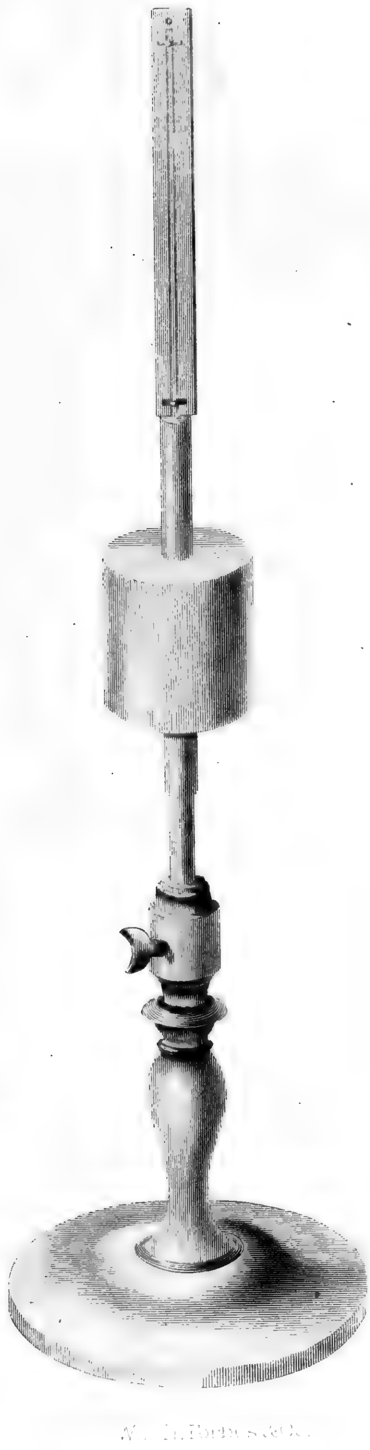
Figure 1 (Plate I.) will give a clear idea of this instrument placed on its wooden stand, which is so contrived that the instrument may be placed higher or lower at pleasure.

The foregoing description of this instrument is so particular that the figure will be easily understood without any further illustration. The cylindrical vessel is represented placed on the stand, with its thermometer in its place.

As, in some of the first experiments I made with this instrument, I found it difficult to apply the coverings which I used to the ends of the body of the instrument, I endeavoured, by covering up those ends with a permanent and very warm covering, to oblige most of the heat to pass off through the vertical sides of the instrument, to which it was easy to fit almost any kind of covering, and more especially coverings of various

PLATE I

Fig 1



thicknesses of confined air, the relative warmth of which I was very desirous of ascertaining.

The means I employed for covering up the ends of the instrument were as follows. Having provided two thin cylindrical wooden boxes (like common pill-boxes, but much larger), something less in diameter than the body of the instrument, and $2\frac{1}{2}$ inches deep, I dried them as much as possible; and, after having varnished them within and without with spirit varnish, I covered them within and without with fine wove writing-paper, and then gave the paper three coats of the same varnish. I then perforated the bottoms of these boxes with round holes, just large enough to admit the neck of the instrument, and the cylindrical projection at its bottom; and then inverted them over the two ends of the instrument, filling the boxes at the same time with *eider-down.*

These boxes were fixed and confined in their places by means easy to be imagined; and, in order to confine the heat still more effectually, each of the boxes was covered on the outside with a cap of fur, as often as the instrument was used; as was also that part of the neck of the instrument which projected above the box.

Two of the instruments, which I shall distinguish by the numbers 1 and 2, were covered up at their ends in this manner; the other two instruments, No. 3 and No. 4, were left in the state represented by the Figure 1; that is to say, the ends of their cylindrical bodies were not covered with permanent coverings.

In each experiment, two similar instruments (No. 1 and No. 2, for instance, or No. 3 and No. 4) were used, the one *naked*, and the other *covered;* and, as the naked instrument always served as a standard, with which the results of the experiments made with the other were compared, it is evident that this arrangement rendered the general results of the experiments much more satisfactory and conclusive than they could possibly have been, had the experiments made on different days and with various kinds of covering been made singly, or unaccompanied by a fixed and invariable standard.

The experiments were made and registered in the following manner: The two instruments used in the experiment, placed on their wooden stands, being set down on the floor, were filled to within about 1½ inch of the tops of their cylindrical necks with boiling hot water; and, a thermometer being put into each of them, they were placed at the distance of three feet from each other, on a large table, in a corner of a large quiet room,* where they were suffered to cool undisturbed. Near them on the same table, and at the same height above the table, there was placed another thermometer (suspended in the air to the arm of a stand), by which the temperature of the air of the room was ascertained from time to time.

No person was permitted to pass through the room while an experiment was going on; and in order to prevent, as far as it was possible, all those currents of air in the room which were occasioned by partial heat, produced by the light which came in at the windows, the window-shutters were kept constantly shut; one of them only being opened for a moment, now and then, just to observe the thermometers, and note down the progress of the experiment.

* This room, which is adjoining to my laboratory, in my house at Munich, is 19 feet wide, 24 feet long, and 13 feet high.

The results of each experiment were entered on a separate sheet of paper; which paper was previously prepared for that use by being divided into separate vertical columns by lines drawn with a pen, and ruled in parallel horizontal lines with a lead-pencil.

"*Experiments on Heat, made at Munich, 11th March, 1803. The large cylindrical Vessels, No. 1 and No. 2 (made of thin sheet brass), were filled with hot Water, and exposed to cool in the Air of a large quiet Room. The Ends of both these Instruments were well covered with warm Clothing, Furs, &c. The vertical polished Sides of No. 1 were* naked. *The Sides of No. 2 were* covered *with one Thickness of fine white* Irish Linen, *which had been worn, strained over the metallic Surface.*"

Time.		Temperature		Temperature of the Air.	Time.		Temperature		Temperature of the Air.
h.	min.	of No. 1, *naked.*	of No. 2, *covered.*		h.	min.	of No. 1, *naked.*	of No. 2, *covered.*	
		°	°	°			°	°	°
10	10	126½	126	43¼	4	..	61¾	53½	43½
..	30	109½	106½	43½	..	30	59½	52	..
..	45	105	100⅛	43¾	5	30	57	49¾	42½
11	..	101¼	94¾	44	6	..	55½	49⅛	..
..	2½	..	94	..	..	30	54¼	48¼	..
..	15	97½	90¼	..	7	..	53½	47½	42
..	30	94	86¼	..	8	..	51½	46½	..
..	39	..	84	..	9	..	50	45¾	..
..	45	91¼	82½	..	10	..	49	45	..
12	..	88½	79⅜	..	8	12th Mar.	43	42	40
..	15	85½	76	..	The instruments were now removed into a warm room.				
..	25	84	..	..	8	2	43	42	62
..	30	..	74½	..	..	32	44¾	44¾	62½
..	45	80	70	..	..	47	46	46½	63
1	..	78	68⅛	..	9	24	48	49½	..
..	30	74¼	64¼	..	10	..	50	52	..
2	..	71⅛	61½	43¾	..	41	51½	53⅞	..
..	30	68⅛	58¾	43½	12	..	54	56½	..
3	..	65¾	56¾	..	12	26	54½	57	..
..	30	63½	54¾	..	An end was now put to the experiment.				

The above is an exact copy of one of these register-sheets, and contains the results of an actual and very interesting experiment, which lasted 26 hours.

Though it was easy to discover, by a single glance at the register, whether a covering which was put over one of the instruments prolonged the time of its cooling or not; yet, in order to compare the results of different experiments, and particularly of such as were made on different days, so as to determine with precision *how much* warmer one kind of covering was than another, it was necessary to fix on some particular interval in the scale of the thermometer, or number of degrees, commencing at some certain invariable number of degrees above the temperature of the air by which the instrument was surrounded, in order that the warmth of the covering, or its power of confining heat, might with certainty be estimated by the time employed in cooling through that interval.

By the results of a great number of experiments I found that the same instrument cooled through any given (small) number of degrees (10 degrees, for instance) in very nearly the same time, whatever was the temperature of the air of the room; provided always, that the point from which these 10 degrees commenced was at the same given number of degrees above the temperature of the air at the time being.

The interval I chose for comparing the results of my experiments is that which commences with the *fiftieth*, and ends with the *fortieth*, degree of Fahrenheit's thermometer *above the temperature of the air in which the instrument is exposed to cool.* When, for instance, the air was at 58°, the interval commenced at the 108th degree, and ended at the 98th. When the air was at 64½°, it commenced at 114½°, and ended at 104½°.

That the same instrument, exposed to cool in the air, does in fact cool the same number of degrees in the same time, very nearly, when the given interval of the scale of the thermometer is reckoned from the same height, or given number of degrees above the temperature of the air at the time when the experiment is made, will appear from the following results of 11 different experiments, made on different days, and when the air in which the instrument was exposed to cool was at different degrees of temperature.

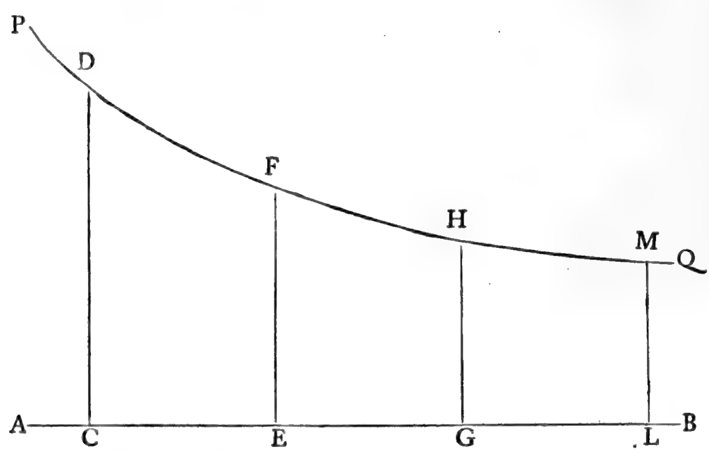
The large cylindrical vessel, No. 1, having its two ends well covered up with eider-down, furs, &c., its vertical sides being exposed *naked* to the air, in a large quiet room, was found to cool 10 degrees, *viz.* from the 50th to the 40th degree above the temperature of the air in which it was exposed, as follows: —

Temperature of the air.	Degrees cooled.	Time employed in cooling.
°	° °	
44	from 94 to 84	55 minutes.
45¼	" 95¼ to 85¼	55½ "
48	" 98 to 88	55¼ "
51½	" 101½ to 91½	55½ "
52	" 102 to 92	55 "
54	" 104 to 94	54¼ "
44	" 94 to 84	55⅝ "
42½	" 92½ to 82½	55⅓ "
45	" 95 to 85	56 "
46	" 96 to 86	55 "
44	" 94 to 84	55⅓ "

The fact which these experiments are here brought to prove has likewise been confirmed by other experiments, made with other instruments, and at times when the temperature of the air has been as high as 64°; but I will not take up the time of the Society by giving a particular account of them in this place.

As it sometimes happened, though very seldom, in the course of an experiment (which commonly lasted several hours) that I was called away, and was not present to observe the thermometer at the moment of the passage of the mercury through one or both of those points of its scale which formed the limits of the given interval chosen as the standard for a comparison of the results of the experiments with each other, it became a matter of considerable importance to find means for supplying these accidental defects, and ascertaining the points in question by interpolation.

In order to facilitate the means of doing this, I endeavoured to investigate the law of the cooling of hot bodies in a cold fluid medium; and I found reason to conclude,



That if, on the right line AB, a perpendicular CD be taken, equal to the difference of the temperatures of the hot body and of the colder medium, expressed in degrees of the thermometer; and, after a certain given time, represented by CE, taken on the line AB at the point E, another perpendicular EF be erected, and EF be taken equal to the difference of the temperatures after the time represented by CE has elapsed; and if the perpendiculars GH and LM be drawn, representing the difference of the temperatures after the times EG and GL have elapsed, a curved line PQ drawn through the points D, F, H, M, will be the logarithmic curve; or, if it vary from that curve, its variation, within the limits answering to a change of temperature amounting to a few degrees (especially if they be taken when the temperature of the hot body is about 40 or 50 degrees above that of the medium), will be so very small that no sensible error will result from a supposition that it is the logarithmic curve, in supplying, by computation, any intermediate observations which happen to have been neglected in making an experiment.

These computations are very easily made, with the assistance of a table of logarithms, in the following manner.

Supposing CD, CG, and GH, to have been determined by actual observation; and that it were required to ascertain, by computation, the absciss CE, corresponding to any given intermediate ordinate EF, or (which is the same thing) to determine at *what time* the cooling body was at any given intermediate temperature (= EF) between that (= CD) which it was found by observation to have at the point C, and that (= GH) which it was found to have after the time represented by the line GC had elapsed;

It is log. CD — log. GH is to CG as 1 to *m* (= modulus = the subtangent of the curve at the point D.)* And CE = *m* × log. CD — log. EF.

* The subtangent shows in what time the instrument would cool down to the tem-

If, for instance, in the experiment of the 11th March, (the details of which have just been given) the time when the instrument No. 2, in cooling, passed the important point of 94° had not been observed, this neglect might have been supplied, by computation, in the following manner.

It is CD = $94\frac{3}{4}°$, the nearest *observed* temperature higher than EF (= 94°), and GH = $90\frac{1}{4}$, the nearest observed temperature below that of 94°; and CG = 15 minutes, or 900 seconds = the time elapsed between the two observations.

$$\begin{aligned} \text{It is log. } 94\tfrac{3}{4} &= 1.9765792 \\ \text{And log. } 90\tfrac{1}{4} &= \underline{1.9554472} \\ \text{Log. CD} - \text{log. GH} &= 0.0211320 \end{aligned}$$

And 0.0211320 is to 900 (= CG) as 1 to 42590 = *m*.

$$\begin{aligned} \text{And again, log. } 94\tfrac{3}{4} &= 1.9765792 \\ \text{Log. } 94 &= \underline{1.9731279} \\ \text{Log. CD} - \text{log. EF} &= 0.0034513 \end{aligned}$$

42590 × 0.0034513 (= *m* × log. CD — log. EF) = 147 seconds = 2 minutes and 27 seconds; which differs very little from $2\frac{1}{2}$ minutes, the observed time.

If, from the temperature observed at 11h. 30 min. = $86\frac{1}{4}°$, and the temperature observed at 11h. 45 min. = $82\frac{1}{2}°$, and the time which elapsed between these two

perature of the air in which it is placed, were its velocity of cooling at the point D to be continued *uniformly* from that point; and, as the subtangent of the logarithmic curve is *constant*, if PQ were the logarithmic curve, it would follow that the velocity with which a hot body cools in a fluid medium is everywhere such, that, were *that velocity* to be continued uniformly, the body would be cooled down to the temperature of the medium *in the same time*, whatever might be the excess of the temperature of the hot body above that of the medium, at the moment when its velocity of cooling became uniform.

observations (= 15 minutes), we were to determine by computation the time when the instrument was at the temperature of 84° (the lower point of the standard interval of 10 degrees answering to the temperature of the air, = 44°, in which the instrument was cooled), it will turn out, 8 minutes and 55 seconds after 11h. 30 min. The observed time was 11h. 39 min.; which differs from the computed time no more than 5 seconds.

If it were strictly true, as a very great philosopher and mathematician has advanced, that the velocity with which a hot body, exposed to cool in a cold fluid medium, parts with its heat is as the difference of the temperatures of the body and of the medium, it is most certain that the curve PQ could be no other than the logarithmic curve. Perhaps it may be so in fact, and that the variations from it which my experiments indicated were owing solely to the imperfection of the divisions of our thermometers. If it be so, it is not impossible to divide the scale of a thermometer in such a manner as to indicate with certainty *equal increments of heat*, as thermometers ought to do; but this is not the proper place to enlarge on this subject. I may perhaps return to it hereafter.

Passing over in silence a number of experiments I made in order to get thoroughly acquainted with my new instruments, and to assure myself that the results of similar experiments made with them were uniform and might be depended on, I shall now proceed to give an account of several experiments made with pointed views, the results of some of which were very interesting.

Experiment No. 1.—The large cylindrical vessel No. 1, with its ends covered with warm clothing, in the manner before described, and its vertical sides (which were polished, and very clean and bright) exposed naked to the air, was filled with water nearly boiling hot, and placed on its wooden stand, on a table, in a large quiet room to cool; the air of the room being at the temperature of 45° Fahrenheit.

Another cylindrical vessel, No. 2, in all respects like No. 1, and with its ends covered in the same manner, but with its vertical sides covered with a single covering of fine Irish linen (such as is sold in London for about 4 *s. per* yard), closely applied to the body of the instrument, was filled with hot water at the same time, and placed on the same table to cool.

This experiment lasted many hours; and, in that period, the temperature of the water in each of the instruments was carefully observed and noted down a great number of times.

The result of this experiment (the details of which have already been given) was very remarkable.

While the instrument No. 1, whose sides were *naked*, employed 55 minutes in cooling from the point of 94° to that of 84°, the instrument No. 2, whose sides were *covered with linen*, cooled through the same interval in 36½ minutes.

Hence it appears that clothing may, in some cases, expedite the passage of heat out of a hot body, instead of confining it in it.

Desirous of seeing whether the same covering would, or would not, expedite the passage of heat *into* the instrument, after having suffered both instruments to cool down to the temperature of about 42°, I removed them into a warm room, in which the air was at the temperature of 62°; and I found that the instrument

No. 2, which was clothed, acquired heat considerably faster than the other, No. 1, which was naked.*

The discovery of these extraordinary facts surprised me, and excited all my curiosity; and I immediately set about investigating their cause.

As it is well known that air adheres with considerable obstinacy to the surfaces of some solid bodies, I conceived it to be possible that the particles of air in immediate contact with the surface of the cylindrical vessel No. 1, might in fact be so attached to the metal as to adhere to it with some considerable force; and, if that were the case, as confined air is known to constitute a very warm covering, it appeared to me to be possible that the cooling of the vessel No. 1 might have been retarded by such an invisible covering of confined air; which covering, in the experiment with the vessel No. 2, had been displaced and in a great measure driven away by the colder covering of linen by which the body of the instrument was closely embraced.

I conceived that the linen must have accelerated the cooling of the instrument, either by facilitating the approach of a succession of fresh particles of cold air, or by increasing the effects of *radiation;* and, with a view to elucidate that important point, the following experiments were made.

Experiment No. 2. — Removing the linen with which the instrument No. 2 was clothed, I now covered the sides of that instrument with a thin transparent coating of glue; and, when it was quite dry and hard, I again filled the two instruments (No. 1 and No. 2)

* The details of this experiment (which was made on the 11th of March, 1803) may be seen on page 29.

with hot water, and observed the times of their cooling as before.

Result, or time of cooling 10 degrees, reckoned from the 50th to the 40th degree above the temperature of the air in which the instruments were exposed to cool: —

Instrument No. 1, sides *naked*, ...	55 min.
Instrument No. 2, sides *covered with one coating of glue*,	43½ "

When we consider this experiment with attention, we shall find reason to conclude, that if it were by facilitating the approach and temporary contact of a succession of fresh particles of the cold air of the room to the surface of the glue (which was now in fact become the surface of the hot body), that the cooling of the instrument was accelerated, the metal being as completely covered, and the air, supposed to be attached and fixed to its surface, as completely excluded by one coating of the glue as it could be by two or more, two coatings could not possibly accelerate the cooling of the instrument more than one; but if, on the other hand, the cooling of the instrument in this experiment was accelerated, not by facilitating and accelerating the motions of the circumambient cold air, but by facilitating and increasing those *radiations* which are known to proceed from hot bodies, I conceived that two coatings of the glue might possibly accelerate the cooling of the vessel more than one. In order to put this conjecture to the test, I made the following decisive experiment.

Experiment No. 3. — I now gave the instrument No. 2 a second coating of glue; and, when it was thoroughly dry, I repeated the experiment last mentioned, with the above variation; when I found the results to be as follows: —

	Time of cooling the 10 degrees in question.
The instrument No. 1, *naked* metal, ...	$55\frac{1}{3}$ min.
The instrument No. 2, *covered* with two coatings of glue,	$37\frac{5}{6}$ "

Finding that two transparent coatings of glue facilitated the cooling of this instrument even more than one coating, I washed off all the glue with warm water; then, making the instrument as clean and bright as possible, I covered its sides with a coating of very fine, transparent, and colourless spirit varnish; and, after this coating of varnish had become quite dry and hard, I repeated the experiment above mentioned; and, finding that this covering, like that of glue, expedited the cooling of the instrument, I first added a *second* coating of the varnish, and repeated the experiment again, and then added two coatings more, making *four* in all. Finding that the cooling of the instrument was more and more rapid, as the thickness of the varnish was increased, I now added four coatings more, making *eight* coatings in the whole, giving time for each new coating to dry thoroughly before the next was applied; but I found, on repeating the experiment with this thick covering of varnish, that I had passed the limit of thickness which produced the greatest effect.

In order that the result of these experiments with coatings of different thicknesses of spirit varnish may be seen at one view, I shall here place them all together; and I shall place by the side of each the result of the standard experiment, which was made at the same time with the instrument No. 1, the sides which were *naked*.

	Time employed in cooling through the given interval of 10 degrees. Instrument No. 1, *varnished.*	Instrument No. 2, *naked.*
Experiment No. 4. — 1 coating of varnish,	42 min.	55½ min.
Experiment No. 5. — 2 coatings, . . .	35¾ "	55¼ "
Experiment No. 6. — 4 coatings, . . .	30¼ "	55½ "
Experiment No. 7. — 8 coatings, . . .	34¼ "	55 "

Experiment No. 8. — Desirous of finding out what effect *colour* would produce, I now painted the sides of the instrument No. 2 *black*, with lamp-black mixed up with size (this paint being laid upon the eighth coating of the varnish), and, repeating the experiment, its results were as follows: —

	Time employed in cooling through the given interval.
The instrument No. 1, *naked*, ...	55¼ min.
The instrument No. 2, covered with 8 coatings of varnish, and painted black, ...	34 "

Experiment No. 9. — Finding that the painting of this thick coating of varnish *black* rendered the covering still colder, or accelerated the cooling of the instrument, I now washed off the black paint with warm water; then washing off all the varnish with hot spirit of wine, I painted the metallic sides of the instrument of a black colour with lamp-black and size; and when the paint was quite dry, I repeated the experiment so often mentioned, when the results were as follows: —

	Time employed in cooling through the given interval.
The instrument No. 1, sides *naked*, ...	55⅛ min.
The instrument No. 2, *painted black*, ...	35 "

Experiment No. 10. — In order to find out whether the *black* colour had any particular efficacy in expediting the cooling of the instrument, or whether another colouring substance would not produce the same effect, when mixed up with the same size, I now washed off the black paint and painted the sides of the instrument *white*, with whiting mixed up with size; and, on repeating the experiment, the results were as follows: —

	Time of cooling through the given interval.
The instrument No. 1, *naked*, ...	$55\frac{1}{3}$ min.
The instrument No. 2, *painted white*, . . .	36 "

As in both the two last experiments it was found necessary to paint the body of the instrument three or four times over, in order to cover the polished metal so completely as to prevent its shining through the paint; this, of course, occasioned the surface of the metal to be covered with a thick coating of size, which, no doubt, affected very sensibly the results of the experiment, and rendered it impossible to determine, in a satisfactory manner, what the effects really were, which were produced by the *different colours* used in the two experiments.

Experiment No. 11. — With a view to throw some more light on this interesting subject, having washed off the paint from the instrument No. 2, I now rendered its sides of a perfectly deep black colour, by holding it over the flame of a wax candle; and, repeating the usual experiment, the results were as follows: —

	Time of cooling through the standard interval.
The instrument No. 1, *naked*, ...	$55\frac{7}{8}$ min.
The instrument No. 2, *blackened*, ...	$36\frac{1}{8}$ "

In order to ascertain the quantity of matter which composed this black covering, I weighed a small piece of clean and very fine linen; and, having wiped off with it all the black matter from the body of the instrument No. 2, in such a manner that the whole of it remained attached to the linen, I weighed it again, and by that means discovered that the whole of this black substance, which had so completely covered the sides of the instrument (a surface of polished brass = 50 superficial inches) that the metal did not shine through it in any part, weighed no more than $\frac{1}{18}$ of a grain Troy.

How this very thin covering, which, if the specific gravity of the black matter were only equal to that of water, would amount to no more than $\frac{1}{4500}$ of an inch in thickness, could expedite the cooling of the instrument, in the manner it was found to do, is what still remains to be shown; but, before I proceed any farther in these abstruse inquiries, I shall make a few observations relative to the results of the foregoing experiments.

Although we may with safety presume, that the velocities with which the heat escaped *through the sides of the instruments* * were nearly as the times inversely taken up in cooling through the given interval of 10 degrees; yet, as some heat must have made its way, in the course of the experiment, *through the ends of the instrument*, notwithstanding all the care that was taken to prevent it by covering them up with warm clothing, it is necessary, in order to be able to compare the results of the preceding experiments in a satisfactory manner, to

* I have found myself obliged in this, as in many other places, to make use of language which is far from being as correct as I could wish. I do not believe that heat ever *makes its escape* in the manner here indicated; but I could not venture to use uncommon expressions in pointing out the phenomena in question, however well adapted such expressions might be to describe the events which really take place. If it should be found that *caloric*, like *phlogiston*, is merely a creature of the imagination, and has no real existence (which has ever appeared to me to be extremely probable), in that case, it must be incorrect to speak of heat as *making its escape* out of one body, and *passing* into another; but how often are we obliged to use incorrect and figurative language, in speaking of natural phenomena!

find out how much of the heat made its escape through the covered ends of the instruments, during the time the instruments were cooling through the interval in question.

In order to determine that point, I now removed the covering from the ends of the instrument No. 1; and, when it was quite naked, I found, on making the experiment, that it cooled through the given interval in $45\frac{1}{2}$ minutes.

When its two ends and its cylindrical neck were covered up with warm clothing, I found, by taking the mean of the results of several experiments, that it required $55\frac{1}{2}$ minutes to cool through the same interval.

On measuring the instrument with care, I found its dimensions as follows: —

	Inches.
Diameter of the body of the instrument, . .	= 4.03
Length of the body, ...	= 3.96
Diameter of the neck of the instrument, . .	= 0.8
Length of the neck, ...	= 4.

The superficies of the different parts of the instrument are therefore as follows: —

Superficies of the vertical sides of the body ($= 4.03 \times 3.14159 \times 3.96$) $= 50.136$ inches.

Superficies of the flat circular bottom of the instrument, ($= 4.03 \times 3.14159 \times \frac{4.03}{4}$) $= 12.755$ inches; deducting nothing for that part which is covered by the end of the tube, which serves as a support for the instrument.

Superficies of the flat circular top of the instrument (after deducting 0.502 of a superficial inch for the circular hole in its centre, made to receive the lower end of the cylindrical neck) $= 12.253$ inches.

Superficies of the cylindrical neck of the instrument $(= 0.8 \times 3.14159 \times 4) = 10.051$ inches.

Supposing, now, that the heat passes with equal velocity through the surface of all the different parts of the instrument, when the instrument is naked, we can determine the quantity of heat which escaped through the ends and neck of the instrument in the experiments in which those parts of the instrument were covered with warm clothing.

The whole of the metallic surface exposed to the air, in the experiments made with the instrument when it was quite naked, amounted to 85.195 superficial inches, namely: —

		Inches.
Surface of its vertical sides,	=	50.136
Surface of its lower end,	=	12.755
Surface of its upper end,	=	12.253
Surface of its neck,	=	10.051
Total surface,	=	85.195

When the instrument was exposed quite naked to the air, it was found to cool through the standard interval of 10 degrees in $45\frac{1}{2}$ minutes.

Assuming, now, any given number as the measure of the whole quantity of heat given off by the instrument during the period above mentioned, we can ascertain what part or proportion of that quantity passed off through the sides of the instrument; and what part of it must have made its escape through its ends, and through the sides of its neck.

As the quantities of heat given off are supposed to have been as the quantities of surface exposed to the air, if we suppose the whole quantity of heat lost by the instrument to be = 10,000 parts, the quantity which passed through the vertical sides of the instrument in 45½ minutes, in the experiment, must have amounted to 5885 parts. For, the whole of the surface of the instrument, = 85.195 superficial inches, is to the whole of the heat given off, = 10,000, as the surface of the vertical sides of the instrument, = 50.136 superficial inches, to the quantity of heat which must have passed off through that surface in the given time, = 5885.

Now, as we may with safety conclude that the quantity of heat which passes off through a *given surface* must be as the times elapsed, all other circumstances being the same, we can determine how much of the heat given off by the instrument, in those experiments in which its ends were covered, passed through the sides of the instrument; and, consequently, how much of it must have made its way through its ends and neck, notwithstanding their being covered.

The instrument with its ends and neck covered up with eider-down, furs, &c., was found to cool through the standard interval of 10 degrees in 55½ minutes. Now, as only 5885 parts of heat were found to pass through the naked vertical sides of the instrument in 45½ minutes, no more than 7015 parts could have passed through the same surface in 55½ minutes; consequently, the remainder of the heat lost by the instrument in the experiment in question, amounting to 2985 parts, must necessarily have made its way through the covered ends and neck of the instrument in the given period, 55½ minutes.

Taking it for granted that these computations are well founded, we may now proceed to a more exact determination of the relative quantities of heat which made their way through the sides of the instrument

No. 2, when its sides were exposed naked to the air, and when they were covered with the different substances which appeared to facilitate the escape of the heat.

In the experiment No. 11, when the sides of the instrument were made quite black by holding it over the flame of a wax candle, the instrument cooled through the standard interval of 10 degrees in $36\frac{1}{8}$ minutes.

In that time a quantity of heat = 1942 parts must have passed off through the covered ends and neck of the instrument; for, if a quantity = 2985 parts could pass off that way in $55\frac{1}{2}$ minutes, the quantity above mentioned (= 1942 parts) must have escaped in $36\frac{1}{8}$ minutes.

This quantity, = 1942 parts, taken from the whole quantity, = 10,000 parts, lost by the instrument in cooling through the interval in question, leaves 8058 parts for the quantity which made its escape through the sides of the instrument in the experiment in question.

Now, if a quantity of heat = 7015 parts, requires $55\frac{1}{2}$ minutes to make its way through the naked sides of the instrument (as we have just seen), it would require $63\frac{3}{4}$ minutes for the quantity in question, = 8058 parts, to pass off through the same surface.

But, when that surface was blackened over the flame of a candle, that quantity of heat passed off through it in $36\frac{1}{8}$ minutes.

Hence it appears, that the velocity with which heat is given off from the naked surface of a heated metal exposed to cool in the air, is to the velocity with which it is given off by the same metal when its surface is blackened in the manner above described, as $36\frac{1}{8}$ to $63\frac{3}{4}$, or as 5654 to 10,000, very nearly; for the velocities are as the times of cooling, inversely.

Again, in the experiment No. 6, the sides of the instrument No. 2 being covered with four coatings of spirit varnish, the instrument was found to cool through the given interval of 10 degrees in $30\frac{1}{4}$ minutes.

In that time, a quantity of heat = 1627 parts, must have made its way through the covered ends of the instrument; and the remainder, = 8373 parts, must have made its way through its varnished sides.

This quantity, = 8373 parts, would have required $66\frac{1}{4}$ minutes to have made its way through the naked sides of the instrument; and, as it actually made its way through the varnished sides of the instrument in $30\frac{1}{4}$ minutes, it appears that the velocity with which the heat was given off from the naked metallic surface, was to the velocity with which it was given off from the same surface covered with four coatings of spirit varnish, as $66\frac{1}{4}$ to $30\frac{1}{4}$, or as 10,000 to 4566.

Without pursuing these computations any farther at present, and without stopping to make any remarks on the curious facts they present to us, I shall hasten to experiments, from the results of which we shall obtain more satisfactory information. But, before I proceed any farther, I must give an account of an instrument I contrived for measuring, or rather for *discovering*, those very small changes of temperature in bodies, which are occasioned by the radiations of other neighbouring bodies, which happen to be at a higher or at a lower temperature.

This instrument, which I shall take the liberty to call a *thermoscope*, is very simple in its construction. Like the hygrometer of Mr. Leslie (as he has chosen to call his instrument), it is composed of two glass balls, attached to the two ends of a bent glass tube; but the balls, instead of being near together, are placed at a considerable distance from each other; and the tube which connects them, instead of being bent in its middle, and its two extremities turned upwards, is quite straight in the middle, and its two extremities, to which its two balls are attached, are turned perpendicularly upwards, so as to form each a right angle with the middle part of the tube, which remains in a horizontal position.

At one of the elbows of this tube there is inserted a short tube of nearly the same diameter, by means of which a very small quantity of spirit of wine, tinged of a red colour, is introduced into the instrument; and, after this is done, the end of this short tube (which is only about an inch long) is sealed hermetically; and all communication is cut off between the air in the balls of the instrument and in its tube and the external air of the atmosphere.

A small *bubble* of the spirit of wine (if I may be allowed to use that expression) is now made to pass out of the short tube into the long connecting tube; and the operation is so managed that this bubble (which is about $\frac{3}{4}$ of an inch in length) remains stationary, at or near the middle of the horizontal part of the tube, *when the temperature (and consequently the elasticity) of the air in the two balls, at the two extremities of the tube, is precisely the same.*

By means of a scale of equal parts, attached to the horizontal part of the connecting tube, the position of the bubble can be ascertained, and its movements observed.

If now, the bubble being at rest in its proper place, one of the balls of the instrument be exposed to the calorific rays which proceed in all directions from a hot body, while the other ball is defended from those rays by a screen, the air in the ball so exposed to the action of these rays will be heated; and, its elasticity being increased by this additional heat, its pressure will no longer be counterbalanced by the elasticity of the colder air in the other ball, and the bubble will be forced to move out of its place and to take its station nearer to the colder ball.

By presenting two hot bodies at the same time to the two balls of the instrument, taking care that each ball shall be defended from the action of the hot body presented to the opposite ball, the distances of these hot bodies from their respective balls may be so regulated that their actions on those balls may be equal, however the temperatures of those hot bodies may differ, or however different may be the quantities or intensities of the calorific rays which they emit.

The instrument will show, with the greatest certainty, when the actions of these hot bodies on their respective balls are equal; for, until they become *unequal,* the bubble will remain immovable in its place.

And, when the actions of two hot bodies on the instrument are equal, the relative intensities of the rays they emit may be ascertained by the distances of the bodies from the balls of the instrument.

If their distances from their respective balls are equal, the intensities of the rays they emit must, of course, be equal.

If those distances are unequal, the intensities will probably be as the squares of the distances, inversely.

A distinct and satisfactory idea may be formed of the instrument I have been describing, from Fig. 2 (Plate II.).

AB is a board, 27 inches long, 9 inches wide, and 1 inch thick, which serves as a support for the bent tube CDE, at the two extremities of which the two balls are fixed. The two projecting ends of the tube, C and E, which are in a vertical position, are each 10 inches long; and the horizontal part D of the tube, which is fastened down on the board, is 17 inches in length.

The balls are each 1.625 inches in diameter. The diameter of the tube is such, that 1 inch of it in length would contain 15 grains Troy of mercury.

The pillar F, which, by means of a horizontal arm projecting from it, serves for supporting the circular vertical screen represented in the figure, is firmly fixed in the board AB.

This circular screen (which is made of pasteboard, covered on both sides with gilt paper) serves for preventing one of the balls of the instrument from being affected by the calorific rays proceeding from a hot body which is presented to the opposite ball.

Besides the circular screen represented in the figure, several other screens are used in making experiments; for the instrument is so extremely sensible, that the naked hand presented to one of the balls, at the distance of several inches, puts the bubble in motion; and it is affected very sensibly by the rays which proceed from the person who approaches it to make the experiments, unless care be taken, by the interposition of screens, to prevent those rays from falling on the balls. These screens can be best and most readily made by providing light wooden frames, about two feet square, and half an inch in thickness, and covering them on

Fig II

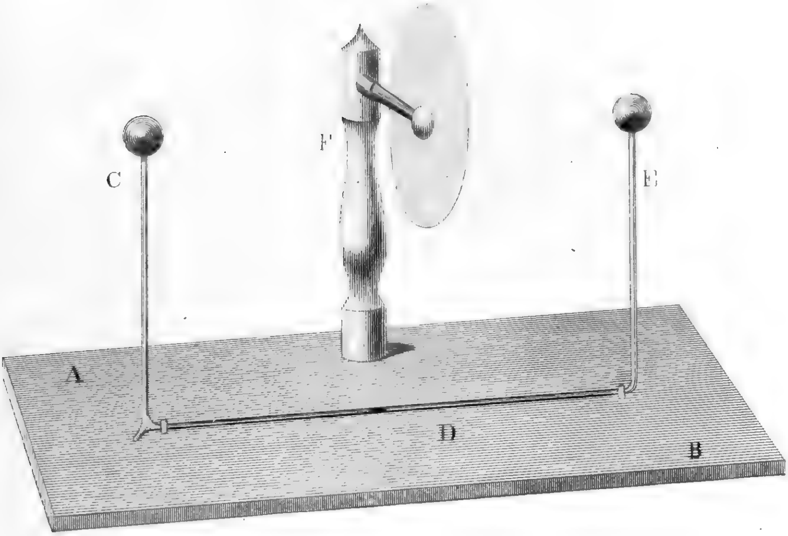
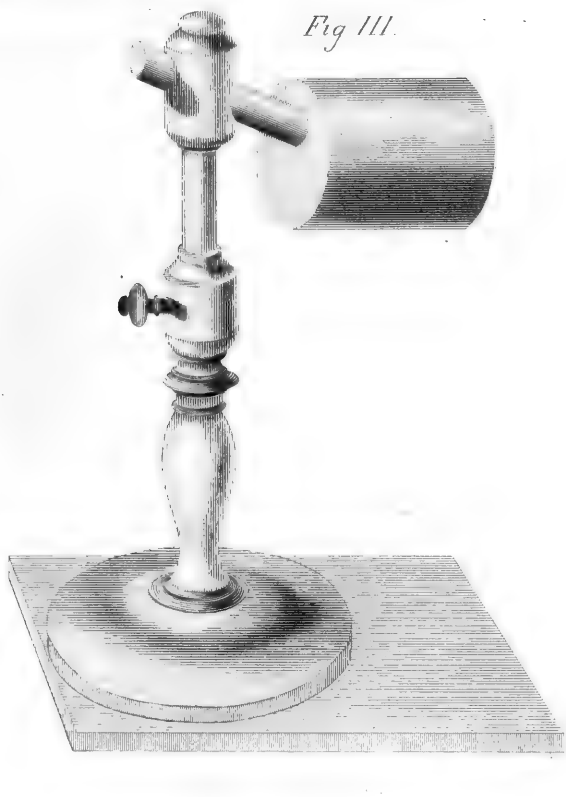


Fig III.



both sides, first with thick cartridge paper, and then with what is called gilt paper; the metallic substance (copper) with which one side of the paper is covered being on the outside.

To support a movable screen of this kind in a vertical position, it must of course be provided with a foot or stand. Those I use are fastened to one side of a pillar of wood by two screws, one of which passes through the centre of the screen where the cross-bars belonging to the frame of the screen meet, and the other through the middle of the piece of wood which forms the bottom of the screen. This pillar of wood, which is turned in a lathe, is $12\frac{1}{2}$ inches high, and is firmly fixed, at its lower end, in a piece of wood 8 inches square and 1 inch thick, which serves as a stand or foot for supporting it.

As, in making experiments with this *thermoscope*, it is frequently necessary to remove the hot bodies that are presented to it farther from it or to bring them nearer to it, in order that this may be done easily and expeditiously by one person, and without its being necessary for him to remove his eye from the bubble (which he should constantly have in his view), I make use of a simple machine, which I have found to be very useful.

It is a long and shallow wooden box, open at both ends. It is 6 feet long, 12 inches wide, and 5 inches deep, measured on the outside; its vertical sides are made of $1\frac{1}{2}$-inch deal; its bottom and top, of inch deal. A part only of the top or cover of this box is fixed down on the sides, and is immovable. The part of the cover which is fixed, and on which the thermoscope is placed, occupies the middle of the box, and is 13 inches in length. On the right and left of this fixed part, the top of the box is covered by a sliding board, 2 feet 3 inches long, which passes in deep grooves, made to receive it, in the sides of the box. A rack is fixed to the under side of each of these sliding boards; and there is a small cog wheel in the box, the axis of which passes through the sides of the box, and is furnished with a winch in the front of the box. By turning round these wheels by means of their winches (both of which can be managed by the same person, at the same time), the sliders may be moved backwards and forwards at pleasure.

In order to ascertain with facility and dispatch the distances of the hot bodies from their respective balls, the top of the front side of the wooden box is divided into inches on each side of the fixed part of the cover of the box; and there is a *nonius* belonging to each of the sliders, which is placed in such a manner as to indicate, at all times, the exact distance of the hot body from its corresponding ball.

The level of the upper surface of that part of the cover which is fixed is about $\frac{1}{8}$ of an inch higher than the level of the upper surface of the sliders, in order that, when a thermoscope longer than this fixed part is placed on it, the sliders may pass freely under its two projecting ends without deranging it.

It is evident, from this description, that by placing the thermoscope on the fixed part of the cover of the box, with its two balls in a line parallel to the axis of the box, and by placing the two hot bodies presented to the two balls of the instrument (elevated to a proper height) on stands set down on the sliders, an observer, by taking the two winches in his hands, keeping his eye fixed on the bubble, may, with the greatest facility, so regulate the distances of the hot bodies from their respective balls that the bubble shall remain immovable in its place.

In order to be able to ascertain precisely the temperatures of the hot bodies presented to this instrument, and in order that their surfaces might be equal, two equal cylindrical vessels, of thin sheet brass, with oblique cylindrical necks, were provided, of the form represented in Figure 3 (Plate II.).

This cylindrical vessel, which is placed in a horizontal position in order that its flat bottom may be presented *in a vertical position* to one of the balls of the thermoscope, is so fixed to a wooden stand, of a peculiar construction, that it may be raised or lowered at pleasure. This is necessary, in order that its axis may be in the continuation of a line passing through the centres of the two balls of the thermoscope.

This cylindrical vessel is 3 inches in diameter and 4 inches in length, and its oblique cylindrical neck is 0.86 of an inch in diameter and 3.8 inches in length.

The neck of this vessel is inserted *obliquely* into its cylindrical body, in order that the water with which it is occasionally filled may not run out of it, when the body of the vessel is laid down in a horizontal position, in the manner represented in the above-mentioned figure.

A thermometer, with a cylindrical bulb 4 inches in length, being inserted into the body of this vessel, through its neck, shows the temperature of the contained water.

Care is necessary, in constructing a thermoscope, to choose a tube of a proper diameter; if its bore be too small, it will be found very difficult to keep the spirit of wine in one mass; and if it be too large, the little horizontal column it forms (which I have called a bubble) will be ill defined at its two ends, which will render it difficult to ascertain its precise situation. After a number of trials I have found that a tube, the bore of which is of such a size that 1 inch of it in length contains about 15 or 18 grains Troy of mercury, answers best. For a tube of that size the balls may be about $1\frac{1}{2}$ inch in diameter; and they should both be painted black with Indian ink, which renders the instrument more sensible.

I have an instrument of this kind, the tube of which is quite filled with spirit of wine, excepting only the space occupied by a small bubble of air, which is introduced into the middle of the horizontal part of the tube; but it does not answer so well as those which contain only a very small quantity of that liquid, sufficient to form a small bubble.

But, without enlarging any farther, at present, on the construction of these instruments, I now proceed to give an account of the experiments for which they were contrived.

Having found abundant reason to conclude, from the results of the experiments of which an account has already been given, that all the heat which a hot body loses when it is exposed in the air to cool is not given off to the air which comes into contact with it, but that a large proportion of it escapes in rays, which do not heat the transparent air through which they pass, but, like light, generate heat only when and where they are stopped and absorbed, — I suspected that in every case when, in the foregoing experiments, the cooling of my instruments was expedited by coverings applied to their metallic surfaces, those coverings must, by some means or other, have facilitated and accelerated the emission of calorific rays from the hot surface.

Those suspicions implied, it is true, the supposition that different substances, heated to the same temperature, emit unequal quantities of calorific rays; but I saw no reason why this might not be the case in fact; and I hastened to make the following experiments, which put the matter beyond all doubt.

Experiment No. 12.—Two equal cylindrical vessels, made of sheet brass, and polished very bright, each 3 inches in diameter and 4 inches long, suspended by their oblique necks in a horizontal position (being placed on their wooden stands), were filled with water at the temperature of 180°; and their circular flat bottoms were presented in a vertical position to the two balls of the thermoscope, at the distance of 2 inches.

When the two hot bodies were presented, at the same moment, to the two balls of the instrument, or, what was still better, when two screens were placed before the two balls, at the distance of about an inch, and, after the hot bodies were placed, these screens were both removed at the same instant, the small column of spirit of wine, which I have called a *bubble*, remained immovable in its place, in the middle of the horizontal part of the tube of the instrument.

If one of the hot bodies was now brought nearer the ball to which it was presented (the other hot body remaining in its place), the bubble immediately began to move from the hot body which was advanced forward, towards the opposite ball to which the other hot body was presented.

If, instead of advancing one of the hot bodies nearer the ball to which it was presented, it was drawn backward to a greater distance from it, the action of its calorific rays on the ball was diminished by this increase of distance; and, being overcome by the action of the rays from the hot body presented to the opposite ball (at a smaller distance), the bubble was forced out of its place, and obliged to move towards the ball which had been drawn backward.

When one of the hot bodies only was presented to one of the balls, the bubble was immediately put in motion, and by bringing the hot body nearer to the ball, it might be driven quite out of the tube into the opposite ball; this, however, should never be done, because it totally deranges the instrument, as it is easy to perceive it must do.

Having, by these trials, ascertained the sensibility and the accuracy of my instrument, I now proceeded to make the following decisive experiment.

Experiment No. 13. — Having blackened the flat circular bottom of one of the cylindrical vessels by holding it over the flame of a wax candle, I now filled both vessels again with water at the temperature of 180° F., and presented them, as before, to the two opposite balls of the instrument at equal distances.

The bubble was instantly driven out of its place by the superior action of the blackened surface, and did not return to its former station till after the vessel which was blackened had been removed to more than 8 inches from the ball to which it was presented; the other vessel, which had not been blackened, remaining in its former situation, at the distance of 2 inches from its ball.

The result of this experiment appeared to me to throw a new light on the subject which had so long engaged my attention, and to present a wide and very interesting field for farther investigation.

I could now account, in a manner somewhat more satisfactory, for those appearances in the foregoing experiments which were so difficult to explain, — for the acceleration of the passage of the heat out of my instruments, which resulted from covering them with linen, varnish, &c.; and I immediately set about making a variety of new experiments, from which I conceived I should acquire a farther insight into those invisible mechanical operations which take place when bodies are heated and cooled.

Finding so great a difference in the quantities of calorific rays which are thrown off by the polished surface of a metal when exposed *naked* to the cold air and when *blackened*, I now proceeded to make experiments to ascertain whether or not all those substances with which the sides of my cylindrical vessels had been covered, and which had been found to expedite the cooling of those instruments, would also facilitate the emission of calorific rays from the surfaces of the instruments I presented to the balls of my thermoscope; and I found this to be the case in fact.

As the results of all these experiments proved, in the most decisive manner, that all the substances which, when applied to the metallic surfaces of my large cylindrical vessels, had expedited their cooling, facilitated and expedited the emission of calorific rays, I could no longer entertain any doubts respecting the agency of *radiation* in the heating and cooling of bodies. Many important points, however, still remained to be investigated before distinct and satisfactory ideas could be formed respecting the nature of those rays and the mode of their action.

I had hitherto made use of but one metal (brass) in my experiments; and that was not a simple, but a compound metal. The first subject of inquiry which presented itself, in the prosecution of these researches, was to find out whether or not similar experiments made with other metals would give similar results.

Experiment No. 14. — Procuring from a gold-beater a quantity of leaf gold and leaf silver about three times as thick as that which is commonly used by gilders, I covered the surfaces of the two large cylindrical vessels, No. 1 and No. 2, with a single coating of oil varnish; and, when it was sufficiently dry for my purpose, I gilt the instrument No. 1 with the gold leaf, and covered the other, No. 2, with silver leaf. When the varnish was perfectly dry and hard, I wiped the instruments with cotton, to remove the superfluous particles of the gold and silver, and then repeated the experiment, so often mentioned, of filling the instruments with boiling-hot water, and exposing them to cool in the air of a large quiet room.

The time of cooling through the given interval of 10 degrees was just the same as it was before, when the natural surface of these brass vessels was exposed *naked* to the air. I repeated the experiment several times, but could not find that the difference in the metals made any difference in the times of cooling.

Experiment No. 15. — Not satisfied to rest the determination of so important a point on a trial with three metals only, — brass, gold, and silver, — I now provided myself with two new instruments, — the one made of lead, and the other covered with tinned sheet-iron, improperly, in England, called tin.

As the *conducting power* of lead, with respect to heat, is much greater than that of any other metal, I conceived that, if the *radiation* of a body were any way connected with its *conducting power*, the cooling of the water contained in the leaden vessel would necessarily be either more or less rapid than in a vessel constructed of any other metal.

The result of this experiment, as also the results of several others similar to it, showed that heat is given off with the same facility, or with the same celerity, from the surfaces of all the metals.

Is not this owing to their being all equally wanting in *transparency?* And does not this afford us a strong presumption that heat is in all cases excited and communicated by means of radiations, or *undulations*, as I should rather choose to call them?

I am sensible, however, that there is another and most important question to be decided before these points can be determined; and that is, whether bodies are cooled in consequence of the rays they emit or by those they receive.

The celebrated experiment of Professor Pictet, which has often been repeated, appears to me to have put the fact beyond all doubt, that rays, or emanations, which, like light, may be concentrated by concave mirrors, proceed from cold bodies; and that these rays, when so concentrated, are capable of affecting, in a manner perfectly sensible, a delicate air thermometer.

One of the objects I had principally in view, in contriving the before-described instrument, which I have called a thermoscope, was to investigate the nature and properties of those emanations, and to find out, if possible, whether they are not of the same nature as those calorific rays which have long been known to proceed from hot bodies.

My first attempts, in these investigations, were to ascertain the existence of those emanations universally, and to discover what visible effects they might be made to produce independently of concentration by means of concave mirrors.

Experiment No. 16.—My two horizontal cylindrical vessels of sheet brass (of the same form and dimensions), having been made very clean and bright, were fixed to their stands; and, being elevated to a proper height to be presented to the balls of the thermoscope, were set down near that instrument (which was placed on a table in a large quiet room), where they were suffered to remain several hours, in order that the whole of this apparatus might acquire precisely the same temperature.

Daylight was excluded by closing the window-shutters; and, in order that the thermoscope might not be deranged by the calorific rays proceeding from the person of the observer on his entering the room to complete the intended experiments, screens were previously placed before the instrument in such a manner that its balls were completely defended from those rays.

Things having been thus prepared, I entered the room as gently as possible, in order not to put the air of the room in motion, and, approaching the thermoscope, presented first one and then the other cylindrical vessel to one of the balls of the instrument; but it was not in the least degree affected by them, the bubble of spirit of wine remaining immovably in the same place.

Experiment No. 17.—Having assured myself, by these previous trials, that the instrument was not sensibly affected by a bright metallic surface being presented to it, provided the temperature of the metal and that of the instrument were the same, I now withdrew one of the cylindrical vessels, and, taking it into another room, I filled it with pounded ice and water.

Entering the room again, I now presented the flat vertical bottom of this horizontal cylindrical vessel, filled with ice and water, to one of the balls of the thermoscope at the distance of four inches.

The bubble of spirit of wine began instantly to move with a slow, regular motion towards the cold body; and, having advanced in the tube about an inch, it remained stationary.

On bringing the cold body nearer the ball to which it was presented, the bubble was again put in motion, and advanced still farther towards the cold body.

Experiment No. 18. — Although the result of the foregoing experiment appeared to me to afford the most indisputable proof of the *radiation* of cold bodies, and that the rays which proceed from them have a power of *generating cold* in warmer bodies which are exposed to their influence, yet in a matter so extremely curious, and of such high importance to the science of heat, I was not willing to rest my inquiries on the result of a single experiment.

In order to vary the substance, or species of matter, presented cold to the instrument, and at the same time to remove all suspicion respecting the possibility of the effects observed being produced by currents of cold air occasioned in the room by the presence of the cold body, I now repeated the experiment with the following variations.

The thermoscope was laid down on one side, so that the two ends of its tube, to which its balls were attached, instead of being vertical, were now in a horizontal position; and the cold body, instead of being presented to the ball of the instrument on one side of it, and on the same horizontal level with it, was now placed *directly under it*, and at the distance of 6 inches.

This cold body, instead of being a metallic substance, was a solid cake of ice, circular, flat, and about 3 inches thick, and 8 inches in diameter. It was placed in a shallow earthen dish, about 9 inches in diameter below, 12 inches in diameter above, at its brim, and 4 inches deep. The cake of ice being laid down on the bottom of the dish, the top of the dish was covered by a circular piece of thick paper, 14 inches in diameter, which had a circular hole in its centre, just 6 inches in diameter.

This earthen dish, containing the ice, and thus covered, was placed perpendicularly under one of the balls of the thermoscope, at such a distance that the centre of the upper surface of the flat cake of ice was 6 inches below the ball.

The result of this experiment was just what might have been expected: the ice was no sooner placed under the ball of the instrument than the bubble of spirit of wine began to move towards that side where the cold body was placed; and it did not remain stationary till after it had advanced more than an inch in the tube.

Experiment No. 19. — Desirous of discovering whether the surface of a liquid emits frigorific or calorific rays, as solid bodies have been found to do, I now removed the cake of ice from the earthen dish, and replaced it with an equal mass of ice-cold water.

The result of this experiment was, to all appearance, just the same as that of the last. The bubble moved towards the cold body, and took its station in the same place where it had remained stationary before. I found reason, however, to conclude, after meditating on the subject, that although the last experiment proves, in a most decisive manner, that radiations actually proceed from the surface of *water*, yet the proof of the radiation from the surface of ice, afforded by the preceding experiment, is not equally conclusive; for, as the temperature of the air of the room in which these experiments were made was many degrees above the freezing point, it is possible, and even probable, that the surface of the ice was actually covered with a very thin, and consequently invisible, coating of water during the whole of the time the experiment lasted.

Finding reason to conclude that frigorific rays are always emitted by cold bodies, and that these emanations are very analogous to the calorific rays which hot bodies emit, I was impatient to discover whether all cold bodies, at the same temperature, emit the same quantity of rays, or whether (as I had found to be the case with respect to the calorific rays emitted by hot bodies) some substances emit more of them and some less.

With a view to the ascertaining of this important point, I made the following experiments.

Experiment No. 20. — Having found that a metallic surface, rendered quite black by holding it over the flame of a wax candle, emits a much larger quantity of calorific rays when hot, than the same metal, at the same temperature, throws off when naked, I was very curious to find out whether blackening the surface of a cold metal would or would not increase, in like manner, the quantity of frigorific rays emitted by it.

Having blackened, in the manner already described, the flat bottom, or rather end, of one of my horizontal cylindrical brass vessels with an oblique neck, I filled it with a mixture of ice and common salt; and, filling another vessel of the same kind, the bottom of which was not blackened, with the same cold mixture, I presented them both, at the same instant, and at the same distance, to the two opposite balls of my thermoscope.

The result of this experiment was perfectly conclusive: the bubble of spirit of wine began immediately to move towards the ball to which the *blackened* cold body was presented; indicating thereby that that ball was more cooled by the frigorific rays which proceeded from the blackened surface than the opposite ball was cooled by the rays which proceeded from an equal surface of naked metal, at the same temperature.

As this experiment appeared to me to be of great importance, I repeated it several times, and always with the same results; the motion of the bubble, which constituted the index of the instrument, constantly showing that the frigorific rays from the blackened surface were more powerful in generating cold than those which proceeded from the naked metal.

The bubble, it is true, did not move so far out of its place as it had done in the experiments in which hot bodies were presented to the balls; but this was not to be expected, for though I had taken pains, by mixing salt with the ice, to produce as great a degree of cold as I conveniently could, yet still the difference between the temperature of the balls and that of the bodies presented to them was much greater when the hot bodies were used than when the experiments were made with the cold bodies; and it is evident, that the distance to which the bubble is driven out of its place must necessarily be greater or less in proportion as that difference is greater or less.

In those experiments in which the horizontal cylindrical vessels were filled with hot water, and then presented to the balls of the instrument, the temperature of the circular flat surfaces was that of 180°, while the temperature of the air of the room in which those experiments were made, and consequently that of the balls, was about 60°; the difference amounts to no less than 120 degrees of Fahrenheit's scale; but, in these experiments with cold, the difference of the temperatures at the moment when the cold bodies were first presented to the instrument did not probably amount to more than 40, or at the most 50 degrees; and in a very few seconds it must have been reduced to less than 30 degrees, in consequence of the freezing of the water precipitated by the air of the atmosphere on the surface of the vessel containing the cold mixture.

This precipitation of water by the surrounding air was so copious that the brilliancy of the polish of the metallic surface was almost instantly obscured by it, and the vessels were very soon covered with a thick coat of ice. These accidents, which were not to be prevented, affected in a very sensible manner the results of the experiment. The bubble, instead of remaining stationary for some time after it had reached the point of its greatest elongation, as it had done in the experiments with hot bodies, had no sooner reached that point than it began to return back towards the place from which it had set out; and, as often as I wiped off the ice from the surface of the flat end of the vessel which was not blackened, and presented it clean and bright to the ball of the instrument, the bubble began again to move towards the opposite side, — which, by the bye, shows that ice emits a greater quantity of frigorific rays than a bright metallic surface, at the same temperature.

Having frequently observed, on presenting my hand to one of the balls of the thermoscope, that the instrument was greatly affected by the calorific rays which proceeded from it, apparently much more so than it would have been by a much hotter body of the same quantity of surface, but of a different kind of substance, placed at the same distance, I was extremely curious to find out whether *animal substances* do not emit calorific (and consequently frigorific) rays much more copiously than other substances, and whether living animal bodies do not emit them in greater abundance than dead animal matter.

The first experiment I made, with a view to the investigation of this particular point, was as simple as its result was striking and conclusive.

Experiment No. 21. — Having procured a piece of gold-beater's skin (which, as is well known, is one of the membranes that line the larger intestines in cattle, and is exceedingly thin), I moistened it with water; and, applying it, while moist, to the flat circular end of one of my horizontal cylindrical vessels, it remained firmly attached to the surface of the metal when it became dry. I now filled this vessel, and another, of equal dimensions, the end of which was not covered, with hot water (at the temperature of 180°), and presented them both, at the same moment, to the two balls of the thermoscope, and at the same distance.

The bubble of spirit of wine was immediately driven out of its place to a great distance; and did not return to its former station till after the vessel whose end was covered with gold-beater's skin had been removed to a distance from the ball to which it was presented which was *five times* greater than the distance at which the other vessel was placed from the opposite ball.

I was induced to conclude, from the result of this interesting experiment, that an animal substance emits 25 *times* more calorific rays than a polished metallic surface of the same dimensions, both substances being at the same temperature.

Experiment No. 22 — Having emptied both the vessels used in the last experiment, and refilled them with pounded ice and water, I now presented them again to the thermoscope, at equal distances from their respective balls.

The result of this experiment confirmed the conclusion I had been induced to draw from a former experiment of the same kind (No. 13), the motion of the bubble towards the vessel whose surface was covered with gold-beater's skin showing that the rays which proceeded from that animal substance were considerably more efficacious in producing cold than those which proceeded from the naked metal.

The radiation of cold bodies appearing to me to have been proved beyond all doubt by the preceding experiments, I now set about to investigate a very important point which still remained to be determined: I endeavoured to find out whether the intensity of the action of the frigorific rays which proceed from cold bodies, or their power of affecting the temperatures of other warmer bodies, *at equal intervals of temperature,* is, or is not, equal to the intensity of the action of the calorific rays which proceed from hot bodies. To ascertain this point, I made the following very simple and decisive experiment.

Experiment No. 23.—Having placed the thermoscope on a table, in the middle of a large quiet room, at the temperature of 72° F., I presented to one of its balls, at the distance of 3 inches, the flat circular end of one of the horizontal cylindrical vessels (A) above described, with an oblique cylindrical neck, this vessel being filled with pounded ice and water; and, at the same moment, an assistant presented to the opposite side of the same ball of the thermoscope, at the same distance (3 inches), the flat end of the other similar and equal cylindrical vessel (B), filled with warm water at the temperature of 112° F., the opposite ball of the thermoscope being hid and defended, by means of screens, from the actions of the bodies presented to the other ball, as also from the calorific rays which proceeded from the bodies of the persons present.

From this description it appears, that while one of the balls of the thermoscope was so defended by screens that it could not be sensibly affected by the radiations of the neighbouring bodies, the other ball was exposed to the simultaneous action of two equal bodies, at equal distances (two vertical metallic disks, 3 inches in diameter, placed on opposite sides of the ball, at the distance of 3 inches); one of these bodies being at the temperature of 32° F., or 40 degrees below that of the ball, while the other was at 112° F., or 40 degrees above the temperature of the ball.

I knew, from the results of former experiments, that this ball would, at the same time, be heated by the calorific rays from the hot body and cooled by the frigorific rays from the cold body; and I concluded that if its mean temperature should remain unchanged under the influence of these two opposite actions, that event would be a decisive proof of the equality of the intensities of those actions.

The result of the experiment showed that the intensities of those opposite actions were in fact equal; the bubble of spirit of wine, which, by its motion, would have indicated the smallest change of temperature in the ball of the thermoscope to which the hot and the cold bodies were presented, remained at rest.

On removing the cold body a little farther from the ball, — to the distance of 3½ inches, for instance, — the hot body remaining in its former station, at the distance of 3 inches, the bubble began immediately to move towards the opposite ball of the thermoscope, indicating an increase of heat in the ball exposed to the actions of the hot and the cold bodies; but, when the hot body was removed to a greater distance, the cold body remaining in its place, the bubble indicated an increase of cold.

The celerity with which the ball of the thermoscope acquired heat or cold might be estimated by the velocity with which the bubble of spirit of wine advanced or retired in its tube; but, on the most careful and attentive observation, I could not perceive that it moved faster when the ball was acquiring heat than when it was acquiring cold, provided that the hot and the cold bodies from which the calorific and frigorific rays proceeded were at the same relative distances.

From these experiments, which I lately repeated at Geneva, in the presence of Professor Pictet, Mons. de Saussure, M. Senebier, and several other persons, we may venture to conclude, that, *at equal intervals of temperature,* the rays which generate cold are just as real, and just as intense, as those which generate heat; or, that their actions are equally powerful in changing the temperatures of neighbouring bodies.

On a superficial view of this subject, it might appear extraordinary that so important a fact as that of the frigorific radiations of cold bodies should have been so long unnoticed, while the calorific radiations of hot bodies have been so well known; but, if we consider the matter with attention, our surprise will cease. Those radiations by means of which the temperatures of neighbouring bodies are gradually changed and equalized are not sensible to our feeling unless the intervals of temperature be very considerable; and the constitution of things is such, that, while we are often exposed to the influence of bodies heated several thousand degrees (as measured by the thermometer) above the mean temperature of the surface of the skin, it is very seldom that we have opportunities of experiencing the effects of the radiations of bodies much colder than ourselves, and we have no means of producing degrees of cold which bear any proportion to the intense heats excited by means of fire.

From the result of the experiment of which an account has just been given, it is evident that we should be just as much affected by the calorific rays emitted by a cannon bullet at the temperature of 160 degrees of Fahrenheit's scale (= 64 degrees above that of the blood) as by the frigorific rays of an equal bullet, ice cold, placed at the same distance; and that a bullet at the temperature of freezing mercury could not affect us much more sensibly, by its frigorific rays, than an equal bullet at the temperature of boiling water would do by its calorific rays; — but at these comparatively small intervals of temperature, the radiations of bodies are hardly sensible, and could never have been perceived, much less compared and estimated, without the assistance of instruments much more delicate than our organs of feeling. Hence we see how it happened that the frigorific radiations of cold bodies remained so long unknown. They were suspected by Bacon; but their existence was first ascertained by an experiment made at Florence towards the end of the seventeenth century. And it is not a little curious, that the learned academicians who made that experiment, and who made it with a direct view to determine the fact in question, were so completely blinded by their prejudices respecting the nature of heat that they did not believe the report of their own eyes; but, regarding the reflection and concentration of cold (which they considered as a negative quality) as *impossible*, they concluded that the indication of such reflection and concentration which they observed must necessarily have arisen from some error committed in making the experiment.

Happily for the progress of science, the matter was again taken up, about twenty years ago, by Professor Pictet; and the interesting fact, which the Florentine academicians would not discover, was put beyond all doubt. But still, this ingenious and enlightened philosopher did not consider the appearances of a reflection of cold, which he observed in his experiments, as being *real;* nor was he led by them to admit the existence of frigorific emanations from cold bodies, analogous to those calorific emanations from hot bodies which he calls radiant heat. He everywhere speaks of the reflection of cold (by metallic mirrors) as being merely *apparent;* and it is on that supposition that the explanation he has given of the phenomena is founded.

On a supposition that the *caloric* of modern chemists has any real existence, and that heat, or an increase of temperature in any body, is caused by an *accumulation* of that substance in such body, the reflection of cold would indeed be impossible; and the supposition that such an event had taken place would be absurd, and could not be admitted, however striking and convincing the appearances might be which indicated that event. But, to return from this digression: —

Having found that the intensity of the calorific rays emitted by a hot body, at any given temperature, depends much on the surface of such body, — that a polished metallic surface, for instance, throws off much fewer rays than the same surface, at the same temperature, would emit if painted, or blackened in the smoke of a lamp or candle, — I was desirous of finding out whether the frigorific rays from cold bodies are affected in the same manner, by the same means, and in the same degree.

It was to ascertain that point that the experiment No. 20 was made; and although the result of that experiment afforded abundant reason to conclude that those substances which, when hot, throw off calorific rays in the greatest abundance, actually throw off great quantities of frigorific rays when they are cold, yet, as the relative quantities of these rays could not be exactly determined by that experiment, in order to ascertain so important a fact I had recourse to the following simple contrivance.

Experiment No. 24.—Having found, by the result of the last experiment (No. 23), that the calorific emanations of a circular disk of polished brass, 3 inches in diameter, at the temperature of 112° F., were just counterbalanced by the frigorific emanations of an equal disk of the same polished metal, at the temperature of 32° F., placed opposite to it, so that one of the balls of the thermoscope placed between these two disks, at equal distances, was just as much heated by the one as it was cooled by the other, I now blackened the two disks, by holding them over the flame of a wax candle, and repeated the experiment with them so blackened.

I knew, from the results of former experiments, that the intensity of the calorific radiations from the hot disk would be very much increased, in consequence of its surface being blackened; and I was certain that, if the intensity of the frigorific radiations of the cold disk should not be increased in *exactly the same degree*, the ball of the thermoscope, exposed to the simultaneous actions of these two disks, could not possibly remain at the same constant temperature, that of 72°.

The result of the experiment was very decisive; the bubble of spirit of wine remained at rest,—which proved that the intensities of the rays emitted by the two disks still continued to be equal at the surface of the ball of the thermoscope, which, at equal distances, was exposed to their simultaneous action.

Hence we may conclude, that those circumstances which are favourable to the copious emission of calorific rays from the surfaces of hot bodies are equally favourable to a copious emission of frigorific rays from similar bodies when they are cold.

But it is time to consider these emanations in a new point of view. What difference can there be between calorific rays and frigorific rays? Are not the same rays either calorific or frigorific according as the body at whose surface they arrive is hotter or colder than that from which they proceed?

Let us suppose three equal bodies, A, B, and C, (the globular bulbs of three mercurial thermometers, for instance,) to be placed at equal distances (3 inches) in the same horizontal line; and let A be at the temperature of freezing water, B at the temperature of 72° F., and C at that of 102° F. The rays emitted by B will be *calorific* in regard to the colder body A, but in respect to the hotter body C they will be frigorific; and, from the results of the two last experiments, we have abundant reason to conclude that they will be just as efficacious in heating the former as in cooling the latter.

Before I proceed to give an account of the experiments which were made with a view to determine the relative quantities of rays emitted from the surfaces of various substances, from living animals, dead animal matter, &c. (which I must reserve for a future communication), I shall lay before the Society the results of several experiments, of various kinds, which were made with a view to the farther investigation of the radiations of hot and of cold bodies, and of the effects produced by them.

Experiment No. 25. — Having found, from the results of the experiments No. 21 and No. 22, that great quantities of rays are thrown off from the surface of the animal substance used in those experiments (gold-beater's skin), I now covered the whole of the external surface of one of my large cylindrical passage thermometers (No. 4) with that substance; and, filling it with boiling-hot water, exposed it to cool gradually in the air of a large quiet room, in the manner often described in former parts of this paper; another similar *naked* standard instrument (No. 3) being filled with hot water at the same time, and exposed to cool in the same situation.

The temperature of the air of the room being $51\frac{1}{2}°$, the instruments were found to cool through the standard interval of 10 degrees, namely, from $101\frac{1}{2}$ to $91\frac{1}{2}$, in the following times: —

No. 4, *covered* with gold-beater's skin, .	in $27\frac{3}{4}$ minutes.
No. 3, which was *naked*, . . .	in 45 "

Experiment No. 26. — Being desirous of finding out whether or not the covering of animal matter, which had so remarkably facilitated the cooling of the instrument No. 4, would be equally efficacious in facilitating the passage of heat *into* the instrument, I suffered both instruments to remain in the cold room all night; and, entering the next morning, at half an hour past seven o'clock, I found the temperature of the water in the *naked* instrument, No. 3, to be $50\frac{1}{8}°$; that in the instrument No. 4, which was covered with gold-beater's skin, was $49\frac{1}{4}°$; while the air of the room was at 48°.

At 7 h. 30 m. A. M. I removed both instruments into a warm room, and observed the times of their acquiring heat to be as expressed in the following table.

Times when the observations were made.	Observed Temperature. No. 3, *naked.*	Observed Temperature. No. 4, *covered.*	Temperature of the air of the room.
At 7 h. 30 m.	$50\frac{1}{8}°$	$49\frac{1}{4}°$	64°
7 45	$51\frac{1}{2}$	$51\frac{1}{2}$	$64\frac{1}{2}$
8 ..	$52\frac{1}{2}$	$53\frac{1}{8}$	65
8 15	$53\frac{3}{4}$	$54\frac{7}{8}$	..

Times when the observations were made.	Observed Temperature. No. 3, *naked.*	No. 4, *covered.*	Temperature of the air of the room.
At 8 h. 30m.	54⅜	56	..
8 45	55½	57⅛	..
9 ..	56¼	58½	..
9 30	57½	60	..
10 ..	58¼	61¼	..
10 30	59½	62⅛	..
11 ..	60½	63	..
11 30	61	63½	64½

The results of this experiment, and of several others similar to it, showed, in a manner which appeared to me to be perfectly conclusive, that those substances which part with heat with the greatest facility, or celerity, are those which also acquire it most readily, or with the greatest celerity.

If we might suppose that the temperatures of bodies are changed, not by the rays they *emit*, but by those they *receive* from other neighbouring bodies, this fact might easily be explained; but, without stopping to form any hypothesis for the explanation of these appearances, I shall proceed in my account of the various attempts I have made to elucidate, by new experiments, those parts of this interesting subject which still appeared to be enveloped in obscurity.

As the cooling of hot bodies is so much accelerated by covering their surfaces with such substances as emit calorific rays in great abundance, or with such as are much affected by the frigorific rays of the colder bodies by which they are surrounded, it seems to be highly probable that a comparatively small part of the heat which a body so cooled actually loses is acquired by the air; a much greater proportion of it passing off through that *transparent* fluid, under the form of calorific rays, without affecting its temperature.

If this supposition should turn out to be well founded, the knowledge of the fact would enable us to explain several interesting phenomena, and particularly that most curious process by means of which living animals preserve an equal temperature, notwithstanding the vast quantities of heat that are continually generated in the lungs, and notwithstanding the great variations which take place in the temperature of the air in which they live.

It is evident, that the greater the power is which an animal possesses of *throwing off* heat from the surface of his body, independently of that which the surrounding air takes off, the less will his temperature be affected by the occasional changes of temperature which take place in the air, and the less will he be oppressed by the intense heats of hot climates.

It is well known that *negroes* and people of colour support the heats of tropical climates much better than white people. Is it not probable that their *colour* may enable them to throw off calorific rays with great facility, and in great abundance; and that it is to this circumstance they owe the advantage they possess over white people in supporting heat? And, even should it be true, that bodies are cooled, not in consequence of the rays they emit, but by the action of those frigorific rays they receive from other colder bodies (which I much suspect to be the case), yet, as it has been found by experiment that those bodies which emit calorific rays in the greatest abundance are also most affected by the frigorific rays of colder bodies, it is evident that in a very hot country, where the air and all other surrounding bodies are but very little colder than the surface of the skin, those who by their colour are prepared and disposed to be cooled with the greatest facility will be the least likely to be oppressed by the accumulation of the heat generated in them by respiration, or of that excited by the sun's rays.

With a view to throw some light on this interesting subject, I made the following experiments.

Experiment No. 27. — Having covered the flat ends of both my horizontal cylindrical vessels with gold-beater's skin, I painted one of these coverings (of this animal substance) black, with Indian ink; and then, filling both vessels with boiling-hot water, I presented them, at equal distances, to the two opposite balls of the thermoscope.

The bubble of spirit of wine was immediately driven out of its place by the superior efficacy of the calorific rays which proceeded from the blackened animal substance.

On repeating this experiment a great number of times, and when the water in the vessels was at different degrees of temperature (the temperature being the same in the two vessels in each experiment), the results uniformly indicated that calorific rays were thrown off from the *black* surface in greater abundance than from the equal surface which was not blackened.

Although the results of these experiments appeared to me to be so perfectly conclusive as to establish the fact in question beyond all possibility of doubt, yet, in so interesting an inquiry, I was desirous, by varying my experiments, to bring, if possible, a variety of proofs to support the important conclusions which result from it.

Experiment No. 28. — Having covered the two large cylindrical vessels, No. 3 and No. 4, with gold-beater's skin, I painted one of them black, with Indian ink; and, filling them both with boiling-hot water, I exposed them to cool, in the manner already often described, in the air of a quiet room.

No. 4, which was *blackened*, cooled through the standard interval of 10 degrees in 23½ minutes; while the other, No. 3, which was not blackened, took up 28 minutes in cooling through the same interval.

In a former experiment (No. 25), the instrument No. 4, covered with gold-beater's skin, but not blackened, had taken up 27¾ minutes in cooling through the given interval, as we have before seen.

The results of these experiments do not stand in need of illustration; and I shall leave to physicans and physiologists to determine what advantages may be derived from a knowledge of the facts they establish, in taking measures for the preservation of the health of Europeans who quit their native climate to inhabit hot countries.

All I will venture to say on the subject is, that were I called to inhabit a very hot country, nothing should prevent me from making the experiment of blackening my skin, or at least of wearing a black shirt, in the shade, and especially at night; in order to find out if, by those means, I could not contrive to make myself more comfortable.

Several of the savage tribes which inhabit very cold countries besmear their skins with oil, which gives them a shining appearance. The rays of light are reflected copiously from the surface of their bodies. May not the frigorific rays, which arrive at the surface of their skin, be also reflected by the highly polished surface of the oil with which it is covered?

If that should be the case, instead of despising these poor creatures for their attachment to a useless and loathsome habit, we should be disposed to admire their ingenuity, or rather to admire and adore the goodness of their invisible Guardian and Instructor, who teaches them to like, and to practise, what he knows to be useful to them.

The Hottentots besmear themselves, and cover their bodies, in a manner still more disgusting. They think themselves *fine*, when they are besmeared and dressed out according to the loathsome custom of their country. But who knows whether they may not in fact be *more comfortable*, and better able to support the excessive heats to which they are exposed? From several experiments which I made, with a view to elucidate that point, (of which an account will be given to this Society at some future period,) I have been induced to conclude that the Hottentots derive advantages from that practice exactly similar to those which negroes derive from their black colour.

It cannot surely be supposed that I could ever think of recommending seriously to polished nations the filthy practices of these savages. That is very far indeed from being my intention, for I have ever considered cleanliness as being so indispensably necessary to comfort and happiness that we can have no real enjoyment without it; but still I think that a knowledge of the physical advantages which those savages derive from such practices may enable us to acquire the same advantages by employing more elegant means. A knowledge of the manner in which heat and cold are excited would enable us to take measures for these important purposes with perfect certainty; in the mean time, we may derive much useful information by a careful examination of the phenomena which occasionally fall under our observation.

If it be true that the black colour of a negro, by rendering him more sensible to the few frigorific rays which are to be found in a very hot country, enables him to support the great heats of tropical climates without inconvenience, it might be asked how it happens that he is able to support, naked, the direct rays of a burning sun.

Those who have seen negroes exposed naked to the sun's rays, in hot countries, must have observed that their skins, *in that situation*, are always very shining. An oil exudes from their skin, which gives it that shining appearance; and the polished surface of that oil reflects the sun's calorific rays.

If the heat be very intense, sweat makes its appearance at the surface of the skin. This watery fluid not only reflects very powerfully the calorific rays from the sun which fall on its polished surface, but also, by its evaporation, generates cold.

When the sun is gone down, the sweat disappears; the oil at the surface of the skin retires inwards; and the skin is left in a state very favourable to the admission of those feeble frigorific rays which arrive from the neighbouring objects.

But I shall refrain from pursuing these speculations any farther at present.

I shall now proceed to give an account of several experiments, of various kinds, which were made with a view to a farther investigation of the radiations of cold bodies.

Having found, by several of the foregoing experiments, that the radiations of cold bodies affected my thermoscope very sensibly, even when placed at a considerable distance from it, and in situations where currents of cold air could not be suspected to exist, I was desirous of finding out whether the cooling of a hot body would or would not be *sensibly* accelerated by those rays. To determine that point, I made the following experiment.

Experiment No. 29. — Having provided two conical vessels, made of thin sheet brass, each 4 inches in diameter at the base, and 4 inches high, ending above in a cylindrical neck, 0.88 of an inch in diameter, I enclosed each of them in a cylinder of thin pasteboard, covered with gilt paper, and then covered them up with rabbit-skins, which had the hair on them, in such a manner that no part of these vessels, except their flat bottoms, was exposed naked to the air. I then covered their bottoms with gold-beater's skin, painted black with Indian ink, in order to render them as sensible as possible to calorific and frigorific rays.

This being done, I suspended these two vessels in an erect position, or with their bottoms downwards, to the two opposite horizontal arms of a wooden stand, provided for the experiment; and I placed under each of them a pewter platter, blackened on the inside by holding it over a lighted wax candle.

Each of these platters was 12 inches in diameter, and they were supported on the top of two shallow earthen dishes, each of which was $11\frac{1}{2}$ inches in diameter at its brim; these earthen dishes being supported on circular wooden stands 10 inches in diameter.

A circular piece of thick drawing-paper, $12\frac{1}{2}$ inches in diameter, with a circular hole in its centre, just 6 inches in diameter, was placed on each of the platters, and served as a perforated cover to it.

The stands on which the platters were supported were of such a height that the upper surface of the flat bottom of each of the platters was elevated just 40 inches above the level of the floor of the room; and the horizontal arms of the wooden stand which supported the conical vessels were of such a height that the flat bottoms of these vessels (which were placed perpendicularly over the centres of the platters) were just 4 inches above the flat horizontal surface of the bottoms of the platters.

One of the platters was at the temperature of the air of the room (63° F.), but the other was kept constantly ice-cold, during the whole of the time the experiment lasted, by means of pounded ice and water, which was put into the earthen dish, over which, or rather in which, this platter was placed.

Each of the platters was just 1 inch deep, measured from the level of the top of its brim to the level of the upper surface of the flat part of its bottom; this flat part was about 8 inches in diameter.

The two conical vessels were now filled with boiling-hot water, and the times of their cooling were carefully observed.

From the above description of the apparatus used in this experiment, it is evident that the vessel which was suspended over the ice could not be reached by any streams of cold air that might be occasioned by that ice, or by the cooled sides of the vessel which contained it; for the air which, coming into contact with the sides of that vessel, was cooled by it, becoming specifically heavier than it was before, naturally descended, and spread itself out on the floor of the room ; and the perforated circular sheet of paper, which was laid down horizontally on the platter, effectually prevented any of the air so cooled from being thrown upwards against the bottom of the conical vessel (placed immediately over the platter), by any occasional undulation of the air in the room.

To preserve the air of the room in a state of perfect quietness, not only the doors and windows, but even the window-shutters of the room were kept shut ; so much light only being admitted occasionally as was necessary to observe the thermometers which were placed in the conical vessels.

In order to guard still more effectually the bottoms of the vessels which were cooling from the effects of occasional undulations in the air of the room, over each of these vessels there was drawn a cylindrical covering of very fine thin post paper, the lower open end of which projected just half an inch below the horizontal level of the flat bottom of the vessel. These cylindrical coverings of post paper were made to fit as exactly as possible the cylinders of pasteboard by which the sides of the conical vessels were covered and defended from the air ; and the warm coverings of fur (rabbit-skins) were put over all.

To confine the heat still more effectually, a quantity of eider-down had been introduced between the outside of each conical vessel and its cylindrical neck, and the inside of the hollow cylinder of pasteboard in the axis of which it was fixed and confined.

The result of this experiment was very conclusive. The conical vessel which was suspended over the *ice-cold* pewter platter cooled through the standard interval of 10 degrees (namely, from the point of 50 degrees to that of 40 degrees above the temperature of the air of the room) in 33 minutes and 42 seconds; whereas the other vessel, which was not over ice, required 39 minutes and 15 seconds to cool through the same interval.

Experiment No. 30. — On repeating this experiment the next day, the air of the room still remaining at 63°, the times of cooling through the given interval were as follows: —

	Min.	Sec.
The vessel suspended over the ice-cold platter, in .	33	15
The other vessel, in	39	30

From the results of these experiments (which were made with the greatest possible care) it appears that the radiations of cold bodies act on warmer bodies *at a distance*, and gradually diminish their temperatures.

It will likewise be evident, when we consider the matter with attention, that the cooling of the vessel which was suspended over the ice-cold platter was in fact considerably more accelerated by the frigorific radiations from that cold surface than it appears to have been when we estimate the effects produced simply by the difference of the times taken up in the cooling of the two vessels, without having regard to any other circumstance.

These times are, no doubt, inversely as the velocities of cooling; but, as all the heat lost by the vessels during the time of their cooling did not pass off through their flat bottoms, and as the rays from the cold surface fell on the *bottom only* of the vessel which was suspended over it, without at all affecting its covered sides, the velocity with which the heat made its way through the covered sides of the vessels was the same in both; consequently, more heat must have passed that way, and of course less through the bottom of the vessel, when the time of cooling was the longest, that is to say, in the vessel which was not p aced over ice.

As the cooling of these vessels is a complicated process, I will endeavour to elucidate the subject still farther.

As the two conical vessels were of the same form and dimensions, and contained equal quantities of hot water, the quantities of heat they parted with, in being cooled the same number of degrees, must of course have been equal.

Expressing that quantity by the algebraic symbol a, and putting $x =$ the quantity of heat which passed off through the covered sides of the vessel which was suspended over ice during the time it was cooling through the given interval of 10 degrees, and $y =$ the quantity which passed off through the covered sides of the other vessel during the time that vessel was coo ing through the same interval, the quantity of heat which passed off through the bottom of the vessel which was placed over ice during the time it was cooling through the given interval must have been $= a - x$, and that which passed off through the bottom of the other vessel during the time of its cooling through the same interval $= a - y$.

But, as the velocities of the heat through the covered sides of both vessels must have been equal, the quantities of heat which passed off *that way* must have been as the times of cooling.

The times of cooling in the last-mentioned experiment (No. 30) were as follows: —

	Min. Sec.	Seconds.
Of the vessel suspended over ice, . .	33 15 =	1995
Of the other vessel, ...	39 30 =	2370

x is therefore to y, as 1995 to 2370; consequently, $x = \frac{1995\,y}{2370} = 0.84177\,y$;

And, substituting for x its value $= 0.84177\,y$, the quantities of heat which passed off through the bottoms of the two vessels, in the experiment in question (No. 30), must have been $= a - 0.84177\,y$ for the vessel which was suspended over ice, and $= a - y$ fort he other vessel.

And, as y is greater than $0.84177\,y$, consequently $a - 0.84177\,y$ is greater than $a - y$, or the quantity of heat which passed off *through the bottom* of the vessel which was cooled the most rapidly was greater than that which passed off *through the bottom* of the other vessel; and hence we perceive that the effect produced by the frigorific rays from the cold surface, in the experiments in question, was *greater* than it appeared to be at first sight, when it was estimated by the times of cooling.

To determine exactly *how much* the cooling was accelerated by the presence of the cold body, it is necessary to find out how much heat actually passed off through the bottoms of the two vessels, in the experiments in question. This we will endeavour to do by comparing the results of those experiments with the results of some other experiments of a similar nature.

In the experiment No. 28, a cylindrical vessel of thin sheet brass, 4 inches in diameter, and 4 inches in height, covered with gold-beater's skin painted black with Indian ink, being filled with hot water and exposed to cool in the air of a large quiet room, cooled from the point of 50 degrees to that of 40 degrees above the temperature of the air of the room in $23\frac{1}{2}$ minutes.

The quantity of surface by which this vessel was exposed to the cold air was = 74.5581 superficial inches, exclusive of its neck, which was well covered up with fur.

The quantity of surface which was exposed to the air, in the foregoing experiments with the conical vessels, or the area of the bottom of each of the vessels, was $(4 \times 3.14159) = 12.4263$ superficial inches.

As the diameters and heights of the conical and cylindrical vessels were equal, the contents of the former must have been to the contents of the latter as 1 to 3; and the quantities of heat which they lost in cooling were as their contents.

If now the cylindrical vessel lost a quantity of heat = 3 in $23\frac{1}{2}$ minutes, it would have disposed of a quantity = 1 (equal to that which the conical vessel lost) in one third part of that time, or in 7 minutes and 50 seconds.

But the quantity of surface exposed to the air in the experiment with the cylindrical vessel was to that so exposed in the experiment with the conical vessel as 74.5581 to 12.4263, or as 6 to 1.

Now, as the time in which any given quantity of heat can pass out of any closed vessel into or through any cold fluid medium by which the vessel is surrounded must be inversely as the surface of the vessel, other things being equal, if a quantity of heat = 1 could pass out of the cylindrical vessel in 7 minutes and 50 seconds, it would require 6 times as long, or 47 minutes, to pass out of the conical vessel *through its flat bottom*, supposing no heat whatever to escape through the covered sides of that vessel.

If now the whole of the heat which the conical vessel actually lost would have required 47 minutes to have passed through the bottom of that vessel, it is evident that the quantity which actually passed through that surface, in the experiment in question (No. 30), could not have been to the whole quantity actually lost in a greater proportion than that of the times, or as $39\frac{1}{2}$ to 47.

Assuming any given number — as 10,000, for instance — to represent the whole of the heat lost in the experiment, we can now determine what part or proportion of it passed off through the bottom of the conical vessel, and consequently how much of it must have made its way through its covered sides.

If the whole quantity, = 10,000, would have required 47 minutes to have passed through the bottom of the vessel, the quantity which actually passed through that surface in $39\frac{1}{2}$ minutes could not possibly have amounted to more than 8404, $= a - y$.

For it is 47 minutes to 10,000, as $39\frac{1}{2}$ minutes to 8404. The remainder of the heat, = 10,000 — 8404 = 1396 parts, $(= y)$ must have made its way through the covered sides of the vessel.

And, if a quantity of heat = 1396 required $39\frac{1}{2}$ minutes to make its way through the covered sides of one of the conical vessels, the quantity which made its way through the covered sides of the other in $33\frac{1}{4}$ minutes could not have amounted to more than 1175 parts; and the remainder of that which was actually disposed of in the experiment = 10,000 — 1175 = 8825 $(= a - x,)$ must have passed off through the bottom of the instrument.

Hence it appears, that the quantity of heat which actually passed off through the bottom of the conical vessel which was placed over ice, in $33\frac{1}{4}$ minutes, was to that which passed off in $39\frac{1}{2}$ minutes through the bottom of the other vessel as 8825 to 8404; and consequently, that the velocity with which the heat passed through the bottom of the vessel which was exposed to the frigorific rays from the surface of the cold platter was to the velocity with which it passed through the bottom of the other vessel in the compound ratio of 8825 to 8404, and of $39\frac{1}{2}$ to $33\frac{1}{4}$; or as 10,000 to 8025, which is as 5 to 4, very nearly.

From these experiments and computations it appears that the cooling of the hot body which was placed over the ice-cold platter was sensibly, and very considerably, accelerated by the vicinity of that cold body,— may we not venture to say, by the frigorific rays which proceeded from it?

I made several other experiments similar to those just described, and with similar results; but I shall not take up the time of the Society by giving a detailed account of them. I may, perhaps, at a future time find occasion to mention some of them more particularly.

In the two last-mentioned experiments, as the conical vessels were suspended in an erect position, and had a circular band or hoop of fine post paper, by which the lower end of each of them was surrounded, and which projected downwards half an inch below the horizontal level of the bottom of the vessel, and as the air which came into immediate contact with the bottom of the vessel, and received heat from it (though it became specifically lighter than it was before), could not make its escape *upwards* into the atmosphere, being confined and prevented from moving upwards by the thin projecting hoop of paper, there is no doubt but that the time of cooling was prolonged by this arrangement; for, there being much reason to believe that the propagation of heat downwards, in air, from one particle of that fluid to another, is either quite impossible or so extremely slow as to be imperceptible, as a succession of fresh particles of cold air was prevented from coming into contact with the bottoms of the vessels, but very little heat could have been given off *immediately* to the air in those experiments.

In order to be able to form some probable conjecture respecting the quantity so given off in cases where the succession of fresh particles of air is free and uninterrupted, I made the following experiment.

Experiment No. 31. — The two conical vessels used in the last experiment (which I shall now distinguish by calling the one No. 5 and the other No. 6) being left suspended in the air to the two horizontal arms of their wooden stand, at the height of 44 inches above the floor of the room (the pewter platters, the earthen dishes, and the stands on which they were placed being removed), both the vessels were again filled with boiling hot water, and exposed to cool in the air.

The vessel No. 5 remained in a vertical position, or with its flat bottom in a horizontal position, as before; but the vessel No. 6 was now reclined, so that its axis, and consequently the plane of its flat bottom, made an angle with the plane of the horizon of 45 degrees. In this position of the vessel No. 6, it is evident that the air, heated by coming into contact with its bottom, had full liberty to escape *upwards*, and to make way for other particles of colder air to come into contact with the hot surface and be heated, rarefied, and forced upwards in their turns; and under these circumstances it might reasonably be expected that as much heat as possible would be communicated *immediately* to the air by the hot body, and that the heat so communicated would of course accelerate the cooling of that vessel.

It was in fact cooled in a shorter time than the other, No. 5, which was suspended in a vertical position; but the difference of the times of cooling was very small; which indicates, if I am not mistaken, that a comparatively small quantity of the heat a hot body loses when it is cooled in air is communicated to that fluid, much the greater part of it being sent off through the air, to a distance, in calorific rays.

The vessel No. 5 was found to cool through the standard interval of 10 degrees in 38½ minutes; and No. 6, which was in a reclined position, in 37¼ minutes.

It will no doubt be remarked that the vessel No. 5 cooled somewhat faster in this experiment than it had done in the two preceding experiments (No. 29 and No. 30), when it stood over a pewter platter which (at the beginning of the experiment at least) was at the same temperature as the air of the room.

The calorific rays from the bottom of the vessel heating the platter in some small degree, and still more, perhaps, the upper surface of the perforated sheet of paper which covered it, the frigorific rays from these bodies were, on that account, somewhat less powerful in lowering the temperature of the neighbouring hot body; and the time of its cooling was consequently a little prolonged.

In one of the preceding experiments it cooled through the given interval in 39½ minutes, and in the other in

$39\frac{1}{4}$ minutes; but in this experiment it took up only $38\frac{1}{2}$ minutes in cooling through it, as we have just seen.

Supposing now (what appears to me to be not improbable) that all, or very nearly all, the heat lost by the instrument No. 5 passed off in rays *through* the air, we can ascertain what part of the heat lost by the instrument No. 6 was communicated *to the air* which came into contact with its surface.

Putting the total quantity of heat lost by each of the instruments in cooling through the given interval = 10,000, as we have just seen that a quantity of heat = 1396 passes through the covered sides of each of these instruments in $39\frac{1}{2}$ minutes, the quantities so lost in this experiment must have been as follows: By the instrument No. 5, in $38\frac{1}{2}$ minutes, = 1081; by No. 6, in $37\frac{1}{4}$ minutes, = 1046; and, deducting these quantities so lost (through the covered sides of the instruments) from the total quantity lost by each (= 10,000), we shall find out how much heat passed off *through the bottom* of each of the instruments.

For the instrument	No. 5 it is .	10,000 — 1081 = 9919
And for "	No. 6 .	10,000 — 1046 = 9954

If now the whole of the heat lost through the bottom of the instrument No. 5 passed off *through* the air in rays, as there is no reason to suppose that a less quantity passed off in the same time, *in the same way*, through the bottom of the instrument No. 6, it appears that this last-mentioned instrument must have lost *by radiation*, or in rays which passed *through* the air, a quantity of heat = 9597.

For it is $38\frac{1}{2}$ minutes to 9919 as $37\frac{1}{4}$ minutes to 9597.

And if of the total quantity of heat which passed off through the bottom of the conical instrument No. 6, = 9954, a quantity = 9597 passed off *through* the air in calorific rays, the remainder only (9954 — 9597), which amounts to no more than 357 parts, could have been communicated to the air.

Hence it would appear that when a hot body is cooled in air $\frac{1}{27}$ part only of the heat which it loses is acquired by the air; for 357 is to 9597 as 1 to 27, very nearly. But I shall refrain from enlarging farther on this subject at present.

One of the objects which I had in view in the last experiment was to find out whether the cooling of a hot body in air is or is not sensibly accelerated or retarded by the greater or lesser distance at which the body is placed from other neighbouring solid bodies, when these neighbouring bodies are at the same temperature as the air; and, as a comparison of the result of this experiment with the results of the two preceding experiments so strongly indicated that the cooling of the conical vessel in the preceding experiments had in fact been *retarded* by the vicinity of the pewter platter over which it was suspended, I was now induced to repeat these experiments with some variations.

These investigations appeared to me to be of the more importance, as I conceived that the results of them might lead to a discovery of one of the causes of the warmth of clothing.

Experiment No. 32. — I now placed the pewter platters once more in their former stations, perpendicularly under the bottoms of the two conical vessels, but at the distance of 3 inches only; that which was under the vessel No. 5 being at the temperature of the air of the room

(62°), while that placed under the vessel No. 6 was kept ice-cold, by means of pounded ice and water, which was put into the earthen dish on the brim of which it was supported.

The times of the cooling of the vessels, through the standard interval of 10 degrees, were as follows : —

No. 5	in $40\frac{1}{4}$	minutes.
No. 6, which was over ice, .	in $33\frac{1}{4}$	"

Experiment No. 33. — I repeated this experiment once more, but varied it by bringing the pewter platters still nearer to the bottoms of the conical vessels. The flat horizontal part of each of the platters was now only 2 inches below the flat surface of the bottom of the conical vessel which was suspended over it. Both the platters still remained covered by their flat circular perforated covers of paper; but it should be remembered that the circular hole in the centre of each of these covers was no less than 6 inches in diameter, and consequently that a large portion of the flat part of the bottom of the platter was in full view (if I may use that expression) of the bottom of the vessel which was suspended over it.

The times of cooling in this experiment were as follows : —

No. 5 cooled through the given interval	in $42\frac{3}{4}$	minutes.
No. 6, which was over ice, . .	in $32\frac{1}{2}$	"

The results of these experiments show (what indeed might have been expected, especially on the supposition that the heating and cooling of bodies is effected by means of radiations) that, although the cooling of the hot body suspended over a surface kept constantly cold by artificial means was accelerated by being brought nearer to that cold surface, yet, in a case where the cold surface was less intensely cold, and where its temperature could be sensibly raised by the calorific rays from the hot body, the cooling of the hot body was retarded by a nearer approach of that cold surface.

From the results of these experiments we may safely conclude that, if the hot body, instead of being a conical vessel covered up on all sides except its flat bottom, had been a globe, and if this hot globe had been suspended in the centre of another larger thin hollow sphere (this last being, at the beginning of the experiment, at the same temperature as the air and walls of the room), the vicinity of the surface of this hollow globe to the surface of the hot body would have retarded the cooling of the hot body in the same manner as the cooling of the conical vessel No. 5 was retarded in the foregoing experiments; and if, instead of inclosing the hot body in the centre of a single hollow sphere of any given thickness, it were placed in the common centre of a number of much thinner concentric spheres, of different diameters, the time of cooling would be still more retarded.

By tracing the various operations which would take place in the cooling of the hot body in this imaginary experiment, we shall become acquainted with the nature of those which actually take place when the cooling of a hot body is prolonged by means of warm clothing.

From the results of several of the foregoing experiments we may conclude that, supposing the thin concentric hollow spheres in which the hot body is confined to be made of metal, the cooling will be slower if the surfaces of these spheres are polished than if they are unpolished or blackened; and hence we might very naturally be led to suspect (what is probably true in fact) that the *warmth* of any kind of substance used as clothing, or its power of preventing our bodies from being cooled by the influence (frigorific radiations) of surrounding colder bodies, depends very much *on the polish of its surface.*

If, with the assistance of a microscope, we examine those substances which supply us with the warmest coverings, — such, for instance, as furs, feathers, silk, &c.,— we shall find their surfaces not only smooth, but also very highly polished; we shall also find that, other circumstances being equal, those substances are the warmest which are the finest, or which are composed of the greatest number of fine polished detached threads or fibres.

The fine white shining fur of a Russian hare is much warmer than coarse hair; and fine silk, as spun by the silkworm is warmer than the same silk twisted together into coarse threads; as I found by actual experiments, an account of which has already been laid before this Society and published in the Philosophical Transactions.

I formerly considered the warmth of natural and artificial clothing as depending *principally* on the obstacle it opposes to the motions of the cold air by which the hot body is surrounded; but, by a patient and careful examination of the subject, I have been convinced that the efficacy of radiation is much greater than I had supposed it to be.

From the result of the experiment No. 31, we might be led to conclude that a very small part only of the heat which a hot body appears to lose when it is cooled in air is in fact communicated to that fluid, a much greater portion of it being communicated to other surrounding bodies at a distance; and, in one of my former experiments, a hot body was cooled, though it was placed in a Torricellian vacuum.

These researches appear to me to be the more interesting, as I have long been of opinion that it must be by experiments of this kind (showing in what manner the temperature of bodies are affected reciprocally at different degrees of temperature and at different distances) that the hypothesis of radiation must be established or proved to be unfounded.

When I speak of heat as being communicated to air *immediately* by a hot body which is cooled in it, I mean only that it is not first communicated to other neighbouring bodies, and then given *by them* to the particles of air with which they happen to be in contact. In this last-mentioned way much of the heat, no doubt, which a hot body loses when cooled in air is ultimately communicated to that fluid.

I am far from supposing that the particles of air which, coming into contact with a hot body, are heated in consequence of that near approximation receive heat in any other *manner* than that in which other bodies at a greater distance receive it. If in the one case it be generated or excited by the agency of calorific rays or undulations caused by the hot body, it must, I am persuaded, be excited in the same manner in the other.

The reason why the particle of air which is in immediate contact with a hot body is heated, while other particles near it are not affected by the calorific rays from the hot body which are continually passing by them through the air, is, I conceive, because the particle heated is at *the surface of the fluid* (air), where these rays are either reflected, refracted, or absorbed; but when a ray has once passed the surface of a transparent fluid, it proceeds straight forward, without being farther affected by it, *and consequently without affecting it,* till it comes to the confines of the medium, or to the surface of some other body.

If this hypothesis of the communication, or rather *generation*, of heat and of cold by radiation be true, it will enable us to explain, in a satisfactory manner, what has been called the *non-conducting power* of transparent fluids with respect to heat; for, if heat be really communicated or excited in the manner above described, it is quite evident that *a perfectly transparent fluid* can receive heat only at its surface, and, consequently, that heat cannot be propagated in such a fluid by communication from one particle of the fluid to another.

By a *transparent* fluid I mean such an one as admits the calorific and frigorific rays emitted by hot and by cold bodies to pass freely through it without obstructing their passage or diminishing their intensities.

Whether any of the fluids with which we are acquainted be *perfectly* transparent in this sense of the word or not, I will not pretend to say; but there is reason to think that pure water and air and most other fluids which are transparent to light, possess a high degree of transparency in regard to calorific and frigorific rays, or that they give a very free passage to them when they have once passed their surfaces.

An even or polished surface has been found to facilitate very much the reflection of the rays of light. May it not, in all cases, have an equal tendency to facilitate the reflection of calorific and frigorific rays?

In the experiments with the large cylindrical vessels, where they were exposed *naked* to cool in the air, their surfaces were polished, and they were a long time in cooling. But, when the surface of the vessel was blackened or covered with other substances, the vessel was found to cool much more rapidly.

A large proportion of the frigorific rays from the surrounding colder bodies were, in the former case, reflected at the polished surface of the metallic vessel; but, in the latter case, more of them were absorbed.

When a large drop of water rolls about without being evaporated upon the flat surface of a piece of red-hot iron, the surface of the drop is *polished;* and, the calorific rays being mostly reflected, the water is very little heated, notwithstanding the extreme intensity of the heat of the iron and its nearness to the water.

If the iron be *less hot,* the water penetrates the pores of the oxide which covers the metal, the drop ceases to have a polished surface, acquires heat very rapidly, and is soon evaporated.

If a drop of water be placed on the clean and polished surface of a metal not so easily oxidable as iron, it will retain its spherical form and polished surface under a lower degree of temperature than on iron; and consequently will be less heated, and less rapidly evaporated by a moderate heat.

If a large drop of water be put carefully into a clean silver spoon, previously heated very hot (that is to say, so hot as to give a loud hissing noise when touched with the wetted finger, but much below the heat of red-hot metal), the drop will support, or rather *resist,* this heat for a considerable time; but after the spoon has been suffered to cool down nearly to the temperature of boiling water a drop of water put into it will be evaporated instantaneously.

It appears, from the results of these experiments, to be probable that under high temperatures air is attracted by metals so much more strongly than water that even the weight of a drop of water is not sufficient to force away the stratum of air which covers and adheres to the surface of a metal on which the drop reposes; but at lower temperatures this does not seem to be the case.

The following experiment, which I made several months ago with a view to investigate the cause of the slow evaporation of drops of water placed on hot metals, will, I think, throw much light on this subject.

Experiment No. 34. — Taking a clean polished silver spoon, I blackened the inside of it by holding it over the flame of a wax candle; then, putting a large drop of water into it, I found, as I expected, that the drop took a spherical form, and rolled about in the spoon without wetting its blackened surface.

I now held the spoon over the flame of a candle, and attempted to make the water boil; but I found it to be absolutely impossible. The handle of the spoon became so very hot that I could not hold it in my hand without being burnt, though it was wrapped up in three or four thicknesses of linen; but still the drop of water did not appear to be at all affected by this intense heat. If the bowl of the spoon were touched with the finger, a hissing noise announced that it was extremely hot; but still the water remained perfectly quiet in the spoon without being evaporated.

Having in vain attempted to make this drop of water boil, and not being able to hold the spoon over the flame of the candle any longer on account of the heat of its handle, I now poured the drop into the palm of my hand. I found it to be warm, but by no means scalding hot.

By holding the spoon with a pair of tongs over the flame of the candle for a longer time, I found that a drop of water in the spoon gradually *changed its form*, became less, and was at length evaporated; from being spherical and lucid, it gradually took an oblong form, and its surface became obscure; and when it was evaporated it left a kind of skin behind it, which was evidently composed of the particles of black matter which had by degrees attached themselves to its surface, and which probably had contributed not a little to its being at last heated and evaporated.

The change in the form of the drop of water, and more especially the gradual loss of its lucid appearance, made me suspect that it had turned round during the experiment. If it really did so its motion must either have been extremely rapid or very slow, for, though I examined it with great attention, I could not perceive that it had any rotatory motion.

I will take the liberty to mention another little experiment which I have often made to amuse myself and others, though it may perhaps be thought too trifling to deserve the attention of the Royal Society.

Experiment No. 35. — If a large drop of water be formed at the end of a small splinter of light wood (deal, for instance), and this drop be thrust quickly into the centre of the flame of a newly snuffed candle, which burns bright and clear, the drop of water will remain for a considerable time in the centre of the flame and surrounded by it on every side, without being made to boil, or otherwise apparently affected by the heat; and if it be taken out of the flame and put upon the hand, it will not be found to be scalding hot.

If it be held for some time in the flame, it will be gradually diminished by evaporation; but there is much reason to think that the heat which it acquires is not communicated to it by the flame, but by the wood to which it adheres, which is soon heated by the flame, and even set on fire.

I cannot refrain from just observing that it appears to me to be extremely difficult to reconcile the results of any of the foregoing experiments with the hypothesis of modern chemists respecting the *materiality of heat.*

Deeply sensible of the insufficiency of the powers of the human mind to unfold the mysteries of nature and discover the agents she employs and their mode of action in her secret and invisible operations, and being, moreover, fully aware of the danger of forming an attachment to a false theory, and of the folly of wasting time in idle speculation, I have ever, in my philosophical researches, been much more anxious to discover new facts, and to show how the discoveries of others may be made useful to mankind, than to invent plausible theories, which much oftener tend to misguide than to lead us in the path of truth and science.

There are, however, situations in which an experimental inquirer sometimes finds himself, where it is almost impossible for him to abstain from forming or adopting some general theory for the purpose of explaining the phenomena which fall under his observation, and directing him in his future researches.

Finding myself in that situation at this time, I beg the attention and, above all, the *indulgence* of the Society while I endeavour to explain the conjectures I have formed respecting the nature of heat and the mode of its communication.

Hot and *cold*, like *fast* and *slow*, are mere relative terms; and, as there is no relation or proportion between motion and a state of rest, so there can be no relation between any degree of heat and absolute cold, or a total privation of heat; hence it is evident that all attempts to determine the place of *absolute cold*, on the scale of a thermometer, must be nugatory.

It seems probable that *motion* is an essential quality of matter, and that rest is nowhere to be found in the universe.

We well know that all those bodies which fall under the cognizance of our senses are in motion; and there are many appearances which seem to indicate that the constituent particles of all bodies are also impressed with continual motions among themselves, and that it is these motions (which are capable of augmentation and diminution) that constitute the *heat* or temperature of sensible bodies.

The only effects of which we have any idea resulting from the action of one body on another are a change of velocity or a change of direction, or both. We perceive, it is true, that certain bodies have a power of affecting certain other bodies *at a distance;* but this is no proof that the effects produced are essentially different from those which result from collision; for, if an elastic body be interposed between the two bodies, their actions on each other may be communicated through such intermediate elastic body, which, when the action is at an end, and the effects resulting from it on the two bodies have taken place, will be in the same state precisely in which it was before the action began.

If a bell or any other solid body, *perfectly elastic*, placed in a perfectly elastic fluid, and surrounded by other perfectly elastic solid bodies, were struck and made to vibrate, its vibrations would by degrees be communicated, by means of the undulations or pulsations they would occasion in the elastic fluid medium, to the other surrounding solid and elastic bodies. If these surrounding bodies should happen to be already vibrating, and with the same velocity as that with which the bell is made to vibrate by the blow, the undulations in the elastic fluid occasioned by the bell would neither increase nor diminish the velocity or frequency of the vibrations of the surrounding bodies; neither would the undulations caused by the vibrations of these bodies tend to accelerate or to retard the vibrations of the bell. But if the vibrations of the bell were more frequent than those of the surrounding bodies, the undulations it would occasion in the elastic fluid would tend to accelerate the vibrations of the surrounding bodies; on the other hand, the undulations occasioned by the slower vibrations of the surrounding bodies would retard the vibrations of the bell, and the bell and the surrounding bodies would continue to affect each other until, by the vibrations of the latter being gradually increased and those of the former diminished, in consequence of their actions on each other, they would all be reduced to the same *tone*.

Supposing now that heat be nothing more than the motions of the constituent particles of bodies among themselves (an hypothesis of ancient date, and which always appeared to me to be very probable), if for the bell we substitute a hot body, the cooling of it will be attended by a series of actions and reactions exactly similar to those just described.

The rapid undulations occasioned in the surrounding ethereal fluid, by the swift vibrations of the hot body, will act as calorific rays on the neighbouring colder solid bodies, and the slower undulations, occasioned by the vibrations of those colder bodies, will act as frigorific rays on the hot body ; and these reciprocal actions will continue, but with decreasing intensity, till the hot body and those colder bodies which surround it shall, in consequence of these actions, have acquired the same temperature, or until their vibrations have become isochronous.

According to this hypothesis, *cold* can with no more propriety be considered as the absence *of heat* than a low or grave sound can be considered as the absence of a higher or more acute note ; and the admission of rays which generate cold involves no absurdity and creates no confusion of ideas.

On a superficial view of the subject, it may perhaps appear difficult to reconcile solidity, hardness, and elasticity with those never-ceasing motions which we have supposed to exist among the constituent particles of all bodies ; but a patient investigation of the matter will show that the admission of that supposed fact, instead of rendering it more difficult to form distinct and satisfactory ideas of the causes on which those qualities of bodies depend, will rather facilitate those abstruse researches.

Judging from all the operations of nature, of the causes of which we are able to form any distinct ideas, we are certainly led to conclude that the force of dead matter (and perhaps of living matter also), or its power of affecting, that is to say, of *moving* other matter, or of *resisting its impulse,* depends on its motion.

If, therefore, solid (or fluid) bodies have any powers whatever, either of impulse or of resistance, it appears to me to be more reasonable to ascribe them to the living forces residing in them — to the never-ceasing motions of their constituent particles — than to suppose them to be derived from their want of power, and their total indifference to motion and to rest.

No reasonable objection against this hypothesis (of the incessant motions of the constituent particles of all bodies), founded on a supposition that there is not room sufficient for these motions, can be advanced; for we have abundant reason to conclude that if there be in fact any indivisible solid particles of matter (which, however, is very problematical), these particles must be so extremely small, compared to the spaces they occupy, that there must be ample room for all kinds of motions among them.

And whatever the nature or directions of these internal motions may be among the constituent particles of a solid body, as long as these constituent particles, in their motions, do not break loose from the systems to which they belong (and to which they are attached by gravitation), and run wild in the vast void by which each system is bounded (which, as long as the known laws of nature exist, is no doubt impossible), the form or external appearance of the solid cannot be sensibly changed by them.

But if the motions of the constituent particles of any solid body be either increased or diminished, in consequence of the actions or radiations of other distant bodies, this event could not happen without producing some visible change in the solid body.

If the motions of its constituent particles were *diminished* by these radiations, it seems reasonable to conclude that their elongations would become less, and consequently that the volume of the body would be contracted; but if the motions of these particles were increased, we might conclude, *a priori*, that the volume of the body would be expanded.

We have not sufficient data to enable us to form distinct ideas of the nature of the change which takes place when a solid body is melted; but as fusion is occasioned by heat, that is to say, by an augmentation (from without) of that action which occasions expansion, if expansion be occasioned by an increase of the motions of the constituent particles of the body, it is, no doubt, a certain additional increase of those motions which causes the form of the body to be changed, and from a solid to become a fluid substance.

As long as the constituent particles of a solid body which are at the surface of that body do not, in their motions, *pass by each other*, the body must necessarily retain its form or shape, however rapid those motions or vibrations may be; but as soon as the motion of these particles is so augmented that they can no longer be restrained or retained within these limits, the regular distribution of the particles which they acquired in crystallization is gradually destroyed, and the particles so detached from the solid mass form new and independent systems, and become a liquid substance.

Whatever may be the figures of the orbits which the particles of a liquid describe, the mean distances of those particles from each other remain nearly the same as when they constituted a solid, as appears by the small change of specific gravity which takes place when a solid is melted and becomes a liquid; and, on a supposition that their motions are regulated by the same laws which regulate the solar system, it is evident that the additional motion they must necessarily acquire, in order to their taking the fluid form, cannot be lost, but must continue to reside in the liquid, and must again make its appearance when the liquid changes its form and becomes a solid.

It is well known that a certain quantity of *heat* is requisite to melt a solid, which quantity disappears or remains *latent* in the liquid produced in that process; and that the same quantity of heat reappears when this liquid is congealed and becomes a solid body.

But before I proceed any farther in these abstruse speculations, I shall endeavour to investigate some of the consequences which would necessarily result from the radiations of hot and of cold bodies, supposing those radiations to exist, and their motions and actions to be regulated by certain assumed laws.

And first, it is evident that the intensity of the rays emitted by a luminous point, in a perfectly transparent medium, is everywhere as the squares of the distance from that point inversely; for the intensity of those rays must be as their condensation; and their condensation being diminished in proportion as the space they occupy is increased, if we suppose all the rays which proceed in all directions from any point to set out at the same instant and to move with the same velocity in right lines, these simultaneous rays (or undulations) will in their progress form a sphere, which sphere will increase continually in size as the rays advance; and as all the rays must be found at the surface of this sphere, their intensity or condensation must necessarily be as the surface of the sphere inversely, or as the squares of the distance inversely from the centre of the sphere, or, which is the same thing, from the luminous point from which these rays proceed; the surfaces of spheres being to each other as the squares of their radii.

Supposing now (what, indeed, appears to be incontrovertible) that the intensity of the rays which hot and cold bodies emit, in a medium perfectly transparent, follows the same law, we can determine what effects must be produced by the largeness or smallness of the confined space (of a room, for instance) in which a hot body is placed to cool.

To simplify this investigation, we will suppose this confined space to be a hollow sphere of ice 9 feet in diameter, at the temperature of freezing water; and the hot body to be a solid sphere of metal 2 inches in diameter, at the temperature of boiling water, placed in the centre of it; and we will suppose, farther, that this hollow sphere is void of air, and that the cooling of the hot body is effected solely by the frigorific rays from the ice.

The question to be determined is, in what manner the cooling of the hot body would be affected by increasing the diameter of this hollow sphere of ice.

Let us suppose its diameter to be increased to 18 feet. Its internal surface will then be to the surface of a sphere 9 feet in diameter as the square of 18 to the square of 9, that is to say, as 324 to 81, or as 4 to 1. And as the quantity of frigorific rays emitted are, *cæteris paribus*, as the surface from which they proceed, the quantity of rays emitted by the internal surface of the larger sphere will be to the quantity emitted by the internal surface of the smaller as 4 to 1.

But the intensities of these rays at the common centre of these spheres (where the hot body is placed) being as the squares of the distances from the radiating points inversely, the intensity of the rays from the internal surface of the smaller sphere must be to the intensity of the rays from the internal surface of the larger sphere as 4 to 1, at the common centre of those spheres.

Now, as the time of the cooling of the hot body will depend on the *quantity* of frigorific rays which arrive at its surface, and on the *intensity* of their action, and as the intensity of the rays from the internal surface of the sphere at its centre is diminished in the same proportion as the surface of the sphere is augmented when its diameter is increased, it follows that a hot body placed in the centre of a hollow sphere at any given constant temperature below that of the hot body, will be cooled in the same time, or with the same celerity, whatever may be the size of the sphere.

If this conclusion be well founded (and I see no reason to suspect that it is not so), it will follow, from the principles assumed, that the hot body will be cooled in the same time, in whatever part of the hollow sphere it be situated. And as the cooling of the body is not affected, that is to say, accelerated or retarded, either by the greater or smaller size of the enclosed space in which it is confined, or by its situation in that confined space, so it cannot be in any manner affected either by the form of that hollow space or by the presence of a greater or less number of other solid bodies; provided always, that all these surrounding bodies be at the same constant temperature.

If, however, any of these surrounding bodies, the temperature of which is liable to be sensibly changed during the experiment by the calorific rays emitted by the hot body, be placed *very near* that body, the cooling of that hot body will be retarded, the rays from this neighbouring body, *so heated,* being less frigorific than those from other bodies at a greater distance, which it intercepts.

The results of all my experiments on the cooling of bodies tended uniformly to confirm the above conclusions.

Admitting that the cooling of a hot body is effected solely by the rays which proceed from colder bodies, and that these rays, like those of light, are reflected, refracted, and concentrated, according to certain known laws, by the polished surfaces of mirrors and lenses, it might perhaps be imagined that the cooling of a hot body might be accelerated or retarded by giving it some peculiar form; or by placing near it, and in certain positions with respect to it, two or more highly polished reflecting mirrors.

As these conjectures, if well founded, might lead to experiments from the results of which the truth or falsehood of the hypothesis in question might be demonstrated, it is of much importance that this matter should be thoroughly investigated. I shall therefore beg the indulgence of the Society while I endeavour to examine it with that careful attention which it appears to me to deserve.

When different solid substances, heated to the same degree of temperature, are exposed in the air to cool, those among them which appear to the touch to be the hottest are not those which cool the fastest, or which send off calorific rays through the air in the greatest abundance.

As polished metals reflect a great part of the rays from other bodies which arrive at their surfaces, and as they are neither heated nor cooled by the rays so reflected, their temperatures are slowly changed by the actions of the surrounding bodies at a different temperature.

When a hot polished metallic body is exposed in the air to cool, surrounded by other bodies at the same temperature as that of the cold air, most of the rays from the surrounding bodies are reflected at the polished surface of the hot body; it is evident, then, that two sorts of rays must proceed from the surface of that body, namely, those calorific rays which it emits, and those other rays (which with regard to the surrounding bodies are neither calorific nor frigorific) which it reflects.

On a cursory view of the subject, one might be led to imagine that, as the rays which proceed from the hot metallic body are of two kinds, the energy of the calorific rays, which properly belong to the hot body, might be diminished by those other reflected rays by which they are accompanied, and with which they may be said to be mixed; but a more careful examination of the matter will show that this cannot be the case, that is to say, as long as all the surrounding bodies continue to be at the same temperature. If the temperature of the surrounding bodies be different, such of them will be affected by the reflected rays as happen to be of a temperature different from that from which the ray originated; but still the effects produced by the rays emitted by the hot body will be the same, or their power of effecting changes in the temperatures of other (hotter or colder) bodies will remain undiminished and unchanged.

The reason why their effects are not more powerful than they are found to be, is not because they are mixed with other reflected rays, but because they are few, the greater part of the rays which the hot body actually emits being reflected and turned back upon itself by the reflecting surface by which it is immediately surrounded.

The reflecting surface at which the rays of light which impinge against the polished surface of any solid or fluid body are turned back and reflected is actually situated *without the body*, and even at some distance from it; this has been proved by the most decisive experiments; and there are so many striking analogies between the rays of light and those invisible rays which all bodies at all temperatures appear to emit, that we can hardly doubt of their motions being regulated by the same laws.

Perhaps there may be no other difference between them than exists between those vibrations in the air which are audible and those which make no sensible impression on our organs of hearing.

If the ear were so constructed that we could hear all the motions which take place in the air, we should, no doubt, be stunned by the noise; and if our eyes were so constructed as to see all the rays which are emitted continually, by day and by night, by the bodies which surround us, we should be dazzled and confounded by that insupportable flood of light poured in upon us on every side.

Taking it for granted that these invisible radiations exist, we will endeavour to trace the effects which must necessarily be produced by them, and see if these investigations will not lead us to a discovery of the causes of some appearances which have hitherto been enveloped in much obscurity.

Suppose two concave reflecting mirrors, of highly polished metal, each 18 inches in diameter, and 18 inches focal distance, to be placed opposite to each other at the distance of 10 feet, in a large quiet room, in which the air and the walls of the room remain constantly at the same temperature (that of freezing water, for instance), without any variation.

If we suppose the floor, ceiling, walls of the room, and doors and windows, to be lined with a covering of ice, at the temperature of freezing water, we can then, without any difficulty, conceive that the temperature of the room may remain the same, notwithstanding the presence of hotter bodies, which are brought into it for the purpose of making experiments.

Let us now suppose one of the mirrors to be at the temperature of freezing, and the other at that of boiling water; and let us see what effects they would produce on each other by their radiations.

And first, with respect to the hot mirror, it is evident that it will be cooled, not only by the frigorific rays which proceed from the cold metal of which the opposite mirror is constructed, but also by such of the frigorific rays from the sides of the room as, impinging against the polished reflecting surface of the cold mirror, and being reflected by that surface, happen to fall on the surface of the hot mirror without being reflected by it.

But, as the quantity of rays which the cold mirror *reflects* is greater in proportion as the reflecting surface is more perfect, while the quantity of rays emitted by this cold mirror is less in proportion as its reflecting surface is more perfect, it is extremely probable that the *total* quantity of frigorific rays (emitted and reflected) which, coming from the surface of the cold mirror, impinge against the surface of the hot mirror, will be the same, whatever may be the degree of polish, or reflecting power, of the cold mirror. And, if this be the case, we may conclude that the presence of this mirror will have no effect whatever on the hot mirror; or that it will no more expedite its cooling than any other body, of any other form, would do, at the same distance and occupying the same space.

It might perhaps be imagined that the *form* of the cold mirror might concentrate the rays it emits and reflects, and, by such concentration, produce a greater effect on the opposite mirror than if its surface were flat, or of any other form; but a more attentive examination of the matter will show that no such concentration actually takes place: for, with regard to those rays which are *emitted* by this cold body, as they proceed from each point of its surface *in all directions*, it is perfectly evident that these are not concentrated; and with respect to those which are *reflected*, it is equally certain that they are not concentrated, because, in order to their being concentrated, they must arrive at the surface of the mirror in parallel lines, and in the direction of the axis of the mirror, which, under the given circumstances, is evidently impossible.

Hence we see that the presence of the cold mirror will not tend, in the smallest degree, either to accelerate or to retard the cooling of the hot mirror; that is to say, provided its temperature be not raised by the calorific rays from the hot mirror.

If its temperature be raised by those rays, it will tend to retard the cooling of the hot mirror; but, even in this case, it will not retard it more than any other polished metallic body would do, of any other form, having the same area or quantity of surface opposed to the hot mirror, and being placed at the same distance from it.

By a similar train of reasoning it may be shown that the *form* of the hot body (that of a concave mirror) will contribute nothing to the effect it will produce on the cold mirror, in heating it by the calorific rays it emits; and that it will itself be cooled neither faster nor slower on account of its peculiar form.

Let us now suppose both mirrors to be at the temperature, precisely, of the room (that of freezing water), and that a bullet, or other small body of a spherical form, at the temperature of boiling water, be placed in the focus of one of the mirrors, which mirror we shall call A.

As the rays emitted by this hot body are sent off in right lines, in all directions, in the same manner as light is emitted by luminous bodies, all those rays which fall on the concave polished surface of the mirror A will be reflected (as is well known) in lines nearly parallel to the axis of the mirror; they will consequently fall on the concave polished surface of the opposite mirror B, and, being there again reflected, they will be *concentrated* at the focus of the second mirror.

If now a sensible thermometer, at the temperature of the room, be placed in this focus, it will immediately begin to rise, in consequence of the heat generated in it by the action of these calorific rays, so accumulated in that place.

If, instead of being placed in the focus of this second mirror, the thermometer be placed at a very small distance from that focus, on one side of it, the instrument, however sensible it may be, will not be apparently affected by the rays from the hot body.

This experiment, which is of ancient date, has often been made, and always with the same results.

Let us now suppose the hot body to be removed from the focus of the mirror A, and that a colder body be substituted in place of it. And, in the first place, we will suppose the temperature of this colder body to be that of freezing water, or just equal to that which reigns in the room.

As the rays which bodies at the same temperature send off from one to the other have no tendency to increase or to diminish the temperature of those bodies, the concentration of rays in the focus of the mirror B, proceeding from the ice-cold body placed in the focus of the mirror A, can have no effect on a thermometer, at the same temperature, which is exposed to their action.

If heat be a vibratory motion of the constituent particles of bodies, and if the rays which sensible bodies send off in all directions be undulations in an ethereal elastic fluid by which they are surrounded, occasioned by those motions; as the pulsations in this fluid must be isochronous with the vibrations by which they are occasioned, these pulsations or undulations can neither accelerate nor retard the vibrations of other bodies at the surfaces of which they arrive, provided the vibrations of the constituent particles of such bodies are, at that time, isochronous with the vibrations of the constituent particles of the body from which these undulations proceed. But to return to our experiment.

Suppose now that, instead of this ice-cold body, another much colder — at the temperature of freezing mercury, for instance—be placed in the focus of the mirror A, and that a thermometer at the temperature of freezing water be placed in the focus of the mirror B; what might be expected to be the result of this experiment?—That the thermometer would fall, in consequence of its being cooled by the accumulation of frigorific rays proceeding from this very cold body.

Now this is what actually happened in the celebrated experiment of my ingenious friend, Professor Pictet, of Geneva.

Several attempts have been made to explain the result of that experiment, on the supposition that caloric has a real or material existence, and that radiant heat is that substance, emitted and sent off in right lines in all directions from the surfaces of hot bodies. But none of these explanations appear to me to be satisfactory. One of the most plausible of them is that which is founded on a supposition that caloric is emitted continually, under the form of radiant heat, by all bodies, at all temperatures, but in greater abundance by hot bodies than by such as are colder; and that a body, at the same time that it sends off radiant caloric in all directions to the bodies by which it is surrounded, receives it in return, in greater or less quantities, from all those bodies; that in all cases where a body, in any given time, receives more radiant caloric than it gives off, an accumulation of caloric in the body takes place, in consequence of which accumulation it becomes hotter, but when it gives off more caloric in any given time than it receives, its quantity of caloric is gradually diminished and it becomes colder; and that a constant temperature results from the quantities of caloric emitted and received continually being equal. But besides the difficulty of explaining how, or by what mechanism, it can be possible for the same body to receive and retain, and reject and drive away, the same kind of substance, at one and the same time (an operation not only incomprehensible, but apparently impossible, and to which there is nothing to be found analogous, to render it probable), many other reasons might be brought to show that this hypothesis of the supposed continual interchanges of caloric between neighbouring bodies is very improbable; and, among the rest, there is one which appears to me to be quite conclusive.

As the point in dispute seems to be of great importance to the science of heat, I shall endeavour to examine it with all possible attention; and, in order to put the hypothesis in question to the test, we will see if it will accord with the results of some of the foregoing experiments, which, in order to their being more easily comprehended and examined, I shall elucidate by figures.

Let the two opposite ends of the cylinders A and B (Plate III. Fig. 4) represent the two vertical metallic disks of equal dimensions, which were presented at the same time to the ball of the thermoscope C, in the experiment No. 23.

In that experiment the disk A being at the temperature of 32° F. (that of freezing water), and the disk B at 112° F., while the ball of the thermoscope C and all other surrounding bodies were at 72°, it was found that the temperature of the thermoscope was not changed by the simultaneous actions of these two bodies, the one hot and the other cold.

In order to account for this result on the hypothesis before mentioned, we must begin by supposing that the ball of the thermoscope gives off radiant caloric continually in all directions, and receives it in return from the surfaces of all the bodies by which it is surrounded.

With regard to all these surrounding bodies (excepting the disks A and B), as they are at the same temperature as the ball of the thermoscope (that of 72°), they will give continually to that instrument just as much radiant caloric as they receive from it, and no change of temperature will result from these equal interchanges.

But in respect to the disk A, as that is colder than the ball of the thermoscope, it returns to it a smaller quantity of radiant caloric than it receives from it; consequently the thermoscope receives continually less than it gives: it would of course be gradually exhausted of caloric and become colder were it not for the compensation it receives for this loss from the disk B. This disk, being hotter than the thermoscope, gives to it continually more radiant caloric than it receives from it; and were it not for the simultaneous loss of caloric which the instrument sustains in its interchanges with the cold disk A, its quantity of caloric would be augmented, and it would become hotter.

Now, as the temperature of the ball of the thermoscope is an arithmetical mean between that of the disk A and that of the disk B, it is reasonable to suppose that the thermoscope receives just as much more caloric from B than it gives to it as it gives to A more than it receives from it; and if that be the case in fact, it is evident that the simultaneous actions of the two disks on the ball of the thermoscope (or the traffic which they carry on with it in caloric) can neither tend to increase nor to diminish the original stock of that substance belonging to that instrument; consequently the instrument will neither be heated nor cooled by these interchanges, but will continue invariably at the same constant temperature.

This explanation is plausible, but, before the hypothesis on which it is founded can be admitted, we must see if it will agree with the results of other experiments, — for the greatest care ought always to be used in the admission of hypotheses in physical researches, and in no case can it be more indispensably necessary than where an hypothesis has evidently been contrived for the sole purpose of explaining a single experiment, or elucidating a new fact.

When the surface of the metallic disk B was blackened by holding it over the flame of a candle, the intensity of its radiation at the given temperature (that of 112°) was found to be very considerably increased; and when (being so blackened) it was again presented to the ball of the thermoscope at the same distance as in the last-mentioned experiment, and the cold disk A (at the temperature of 32°) was placed opposite to it at an equal distance, as represented in Fig. 5, the thermoscope, instead of continuing to retain its original temperature (that of 72°), was now gradually heated.

There is nothing, it is true, in that event, which appears difficult to explain on the assumed principles; for, if the quantity of radiant caloric emitted by the disk B be increased by blackening its surface, the quantity received from it by the ball of the thermoscope must be increased also, and that additional quantity must, of course, tend to raise the temperature of the instrument. But here is an experiment which cannot be explained on those principles.

The surface of the cold disk A having been blackened as well as that of the hot disk B, when both disks (blackened) were again presented at equal distances to the ball of the thermoscope, as represented in Fig. 6, it was found that the original temperature of the thermoscope remained unchanged.

The result of this most interesting experiment proves that the ball of the thermoscope was just as much cooled by the influence of the cold blackened disk as it was heated by the hot blackened disk.

Now, as it was found by experiment that the intensity of the radiation of the disk B was *increased* by the blackening of the surface of that disk, we must conclude that the intensity of the radiation of the disk A was likewise *increased* by the use of the same means; but if those radiations be *caloric*, emitted by those bodies (which the hypothesis in question supposes), how did it happen that the ball of the thermoscope, instead of being *more heated* by the additional quantity of caloric which it received in consequence of the blackening of the disk A, was actually *more cooled?*

It may perhaps be said by the advocates for the hypothesis in question, that the blackening of the surface of the disk A caused a greater quantity of caloric to be sent off to it by the ball of the thermoscope. Without insisting on an explanation of the mode of action of the cause which is supposed to produce this effect (which I might certainly do, as the supposition is perfectly gratuitous), I will content myself with just observing that as the surface of the opposite disk *was also blackened*, this supposed augmentation of the quantity of caloric emitted by the ball of the thermoscope, *occasioned by the blackening of the surfaces of the bodies presented to it*, can be of no use in explaining the phenomena in question.

The results of the two last mentioned experiments appear to me to be very important; and I do not see how they can be reconciled with the opinions of modern chemists respecting the nature of heat.

In order to simplify our speculations on this abstruse subject, we have hitherto supposed that *difference of temperature* depends solely on the difference *of the times* of the vibrations of the component particles of bodies. It is possible, however, and even probable, that it depends principally on the *velocities* of those particles; for it is easy to perceive that, the more rapid the motions of those particles are, the greater their elongations must be in their vibrations, and the more, of course, will the volume of the body they compose be expanded.

It is well known that the pulsations occasioned in an elastic fluid by the vibrations of an elastic solid body proceed from that body in all directions, and that these pulsations are everywhere (that is to say, at all distances from the body) isochronous with the vibrations of the solid body; it is known, also, that the mean velocity of any individual particle of the fluid is less in proportion as the distance of the particle is greater from the centre from which these pulsations proceed.

In the case of the pulsations occasioned in the air by the vibrations of sonorous bodies, those pulsations are everywhere isochronous with the vibrations of the sonorous body, and the time, or *frequency*, of these pulsations, determines the *note;* but it is the *velocity* of the particles of the air, or the breadth of the wave, on which the *force* or *strength* of the sound depends; and this velocity becoming less as the distance from the sonorous body increases, the sound is weakened in the same proportion.

There are several circumstances which might lead us to suspect that *colour* depends on the *frequency* of those pulsations which have been supposed to constitute light; and that the *heat* produced by them is in proportion to their *force*.

If this supposition should be well founded, a knowledge of that important fact might perhaps enable us to explain several very interesting phenomena, — the combustion of inflammable bodies, for instance, and the great intensity of the heat which is produced by the *concentration* of calorific rays.

There are several well-known experiments with burning-glasses which show that the intensity of the heat generated by the concentration of the solar rays is not simply as the *condensation* of those rays, but in a higher proportion; and that it depends much on their *direction*, being greater as the angle is greater at which they meet at the focus of the lens.

That fact is certainly very remarkable. It has often been the subject of my meditations, and it has contributed not a little to the opinion I have been induced to adopt respecting the nature of light and of heat. I never could reconcile it with the supposition that heat is caused by the *accumulation* of anything *emitted* by the sun, or by any other body which sends off calorific radiations.

Reserving for a future communication an account of the sequel of my inquiries respecting the subject which I have undertaken to investigate, I shall conclude this long paper with some observations concerning the *practical uses* that may be derived from a knowledge of the facts which have been established by the results of the foregoing experiments.

In all cases where it is designed to *preserve the heat* of any substance which is confined in a metallic vessel, it will greatly contribute to that end if the external surface of the vessel be very clean and bright; but if the object be to *cool* anything quickly in a metallic vessel, the external surface of the vessel should be painted, or covered with some of those substances which have been found to emit calorific rays in great abundance.

Polished tea-urns may be kept boiling hot with a much less expense of spirit of wine (burnt in a lamp under them) than such as are varnished; and the cleaner and brighter the dishes and covers for dishes are made, which are used for bringing victuals on the table, and for keeping it hot, the more effectually will they answer that purpose.

Saucepans and other kitchen utensils which are very clean and bright on the outside may be kept hot with a smaller fire than such as are black and dirty; but the bottom of a saucepan or boiler should be blackened, in order that its contents may be made to boil quickly, and with a small expense of fuel.

When kitchen utensils are used over a fire of sea-coal or of wood, there will be no necessity for blackening their bottoms, for they will soon be made black by the smoke; but when they are used over a clear fire made with charcoal, it will be advisable to blacken them, — which may be done in a few moments by holding them over a wood or coal fire, or over the flame of a lamp or candle.

Proposals have often been made for constructing the broad and shallow vessels (flats), in which brewers cool their wort, of metal, on a supposition that the process of cooling would go on faster in a metallic vessel than in a wooden vessel; but this would not be found to be the case in fact, a metallic surface being ill calculated for expediting the emission of calorific rays.

The great thickness of the timber of which brewers' flats are commonly made is a circumstance very favourable to a speedy cooling of the wort; for, when the flats are empty, this mass of wet wood is much cooled, not only by the cold air which passes over it, but also and more especially by evaporation; and when the flat is again filled with hot wort a great part of the heat of that liquid is absorbed by the cold wood.

In all cases where metallic tubes filled with steam are used for warming rooms or for heating drying-rooms, the external surface of those tubes should be painted or covered with some substance which facilitates the emission of calorific rays. A covering of thin paper will answer that purpose very well, especially if it be black, and if it be closely and firmly attached to the surface of the metal with glue.

Tubes which are designed for *conveying* hot steam from one place to another should either be well covered up with *warm* covering or should be kept clean and bright. It would, I am persuaded, be worth while, in many cases, to gild them, or at least to cover them with what is called gilt paper, or with tin foil, or some other metallic substance which does not easily tarnish in the air.

The cylinders and principal steam-tubes of steam-engines might be covered first with some warm clothing, and then with thin sheet brass kept clean and bright. The expense of this covering would, I am confident, be amply repaid by the saving of heat and fuel which would result from it.

If garden walls painted black acquire heat faster when exposed to the sun's direct rays than when they are not so painted, they will likewise cool faster during the night; and gardeners must be best able to determine whether these rapid changes of temperature are, or are not, favourable to fruit-trees.

Black clothes are well known to be very warm in the sun; but they are far from being so in the shade, and especially in cold weather. No coloured clothing is so cold as black when the temperature of the air is below that of the surface of the skin, and when the body is not exposed to the action of calorific rays from other substances.

It has been shown that the warmth of clothing depends much on the *polish* of the surface of the substance of which it is made; and hence we may conclude that, in choosing the colour of our winter garments, those dyes should be avoided which tend most to destroy that polish; and, as a white surface reflects more light than an equal surface, equally polished, of any other colour, there is much reason to think that white garments are warmer than any other in cold weather. They are universally considered as the coolest that can be worn in very hot weather, and especially when a person is exposed to the direct rays of the sun; and if they are well calculated to reflect calorific rays in summer, they must be equally well calculated to reflect those frigorific rays by which we are cooled and annoyed in winter.

I have found, by direct and decisive experiments (of which an account will hereafter be given to this Society), that garments of fur are much warmer in cold weather when worn with the fur or hair outwards than when it is turned inwards. Is not this a proof that we are kept warm by our clothing, not so much by confining our heat as by keeping off those frigorific rays which tend to cool us?

The fine fur of beasts, being a highly polished substance, is well calculated to reflect those rays which fall on it; and if the body were kept warm by the rays which proceed from it being reflected back upon it, there is reason to think that a fur garment would be warmest when worn with the hair inwards; but if it be by reflecting and turning away the frigorific rays from external (colder) bodies that we are kept warm by our clothes in cold weather, we might naturally expect that a pelisse would be warmest when worn with the hair outwards, as I have found it to be in fact.

The point here in question is by no means a matter of small importance; for until the principles of the warmth of clothing be understood, we shall not be able to take our measures with certainty, and with the least possible trouble and expense, for defending ourselves against the inclemencies of the seasons, and making ourselves comfortable in all climates.

The fur of several delicate animals becomes white in winter in cold countries, and that of the bears which inhabit the polar regions is white in all seasons. These last are exposed alternately, in the open air, to the most intense cold and to the continual action of the sun's direct rays during several months. If it should be true that heat and cold are excited in the manner above described, and that white is the colour most favourable to the reflection of calorific and frigorific rays, it must be acknowledged, even by the most determined sceptic, that these animals have been exceedingly fortunate in obtaining clothing so well adapted to their local circumstances.

The excessive cold which is known to reign, in all seasons, on the tops of very high mountains and in the higher regions of the atmosphere, and the frosts at night which so frequently take place on the surface of the plains below in very clear and still weather in spring and autumn, seem to indicate that frigorific rays arrive continually at the surface of the earth from every part of the heavens.

May it not be by the action of these rays that our planet is cooled continually, and enabled to preserve the same mean temperature for ages, notwithstanding the immense quantities of heat that are generated at its surface, by the continual action of the solar rays?

If this conjecture should be well founded, we should be led to conclude that the inhabitants of certain hot countries who sleep at night on the tops of their houses, in order to be more cool and comfortable, do wisely in choosing that situation to pass their hours of rest.

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EXPERIMENTAL INVESTIGATIONS CONCERNING HEAT

SECTION I.—*Short Account of a new Experiment on Heat.*

I HAVE lately made a new experiment, the result of which appears to me sufficiently interesting to deserve the attention of the Class.

Having found, by experiments often repeated, that metallic bodies, exposed in the free air of a large apartment, are much more speedily heated and cooled when their surfaces have been blackened (over the flame of a candle, for example) than when they are clean and polished, I was curious to know whether the same phenomena would take place when, instead of exposing these bodies in the open air, they should be placed in close metallic vessels, surrounded by a certain thickness of included air, and these vessels should be then plunged in a large mass of hot or cold water. In order to clear up this important point, I made the following experiment:—

A cylindrical vessel of brass, three inches in diameter and four inches long, was enclosed in another larger cylindrical vessel, in the centre of which it was suspended by its neck, so as to touch it in no other part, leaving on all sides an interval of one inch between the vessels.

The external vessel, as well as the smaller one included within it, is made of thin sheets of brass; its diameter is five inches, and its height six. It is one inch and a half in diameter, and six inches high. Its neck is one inch and a quarter in diameter, and two inches and a half long.

The interior vessel is suspended in the centre of the external one by a stopper of cork. This stopper is adjusted to the neck of the external vessel, and there is a cylindrical hole of three quarters of an inch diameter through the cork, and having the same axis; which perforation receives the neck of the interior vessel, and retains it in its place.

The interior vessel was introduced and fixed in its place before the bottom of the exterior vessel was soldered in.

At the centre of the bottom of the great vessel is a small metallic tube, of three quarters of an inch diameter and one inch and a half long, by means of which this instrument is attached to a solid heavy foot of metal, which supports it in a vertical position when the whole instrument is submerged in a vessel of water.

This instrument, which greatly resembles that described in my seventh Essay on the Propagation of Heat in Fluids, which I have called the *passage thermometer*,* may be used to make a number of interesting experiments on the cooling of bodies through different fluids. In the present experiment I employed it in the following manner: —

The interior vessel was entirely filled with hot water to the height of half an inch in its neck, and a good thermometer, having its cylindrical bulb four inches long, was inserted therein. The instrument was then plunged in a mixture of pounded ice and water, and

* See Vol. I. p. 237.

the time was noted by means of the thermometer, during which the hot water in the small vessel became cold.

I was careful to plunge the instrument in this frigorific mixture, so that the large vessel was completely submerged, except the upper extremity of its neck; and I added, from time to time, a sufficient quantity of pounded ice to keep the frigorific mixture constantly and throughout at the temperature of melting ice.

The following were the results afforded by two similar instruments, employed at the same time: —

These two instruments, which I shall distinguish respectively by the letters A and B, are of the same form and dimensions; there is no difference between them but in the state of their surfaces. In the instrument A the exterior surface of the small vessel and the interior surface of the great vessel which encloses it are bright and polished; but in the instrument B the exterior surface of the small vessel and the interior surface of the large vessel are black, having been blackened over the flame of a candle, before the bottom of the great vessel was soldered in its place.

Having filled the interior vessel of each of these instruments with boiling water till the water rose to the height of half an inch in the neck, I placed a thermometer in each; and then plunging both instruments at the same time into a tub filled with cold water, mixed with pounded ice, I observed the course of their refrigeration during several hours.

Each of the instruments was completely submerged in the frigorific mixture, excepting about one inch of the superior extremity of the neck of the exterior vessel, and I was careful to add new quantities of pounded ice, from time to time, in order to keep the frigorific mixture constantly at the precise temperature of melting ice.

As the specific gravity of water at the temperature of three or four degrees of the thermometer of Reaumur is greater than that of melting ice, the water which lies at the bottom of a vessel containing a mixture of water and pounded ice is usually warmer than the fluid which occupies the upper part of the vessel. To remedy this inconvenience, my refrigeratory for the frigorific mixture was a tin vessel, supported on three feet of one inch in length; and I placed this first vessel in a larger one of wood, containing a certain quantity of ice surrounding the bottom and part of the sides of the metallic vessel.

As in the first moments of the experiment the thermometers descended too quickly to be observed with precision, I waited till each of them had arrived at the 55th degree of Reaumur; after which I carefully observed the number of minutes and seconds employed in passing through each interval of five degrees of the lower part of the scale of the thermometer to the fifth degree above zero.

Degrees of the thermometer.	Time employed in cooling			
	By the instrument A.		By the instrument B.	
	m.	s.	m.	s.
From 55 to 50	11	6	7	50
" 50 " 45	13	15	8	10
" 45 " 40	15	12	9	5
" 40 " 35	19	10	10	50
" 35 " 30	22	24	12	18
" 30 " 25	27	50	15	10
" 25 " 20	37	6	21	15
" 20 " 15	54	15	28	15
" 15 " 10	80	25	41	25
" 10 " 5	183	45	85	15
Time employed in cooling from 55° to 5°,	478	4	254	5

The foregoing table exhibits the depression of the thermometers during eight hours employed in the experiment.

It is evident, from the results of this experiment, that the blackened body is constantly cooled in less time than the polished body; but it appears, by the course of the thermometers, that the difference between the quickness of cooling of these two bodies varies, and that this difference was less considerable in proportion as the temperature of the bodies was more elevated in comparison to that of the medium in which they were exposed to cool.

In cooling from the 55th degree to the 50th above the temperature of the surrounding medium, the polished body employed 11 minutes and 6 seconds, and the blackened body employed 7 minutes and 50 seconds to pass through the same interval. But from the 10th to the 15th degree above the temperature of the medium, the polished body employed 183 minutes and 45 seconds, while the blackened body employed only 85 minutes and 15 seconds; but it is extremely probable that this difference between the proportion of the times employed in cooling the two bodies at different temperatures is only apparent, and that it depends on the greater or less time required for the thermometers in the vessels to arrive at the mean temperatures of the masses of water which surround them.

In order to compare the results of this experiment with those I made last year with metallic vessels, polished and blackened, and left to cool in the undisturbed air of a large chamber, it is necessary to ascertain how much time the two bodies in question employed in cooling from the 50th to the 40th degree of Fahrenheit above the temperature of the medium. Now, I found, by observation, that the polished vessel A employed 39 minutes and 30 seconds to pass over that interval of cooling, while the blackened vessel B employed only 22 minutes. These times are in the proportion of 10,000 to 5810. By one of my experiments, made last year, I found that the times employed in passing through the same interval of cooling in the open air by a clean polished metallic vessel, and another of the same form and capacity, but blackened without, were as 10,000 to 5654.

Reflecting on the consequences which ought to result from the radiations of bodies, on the supposition that the temperatures of bodies are always changing by means of these radiations, I was led to the following conclusion: If the intensity of the action of the rays which proceed from a body be universally as the squares of the distances of bodies inversely, which is extremely probable, a hot body exposed to cool in a close place, or surrounded on all sides by walls, ought to cool with the same celerity, or in the same time, whatever may be the magnitude of this enclosure, provided the temperature of the sides or walls be at a constant given temperature; and the results of the experiment here described, in which the hot body was enclosed in a vessel of a few inches diameter, compared with those of several experiments made last year, in which the heated bodies were exposed to cool between the walls of a large chamber, appear to confirm this conclusion.

As to the effect produced by the air in cooling a heated body exposed to cool in a close place filled with that fluid, I have reason to believe that it is much less considerable than has been supposed.

I have shown, by direct and conclusive experiments, that bodies cool and are heated, and that with considerable celerity, when placed in a space void of air; * and by experiments made last year, with the intention of clearing up this point, I found reasons to conclude that when a hot body cools in tranquil air, not agitated by winds, one twenty-seventh only of the heat lost by this body (or, to speak more correctly, which it excites in surrounding bodies) is communicated to the air, all the rest being carried to a distance through the air and communicated by radiation to the surrounding solid bodies.

Section II. — *Experiments on cooling Bodies.*

It is only by careful observation of the phenomena which accompany the heating and cooling of bodies, that we can hope to acquire exact notions of the nature of heat and its manner of acting.

Many experiments have been made by different persons, at different times, with a view to determine what has been called the conducting quality of different substances with regard to heat. I have myself made a considerable number; and it is from their results, often no less unexpected than interesting, that I have been gradually led to adopt the opinions on the nature of heat which I have presumed to submit to the judgment of this illustrious assembly. The flattering attention with which the Class has honoured the three Memoirs I have lately presented encourages me to communicate the continuation of my researches.

All philosophers are agreed in considering glass as one of the worst conductors of heat which exists; and

* In my Essay on the Propagation of Heat in various Substances. See Vol. I. p. 401.

when it is proposed to confine the heat in a body, of which the temperature has been raised, or to hinder its dissipation as much as possible, care is taken to surround the heated body with substances known to be bad conductors of heat.

The results of many of my experiments having led me to suspect that the cooling of bodies is not effected in the manner which is generally supposed, I made the following experiment, with the intention of clearing up this interesting part of the science.

I procured two bottles, nearly cylindrical, of the same form and the same dimensions when measured externally, — one being of glass, and very thick, and the other of tin or tinned iron, which was very thin. Each of them is three inches ten lines in diameter, very nearly, and five inches in height; and each has a neck one inch three lines in diameter, and one inch two lines in height. The glass bottle weighs 13 ounces, 1 gros, and 18 grains poids de marc; and the other thin metallic vessel weighs only 5 ounces, 1 gros, and 65 grains.

Having very exactly weighed the bottle of tinned iron, I found its exterior surface to be 54.462 inches, which give 0.21142 of a line for the thickness of its sides, taking the specific gravity of the metal at 7.8404.

The mean thickness of the sides of the glass bottle is more than six times as great, as may be easily deduced from a calculation founded on the weight of the bottle, the quantity of its surface, and the specific gravity of glass.

Having filled these two bottles with boiling water, I hung them up by slender strings in the midst of the tranquil air of a large chamber, at the height of five feet from the floor, and at the distance of four feet asunder.

The temperature of the air of the chamber, which did not vary a quarter of a degree during the whole time of the experiment, was $9\frac{3}{4}$ degrees of Reaumur's scale.

An excellent mercurial thermometer, with a cylindrical bulb, of four inches long and two lines and a half in diameter, suspended in the axis of each of these bottles, indicated the temperature of the contained water; and the time employed in its cooling for every five degrees of Fahrenheit's thermometer was carefully observed, during eight hours.

The glass being considered as a very bad conductor of heat, and the sides of the bottle being so thick, who would not have expected that the water in this bottle would have been more slowly cooled than that in the very thin bottle of tin?

The contrary, however, was the event; the bottle of glass was cooled almost twice as quickly as that of tin.

While the water included in the bottle of tinned iron employed 56 minutes to pass through a certain interval of cooling, — namely, through ten degrees, between the 50th and 40th degree of the thermometer of Fahrenheit above the temperature of the air of the chamber, — the water in the glass bottle employed only 30 minutes for the same change.

It appears to me that the result of this experiment throws great light on the mysterious operation of the communication of heat.

If we admit the hypothesis that hot bodies are cooled, not by losing or acquiring some material substance, but by the action of colder surrounding bodies, communicated by undulations or radiations excited in an ethereal fluid, the results of this experiment may be easily explained; but, if this hypothesis be not adopted, I cannot explain them.

It might, perhaps, be suspected that the air attached by a certain attraction, but with unequal forces, to the surfaces of the two bottles, might have been the cause of this remarkable difference in the time of their cooling; but those who will take the trouble to reflect attentively on the results of the experiments I have described in a preceding Memoir, which were made with a view to clear up this point, with a metallic vessel first naked, and afterwards with one, two, four, and five coatings of varnish, will be persuaded that this cause is not sufficient to explain the facts.

By a course of experiments made at Munich, last year, of which the details are given in a Memoir sent to the Royal Society of London,* I have found that a given quantity of hot water, included in a metallic vessel of a given form and capacity, always cools with the same quickness in the air, whatever may be the metal employed to construct the vessel; provided always that the external surface of the vessel be very clean, and the temperature of the air the same.

In order that the cooling shall be effected in the same time, nothing more is required than that the external surface of the vessel be truly metallic, and not covered with oxide, or other foreign bodies.

On the inquiry, what quality all the metals might have in common, and possess in the same degree, to which this remarkable equality of their susceptibil-

* See p. 23.

ity of cooling might be attributed, I found it in their opacity.

The rays which cannot penetrate the surface of a body must necessarily be thrown back, or reflected; and as the rays of light, which have much analogy with the invisible calorific or frigorific rays, easily penetrate glass, though they are reflected, at least for the greatest part, by metallic surfaces, I suspected beforehand the result of the experiment with the two bottles, — one of glass, and the other of tinned iron.

The state of a heated body, or a body which contains a certain quantity of caloric, has been compared to that of a sponge which contains a certain quantity of water. Supposing this comparison to be just, we might compare the loss of heat by the emission of the calorific rays to the loss of water by evaporation. Let us try if this comparison can supply us with the means of throwing some light on the interesting subject of our researches.

Instead of the sponge filled with water, let us substitute the earth, and suppose, for a moment, that the earth is everywhere equally heated, and its surface, in all parts, covered with a bed of the same kind of soil, equally moist.

As a square league in a mountainous country contains more surface, or more superficial acres than a square league situated in the plain, it is evident that more water would be evaporated from the whole surface of the earth in a given time, if the earth were covered with mountains than if its surface were an immense plain, and, consequently, that more caloric ought to be projected from the surface of any solid body broken with asperities, than from the surface of another body, of the same form and dimensions, which is smooth or well polished.

This reasoning appears to me to be just, and, if I am not deceived, the conclusions which may be drawn from the facts in question, well confirmed by experiment, ought to be considered as demonstrative. I have taken every possible care to establish these facts; and the results of all my experiments have constantly shown that more or less perfect polish, or the greater or less brightness of the surface of a metallic vessel, does not sensibly influence the time of its cooling.

I took two equal vessels of brass, and polished the external surface of one of them as highly as possible; and I destroyed the polish of the other by rubbing it in all directions with coarse emery. When these two vessels were filled with hot water, I did not find that the unpolished vessel employed more or less time in cooling than that which was polished.

I was careful to wash the surface of the unpolished vessel effectually with water, before the experiment; as I knew that if I did not take the precaution of removing all the dirt which might be lodged in the asperities of the surface, the presence of these small foreign bodies would influence the result of the experiment in a sensible manner.

We ought carefully to distinguish those surfaces which appear unpolished to our eyes, but which in fact are not so, from those which reflect little or no light.

It is more than probable that the surface of a metal is always polished, and even always equally so in all the cases wherein the metal is naked and clear and clean, notwithstanding all the mechanical means which may be used to scratch its surface and break the glare of its lustre.

Let us return to the comparison of the evaporation of water from the surface of the earth, with the emission of caloric radiating from the surface of a heated body, and let us suppose, for an instant, that the evaporation of the water from the surface of the earth does not depend on the heat of the earth itself, but that it is caused merely by the influences of surrounding bodies, — as, for example, by the rays of light received from the sun. It is evident that, in this case, the evaporation could not be sensibly greater in a mountainous country than in the plain; and by an easy analogy we see that if hot bodies be cooled, not in consequence of the emission of some material substance from their surfaces, but by the positive action of rays sent to them by colder surrounding bodies, the more or less perfect polish of their surfaces ought not sensibly to influence the rapidity of their cooling.

This is precisely what all my experiments concur to prove.

I have long sought, and with that patience which the love of the sciences inspires, to reconcile the results of my experiments with the opinions generally received concerning the nature of heat and its mode of action, but without being able to succeed.

It is in the hands of two of the most illustrious bodies of learned men that ever existed that I have thought it incumbent on me to deposit my labours, my discoveries, my doubts, and my conjectures.

I am earnestly desirous of engaging the philosophers of all countries to turn their attention towards an object of inquiry too long neglected.

The science of heat is not only of great curiosity, from the multitude of astonishing phenomena it offers to our contemplation, but it is likewise extremely interesting from its intimate connection with all the useful arts, and generally with all the mechanical occupations of human life.

Without a knowledge of heat, it is not possible either to excite it with economy or to direct its different operations with facility and precision.

Section III. — *Experiments tending to show that Heat is communicated through solid Bodies, by a Law which is the same as that which would ensue from Radiation between the Particles.*

Having made a considerable number of experiments on the passage of heat through fluids, and through different substances in the state of powder, I was curious to ascertain the laws of its propagation through solid bodies, particularly metals.

I hoped this discovery would furnish some additional data to confirm or refute the opinions I had adopted concerning heat and its manner of acting; and it will be seen by the results that my expectations were not frustrated.

Having procured two cylindrical vessels of tin, each six inches in diameter and six inches high, I fastened them together, by means of a solid cylinder of copper, six inches long and an inch and a half in diameter, which was fixed horizontally between the two tin vessels. The extremities of the cylinder passed through two holes, an inch and a half in diameter, made for the purpose in the sides of the vessels, midway between the bottom and top, and were soldered fast in them.

Each of the vessels was made flat on the side where the copper cylinder was fastened, so that the extremity of the cylinder did not project into the vessel, but was level with the flattened part.

This instrument was supported at the height of eight inches and a half above the table on which it stood, by means of three feet, — two fixed to one of the vessels, and one to the other.

One of these vessels being filled with boiling water, the other with water at the freezing point, as the two extremities of the cylinder were placed in immediate contact with these two masses of fluid, a change of temperature must necessarily take place by degrees in all the interior parts of the cylinder. For the purpose of observing this change, three vertical holes were made in the cylinder, into which were introduced the bulbs of three small mercurial thermometers. One of the holes was in the middle of the cylinder, the others midway between the centre and either extremity.

Each of these holes is four lines in diameter, and eleven lines and a half deep; so that the bulbs of the thermometers, which are three lines in diameter, were all in the axis of the cylinder.

When the thermometers were put in their places, the holes were filled with mercury, in order to facilitate the communication of heat from the metal to the bulb of the thermometer.

To keep the hot water constantly boiling, a spirit-lamp was placed beneath the vessel containing it; and to keep the cold water constantly at the temperature of melting ice, fresh portions of ice were added to it, from time to time.

The thermometers are graduated to Fahrenheit's scale, the freezing point being marked 32°, and that of boiling water 212°.

As the first and most important object I had in view was to learn at what temperature the three thermometers would become stationary, I did not very carefully notice the progress of the thermometers toward this point; but as soon as they appeared nearly stationary, I observed them with the greatest attention for near half an hour.

To distinguish the three thermometers, I shall call that nearest the boiling water B, that in the centre C, and that nearest the cold water D.

The following are the progress and results of an experiment made the 28th of April, 1804, the temperature of the air being 78° of Fahrenheit.

Time.			Temperature of the hot water.	Temperature marked by the thermometer B.	Temperature marked by the thermometer C.	Temperature marked by the thermometer D.	Temperature of the cold water.
h.	m.	s.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
1	52	15	212	160	130	105	32
—	53	30	—	160½	131	105½	—
—	55		—	161	131¾	106	—
—	56	30	—	161¾	132	106½	—
—	58		—	162	132½	107	—
2	0	0	—	162	132¾	107½	—
—	1	30	—	162	133	107½	—
—	4		—	162	132¼	106½	—
—	6		—	162	132	106	—
—	9		—	162	132¾	106½	—
—	11		—	162	132¾	106½	—
—	28		—	162	132¾	106½	—

Before I proceed to examine more minutely the results of this experiment, I will endeavour to show

Fig. 1.

Fig. 2.

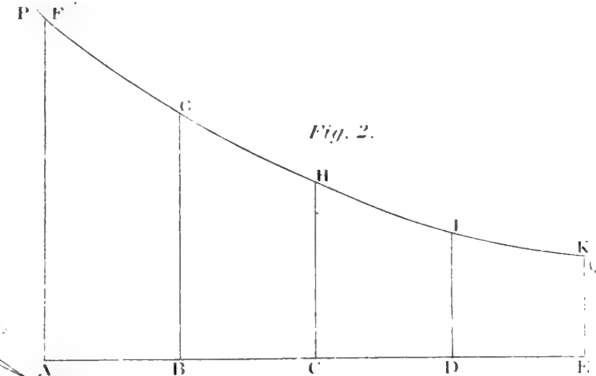


Fig. 3.

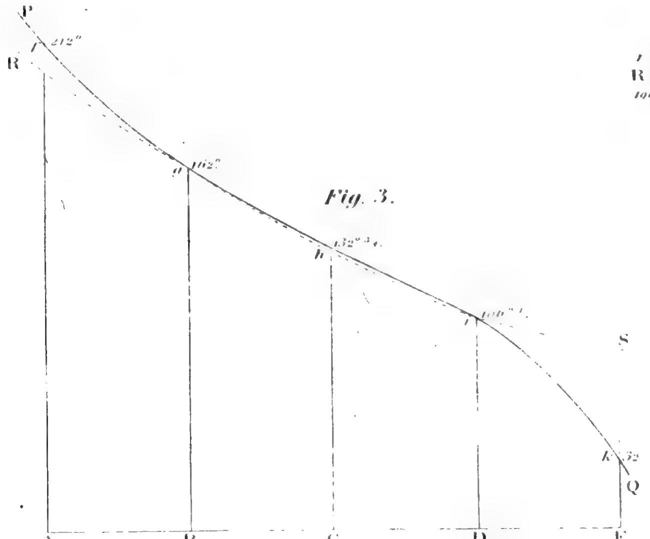
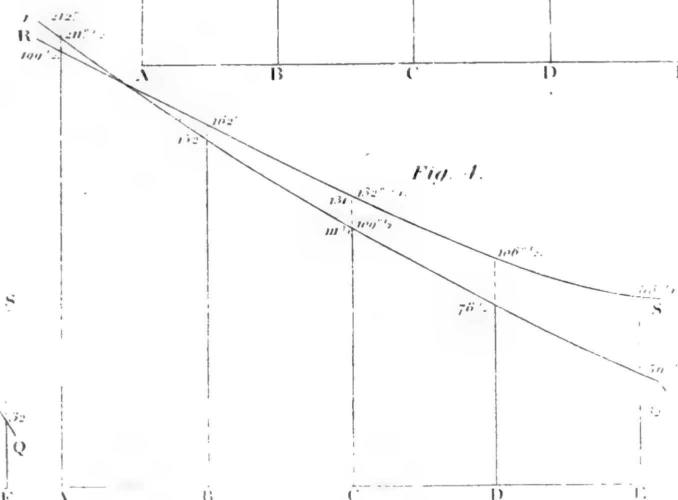


Fig. 4.



those results which it ought to have exhibited, on the supposition that heat is propagated, even in the interior of solid bodies, by *radiations* emanating from the surfaces of the particles composing these bodies.

On this supposition, we must necessarily consider the particles that compose bodies as being *separate from each other*, and even by pretty considerable distances, compared with the diameters of these particles; but there is nothing repugnant to the admission of this supposition; on the contrary, there are many phenomena which apparently indicate that all the solid bodies with which we are acquainted are thus formed.

To see now by what law heat would be propagated in a solid cylinder, let us represent the axis of this cylinder by a right line A E, Plate IV. Fig. 1; and let us begin with supposing that the cylinder consists of three particles of matter only, A C E, placed at equal distances in that line.

Let us farther suppose that the extremity, A, of the cylinder is constantly at the temperature of boiling water, while its other extremity, E, remains invariably at the freezing point.

By an experiment, of which I have already given an account to the Class,* I found that when two equal bodies, A B, one hotter than the other, are isolated and placed opposite each other, the intensities of their radiations are such, that a third body, C, placed in the middle of the space that separates them, will acquire a temperature, by the simultaneous action of these radiations, which will be an arithmetical mean between those of the two bodies A and B.

From the result of this experiment we have ground

* See the preceding paper.

to conclude that if the cylinder were composed of three particles of matter only, A, C, E, the particle C, which is in the middle of the cylinder, must necessarily have the arithmetical mean temperature between that of A and that of E, which are at the two extremities of the cylinder; that is to say, between 212° and 32° of Fahrenheit, which is 122°.

Now let us interpose between the particles A, C, and E, two other particles, B, D, and see whether the introduction of these two particles will make any change in the temperature of the particle C that occupies the middle of the cylinder.

If the particle B be placed in the middle of the space comprised between the extremity, A, of the cylinder and its middle, C, it ought to acquire a mean temperature between that of the extremity, A, of the cylinder, and that of the point C, namely that of 167°, the mean between 212° and 122°; and if the particle D be placed in the midst of the space comprised between the middle of the cylinder and its other extremity, E, this particle ought to acquire a mean temperature between that of the middle of the cylinder and that of its extremity, E; it ought then to have the temperature of 77°.

From this new arrangement, the particle C, situate in the middle of the cylinder, will find for its neighbours, on one side the particle B, at the temperature of 167°, and on the other the particle D, at that of 77°. The point in question is, whether the presence of these two particles will make any change in the temperature of the particle C, or not.

In the first place, it is evident that if the calorific influences of the particle B on the particle C be as efficacious in heating it as the frigorific influences of the particle D be in cooling it, the temperature of the particle C ought not to be changed. But experience has shown that, at equal distances and equal intervals of temperature, the calorific influences of hot bodies and the frigorific influences of cold bodies are exactly equal; and as the distance from B to C is equal to the distance from D to C, while the interval of temperature between B and C, $= 45^\circ$, is the same as that between D and C, $= 45^\circ$, it is evident that the temperature of the particle C, which is in the middle of the cylinder, can be no way affected by the introduction of the intermediate particles B and D.

By the same way of reasoning may be proved, that the introduction of an indefinite number of intermediate particles would produce no change in the temperature of the middle of the axis of the cylinder, or in any part of it; and if the introduction of an indefinite number of intermediate particles make no change in the state of a thermometer placed in the middle of the axis of the cylinder, we may conclude that the thermometer would remain equally stationary if the number of intermediate particles were increased till they had that proximity to each other which is necessary to constitute a solid body. If, instead of a single row of particles in a right line, there were a bundle composed of an indefinite number of such rows placed side by side, forming a solid cylinder, the temperature in the different parts of the line A E would remain the same.

From this reasoning we may infer that the temperatures of the different parts of the cylinder should decrease in arithmetical progression from one extremity of the cylinder to the other.

But it is evident that this law of decrement of temperature could take place only in the single case of the surface of the cylinder being completely isolated, so as to be no way affected by the action of surrounding bodies, which is absolutely impossible.

The circumstances under which the experiments were made are very different from those here taken for granted. The bodies we subject to experiment are constantly surrounded on all sides by the air and other bodies which act on our instruments continually, and often in a very perceptible manner; and we can never hope to isolate a cylinder so completely that the apparent progress of heat in its interior shall perceptibly obey the law we have just discovered. In common cases it deviates widely from this law.

As the causes of this deviation are well known, we will see whether there be no means of appreciating their effects.

The surface of the cylinder being surrounded by the atmospheric air and other bodies, all which are of a known and sensibly constant temperature, we may determine the comparative effects of these bodies on the different parts of the surface of the cylinder.

In those parts of the cylinder which are hotter than the air and other surrounding bodies, the surface of the cylinder will be cooled by the action of these bodies; but if one of the extremities of the cylinder be colder than the atmospheric air, those parts of the cylinder which are colder than the circumambient fluid will be *heated* by its influence and that of the surrounding bodies.

We will begin with examining the case where the coldest extremity of the cylinder is at the same temperature as the surrounding air. Let us suppose, then, that the experiment with boiling water at the one end and freezing at the other be made when the temperature of the air is at the freezing point, or 32° of Fahrenheit.

In this case it is evident that the surface of the cylinder must everywhere be *cooled* by the influence of the surrounding atmosphere. The question, then, is to determine the comparative effects, or the relative quantities of refrigeration or loss of heat, that must take place *in the different parts of the cylinder;* and, in the first place, it is clear that the hotter a given part of the cylinder is, the more heat it must lose in a given time, by the influence of the surrounding cold bodies; whence we may conclude that the refrigeration of the surface of the cylinder by the influence of the air and other surrounding cold bodies must necessarily diminish from the extremity of the cylinder, A, which is in contact with the hot water, to its extremity, E, which is in contact with the cold.

From reasoning which appears incontrovertible, and which the results of a great number of experiments appear to confirm, it has been concluded that the celerity with which a hot body placed in a cold medium is cooled is always proportional to the difference between the temperature of the hot body and that of the medium. Considering this conclusion as established, we may determine *a priori* what ought to be the gradation of temperatures in the interior of a given solid cylinder surrounded by air, one extremity of which is in contact with a considerable body of boiling water, while the other is similarly in contact with cold.

We have seen that, if the surface of the cylinder were perfectly isolated, the decrease of temperature from the hottest extremity of the cylinder, A, to its other extremity, E, which is in contact with cold water, would be *in arithmetical progression*, and it has just been shown that the decrease must necessarily be accelerated by the action of the air and other surrounding cold bodies.

But the acceleration of the decrease of temperature in those parts of the cylinder which are toward the cold extremity, depending on the action of the air and surrounding bodies, must be continually diminishing in proportion as the temperature of the surface of the cylinder approaches nearer and nearer that of the air; and hence we may conclude that, if a given number of points, at equal distances from each other, be taken in the axis of the cylinder, the temperatures corresponding with these points will be *in geometrical progression.*

We may represent the progress of the decrease of temperature by Plate IV. Fig. 2.

In a right line A E, representing the axis of the cylinder, if we take the three points B, C, and D, so that the distances A B, B C, C D, and D E shall be equal, and, erecting the perpendiculars A F, B G, C H, D I, EK, take A F = the temperature of the cylinder at its extremity A, B G = its temperature at the point B, and so of the rest; the ordinates A F, B G, &c. will be in geometrical progression, while their corresponding abscisses are in arithmetical progression; consequently the curve, P Q, which touches the extremities of all these ordinates, must necessarily be the *logarithmic* curve.

We will now see whether the results of experiment agree with the theory here exhibited, or not.

To form our judgment with ease and, as it were, at a single glance, of the agreement of our theory with the results of the experiment of which I gave an account at the beginning of this Memoir, we have only to represent these results by a figure in the following manner.

On the horizontal line A E, Fig. 3, representing the axis of the cylinder employed in the experiment, we will take three points, B, C, and D; one, C, in the middle of the axis, being the situation of the central thermometer, the other two, B and D, at the intermediate points which the other two thermometers occupied between the middle of the axis and its two extremities.

Erecting the perpendiculars A *f*, B *g*, C *h*, D *i*, and E *k*, on the points A, B, C, D, and E; and taking the ordinate A $f = 212$, the temperature of boiling water, B $g = 162$, the temperature indicated by the thermometer B, C $h = 132\frac{3}{4}$, the temperature indicated by the thermometer C, D $i = 106\frac{1}{2}$, the temperature given by the thermometer D, and lastly, E $k = 32$, the temperature of water mixed with pounded ice,—a curve, P Q, passing through the points *f*, *g*, *h*, *i*, *k*, ought to be the *logarithmic;* that is, supposing the temperature of the surrounding air to be constantly at the temperature of melting ice during the experiment.

But the experiment in question was made when the temperature of the air was at 78° F.; consequently, reckoning from a certain point, taken in the length of the cylinder, where the temperature was at 78°, to the extremity, E, the influence of the surrounding air, instead of *cooling* the surface of the cylinder, *heated* it; and it is evident that the curve, P Q, must necessarily in this case have a point of inflection.

In fact, it appears on a simple inspection of the figure, that the curve, P Q, has a point of inflection; but we see, likewise, that this curve is not regular. That branch which is concave toward the axis of the cylinder is not similar to the adjoining portion of the curve, of equal length, which is convex toward that axis, as it ought to be according to our theory; and even the part of the curve which is convex toward the axis, A E, differs sensibly from the logarithmic, particularly toward its extremity, P.

It ought necessarily to differ from this curve *as far as the divisions of our thermometers are defective;* but the deviation between the ordinates, A *f* and B *g*, indicated by the results of the experiment in question, appears to me much too considerable to be ascribed to the imperfection of our thermometers.

To see how far the curve, P Q, differs from the logarithmic, we have only to draw a logarithmic curve, R S, through the points *g* and *i*, and we shall find, that the ordinates corresponding to the points

	A,	B,	C,	D,	E,
	°	°	°	°	°
Instead of being	212.00	162	132¾	106½	32.00
Will be	199.55	162	131	106½	86.35
Difference	—12.45	0	—1¾	0	+54.35

The very great difference that exists between the temperature of cold water and that indicated by the results of the experiment for the extremity of the cylinder which was in contact with this water led me to suspect that it was owing to the quality possessed by water in common with other fluids, which renders it *a very bad conductor of heat.*

If it be true, as I believe I have elsewhere proved, that there is no sensible communication of heat between the adjacent particles of a fluid, from one to another, and that heat is propagated through fluids only in consequence of a motion of their particles, resulting from a change in their specific gravity, occasioned by their being heated or cooled; as the specific gravity of water is very little altered by an inconsiderable change of temperature when this fluid is near the freezing point, it might have been foreseen that a solid body a little heated, and plunged into cold water, would be very slowly cooled.

The result of the following experiment, which I made with a view to elucidate this point, will put the fact out of all doubt.

The three thermometers being stationary, one, B, at 162°, the second, C, at $132\frac{3}{4}$°, and the third, D, at $106\frac{1}{2}$°, the water in contact with one of the extremities of the cylinder being still boiling, while the water mixed with pounded ice, which was in contact with the other extremity, was constantly at the temperature of melting ice, I began to stir this mixture of ice and water pretty briskly with a little stick, and I continued to stir it uninterruptedly and with the same velocity for 22 minutes.

I had scarcely begun this operation when I had a proof that my conjectures were well founded. The mercury in the three thermometers immediately began to descend, and did not stop till it had fallen very considerably.

The thermometer B fell from 162° to 152°; C from $132\frac{3}{4}$° to $111\frac{3}{4}$°; and D from $106\frac{1}{2}$° to $78\frac{1}{2}$°.

On comparing these numbers, we find that, in consequence of the agitation of the cold water for 22 minutes, the thermometer B fell 10° of Fahrenheit's scale, the thermometer C 21°, and the thermometer D 28°.

As soon as I had ceased to stir the cold water the three thermometers began to rise, and at the end of a quarter of an hour they had all reached the points from which they set out at the beginning of this operation.

To facilitate the comparison of the results of these two experiments, — one made with cold water at rest, the other with the same water in a state of constant agitation, — I have represented them in Fig. 4.

In the first place, we shall learn several very interesting facts by simple inspection of this figure; we shall see, —

1st. That the progress of refrigeration — or, to speak more properly, *the decrease of temperature* — was everywhere much more rapid when the cold water in contact with the extremity, E, of the cylinder was agitated than when it was at rest.

2dly. That the extremity of the cylinder in contact with this water was constantly near 30° colder in the first case than in the second.

3dly. We shall see that the progress of refrigeration was everywhere, and in both the experiments, such nearly as our theory points out.

The decrease of temperature toward the middle of the cylinder was so regular that it is more than probable the apparent irregularities toward the two extremities were occasioned solely by the difficulty which a body of water finds in communicating its mean temperature to a solid with which it is in contact.

The boiling water being in continual motion, owing to its ebullition, it had a great advantage over the cold water, which was at rest, in communicating its temperature to the extremity of the cylinder it touched; but I have found, notwithstanding this, that by agitating the boiling water strongly with a quill, and particularly when with the quill I made a rapid friction against the end of the cylinder immersed in the boiling water, I occasioned all the thermometers to rise several degrees.

It may perhaps be imagined, at first sight of the results of the experiment, that as the three thermometers, which occupied the parts about the middle of the axis of the cylinder, did not indicate a decrease perfectly agreeing with the theory, the theory itself cannot be true; but a moment's reflection will show that this inference would be too hasty, and that the difference between the theory and the results of our experiments, far from proving anything adverse to the theory, serve on the contrary to render it more probable.

The results of such experiments can never agree with the theory, except the divisions of our thermometers be perfectly accurate; but it is well known to every one who has any knowledge of natural philosophy that the divisions of our thermometers are defective.

One of the objects I had in view in the experiments of which I have just given an account to the Class, and in several others which I intend to make without delay, is to improve the division of the scale of the thermometer, in order to render this valuable instrument of greater utility in the delicate investigations of natural philosophy.

It appears certain that the increase of the elasticity of air by heat is much more nearly proportionate to the increase of temperature than the dilatation of mercury or any known fluid; consequently, it is the air thermometer we ought to endeavour to improve, and which must ultimately afford us the most accurate measure of heat that it is possible for us to procure.

Section IV. — *The Heat produced in a Body by a given Quantity of solar Light is the same whether the Rays be denser or rarer, convergent, parallel, or divergent.*

In all cases where the rays of the sun strike on the surface of an opaque body without being reflected, heat is generated and the temperature of the body is increased; but is the *quantity* of heat thus excited always in proportion to the quantity of light that has disappeared? This is a very interesting question and has not hitherto found a decisive solution.

When we consider the prodigious intensity of the heat excited in the focus of a burning mirror or a lens, we are tempted to believe that the concentration and condensation of the solar rays increase their power of exciting heat; but, if we examine the matter more closely, we are obliged to confess that such an augmentation would be inexplicable. It would be equally so on both the hypotheses which natural philosophers have formed of the nature of light; for if light be analogous to sound, since it has been proved, both by calculation and experiment, that two undulations in an elastic fluid may approach and even cross each other without deranging either their respective directions or velocities, we do not see how the concentration or condensation of these undulations can increase their force of impulse; and if light be a real emanation, as its velocity is not altered either by the change of direction it undergoes in passing through a lens or by its reflection from the surface of a polished body, it seems to me that the power of each of these particles to excite or impart heat must necessarily be the same after refraction or reflection as before, and consequently, that the heat communicated or excited must be, in all cases, as the quantity of light absorbed.

I have just made some experiments which appear to me to establish this fact beyond question.

Having procured from the optician Lerebours two lenses perfectly equal, and of the same kind of glass, 4 inches in diameter, and of $11\frac{1}{2}$ focus, I exposed them at the same time to the sun, side by side, about noon, when the sky was very clear; and by means of two thermometers, or reservoirs of heat, of a peculiar construction, I determined the relative quantities of heat that were excited in given times by the solar rays at different distances from the foci of the lenses.

The two reservoirs of heat are a sort of flat boxes of brass filled with water. Each of these reservoirs is 3 inches $10\frac{1}{2}$ lines in diameter, and 6 lines thick, well polished externally on all sides except one of its two flat faces, which was blackened by the smoke of a candle. On this face the solar rays were received in the experiments.

Each of these reservoirs of heat weighs when empty 6850 grains, *poids de marc* (near a pound troy), and contains 1210 grains of water (about 2 oz. 2 dwts.).

Taking the capacity of brass for heat to be to that of water as 1 to 11, it appears that the capacity of the metallic box weighing 6850 grains is equal to the capacity of 622 grains of water; and, adding this quantity of water to that contained in the box, we shall have the capacity of the reservoir prepared for the experiments equal to that of 1832 grains of water.

Each reservoir is kept in its place by a cylinder of dry wood, one of the extremities of the cylinder being fixed in a socket in the centre of the interior face of the reservoir; and each reservoir has a little neck, through which it is filled with water, and which after receives the bulb of a cylindrical thermometer, that reaches completely across the inside of the box in the direction of its diameter.

The two reservoirs of heat, with their two lenses, are firmly fixed in an open frame, which being movable in all directions by means of a pivot and a hinge, the apparatus is easily directed toward the sun, and made to follow its motion regularly, so as to keep the solar spectra constantly in the centres of the blackened faces of the reservoirs.

In order that the quantities of light passing through the two lenses should be perfectly equal, a circular plate of well-polished brass, in the centre of which is a circular hole $3\frac{1}{2}$ inches in diameter, is placed immediately before each of the lenses.

When the reservoirs of heat are placed at different distances from the focuses of their respective lenses, the diameters of the solar spectra which are formed on the blackened faces of the reservoirs are necessarily different; and as the quantities of light are equal, its density at the surface of each reservoir is inversely as the square of the diameter of the spectrum formed on that surface.

Experiment No. 1. — In this experiment the reservoir A was placed so near the focus of the lens, between the lens and the focus, that the diameter of the solar spectrum falling on it was only half an inch, or 6 lines, while the reservoir B was advanced so far before the focus that the spectrum was 2 inches in diameter, or 24 lines.

As the quantities of light falling on both were equal, the density of the light at the surface of the reservoir

A was to the density of that at the surface of the reservoir B as the square of 24 to the square of 6, or as 16 to 1.

I imagined that if the quantity of heat which a given quantity of light is capable of exciting depended any way on its density, as the densities were so different in this experiment, I could not fail to discover the fact by the difference of time which it would require to raise the two thermometers the same number of degrees.

Having continued the experiment more than an hour, on a very fine day, when the sun was near the meridian and shone extremely bright, I did not find that one of the reservoirs was heated perceptibly quicker than the other.

Experiment No. 2. — I placed the reservoir of heat A still nearer the focus of the lens, in a situation where the solar spectrum was only $4\frac{3}{4}$ lines in diameter, and where blackened paper caught fire in two or three seconds; and I removed the reservoir B still farther from the focus, advancing it forward till the diameter of the spectrum was 2 inches 3 lines.

The densities of the light at the surfaces of the reservoirs in this experiment were as 32 to 1.

The temperature of the reservoirs as well as that of the atmosphere, at the beginning of the experiment, was $54°$ F., $= 9\frac{7}{9}°$ R.

The reservoir A, after having been exposed to the action of very intense light near the focus of the lens for 24 minutes 40 seconds, was raised to the temperature of $80°$ F., $= 21\frac{1}{3}°$ R.

The reservoir B, which was much farther from the focus of its lens, was raised to the same temperature, 80° F., a little more quickly, or in 23 minutes 40 seconds.

To raise the temperature of the reservoir A to 100° F., = $30\frac{2}{9}$° R., it was necessary to continue the experiment for 1 hour 15 minutes 10 seconds, reckoning from the commencement of it; but the reservoir B reached the same temperature in 1 hour 12 minutes 10 seconds.

The progress of this experiment from the beginning to the end is exhibited in the following table.

Increases of Temperature.	Time taken.	
	By A.	By B.
	m. s.	m. s.
From 54° to 80° F.	24 40	23 40
80 85	7 45	7 30
85 90	9 55	9 0
90 95	13 30	13 0
95 100	19 20	19 0
From 54° to 100°	75 10	72 10

This experiment was begun at 7 minutes 30 seconds after 11, and finished at 22 minutes 40 seconds after 12, the sky being perfectly clear during the time.

On comparing all the results of this experiment, we see that the reservoir A, which was placed very near the focus, was more slowly heated than the reservoir B, which was at a considerable distance from it. The differences of time, however, taken to heat them an equal number of degrees were very trifling, and I think may be easily explained without supposing the condensation of light to increase its faculty of exciting heat.

In both the preceding experiments the solar rays striking on the reservoirs of heat were *convergent*, and they were even equally so on both sides. To determine whether *parallel* rays have the same power of exciting heat as convergent rays, I made the following experiment.

Experiment No. 3. — Having removed the lens from before the reservoir B, I suffered the direct rays of the sun to fall on the blackened face of the reservoir, through the circular hole, $3\frac{1}{2}$ inches in diameter, in the round brass plate which had been constantly placed before that lens in the preceding experiments.

The reservoir A was placed behind its lens as in the former experiments, and at the place where the solar spectrum had 6 lines diameter.

Having exposed this apparatus to the sun, I found that the reservoir B, on which the direct rays fell, was heated sensibly quicker than the reservoir A, which was exposed to the action of the concentrated rays near the focus of the lens.

The temperature of the apparatus and of the atmosphere at the beginning of the experiment being $53°$ F., $= 9\frac{1}{3}°$ R, the reservoir A required 23 minutes 30 seconds to raise it to the temperature of $80°$ F., $= 21\frac{2}{9}°$ R.; but the reservoir B, which was exposed to the direct rays of the sun, acquired the same temperature in 18 minutes 30 seconds.

To reach the temperature of $100°$ F., $= 30\frac{2}{9}°$ R., took the reservoir A 1 hour and 3 minutes, but the reservoir B 47 minutes 15 seconds only.

The following table will show the progress of this experiment from the beginning to the end.

Increases of Temperature.	Time taken.			
	By A.		By B.	
	m.	s.	m.	s.
From 53° to 65° F.	8	26	7	0
65 70	4	10	3	15
70 75	5	10	3	45
75 80	5	40	4	30
80 85	7	0	4	45
85 90	7	30	5	45
90 95	10	30	8	0
95 100	13	10	10	15
100 105	20	0	14	45
From 53° to 105°	81	36	62	30

As a considerable part of the light that fell on the lens before the reservoir A was lost in passing through it, it is evident that the quantity received by this reservoir was less than that received by the reservoir B, which was exposed to the direct rays of the sun; and we have seen that the latter was heated more rapidly than the former.

As we know not exactly how much light was lost in passing through the lens, we cannot determine from the results of this experiment whether convergent rays be more or less efficacious in exciting heat than parallel rays; but the difference in the times of heating was not greater, as it appears to me, than we might have expected to find it, supposing it to be occasioned solely by the difference between the quantities of light acting on the reservoirs.

The result of the following experiment will establish this point beyond doubt.

Experiment No. 4. — Having replaced the lens belonging to the reservoir B, I adjusted this reservoir to such a distance between the lens and its focus that the solar spectrum was one inch in diameter; and I placed the reservoir A at the same distance beyond its focus.

As the quantities of light directed toward both were equal, and as the diameters of the spectra, and consequently the densities of the light that formed them were also equal, there could be no difference between the results of the experiments with the two reservoirs, except what was occasioned by the difference in the *direction* of the rays that formed the spectra. On one hand these rays were *convergent*, and on the other *divergent*; and I had inferred that if parallel rays were in reality less efficacious in exciting heat than convergent rays, as some philosophers have supposed, *divergent* rays must be still less efficacious than parallel rays, and consequently much less than convergent rays.

Having made the experiment with all possible care, I found no sensible difference between the quantities of heat excited in a given time by divergent and convergent rays.

The following are the particulars of the progress and results of this experiment.

Increases of Heat.	Time taken. By A, with divergent Rays.		Time taken. By B, with convergent Rays.	
	m.	s.	m.	s.
From 60° to 65° F.	4	50	4	50
65 70	4	55	5	0
70 75	5	27	5	25
75 80	6	13	6	15
From 60° to 80°	21	25	21	30

From the results of all the experiments of which I have just given an account to the Class, we may conclude that the quantity of heat excited or communicated by the solar rays is always, and under all circumstances, as the quantity of light that disappears.

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REFLECTIONS ON HEAT.

THE most excellent gift which man has received from the Author of his being is the power which he possesses of freeing himself from the prejudices arising from the deceptive testimony of his senses, and of penetrating into the mysteries of Nature.

The animals see as we do, without doubt, that the sun, moon, and stars rise and set; man in a state of nature, when his attention is aroused, discovers irregularities in these movements; the man of genius, however, does not allow himself to be deceived by appearances, but causes to come forth from this confusion that vast and wonderful system of laws which govern the mechanism of the Universe.

The first step in science is to observe facts attentively, and in their proper connection; the second is to learn to doubt. The sublime in science consists in employing it to extend the power and increase the innocent enjoyments of the human race.

There is no branch of the physical sciences which is so intimately connected with all the every-day occupations of man as that of *Heat*, and consequently there is no one of them which interests him so closely.

Fire is the most universal and active agent with which we are acquainted, and it is to the power which he has been able to acquire over this wonderful principle that man owes the supernatural strength which has made him superior to all animals, and master of land and sea.

It is not at all surprising that an agent at once so powerful and so manageable, so beneficent and so terrible, should have become an object of admiration and even of adoration among the nations of the earth; but it is more than surprising that a subject, the investigation of which is of such interest, should have been for so long a time neglected.

This indifference to an object at once so curious and so interesting can only be attributed to that lack of attention with which men always regard those things that they are accustomed to have before them at all times.

A proof that our knowledge on the subject of heat is still extremely limited and imperfect lies in the difference of opinion which exists among the learned on the nature of heat and its mode of action. Some regard it as a *substance*, others as a *vibratory motion* of the particles of matter of which a body is composed.

Those who have adopted the hypothesis of a peculiar calorific substance which they call *caloric* suppose that the heating of a body is always the result of an *accumulation* of this substance in the body; on the other hand, those who regard heat as a vibratory motion which is conceived to exist always with greater or less rapidity among the particles of all bodies, consider heat as an *acceleration* of this motion.

On the hypothesis of vibratory motion, a body which has become cold is thought to have lost nothing except motion; on the other hypothesis it is supposed to have lost some material substance, that is, caloric.

The eminent French philosophers, who proposed, twenty-five years ago, the modern hypothesis of caloric, far from considering the existence of this substance as proved, always speak of it with that modest reserve which characterizes men of superior excellence. They propose the word in order to avoid circumlocutions and to render the language of science more concise, rather than to introduce a new opinion.

One of these philosophers, whom science and mankind still mourn, thus expresses himself in his admirable *Traité Elémentaire de Chimie:* "In the labours which M. de Morveau, M. Berthollet, M. de Fourcroy, and myself have performed in common on the reform of the language of chemistry, we have felt that we ought to banish from it those circumlocutions which render the form of expression longer and more cumbersome, less exact, and less clear, and which not seldom even do not allow of ideas sufficiently well defined. We have, therefore, designated the cause of heat, the eminently elastic fluid which produces it, by the name *caloric.* Independently of the fact that this expression answers our purpose in the system which we have adopted, it has besides another advantage, that of being able to adapt itself to all sorts of opinions, since, strictly speaking, we are not even obliged to suppose that caloric is a material substance."

If the point in question, the existence or non-existence of caloric, were less important we might be content with leaving it undecided; but the use of heat is so general, and the art of exciting and directing it is so intimately connected with the perfecting of all the mechanical arts and with a great number of domestic applications, that we cannot take too much trouble in becoming acquainted with it.

Without entering into the details of the various experiments which have been performed in order to determine the nature of heat, I will limit myself in this memoir to some of the principal results of these researches.

A very remarkable phenomenon, and one which must have been noticed as soon as men had any acquaintance with fire, is the radiation from solid bodies as soon as they become very warm.

When a solid body — a bar of iron, for example — is at about the temperature of the surrounding air, we do not see or perceive anything which indicates that it possesses a radiating surface; but if we heat it strongly in the glowing fire of a forge, the body changes color, becomes at first red, then white, is visible in the dark, lights up surrounding objects, and warms in a sensible degree all objects which are struck by the rays which it emits in all directions.

If we allow it to cool slowly in the undisturbed air of a dark room, we see that it changes color again; from white it becomes red, then a darker red; the light which it gives forth gradually diminishes; the intensity of its calorific rays becomes less at the same time, and soon it ceases to shed light round about it.

It continues, however, to emit from its surface calorific rays for some time after it has ceased to be luminous, as may be perceived by holding the hand near it.

The calorific rays which very warm bodies send off from their surfaces pass through the transparent air without heating it, nor do they heat sensibly those bodies at whose surfaces they are reflected.

These very important facts, which ought not to be forgotten, have been established by the results of a large number of experiments.

We have thus taken one sure step in the investigation of heat. We see that very warm bodies emit from their surfaces rays which, passing (like rays of light) through the air excite, at a distance, heat at the surfaces of the surrounding objects on which they fall *without being reflected.*

The existence of the calorific rays, which we are now discussing, being actually proved, and their manner of acting being evidently as I have described it, it is important to ascertain whether the knowledge of these facts be not sufficient to form a theory of heat which will explain all these phenomena.

A theory which should have the advantage of explaining the communication of heat by a *single* method, at once simple and easily understood, would be preferable, it seems to me, to one which, in order to explain various phenomena, would be obliged to admit *two different* modes of the communication of heat.

In order to form a clear and exact idea of the rays in question and of the effects which they are capable of producing, we must go back to their mechanical cause, and consider them with regard both to their existence and to their operation.

There are two ways of looking at the radiation from an object; the first, by conceiving the rays as emanations of an actual substance thrown off from the surface of the body; the second, by considering these rays as *undulations* which, starting from every point of the surface of the radiating object, are propagated in all directions in straight lines in an elastic fluid which surrounds it on every side.

The system of Newton *supposes* that the rays of light are real emanations.

Sound, with which we are better acquainted than we are with light, affords us an example of radiation or undulation in an elastic fluid which most certainly is not an emanation.

We have sufficiently clear ideas of the mechanical operations by means of which the undulations of the air which constitute sound are excited and propagated; but we have no conception of any possible mechanical operation by means of which a *material substance* could be sent forth continually and *in all directions* from the surface of a body.

In physics, in order that an hypothesis may be admitted, it must be founded on the supposition of a *conceivable* mechanical operation.

In order that the theory of heat which is founded on the vibratory hypothesis may be admitted, it is necessary to show that the vibrations in question can exist, and that they can cause the rays or undulations which objects emit from their surfaces, and by means of which we suppose that bodies of different temperatures influence each other even at a distance, bringing about reciprocal and simultaneous changes of temperature, so that little by little they arrive at a common and intermediate temperature.

If the particles which compose a body do not touch each other (an opinion which is generally received, and which appears very probable), as there is no doubt that these particles are continually drawn one towards another by the recognized force of universal gravitation, it is impossible to conceive how, in an assemblage of particles which form a tangible solid body, these particles can preserve their relative situations without being in motion.

From this course of reasoning we might conclude that the particles which compose a body are of necessity in motion; and if we admit the existence of an eminently elastic fluid, — an ether which fills all space throughout the universe, with the exception of that occupied by the scattered particles of ponderable bodies, — it is easy to conceive that the movements of the particles which make up material objects must cause undulations in this fluid; and, on the other hand, the undulations of this fluid must affect to a sensible degree and modify the motions of the particles of these bodies.

It might perhaps seem that these motions among the particles of solid bodies would be incompatible with the preservation of the forms of those bodies; but by reflecting attentively on this subject it will be found that such motions as are here supposed can well exist without diminishing at all the stability of the external form of the bodies.

It would follow necessarily, from the state of things supposed by the hypothesis in question, first, that the sum of the active forces in the universe must always remain constant, in spite of all actions and reactions taking place among the various bodies; secondly, that the particles of all ponderable bodies must of necessity have the property of producing radiations.

Now, if we admit the existence of the *ether*, it is possible to explain the radiations of bodies in still another manner; it is by supposing that the particles are kept apart from each other, not in consequence of the action of the centrifugal force of those particles, but by atmospheres composed of ether or of some other fluid unknown to us, which is extremely elastic, and that it is by the very rapid vibrations which take place in these atmospheres that those undulations in the surrounding ether are excited by means of which the temperature of objects is altered.

The adoption of this latter hypothesis will reconcile, to a certain extent, the theory of vibrations with that of a calorific substance; but still the heating of a body cannot be regarded, in any respect, as the result of the *accumulation* of this substance, but as the *acceleration* of its motion.

In order to establish on a firm foundation the theory of heat which is based upon the vibratory hypothesis, it is necessary not only to show that the vibrations in question are possible, but also to prove that the undulations which they should cause do really exist.

In the ordinary condition of things, the objects which surround us do not afford any indication of radiation, nor do they produce any effect capable of manifesting itself to any one of our senses in such a way as to lead us to suspect that they possess radiating surfaces. But the philosopher who aims at penetrating into the mysteries of Nature must be continually on his guard that he may not be deceived either by the testimony or by the silence of his senses.

In the first place it is evident that our various organs were formed with reference to the daily wants of life; and that, if they were too sensitive, the pleasure which they afford us would be turned into actual pain.

If our ears had been constructed so as to be sensibly affected by all the vibrations which take place in the air, we should, without doubt, be stunned by the intolerable noise, even in the deepest retirement; and if our eyes took cognizance of all the rays that strike them, we should be dazzled by an insupportable flood of light, even in the darkest night.

It is well known that, if the vibrations of a sonorous body be less frequent than 30 in a second, or more frequent than 3000 in a second, the undulations of the air caused by these vibrations do not perceptibly affect our organs of hearing; and it is very probable that the range of our organs of sight is still more limited.

When we have found strong reasons for suspecting the existence of agents which fail to manifest themselves to our senses, we ought to employ all our skill in devising means for compelling them to discover themselves and to unveil the mysteries of their invisible operations.

By means of an instrument which I have called a *thermoscope*, and which is extraordinarily sensitive, I have found not only that all bodies at all temperatures emit rays, but also that the rays emanating from cold bodies are as effectual in cooling warm bodies as the rays from the latter are effectual in warming cold bodies.

The principal part of the instrument of which I have made use in these delicate experiments consists of a long glass tube bent at both ends, and having at each extremity a very thin glass bulb an inch and a half in diameter. The middle portion of this tube, which is straight, is placed in a horizontal position, while the two end portions, whose extremities are the two bulbs, are turned upwards in such a way as to form right angles with the horizontal portion of the tube. The horizontal portion is from 15 to 16 inches in length, and each of the two end portions, which are vertical, is from 6 to 7 inches long. The internal diameter of the tube should be about half a line.

By means of a little glass *reservoir*, an inch in length and a line in internal diameter, inserted in the tube at one of the elbows, there is introduced into the interior of the instrument a small quantity of coloured spirit of wine (exactly enough to fill the reservoir without interfering with the free passage of the air from one bulb to the other); this being done, the extremity of the reservoir is sealed hermetically, and all communication between the air enclosed in the instrument and the air of the outside atmosphere is forever interrupted.

The instrument is adjusted and prepared for use as follows: —

The bulb which is farthest from the reservoir having been warmed slightly with the hand, the instrument is suddenly turned over, so as to bring the reservoir uppermost, and in this way a small quantity of the spirit of wine passes from the reservoir into the horizontal part of the tube; restoring immediately the instrument to its natural position, the observer withdraws himself from it, and waits for the small quantity of spirit of wine which has passed into the horizontal part of the tube to become stationary; this will be as soon as the two bulbs have acquired the same temperature.

The little bubble of spirit of wine, which serves as the index of the instrument, and which may be about three quarters of an inch long, should become stationary nearly in the middle of the horizontal portion of the tube; if it is too near either of the elbows it must be returned to the reservoir, and the operation performed anew.

When this delicate operation is finished, the instrument is ready for use. The method of employing it is as follows: —

One of the two bulbs is protected from the influence (calorific or frigorific) of the warm or cold bodies presented to the other bulb by means of light screens covered with gilt paper; when the air in this latter bulb is warmed or cooled by a body warmer or colder than the thermoscope to which it is thus presented, the elasticity of the air is affected by this change of temperature, and the little bubble or column of spirit of wine which is in the horizontal portion of the tube is compelled to move and to take a new position.

The direction of the motion of this bubble indicates the nature of the change which has taken place in the temperature of the air which is enclosed in the bulb to which the body is presented, and the distance traversed by the bubble is the measure of the increase or diminution of the elasticity, and, as a consequence, of the temperature of that air.

If the bubble recedes from the bulb to which the object experimented upon is presented, it is evident that the air enclosed in the bulb has been heated by the influence of this body; but when the bubble of spirit of wine advances towards this bulb, we have a proof that the air in the bulb has been cooled.

The *rapidity* with which the bulb moves is proportional to the *intensity* of the action of the object presented to the instrument.

In order to compare the intensity of the calorific or frigorific actions of two different objects, they are presented at the same time to the two bulbs of the instrument, and their respective distances from the bulbs so regulated that the bubble of spirit of wine remains at rest in its proper position.

In this case it is evident that the action of the two objects, each on the bulb to which it is presented, is of precisely the same amount; hence we can calculate the relative intensity of the radiation of each one of the two objects from the extent of the surface presented to the bulb, and from the square of its distance from the bulb.

If we desire to compare the calorific action of a warm body with the frigorific action of a cold body, we begin by protecting one of the bulbs of the instrument by the screens, and then present to the other bulb the two objects, — regulating their respective distances in such a manner that their actions exerted at the same time produce equal effects, that is, so that one warms the bulb as much as the other cools it.

The equality in the amount of action is denoted by the remaining at rest of the bubble of spirit of wine which serves as the index of the instrument, and when this equality is established, the relative intensity of the radiation from the objects in question is calculated from the amount of surface which they respectively present to the bulb, and from the squares of their distances from it.

The sensibility of this instrument is so great that, when it is at a temperature of 15° or 16° of Reaumur's scale, if the hand be presented to one of the bulbs at a distance of three feet, the heat radiating from the hand is sufficient to cause the bubble of spirit of wine to move forward several lines; and the cooling influence of a blackened metallic disk four inches in diameter, at the temperature of melting ice, is such that, when presented to the bulb at a distance of 18 inches, it causes the bubble to advance in the opposite direction with a rapidity which is very perceptible to the eye.

By means of this instrument I have discovered, first, that all bodies at all temperatures (cold bodies as well as warm ones) emit continually from their surfaces rays, or rather, as I believe, *undulations*, similar to the undulations which sonorous bodies send out into the air in all directions, and that these rays or undulations influence and change, little by little, the temperature of all bodies upon which they fall without being reflected, in case the bodies upon which they fall are either warmer or colder than the body from the surface of which the rays or undulations proceed; secondly, that the intensity of the rays from different bodies *at the same temperature* is very different, and that it is less in bodies which reflect the rays of light than in those which absorb them, less in the metals than in their oxides, less in opaque and polished bodies than in those which are imperfectly transparent and unpolished, (a surface of brass, for instance, emits four times as large a quantity of rays at a given temperature when it is covered with a coating of oxide, and five times as large a quantity when it is blackened by the flame of a candle, as when the surface of the metal is clean and well polished); thirdly, that the rays which bodies of the same temperature send out to each other have no tendency to bring about any change of temperature in these bodies; fourthly, that the rays which any body whatever, at a given temperature, sends continually from its surface in all directions, are calorific or frigorific with regard to other bodies on which they fall, according as these latter are less warm or warmer than the body from which the rays come; so that the same rays are calorific as regards all bodies less warm than the one from which they proceed, and frigorific as regards all those which are warmer than this body.

From these facts we might conclude *a priori*, that those bodies which, when warm, give off many calorific rays would, when colder than the surrounding objects, give off to them many frigorific rays. This is exactly what my experiments have made evident to me.

In experiments made with bodies of the same size, and of the same material, the intervals of temperature being equal, the frigorific influences of cold bodies have always appeared as real and effective as the calorific influences of warm bodies.

To one of the bulbs of a thermoscope, the temperature of which was 20° of Reaumur's thermometer, were presented at the same time and at equal distances two disks of metal of the same diameter. The temperature of one of these disks was 0° (that of melting ice), that of the other was 40°. The index of the instrument by remaining at rest showed that the bulb was cooled by the rays from the cold body as much as it was heated by the rays from the warm body.

If the surface of one of the disks, it matters not of which, is blackened, the intensity of the radiation from this blackened disk is increased to such an extent that the other can no longer counterbalance it; but if the second one is blackened also, the equality of action is immediately re-established.

If the emanations from warm and cold bodies are really undulations in an extremely rare and elastic fluid which has been called *ether*, the communication of heat and cold ought to be similar to the communication of sound; and all the mechanical contrivances which have been invented to increase the intensity of sound ought to be just as applicable for increasing the effects produced by these emanations from warm and cold bodies;

and, indeed, I found that a speaking-tube (a conical brass tube, well polished on the inside) placed between one of the bulbs of the thermoscope and a hollow ball of thin copper 3 inches in diameter, which, being filled with pounded ice, was presented to it at a distance of 12 inches, increased more than three times the effect of the cold body.

To use a rather strong metaphor, but one which expresses perfectly the idea which I have conceived of the mechanical operation in question, I will say that the cold ball *spoke* at the larger opening of the speaking-tube while the bulb of the thermoscope *listened* at the smaller opening.

If it is true that the particles which make up all material bodies are agitated continually by very rapid vibratory motions, and that, in consequence of these motions, all bodies at all temperatures send continually from every point of their surfaces rays or undulations similar to the undulations caused in the air by the vibration of sonorous bodies; and if bodies of different temperatures act one upon another at a distance, by means of these rays or undulations, working simultaneously an interchange in temperature and gradually bringing about a mean intermediate temperature, — we ought then to regard the cooling of a warm body as the result of the actual and positive operation of the surrounding bodies less warm than itself; and since the rays coming from warm bodies, and, as a consequence, from cold bodies, are reflected in great measure by the polished surfaces of opaque bodies, and since the rays which are reflected produce little or no effect on the bodies at whose surfaces they are reflected, we might conclude *a priori* that opaque polished bodies ought to cool or become warm more slowly than bodies imperfectly transparent and unpolished.

I will now detail the results of a series of experiments made with a design of throwing light on this point, so important in the science of heat.

I had made two cylindrical vessels, four inches in diameter and four inches high, of thin sheet brass, well polished on the outside. Having blackened one of them over the flame of a candle, I filled them both with boiling water, and left them at the same time to cool in the air of a large quiet room. The one which was blackened cooled almost twice as fast as the one whose metallic surface remained bright and clean. When the two vessels had become of the same temperature as that of the room in which they were situated, they were removed into a room warmed by a stove, and I found that the blackened vessel was heated twice as quickly as the other.

The blackened vessel was cleaned and covered with a single covering of fine linen, fitting closely to the body of the instrument. Repeating the experiments with the two vessels, that which was exposed naked to the cold air took up 45 minutes in cooling through an interval of 10 degrees on Fahrenheit's scale, that is, from the 50th to the 40th degree above the temperature of the room; the other vessel, covered with a *coat* of fine linen, took up only 29 minutes in cooling through the same interval.

When the two vessels had become of the same temperature, they were removed into a warm room, and I found that the vessel which was clothed with linen acquired heat faster than the one whose surface was naked.

If the results of these experiments do not furnish a conclusive proof of the radiation from all bodies, and that it is by means of these radiations from surrounding objects that the temperature of a given body is changed, they certainly lend to this conjecture a great degree of probability.

Several other similar experiments were undertaken in order to throw light on this point, and results were invariably obtained which tended to confirm the hypothesis in question.

Of all known bodies the metals are the most *opaque*, and it appears that they are so to an equal degree; it appears also that a naked metallic surface, or one that is free from all dirt, is always polished in spite of those irregularities of form by which the brilliancy of its metallic lustre is broken up and apparently diminished. If these conjectures are well founded, we may conclude that all metals are equally competent to reflect from their surfaces the rays that impinge upon them; and if objects are heated and cooled by rays from surrounding objects, we might conclude not only that of all known bodies the metals ought to acquire heat or become cold the least rapidly, but also that they ought to acquire heat or become cold with the same degree of difficulty or rapidity.

To put these suppositions to the test of experiment, I procured several cylindrical vessels, of the same form and dimensions but of different metals, and I found that they did indeed all cool or acquire heat in the same time. There were vessels of brass, tin, lead, and others covered with thin coatings of gold and silver; each vessel was four inches in diameter and four inches high, and when filled with boiling water and exposed, in winter, to the air of a large quiet room, they all passed, in cooling, through the given interval of 10 degrees in from 45 to 46 minutes.

This equality in the degree of readiness with which all the metals become cool or acquire heat is certainly very remarkable; and it seems to me very difficult of explanation except by adopting the hypothesis that heat is communicated by means of radiations.

As it might be supposed that a film of air, attached by a certain force of attraction to the surfaces of the metallic vessels, could have caused this apparent equality in their rate of cooling, I made the following experiments to elucidate this point.

One of the two brass vessels was covered, first with one, next with two, then with four, and finally with eight coatings of spirit varnish, and the experiment with the two vessels was repeated with each of these coatings. While the vessel, the surface of which was bare, cooled invariably through the given interval of 10 degrees in 45 minutes, the other vessel, which was varnished, cooled more or less rapidly according to the thickness of the coating of varnish with which its surface was covered, but always in a sensible degree more rapidly than the one whose surface was naked: —

	Minutes.
With one coating of varnish it cooled in	$34\frac{1}{2}$
With two coatings, in	29
With four coatings, in	$24\frac{1}{2}$
And with eight coatings, in	27

As the film of air which is supposed to have been attached to the surface of the vessel when this metallic surface was not covered with varnish ought to have been as completely driven off by *one* coating of varnish as by *two* or by a greater number, it seems very difficult to reconcile the results of these experiments with the supposition that a film of air attached to the surfaces of all the vessels, made as they were of different metals, was the cause of their cooling all equally slowly.

When I repeated the experiment with a vessel of glass, and with one of tinned iron of the same form and dimensions, I found that the glass vessel cooled much more rapidly in the air than the one made of tinned iron, although its walls were six times as thick as those of the latter. In water the vessel of tinned iron cooled most rapidly.

The results of all these experiments, and of a great number of others which it would take too long to detail here, convinced me that the ease with which a body is heated or cooled depends very much on the nature of the surface of that body, — these operations going on more slowly and with more difficulty as the surface of the body is more capable of reflecting the rays which fall upon it; I was therefore impatient to submit the theory of heat which I had adopted to the most searching of tests, by employing it to explain some of the grand and interesting phenomena of nature.

Close to us there occurs a most interesting phenomenon, and one which, assuredly, is calculated to excite our curiosity.

The people who inhabit hot countries are black, while those who dwell in cold climates are white.

What advantages do the negroes derive from their colour which makes them better fitted than the whites for supporting without inconvenience the excessive heats of their scorching climate?

In all climates a large amount of heat is necessarily excited in the lungs by the act of breathing; and when man is placed in a situation where the air and all objects about him are almost as warm as his blood, the surface of his body ought to be of such a character as to be readily cooled; else the rays, very slightly cooling in their action, which reach him from the surrounding objects, would not suffice to free him from the heat generated continually in his lungs, and he would soon find himself oppressed and overcome by the accumulation of this heat.

In a cold country, where the cooling of the surface of a body by the cold objects which surround it is more than sufficient to counterbalance the heat continually produced by respiration, the body can be protected from this excessive cooling action by clothing; but we know of no sort of clothing fitted to promote sufficiently the cooling of the human body in a very hot climate.

What has Nature done to supply this want? She has given to the inhabitants of hot countries a black skin; this colour gives to the negro such facility for becoming cool that he feels perfectly comfortable in a situation where a white man would be overcome by the heat. But, in return, the negro shivers with cold in a climate which the white man finds perfectly agreeable.

Every one knows that a black surface reflects fewer rays of light than a white surface; and the results of all the experiments performed by myself and by others seem to show that those surfaces which are of such a character as to reflect light also reflect the calorific or frigorific rays which all bodies send continually from their surfaces; and if the temperature of a body is changed in consequence of the action of surrounding bodies through these radiations, it is seen clearly why the negro suffers less from the heat of the tropics, and more from the cold of the polar regions, than the man with a white skin.

But when the negro is exposed to the action of calorific rays — to those of the sun, for instance — must he not be heated more than a white man? It would be so, without doubt, if Nature had not foreseen the danger and provided means for warding off the evil.

When the negro is exposed to the rays of the sun, an oily matter appears immediately at the surface of his skin, and causes it to shine; the calorific rays which fall upon it are reflected to a great extent, and he finds himself but little heated.

The sun sets, or the negro enters his hut; the oil which covers the surface of his body retires under his skin, and he retains all the advantages which his colour affords in aiding him to become cool.

If a coating of oil on the skin serves to protect the body from the too violent action of calorific rays, it ought to serve also, without doubt, to protect it from the too violent action of frigorific rays in very cold countries, especially in winter, when the sun never rises. And, indeed, do not the Laplanders besmear themselves with oil?

But in the case of a question of so great interest, I wished to omit nothing which might throw light upon it.

The following experiment seemed to me to establish beyond doubt the principal facts.

Having covered two of my cylindrical vessels with an animal substance, namely, with gold-beater's skin, I painted one of them black with Indian ink, leaving the other of its natural white color. Having filled both of the vessels with hot water, I left them, at the same time, to cool in the air of a large quiet room.

The vessel covered with a black skin represented a negro; the one covered with a white skin represented a white man.

The negro cooled considerably more rapidly than the white man, requiring $23\frac{1}{2}$ minutes to cool through the usual interval of 10 degrees, while the white man required 28 minutes to cool through the same interval.

This interesting experiment was made at Munich, the 26th of March, 1803. The results of these experiments need no illustration; and I leave to physiologists and physicians to determine what advantages may be derived from them in taking measures for the preservation of the health of white men who are called upon to dwell in hot countries.

[This paper is translated from the Moniteur Universel, 9 Messidor, An 12 (June 26, 1804).]

HISTORICAL REVIEW

OF THE

VARIOUS EXPERIMENTS OF THE AUTHOR ON THE SUBJECT OF HEAT.

A WRITER who directs the attention of the public to a work upon a subject as important as it is difficult of investigation must assuredly be allowed at the very outset to state modestly the reasons which entitle him to a hearing. It is also equally true that a natural philosopher can with justice lay claim to the confidence and approbation of the learned only so far as his claims are based upon his own labours, upon toilsome and accurate observations, as well as upon experiments planned and executed with all possible care.

To engage in experiments on heat was always one of my most agreeable employments. This subject had already begun to excite my attention when, in my seventeenth year, I read Boerhave's admirable Treatise on Fire. Subsequently, indeed, I was often prevented by other matters from devoting my attention to it, but whenever I could snatch a moment I returned to it anew, and always with increased interest. Even now this object of my speculations is so present to my mind, however busy I may be with other affairs, that everything taking place before my eyes, having the slightest bearing upon it, immediately excites my curiosity and attracts my attention.

This habit of many years' standing, by force of which I seize with the greatest eagerness, and endeavour to investigate, each and every phenomenon related even in the slightest manner to heat and its operations which comes to my knowledge, has suggested to me almost all the experiments that I have performed with reference to this subject.

In the year 1778 I was engaged in investigating the force of gunpowder and the velocity of bullets discharged from fire-arms. For this purpose I discharged many times a musket-barrel which was loaded in various ways, and which rested on two iron rods, perfectly free (that is, without any stock), in a horizontal position, about four feet from the ground.* This gave me occasion to make a very striking observation.

Since these experiments were intended principally to determine, from the recoil of the barrel, the velocities with which the bullets were discharged, it was first necessary to ascertain how much the weight of the powder which caused the discharge of the bullets had to do with this recoil. In order to solve this problem, I made several successive experiments, — some with a charge of powder without any bullet, and some with two, three, or even four bullets, one upon another.

According to my usual practice, I seized the piece with my left hand immediately after each discharge, in order to hold it firmly until I had wiped it out with some tow fastened to the rammer. I was therefore not a little astonished to notice, on this occasion, that

* A detailed description of these investigations may be found in the seventy-first volume of the Philosophical Transactions, and in the first volume of my Philosophical Papers, which was published at London, in the year 1802, by Cadell and Davies. See also Vol. I. p. 1.

the barrel was always hotter when the charge had consisted of powder alone than when loaded with one or more bullets.

I had, up to this time, no suspicion but that the piece, on being discharged, became warm as an immediate consequence of the heat caused by the burning of the gunpowder; now, however, I was convinced by the result of the above-mentioned experiment, that this supposition was entirely without foundation.

For if we should hold that the gun in question was actually heated by the inflammation of the powder, since the flame would issue from the piece much more rapidly when the charge consisted of powder alone than when the same charge had to force out one or more bullets, it would follow that a much higher degree of temperature would be reached in the latter case than in the former. But since the above-mentioned experiment shows the contrary, it follows that the heating of the piece in question is not due to the combustion of the powder, but to the vibrations caused by the concussion within the barrel, and to the operation, as rapid as it is brief, of the elastic fluid generated by this combustion.

No one is ignorant of the fact that a heavy blow is much more effective in producing heat in a solid body than a lighter one; and if the hypothesis be well founded that heat is nothing more than a continuous, more or less rapid, vibratory motion among the particles of solid bodies, this phenomenon is easily explained.

Nothing is more certain than that the shock taking place within the barrel, in the case of the above-mentioned experiment, by the combustion of the powder, was more vibrating or heavier when the charge was fired without a bullet than when the elastic fluid generated by the combustion was obliged, in order to get room for action, to push slowly before it one or more balls, which were anything but light. On careful consideration it seems to me that this circumstance is more than sufficient to explain in a satisfactory manner the results of the experiments in question, although I am perfectly free to confess that I never could reconcile myself to the hypothesis which has been developed with regard to *caloric*.

The above-mentioned occurrence made so deep an impression upon me, that I could hardly wait long enough to procure the necessary instruments before undertaking a number of successive experiments upon heat, in order to arrive at some conclusion with regard to its character, as well as to the manner of its operation.

I proposed, first of all, to undertake various experiments on what has since been called the *specific heat* of bodies. For this purpose, I procured from Mr. Fraser, New Bond Street, London (now physical and mathematical instrument maker to the King of England), a considerable number of solid balls of precisely the same diameter, namely, one inch. Some of these balls were of gold, some of silver; in short, they all were of one metal or another, or of some solid substance easily turned in a lathe. Each of these balls was suspended by a thin silken cord, and I proposed to heat the balls in certain liquids up to a given temperature, and then to plunge them into a known quantity of water which had been cooled in the same proportion. I drew this inference, — that the degree of temperature which the balls communicated to the known amount of water, as shown by the thermometer, would be more than sufficient to calculate therefrom the proportional amount of heat necessary to bring to the same temperature the balls and an equal quantity of water.

I had already begun upon these experiments, but before I could finish them the war made it necessary for me to go to America. These researches were therefore interrupted for several years; and when, after the peace of 1783, I returned to England, I learned that Wilkin, in Sweden, had already carried out exactly what I had proposed to myself. Since I had not the slightest occasion to doubt the accuracy of the experiments performed by this philosopher, I laid aside, as useless, the apparatus which I had designed for my own investigations.

In the following year I left England and went to Bavaria, where I was received into the service of the late Elector. I brought with me several instruments belonging to the above-mentioned apparatus, which are still to be seen in the museum of the military school in Munich.

For more than twenty years I have never in any of my writings mentioned either my project and the preparations made for carrying out experiments on this point, or the experiments I really made and which agree with those of Wilkin, simply because I hate, and always have hated, the character of a man who appropriates the discoveries of another. I speak of them now rather to convince the public that I have long thought about this subject, than from any motive which might perhaps have its origin in personal vanity.

My relations at the court at Munich, and that, too, with a prince who was much interested in the promotion of knowledge, afforded me during a period of four years abundance of leisure to pursue, almost without interruption, my physical investigations, and I employed this leisure in making a considerable number of experiments on heat.

In the years 1785 and 1786 I was occupied in researches as to the manner in which heat passes through various substances and communicates itself still farther. A detailed description of these experiments is to be found in the two papers which I inserted in the Philosophical Transactions of the Royal Society of London. The first is in the seventy-sixth, the other in the eighty-third, volume of this work. For the latter I received the gold medal which this Society is accustomed to confer annually.*

In the summer of 1785 I discovered that heat could be transmitted through, or excited in, a Torricellian vacuum.

Since this discovery has contributed not a little towards strengthening me in the opinion which I have since adopted with regard to the real character of heat, I do not consider it at all superfluous to give here, with all its details, an account of the experiment by which this fact was established beyond doubt. This experiment was conducted as follows.

After a skilful workman in Mannheim, Artaria by name, had succeeded in fixing firmly the globular bulb of a mercurial thermometer, half an inch in diameter, in the centre of another glass bulb an inch and a half in diameter, the space between the outer surface of the thermometer bulb and the inner surface of the outside ball, or the *globe*, was filled with mercury by means of a

* These papers were printed in 1797, in my eighth Essay. See Vol. I. p. 401.

barometer tube which was soldered to a small hollow tube or point projecting outwards from the globe. This projection extended downwards when the thermometer fastened to the globe was in its natural upright position.

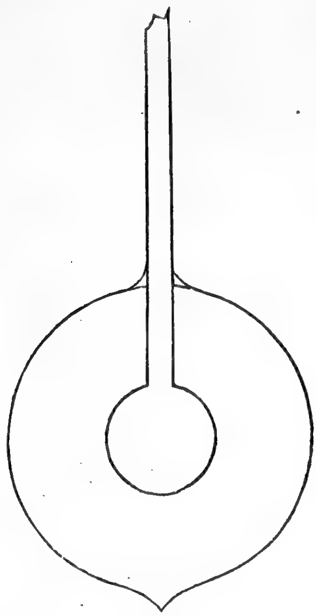
As soon as the vacant space inside of the globe and around the thermometer bulb, as well as the barometer tube (thirty-six inches in length), was filled with mercury, the end of the tube was dipped into a vessel of mercury; the tube was then inverted and brought into a perpendicular position, so that the globe in which the thermometer was fastened was at the top.

Since the instrument was converted in this way into a true barometer, the mercury in the globe and in the upper part of the barometer tube fell until the upper surface of the mercury in the tube was twenty-eight inches above the surface of the mercury in the vessel, where it remained at rest, being kept at this height by the pressure of the outside air. A lighted wax-candle was now held at the upper part of the tube where it entered the globe, and where the diameter of the tube had previously been contracted, and the flame was directed, by means of a blow-pipe, against that part of the tube which it was desired to melt together.

As the glass was softened by the heat, the pressure of the outside air immediately forced the walls of the tube together; the whole operation was successful.

The barometer tube was then detached, and the bulb of the thermometer was now surrounded on all sides by a vacuum, as may be seen from the figure on the opposite page.* The thermometer was filled with mercury,

* See also Vol. I., Plate to p. 404, Fig. 1.



and provided with a scale, and I could then scarcely master my impatience and wait for the time when I should satisfy myself whether heat would be able to pass through this vacuum.

I now put the apparatus into a vessel filled with water at 18° Reaumur, and left it there until I was sure (from the scale of the instrument) that the bulb filled with mercury, which was in the centre of the vacuum, had reached this temperature of 18 degrees. I then took the instrument out of this vessel, and held it for some minutes in another full of hot water, which was kept constantly boiling by a lamp placed under it.

Since the mercury in the tube of the thermometer began to rise, although slowly, there remained no longer any doubt that the heat of the boiling water really passed through the vacuum into the bulb of the thermometer.

The mercury in the thermometer rose in the following manner: After the instrument had remained in the boiling water 1 min. 30 sec. the mercury had risen from 18° to 27°. After the lapse of 4 minutes, it had risen to $44\frac{9}{10}$°, and at the end of 5 minutes to $48\frac{1}{5}$°.

In order to estimate more accurately the relative rapidity with which heat passed through a vacuum and through air, I broke off the end of the small pointed tube which projected from the under side of the globe so that the air could freely enter the globe; I then melted the tube together a second time, by means of a candle; cooled my apparatus in water, and plunged it, as soon as it had acquired the temperature of this water, that is 18°, again into boiling water. The mercury rose much more rapidly than in the preceding experiment.

The manner in which the temperature gradually increased in both experiments is shown in the following table.

When the spherical reservoir of the mercurial thermometer, which was fastened in the centre of a glass globe an inch and a half in diameter, was plunged into boiling water, the times of ascent were as follows: —

	In a Torricellian vacuum. (Exp. No. 1.)		Surrounded by air. (Exp. No. 2.)	
	Time elapsed.	Heat acquired.	Time elapsed.	Heat acquired.
Upon being plunged into boiling water		18°		18°
	m. s.	°	m. s.	°
After remaining in it	1 30	27	0 45	27
	4 0	$44\frac{9}{10}$	2 10	$44\frac{9}{10}$
	5 6	$48\frac{1}{5}$	5 0	$60\frac{9}{10}$

From the results of these experiments it is evident that the heat increases nearly twice as fast when the bulb is surrounded by air as when it is in a vacuum.

I afterwards performed other experiments of the same kind without discovering the least difference from those mentioned above. It would take too much time and space to describe them all here. They are to be found, however, in my memoir in the Philosophical Transactions and in my eighth Essay.

I had subsequently several instruments of the same sort made, in order to repeat and vary my experiments. Sometimes I observed the time which they took in cooling, sometimes that necessary for the heat to penetrate them. Sometimes I performed the experiment in the open air, sometimes in water. All these experiments gave the same result, namely, that the thermometer bulb in a vacuum became warm or cold as the case might be, the only difference being that it always took nearly twice as long to effect this change of temperature as was required when the bulb was surrounded by air.

The passage of heat through a vacuum was a fact of such importance in the investigation of the nature of heat, that I wished to confirm it by experiments which would not allow a shadow of doubt.

That part of the thermometer tube which was inserted in the glass globe was in contact with this globe. Hence the thought might suggest itself that a part of the heat received or given out by the thermometer bulb, which was surrounded by the vacuum, was communicated by means of the tube of the thermometer, since a portion of this tube was surrounded by air or water in which the heating or cooling was effected. In order to be fully satisfied as far as this circumstance was concerned, it occurred to me to repeat the experiment with a thermometer suspended by a very fine silken thread in the middle of a glass body of such size that the thermometer with its tube was entirely contained in it.

This glass body was then voided of air by means of mercury.

The results of the experiments performed with these instruments differed little or not at all from those made with the apparatus previously described, therefore the fact of the transmission of heat through the Torricellian vacuum was established beyond any doubt.

These results are sufficiently known to the learned world; now the question arises as to how these results can be reconciled with the theory which at the present day has been adopted in regard to *caloric*. I must confess freely, that, however much I might desire it, I never could reconcile myself to it, because I cannot by any means imagine how heat can be communicated in two ways entirely different from each other.

Philosophers have made little or no mention of the results of these investigations: I do not assume to explain their silence; if I myself mentioned them as little as they, it is easy to imagine the cause of my silence. It will at least be admitted that I have pointed out plainly enough the doubts which the results of my experiments could give rise to.

I afterwards undertook many other experiments to determine accurately the various degrees of rapidity with which heat passes into mercury when surrounded by common or atmospheric air, by air saturated with moisture, by carbonic acid gas, and by air brought to various degrees of density.

In the year 1787 I made a series of experiments which are described in the Philosophical Transactions for 1792; my principal object was to investigate the conducting power with regard to heat possessed by various substances, especially by those which we are accustomed to use for clothing. The instrument which I used in these experiments, and which I called a *passage-thermometer*, differs but slightly from that described above. I fixed the bulb of a mercurial thermometer half an inch in diameter within a glass globe an inch and a half in diameter, with a long cylindrical neck; I then filled the space between the outer surface of the thermometer bulb and the inner surface of the glass globe with a certain quantity of the substance whose conducting power was to be determined, and allowed the instrument to cool in a mixture of pounded ice and water. As soon as the thermometer showed me that its bulb (which was in the middle of the glass globe) had acquired and retained constantly the temperature of the cooling mixture (that is, 0° of Reaumur's scale), I took the apparatus out of this cold mixture, plunged it into boiling water, observed the times required for the heat to pass into the bulb of the thermometer through the surrounding substance, and inserted them in a table, noting every ten degrees as accurately as possible.

Since the water into which I plunged my apparatus was kept constantly boiling, it is evident that the outside of the instrument, that is, the outer surface of the globe, was always of the same temperature; hence the more or less rapid heating of the thermometer bulb within the globe indicated the resistance which the covering of the bulb offered to the passage of the heat from the inner surface of the globe to the bulb of the thermometer.

In this way I made several experiments; but as I was inconvenienced by the steam rising from the boiling water, and so experienced difficulty in noting the rising and falling of the mercury, I changed my method of operation, and no longer observed the time necessary for the instrument to grow warm, but that necessary for it to grow cold.

When, therefore, my apparatus, plunged in boiling water, had acquired such a temperature that the mercury had reached 77° of Reaumur's scale, I took it out of the boiling water and held it in the air, over the large vessel filled with pounded ice and water, ready to plunge it into this cooling mixture the very moment that the mercury had fallen to 75°.

As soon as the mercury had reached this division of the scale, I plunged my apparatus immediately into the cooling mixture, and holding at the same time at my ear a watch which beat half-seconds (which I carefully counted), I waited for the moment when the mercury had fallen to 70°. I then noted and recorded the time elapsed, and in the same way observed the time when the mercury had fallen to 60°, and thus proceeded, noting every ten degrees, until the apparatus had cooled to the temperature of 10°.

Sometimes the apparatus cooled to such an extent that the mercury in the thermometer stood at 0°; this, however, took up much time, and was attended with no particular advantage, as the determination of the times taken up in cooling from 70° to 10° was quite sufficient for calculating the conducting power of every sort of covering; on this account I generally ended the experiment when the mercury had just passed the 10° mark on the scale.

During the time of cooling the apparatus in ice and water, I moved it about in the mixture very slowly and constantly from one place to another; moreover, I always mixed the water with such a quantity of ice that the temperature of this mixture remained constant.

Since in such experiments the thermometer bulb in the middle of the glass globe was entirely surrounded as well by the air contained in the globe as by the substances of which the covering consisted, I made a few experiments to determine the time necessary for the bulb of the thermometer to become cold again when the globe contained nothing but air. I thus learned that when the apparatus previously warmed in boiling water was plunged into the mixture of cold water and pounded ice, it required 576 seconds to cool from 70° to 10° Reaumur.

The following table contains the results of several experiments undertaken with a view to determine the relative warmth of various substances such as are commonly used for clothing.

I only remark, in addition, that I always determined the amount of the substance by weight (16 grains standard weight), and endeavoured to distribute it as equally as possible in the globe, and in such a manner that the bulb of the thermometer was surrounded by it.

Gradual Loss of Heat.	Substances used for Covering.							
	Air.	Raw Silk.	Sheep's-wool.	Cotton-wool.	Fine Lint.	Beaver's Fur.	Hare's Fur.	Eider-down.
	Time elapsed	Time elapsed.	Time elapsed	Time elapsed.	Time elapsed	Time elapsed.	Time elapsed.	Time elapsed.
From 70° to 60°	38″	94″	79″	83″	80″	99″	97″	98″
60 50	46	110	95	95	93	116	117	116
50 40	59	133	118	117	115	153	144	146
40 30	80	185	162	152	150	185	193	192
30 20	122	273	238	221	218	265	270	268
20 10	231	489	426	378	376	478	494	485
From 70° to 10°	576	1284	1118	1046	1032	1296	1315	1305

In order to determine what influence the *density* of a covering or clothing of a given thickness exerted on the warmth of this covering or on its power to confine heat, I made three consecutive experiments with different quantities of one and the same substance, namely, with eider-down. For the first experiment I took 16 grains of this substance, for the second 32 grains, and for the third 64 grains. In all cases I used the same apparatus, so that the thickness of the covering always remained the same.

The results of these three experiments are contained in the following table.

Loss of Heat.	The covering of Eider-down consisted of the following quantities of the substance.		
	16 grains.	32 grains.	64 grains.
	Time elapsed	Time elapsed.	Time elapsed.
From 70° to 60°	97″	111″	112″
60 50	117	128	130
50 40	145	157	165
40 30	192	207	224
30 20	267	304	326
20 10	486	565	658
From 70° to 10°	1304	1472	1615

Having convinced myself by these experiments that the *density* of any covering or clothing exercises a very considerable influence on its power to confine heat, its *thickness* remaining the same, I now sought to discover what effect the internal structure or constitution of the covering has on this power, its mean density and its thickness remaining the same.

By the expression *internal structure* I mean the state of division, whether fine or coarse, of the substance of which the covering consists, in the space which it occupies. This substance may be very fine and of delicate texture, and may be equally distributed through the whole space occupied by it, — as raw silk, for example; or it may be coarser and have larger interstices, — as, for example, a covering consisting of bits of stout sewing-thread, or one consisting of ravellings of cloth.

If heat really passed *through* the substances of which the covering is made, and if the efficiency of such a covering in restraining the same depended solely on the greater or less difficulty which the heat meets in passing through the solid parts of the covering, in that case the warmth of a covering would be, *cæteris paribus*, the same as that of the raw materials employed in its construction. It is evident, however, from the foregoing experiments, as well as from those to be detailed hereafter, that heat is not propagated in any such manner.

In one of my previous experiments I had endeavoured to determine the warmth of 16 grains of raw silk, which I had distributed equally in a certain space about the bulb of a thermometer. I now repeated this experiment twice, but with this difference: the first time I surrounded the bulb of the thermometer with 16 grains of a sort of lint made from a piece of white taffety; the second time with 16 grains of white sewing-silk, cut into small pieces, two inches long. The results of these experiments are recorded in the following table. My apparatus was warmed in boiling water, and then cooled in a mixture of water and pounded ice.

Loss of Heat.	Substances of which the Covering consisted.		
	Raw Silk, 16 grains.	Ravellings of Taffety, 16 grains.	Silk Threads, 16 grains.
	Time elapsed.	Time elapsed.	Time elapsed.
From 70° to 60°	94″	90″	67″
60 50	110	106	79
50 40	133	128	99
40 30	185	172	135
30 20	273	246	195
20 10	489	427	342
From 70° to 10°	1284	1169	917

Having convinced myself by these experiments that the fineness of the particles or fibres of the substance used as a covering contributes very much to the warmth of the same, I made the following experiments to determine what effect the condensing of the covering would have, the quantity of matter of which it was composed remaining the same, but the thickness being decreased.

As I had already, by means of the foregoing experiments, determined the warmth of coverings of raw silk, wool, cotton, and linen when taking 16 grains of each substance, and making thereof, about the bulb of a thermometer, a globular covering half an inch thick, I now took 16 grains of moderately coarse threads of each of these four substances, and with them I made four new experiments.

Instead of filling with these threads the entire space between the bulb of the thermometer and the inner surface of the globe, in the middle of which was the bulb, I wound it around the bulb of the thermometer, so that the latter looked exactly like a little ball.

I now introduced, as before, the thermometer bulb thus enveloped, into the middle of a glass globe an inch and a half in diameter; to this globe was attached a *neck* ten inches long, and of such a width as to allow of the insertion of the bulb of the thermometer wrapped up as described above, together with the attached scale.

The results of these four experiments may be seen in the following table; and that they may the more easily be compared with those made with the same quantity of the substances, but differently disposed, I have placed side by side the results of the comparative experiments.

Loss of Heat.	The Bulb of the Thermometer was covered with 16 grains of one of the following substances.							
	Silk.		Wool		Cotton.		Linen.	
	Raw.	In threads.	Raw.	In threads.	Raw.	In threads.	Raw.	In threads.
From 70° to 60°	94″	46″	79″	46″	83″	45″	80″	46″
60 50	110	62	95	63	95	60	93	62
50 40	133	85	118	89	117	83	115	83
40 30	185	121	162	126	152	115	150	117
30 20	273	191	238	200	221	179	218	180
20 10	489	399	426	410	378	370	376	385
From 70° to 10°	1284	904	1118	934	1046	852	1032	873

It would carry me too far if I brought forward in detail all the experimental results obtained in my researches undertaken to investigate the manner in which heat propagates itself through the various coverings. In my printed memoirs I have said all upon this subject that can with reason be said. For the present I have indicated clearly, not only the course upon which I entered at the very beginning of my researches, but also the object I had in view. Philosophers may decide whether this course was the right one, and whether I pursued it with zeal and perseverance.

The few remarks and observations which follow were occasioned by my researches made at that time.*

All the different substances which I had yet made use of for covering the bulb of the thermometer (which was contained within a glass globe an inch and a half in diameter) had in a greater or less degree confined the heat and prevented it from passing into or out of the bulb of the thermometer as rapidly as it would otherwise have done. Here then arose the important, and as yet unanswered question, how and by what mechanical operation had the coverings in question produced these effects?

This much is certain, that the slowness of the cooling of the bulb of the thermometer cannot by any possibility be a result of the non-conducting powers of those substances of which the coverings consisted, considered simply as having hindered the passage of the heat, for if, instead of regarding them merely as bad conductors of heat, we were to suppose them to have been totally impervious to heat, still their volumes — that is, the sum of all their solid parts or fibres — would be so inconsiderable in proportion to the space they occupied, that they would either have produced no effect on the air filling their interstices, or this air would have been sufficient of and for itself to have conducted all the heat communicated in less time than was actually taken up in the experiments. Here is the proof of this statement.

The diameter of the glass globe being 1.6 inches, its contents amounted to 2.14466 cubic inches. The di-

* See my eighth Essay, Vol. I. p. 455.

ameter of the thermometer bulb was 0.55 of an inch, and its contents 0.08711 of a cubic inch. Taking now from the contents of the globe (2.14466 cubic inches) the contents of the thermometer bulb (0.08711 of a cubic inch), there remain 2.05755 cubic inches as the measure of the space occupied by the substances by which the bulb of the thermometer was surrounded.

Although the above-mentioned substances *occupied* this space, they were very far from *filling* it, as will be observed without my calling attention to the fact; on the contrary, this space contained a large quantity of air, which occupied and filled the small interstices of the substances in question.

For example, in one of the experiments the bulb was covered with 16 grains of raw silk. As I had already learned from experiment that the specific gravity of the silk was to that of water as 1734 to 1000, it follows that the volume of 16 grains of silk was equal to the volume of 9.4422 grains of water. Further, as 1 cubic inch of water weighs 253.185 grains, it follows incontrovertibly that the space occupied by 9.4422 grains of water can be reckoned at the highest at 0.037294 of a cubic inch, and this amount of water (9.4422 grains) corresponds in volume to 16 grains of silk.

We know, however, that the space which this small quantity of silk (0.037294 of a cubic inch) occupies is 2.05755 cubic inches; hence it appears that, since 0.037294 is to 2.05755 as 1 is to 54, the silk which I used in the experiment in question could not fill more than $\frac{1}{55}$ of the space in which it was confined.

The longer we meditate upon these investigations, the more we are struck by the importance of the results that follow from them. I have never been able to explain them without rejecting altogether that hypothesis according to which it is supposed that the heat which may be in the air is communicated directly from one particle of this fluid to another.

My researches on the propagation of heat in liquids are sufficiently well known.* From them it has probably been seen how and in what manner I was compelled by the results of my numerous experiments to adopt the opinion with regard to this subject which I have developed in my various writings.

I have examined with the greatest care the objections which have been offered to the deductions which I have drawn from my experiments, and I can assert with truth — and to say this is a duty I owe to myself — that neither in these objections nor in the result of any new experiment, as far as my knowledge extends, has the least thing occurred which could serve as a reason for altering my opinion in regard to this subject. In a paper which I sent last year to the Royal Society at London,† I think that I have proved that water is really a non-conductor of heat, as I suspected six years ago.

I have now only a few words to say in addition, about the various experiments which I made at different times, to enable me (if it were in any way possible) to answer decisively that important and much contested question as to the materiality of heat, about which philosophers have striven for so long a time.

Those who regard heat as a substance must, of necessity, assume that it possesses weight. If now the

* A detailed description of my investigations in regard to this interesting subject is contained in my seventh Essay, which appeared in London in the year 1797, in two parts, together 188 octavo pages. See also Vol. I. p. 239.

† See p. 274.

heating of a body is caused by the accumulation of this substance in the body, it follows naturally that the body must be heavier when it is warm than when it is cold. Some natural philosophers have sought to determine this point; I feel confident, however, that no one has made more decisive experiments in this direction than myself.*

I was provided with excellent instruments, and spared neither trouble nor expense to arrive, by means of my experiments, at a certain and convincing result. The results obtained are, in few words, as follows.

I had a ball of very fine gold made, and weighed it when perfectly cold, and again after heating it to such a temperature that it was on the point of melting. Further, I weighed a considerable amount of water, which I had sealed hermetically in a flask, first in its liquid state, then at the temperature of melting ice, then as actual ice, and then again at its original temperature. All these experiments convinced me that the weight of a body is not changed in the least by heat.

Now although, as a consequence of the results of these experiments, I was only still more strengthened in those doubts which a number of other natural phenomena had raised in my mind with regard to the existence of caloric, still I saw at the same time only too well that the essential point of the controversy was far from being decided thereby. The defenders of caloric would still object (as they have actually done) that this substance is far too subtile to be weighed upon our ordinary balances.

* A paper in which are described in detail all my experiments upon this subject may be found in the Philosophical Transactions for 1799. See also page 1 of this volume.

After I had long meditated upon a way of putting this interesting problem entirely out of doubt by a perfectly conclusive experiment, I thought finally that I had discovered it, and I think so still.

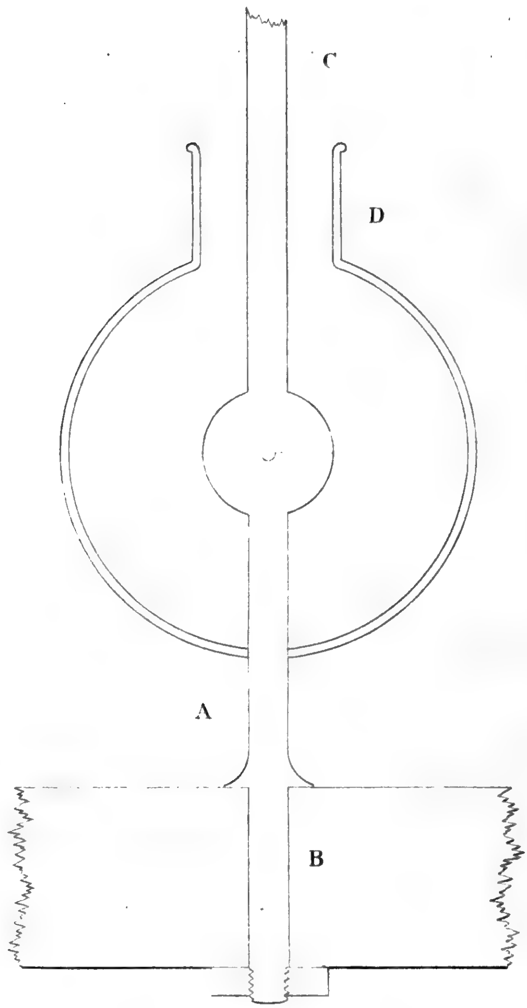
I argued that if the existence of caloric was a fact, it must be absolutely impossible for a body or for several individual bodies, which together made one whole, to communicate this substance continuously to various other bodies by which they were surrounded, without this substance gradually being entirely exhausted.

A sponge filled with water, and hung by a thread in the middle of a room filled with dry air, communicates its moisture to the air, it is true, but soon the water evaporates and the sponge can no longer give out moisture. On the contrary, a bell sounds without interruption when it is struck, and gives out its sound as often as we please without the slightest perceptible loss. Moisture is a substance; sound is not.

It is well known that two hard bodies, if rubbed together, produce much heat. Can they continue to produce it without finally becoming exhausted? Let the result of experiment decide this question.

It would be too tedious to describe here in detail all the experiments which I undertook with a view of answering in a decisive manner this important and disputed question. They may be found in my memoir On the Source of Heat excited by Friction. I have had it printed in the Philosophical Transactions for the year 1798; still these experiments bear too close a relation to my later researches on heat for me to omit attempting at least to give the reader a clear idea of the experiments and of their results.

The apparatus which I used in these investigations


C
D
A
B

is too complicated to be represented in this place;* still it will not be difficult for the reader, with the help of the accompanying figure (see Plate V.), to form a conception of the principal experiments and their results.

Let A be the vertical section of a brass rod which is an inch in diameter and is fastened in an upright position on a stout block, B; it is provided at its upper end with a massive hemisphere of the same metal, three and a half inches in diameter. C is a similar rod, likewise vertical, to the lower end of which is fastened a similar hemisphere. Both hemispheres must fit each other in such a way that both the rods stand in a perfectly straight vertical line.

D is the vertical section of a globular metallic vessel twelve inches in diameter, which is provided with a cylindrical neck three inches long and three and three-quarters inches in diameter. The rod A goes through a hole in the bottom of the vessel, is soldered into the vessel, and serves as a support to keep it in its proper position.

The centre of the ball, made up of the two hemispheres which lie the one upon the other, is in the centre of the globular vessel, so that, if the vessel is filled with water, the water covers the ball as well as a part of each of the brass rods.

If now the hemispheres be pressed strongly together, and at the same time the rod C be turned, by some means or other, about its axis, a very considerable quantity of heat is generated by means of the friction which takes place between the flat surfaces of the two hemispheres.

* See Vol. I., Plate to p. 493.

The quantity of the heat excited in this manner is exactly proportional to the force with which the two surfaces are pressed together, and to the rapidity of the friction. When this force was equal to the pressure of ten thousand pounds, and when the rod was turned with such rapidity about its axis that it revolved thirty-two times a minute, the quantity of heat generated by the continual rubbing of the two surfaces together was extraordinarily great. It was equal to the quantity given off by the flame of *nine* wax-candles of moderate size all burning together.

The quantity of heat generated in this manner during a given time is manifestly the same, whether the globular vessel D is filled with water, and the surfaces of the two hemispheres rub on each other in this liquid, or whether there is no water in the vessel, and the apparatus by which the friction is produced is simply surrounded by air.

The source of the heat which is generated by this apparatus is *inexhaustible.* As long as the rod C is turned about its axis, so long will heat be produced by the apparatus, and always to the same amount.

If the globe-shaped vessel D is filled with water, this water becomes hotter and hotter, and finally begins to boil. I have myself in this way boiled a considerable quantity of water.

If this experiment is performed in winter when the temperature of the air is but little above the freezing-point, and if the vessel D is filled with a mixture of water and pounded ice, the quantity of heat caused in a given time by the rubbing together of the two surfaces can be expressed very exactly by the amount of ice melted by this heat.

Since the apparatus affords heat continuously, and always to the same amount, we can melt in this way as much ice as we please.

But whence comes this heat? This is the contested point, to determine which was the real aim of the experiment.

It is certain that it comes neither from the decomposition of the water nor from the decomposition of the air Various experiments on this point, which I have described at length in my memoir in the Philosophical Transactions, are more than sufficient to establish this fact beyond doubt.

Just as little does it come from a change in the capacity for heat brought about by friction in the metal of which the hemispheres are composed. This is shown, first, by the continuance and uniformity of the production of the heat; and, secondly, by an experiment bearing directly on this point, by which I am convinced that not the slightest change had taken place in the capacity of the metal for heat.

Just as little does it come from the rods which are attached to the hemispheres, for these rods were always warm, the hemispheres communicating heat to them.

Much less could this heat come from the air or the water immediately surrounding the hemispheres, for the apparatus communicated heat to both these fluids without cessation.

Whence, then, came this heat? and what is heat actually?

I must confess that it has always been impossible for me to explain the results of such experiments except by taking refuge in the very old doctrine which rests on the supposition that heat is nothing but a vibratory motion taking place among the particles of bodies.

A bell, on being struck, immediately gives forth a sound, and the oscillations of the air produced by these vibrations forthwith cause a quivering motion in those bodies with which they come in contact. On the other hand, a sponge filled with water cannot give off its moisture to the bodies in its vicinity for any length of time without itself losing moisture.

A very illustrious philosopher, for whom I have always entertained the greatest respect, and whom, moreover, I have the good fortune to count among my most intimate friends, M. Bertholet, has, in his admirable *Essai de Statique Chimique*, attempted to explain the results of this investigation, and to reconcile them with that theory of heat which is founded upon the hypothesis of caloric.

If a man as learned, as honest, as worthy, and as renowned as is M. Bertholet, spares no pains in opposing the errors of a natural philosopher or chemist, one cannot and dare not keep silence unless he wishes to acknowledge himself vanquished. If, however, one can produce proofs — a fortunate thing for all those who find themselves driven to similar self-vindication — that the objections of M. Bertholet have no foundation, he has done very much towards establishing beyond doubt the opinions and facts in question.

I will now endeavour to answer the objections which M. Bertholet has offered to my explanation of the above-mentioned experiments; and, that the reader may be in a position to give to these objections their just value, I will insert them here in the writer's own words.

"Count Rumford has made a curious experiment with regard to the heat which may be excited by friction. He causes a blunt borer to revolve very rapidly (*this borer revolved about its axis only thirty-two times a minute*) in a brass cylinder weighing thirteen pounds, English weight (*the cylinder weighed one hundred and thirteen pounds and somewhat more*), and says that he observed that this borer in the course of two (*one and a half*) hours, and under a pressure equal to 100 cwt., reduced to powder 4145 grains ($8\frac{1}{2}$ ounces Troy) of brass, and that an amount of heat was generated during this operation sufficient to bring to boil 26.38 pounds of water, previously cooled to the freezing-point. He asserts that he did not discover the slightest difference between the specific heat of the metallic dust and that of the brass which had not experienced the friction. Hence he supposes that the heat was excited by the pressure alone, and was not at all due to caloric, as is the opinion of most chemists.

"I will for the present satisfy myself with simply inquiring whether it necessarily follows from this experiment that we must renounce entirely the received theory of caloric, according to which it is regarded as a substance which enters into combination with bodies, or whether this result cannot be explained in a satisfactory manner by applying to the case in question those laws of nature in accordance with which the operations of heat are manifested under other conditions.

"If the evolution of heat be regarded as a consequence of the decrease of volume caused by the pressure, then not only the metallic powder but also all the rest of the brass cylinder must have contributed, though not in an equal manner, to this evolution, by the powerful expansive effort of that portion which experienced the greatest pressure, and consequently acquired the greatest temperature, without being able to assume the dimensions proper to this same temperature on account of the less heated and less expanded parts; consequently there must have arisen, necessarily, a certain condensation of the metal in respect of its natural dimensions, which condensation gradually decreased from the point where the pressure was greatest to the surface. We may suppose that this operation took place in a similar manner in all parts of the cylinder.

"As a consequence of this decrease of volume, an amount of caloric was given out equal to that which would have caused a similar increase of volume, on the supposition, that is, that the specific heat of the metal does not change through this range of the scale of the thermometer, and that the expansions are equal; and this, considering the range of temperatures and the consequent expansions, is probably not far from the truth. The entire amount of heat disengaged would have raised the cylinder to about 180° of Reaumur's scale; and if the expansion of brass by heat is equal to that of iron, which has been found to be $\frac{1}{75000}$ for each degree of the thermometer, the 180 degrees would have caused an expansion of $\frac{18}{7500}$ in each direction, and the decrease of volume must have brought about the same degree of heat if we suppose that the pressure stood in equal relation to this expansion.

"Now there is a change, and sometimes a very considerable one, wrought in the specific gravity of a metal, by percussion, by the action of a fly-wheel, or by the compression of a wire-drawing machine. It appears, for example, that the specific gravity of platina and of iron, on being forged, is thus increased by a twentieth part.

"Hence it appears that the experiment of Count Rumford is far from explaining satisfactorily a property which is well known, and called in question by no one.

"It is easy, it is true, to arrange side by side in an imposing manner the phenomena of heat; if, however, you were to say to one who has little or no knowledge of chemical speculations, 'Count Rumford's cylinder has, in the course of two hours, by means of a violent friction, afforded all the heat required to dissolve in water, without changing its temperature, 15 kilogrammes of ice, or as much as 2 hectogrammes ($6\frac{1}{2}$ ounces) of oxygen would require [*sic*] in its combination with phosphorus,' I do not know at which of these phenomena he would be most astonished.

"The slight changes which can take place in the amount of combined caloric have so inconsiderable an influence on the capacity for work of the caloric within the narrow limits of the thermometric scale, that it cannot be computed. Moreover, we have not, as yet, adequate data for determining the nature of the changes in this respect which take place in a solid body in consequence of the particular condition of condensation into which it has been brought by means of a certain mechanical force, and by degrees of heat differing greatly from each other.

"Besides, Rumford, in the experiment to determine the specific heat of the filings of bell-metal thus obtained, heated them to the temperature of boiling water. But this extremely elastic metal would very naturally as soon as left to itself, and especially during the operation just mentioned, resume that state of expansion and that capacity for heat which is proper to it at a given temperature, so that the effect of the pressure to which it has been subjected partly disappears again, just as a piece of metal which has been hammered resumes its natural properties on being annealed."

In reply to these remarks, I will call to mind what follows.

1st. The discovery which I made, that no considerable change had taken place in the specific heat of the metallic dust produced by the friction, led me in no way to the supposition that the heat excited in the experiment could not come from the caloric set free. I only found that the source of this heat was inexhaustible. To explain this phenomenon, which has never yet been explained, is the point now in question, and I do not see how it can be explained except by giving up altogether the hypothesis adopted in regard to caloric.

2d. If we actually suppose (and it is far from having been proved) that the simple pressing together of a metal is sufficient to expel the caloric contained in it, still the explanation of such a natural phenomenon would be advanced little or none; for since the action of the force which causes the pressure is continuous, the condensation of the metal brought about by this force would in a short time reach its maximum; and if really in this operation ever so much caloric had been disengaged from the metal, still it would very soon disperse. The rubbing surfaces, on the contrary, continue to give forth heat, and that always to the same amount.

3d. In regard to the objection made to the experiment which was undertaken with a view of determining whether a change had taken place in the capacity of the metallic dust for heat, this can very readily be answered, and in such a way that nothing, it seems to me, can be said against it. If the temperature of boiling water were really sufficient to give to these small, forcibly condensed particles of metal the quantity of heat necessary to bring them back to their original condition as far as their capacity for heat is concerned, then, as the water by which the apparatus was surrounded finally began to boil, they must, without doubt, have taken the necessary amount of heat from this water. If, now, these particles of metal received finally from the water the caloric which in the beginning they imparted to it, the question arises, whence came the caloric which served to heat, not only the water, but also the metal and the objects immediately surrounding it?

I am far from desiring to deceive any one by an imposing arrangement of facts; but the facts in my experiments were so very striking that it was altogether impossible for me to help instituting comparisons and making calculations with regard to them which would make them clear, especially to those not yet sufficiently acquainted with such investigations.

I will now close my remarks with an entirely new computation. I will show whether it is probable that the metal could supply all the heat which was produced by friction in the experiment in question. If we are to make this supposition, we must, in the first place, allow that all the heat came directly from the particles of metal which were separated from the solid mass of metal by the friction; for, since the mass remained in the same condition throughout the entire experiment, it is evident that it could contribute in no measure to the effect produced.

We will now inquire how much heat would have been developed if the experiment had been carried on without cessation, until the whole mass of metal had been reduced to powder by the friction.

After the experiment had lasted an hour and a half, there were 4145 grains (Troy) of the metallic dust, and during that time an amount of heat was produced by the friction sufficient to raise 26.58 pounds of ice-cold water to the boiling-point.

Since the mass of metal weighed 113.13 pounds, or 791,910 grains, all this metal would have been reduced to powder if the experiment had lasted uninterruptedly, day and night, for 477½ hours, or for 19 days 21½ hours, and during this time an amount of heat would have been produced sufficient to have raised 5078 pounds of water to the boiling-point.

Since the metal used in this experiment showed a capacity for heat which was to that of water as 0.11 to 1, it is evident that this amount of heat would have been sufficient to raise a mass of the same metal 46,165 pounds in weight through 180 degrees of Fahrenheit's scale, or from the temperature of melting ice to that of boiling water.

This amount of heat would be sufficient to melt a mass of metal sixteen times heavier than that which I used in the experiment.*

* Brass melts at a temperature of 3807° Fahrenheit; copper at 4587°; bell-metal melts more easily than copper; if, however, we suppose that it requires the same heat for fusion, we find by a very simple calculation, that the amount of heat necessary to raise the temperature of 46,165 pounds bell-metal through 180 degrees would be sufficient to raise the temperature of 1811½ pounds through 4587

Is it at all conceivable that such an enormous quantity of caloric could really be present in this body? But even this supposition would be by no means sufficient for the explanation of the fact in question, as I have shown by a decisive experiment that the capacity of the metal for heat has not sensibly altered.

Whence, then, came the caloric which the apparatus furnished in such abundance?

I leave this question to be answered by those persons who believe in the actual existence of caloric.

In my opinion, I have made it sufficiently evident that it was impossible for it to come from the metallic bodies which were rubbed together, and I am absolutely unable to imagine how it can have come from any other object in the neighbourhood of the apparatus, for all these objects received their heat constantly from the apparatus itself.

I will now proceed to give an account of my further investigations on the subject of Heat.

In the summer of the year 1800, I visited Scotland, and on this occasion spent some months in Edinburgh.

It is well known that the University at that place stands in high repute on account of the eminent scholars occupying chairs there for more than fifty years in uninterrupted succession.

One day I found myself in the company of Professor Hope (the successor of the celebrated Black), Professors Playfair and Stewart, and several other persons. We repeated the experiment which Pictet undertook with a view to determine the condensation and contraction

degrees, or to bring this number of pounds to the melting-point. From this calculation it appears that a quantity of bell-metal, the temperature of which is at the melting-point of ice, on being reduced by friction to the state of powder, gives out sixteen times as much heat as would be necessary to melt it.

of air by the cooling influence of cold bodies. It now happened that for the first time my opinion on the subject of heat was publicly announced.

Two metallic mirrors fifteen inches in diameter, with a focal distance of fifteen inches, were placed opposite each other, sixteen feet apart. When a cold body (for example, a glass bulb filled with water and pounded ice) as was the case on this occasion, was placed in the focus of one of the mirrors, and a very sensitive air-thermometer was placed in the focus of the other mirror, the latter thermometer began immediately to fall. If, instead of being placed directly in the focus, the thermometer was removed a short distance from it to one side, the cooling power which in the former case the cold body had exerted upon it was no longer perceptible.

The matter was, however, not allowed to rest with merely repeating the experiment of Pictet just as he describes it, but I was allowed, in addition, to make various changes, that I might lay aside every doubt, and elucidate in the most convincing manner the fact in question.

I expressed my opinion on the results of these experiments in the following words: —

"It is not possible that caloric has an actual existence. The communication of heat and the communication of sound seem to be completely analogous. The cold body in one focus compels the warm body (the thermometer) in the other focus to *change its note.*"

It is owing to a peculiar circumstance, the further discussion of which would be neither appropriate nor useful in this place, that I here introduce word for word the expression which I used on this occasion.

A considerable time before, I had already projected a series of experiments on the subject of radiant heat, and in my sixth Essay, which treats Of the Management of Fire and the Economy of Fuel, published at London in 1797, I had openly announced my purpose of taking the work in hand as soon as possible.

The experiments I have just mentioned as being performed in my presence by Professor Hope determined me not to put off this intention of mine a moment longer.

As soon as I returned to London, I began immediately to make all preparations for my researches. I therefore communicated my intentions to Sir Joseph Banks, at that time President of the Royal Society, also to Mr. Cavendish, because both these gentlemen (as well as myself) were managers of the Royal Institution. As I wished to carry out my experiments in the most decisive manner, and consequently with the apparatus as perfect as possible, — which to all appearance would require a considerable outlay, — I was, at my request, authorized by the managers of the Royal Institution to procure the new instruments needed at the expense of the Institution, with the condition, however, that these instruments should remain at the Institution as its property, and be kept in its cabinet.

As the principal object in this investigation was to establish beyond doubt the cooling emanations from cold bodies, I desired to accumulate the emanations and concentrate them as much as possible, in order, that their action might be so much the more sensible.

Pictet took for his experiment, as is well known, two metallic reflectors, and placed a cold body in the focus of one of them, and a thermometer in the focus of the other. That the cooling influence which the cold object exerted on the thermometer might be doubled, I proposed in my experiment to have two cold bodies and one reflector, and, in order to increase so much the more the cooling effect on the thermometer, I intended to place it in the upper part of an open cylindrical vessel, the two cold bodies, however, being placed somewhat lower.

In Pictet's experiments both reflectors were in a horizontal line, and the thermometer on which the cooling influences were exerted was continually heated by the vertical current from the air above, which was caused necessarily by the cooling of the stratum of air immediately surrounding the thermometer; as a consequence, the frigorific influence of the cold body was lessened by the calorific influence of this current to such an extent that an equilibrium resulted. Still I expected that in my case I should, in all probability, be able to carry the cooling of the thermometer still farther, as I hoped by the arrangement of my apparatus to prevent this current, and at the same time to double the cooling effect.

After long delay on the part of the workmen, the necessary mirrors, four in number, were finally completed. They are now in the physical cabinet of the Royal Institution at London, and are used in the annual lectures on physics. If I am not mistaken, there are several other instruments kept in the same place, which I had expected to use in the projected experiments on the radiation of bodies; but most of the instruments designed for this investigation (made by Mr. Fraser, New Bond Street) were made at my own expense and are still in my possession.

It is only necessary to see this apparatus, which I had made in the summer of 1801, to be immediately convinced that I pursued my researches on the subject of heat zealously and connectedly.

In the beginning of the year 1802 I was recalled to Bavaria. I was, therefore, obliged to leave London in the early part of the month of May, after I had actually begun on a very few only of the experiments which I had planned with so much pains. But as I was firmly resolved to devote myself to them again, as soon as I could obtain any leisure, however little, I took back to Germany with me the greater part of this apparatus which I had procured during my stay in England.

During my journey, I remained three months at Paris, so that I did not reach Munich before the end of August. In the early part of the month of October, however, I began my experiments.

As I had not been able to bring with me from London the four large reflectors belonging to the Royal Institution, and as I could not procure similar ones in Bavaria, I was obliged to change the plan of my investigations, and to try whether it might not be possible to discover the radiation from bodies in some other way, and to make the effects of these radiations manifest without the aid of the concentration brought about by means of the metallic reflectors.

In the first experiments which I undertook, I had this object in view, to determine whether the invisible heating rays which a warm body (a heated stove, for example) gives out are not of the same character as those coming from the sun. For this purpose I procured three cylindrical boxes of very thin, soft wood, precisely alike, four and a half inches in diameter, three inches high, and open above. In each of these boxes, an inch and a quarter from the bottom, I put a circular metallic disk, a quarter of a line in thickness, and of the same diameter as the inside of the box. This disk, which formed a sort of optical screen in the inside of the box, was fastened in its place by a number of very short wooden pegs, which went through the side of the box.

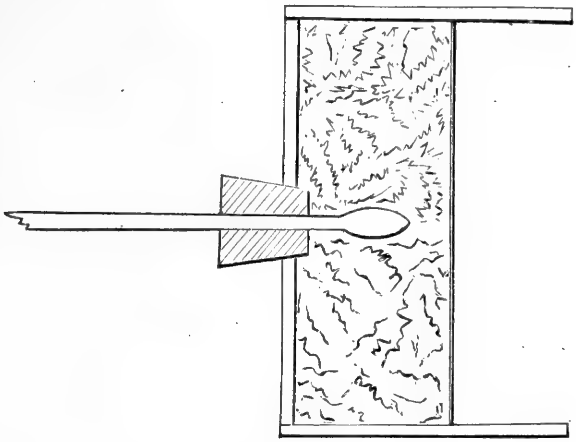
In the middle of the bottom of the box was a circular aperture, three quarters of an inch in diameter, closed by a cork stopper.

In this stopper was a hole of three lines' diameter, into which fitted a small mercurial thermometer provided with an oval reservoir. The divisions of the scale were engraved upon the tube itself.* By means of this stopper the thermometer was introduced into the inside of the box in such a way that its bulb was situated in the axis of the box, and in the middle of the space between the bottom and the metallic disk. This space, which was designed to serve as a reservoir of heat, was filled with a certain quantity of flat silver threads, which had been picked out of old silver lace.

In one box the metallic disk or reflector was brass; in the second, tinned iron; and in the third, ordinary sheet-iron.

* I had four such thermometers made for me in England, and they did me good service throughout the whole course of my experiments on the subject of heat. Their tubes were made of very hard glass, three lines in diameter, and were polished down on one side so as to present a flat surface, on which the divisions of the scale were etched with fluoric acid. The tubes are six or seven inches long, and the bulbs for the mercury are pear-shaped, and consequently not so liable to get broken as cylindrical ones. In the pointed or lower part of the pear, the glass can be quite thick without any disadvantage.

The accompanying figure represents the vertical section of one of these boxes in a horizontal position. The stopper is also shown by diagonal lines, and a part of the thermometer in its proper place.



In order to diminish the loss of heat which might take place through the bottom and the sides of the box, each one was covered inside and outside with well-sized paper, then coated three times with copal varnish, and, in addition to this, they were covered during the experiment with an envelope of fur.

When one of the boxes was placed for a certain length of time in the sun, so that its rays fell vertically upon the metallic disk, there was a certain amount of heat excited in the same; and, as this heat was evenly distributed within the box by means of the metallic threads, it was possible to observe very exactly the various degrees of heat by means of the thermometer; if all three boxes were placed at the same time in the sun, it was possible to determine with certainty the relative amounts of heat excited at the surface of the three different metals used in the experiment.

I was not at all surprised to find that the rays of the sun excited more heat in a given time on the black and unpolished iron disk than on the other two disks, which were bright and polished; I was, however, all the more astonished by an entirely unexpected circumstance which I noticed by chance during the cooling of the instruments which had just been heated by the sun, a circumstance which arrested my attention.

After I had placed the three boxes close together, and had exposed them to the influence of the sun's rays until each had reached its maximum temperature, I took them away from the window at which they had been standing at the time, and put them, bottom upwards, on a cold table in a corner of the room.

As I happened, about a quarter of an hour later, to go past the table, I cast a single glance at the thermometers, which now, in a vertical position, projected from the reversed boxes. To my no slight astonishment, I saw that the box which before contained the most heat (the one which had the iron disk) was now the coldest of all.

This phenomenon surprised me so much the more, as I was convinced that this rapid cooling could not be due to the fact that this box did not have sufficient room to take up just as much heat as the others. For, as I very well knew how much in these experiments depended upon the boxes being precisely alike as regards their contents, I had taken the greatest pains by similar distribution of the silver threads to arrange them alike before I began my experiments.

I cannot allow myself here to give in detail all the conjectures and projected experiments of which this discovery was the cause. I will, therefore, say nothing further except that it made a firm and lasting impression on my mind, and afterwards exerted much influence on the manner in which I carried on my inquiries.

Meanwhile I did not allow this occurrence to hinder me in the least from carrying out to the end the experiments for which I had devised my apparatus. I had, therefore, a cylindrical iron stove put into the middle of a large room, and having surrounded it with fire-screens, I caused all the windows in the room to be opened. When the stove was sufficiently heated, I found that no sensible change had taken place in the mean temperature of the room. I now removed the screens which surrounded the stove, and placed all three of the boxes at the same time in the same position, that is, twenty-four inches from the stove.

The box containing the iron disk, which previously had contained the most heat after standing in the rays of the sun, was also now the warmest after being subjected to the influence of the rays which proceeded, although invisibly, from the stove.

In order to become more closely acquainted with these rays, I had several new instruments constructed; among others, four large air thermometers, and three other thermometers of the same size filled with spirit of wine. The bulbs of these thermometers were an inch and three quarters in diameter, and contained either various substances mixed with air or else simply spirit of wine. The bulb of the first thermometer was filled with air alone; in the second was a mixture of air and eider-down; in the third a mixture of air and very thin flat silver threads; the fourth contained air, eider-down, and, at the same time, flat silver threads.

In the bulb of the first of the thermometers filled with spirit of wine, there was nothing besides this fluid, in the second there was a mixture of spirit of wine and eider-down, and in the bulb of the third thermometer there was a mixture of spirit of wine and flattened silver threads.

If, then, I exposed these thermometers in turn, now to the influence of the rays of the sun, and again to the influence of rays coming from bodies warmed by the fire, I could, from the rapid or gradual heating of the different thermometers, arrive at a sufficiently just conclusion with regard to the identity of the radiations or to the difference between them.

It would be too tedious if I were to describe these experiments here in detail. From some of them I obtained many, in certain respects, very remarkable results,* which allowed me to draw such conclusions as pointed out clearly enough the way in which I must proceed towards the chief object of my researches.

I afterwards procured other thermometers of very large size. Their bulbs are round, and are made of copper; they are four inches in diameter. Their tubes, which are glass, are thirty inches long, and are filled with linseed oil. I use them in experiments designed to determine the relative rapidity with which a warm body (the thermometer itself) cools in different liquids having the same temperature. This instrument is the

* I noticed, among other things, that the thermometer whose bulb contained a mixture of spirit of wine and flat silver threads was much more sensitive in general, and especially to very slight changes of temperature, than another thermometer of the same size, the bulb of which contained only spirit of wine. It would, perhaps, be of advantage to procure similar thermometers to use, if not ordinarily, at least on certain occasions. I am firmly convinced that a thermometer whose bulb is filled with mercury and platina cut into threads will be much more sensitive, that is, will indicate the temperature much more quickly, than a thermometer of the same size filled, as is usual, with mercury alone.

only one that I could ever devise for such experiments without fearing important objections on account of the apparatus employed.

I possess, also, various other thermometers, intended simply to receive and collect within themselves the calorific or frigorific rays which fall upon their surfaces. The reservoir of each consists of two cones of very thin sheet brass, which lie one within the other, and are fastened to each other, on the under side, in such a way that there is an empty space, not quite a line in width, between the inner surface of the outer cone and the outer surface of the inner cone. The inner cone is four inches in diameter, four inches high, and ends in a point above. The diameter of the outer cone is four and a quarter inches, and it ends above in a cylindrical tube three quarters of an inch in diameter and four inches long. In this cylinder is fixed a glass thermometer tube, and if the space between the two cones be filled with linseed oil or coloured spirit of wine, this instrument answers the same purpose as an ordinary thermometer. The scale of this thermometer is fastened firmly to this glass tube.

The outer wall of the instrument is shielded and protected from the calorific or frigorific influences of the surrounding air by means of a cylindrical box of dry wood, thickly coated with varnish, and filled with eider-down; into this the body of the instrument fits. This cover is four and a half inches high, and is, on the inside, of the same diameter as the lower part of the outer cone; and the tube of the instrument, with its attached scale, goes through a hole made in the bottom of the box.

If, now, the outer blackened surface of the inner cone be held in the neighbourhood of and towards an object which is giving off calorific (or frigorific) rays, the heat (or cold) caused by these rays is communicated to the fluid contained in the space between the two cones, and this change of temperature brings about a corresponding change in the level of the liquid in the upper part of the tube; by this means the amount of heat (or cold) communicated can be estimated and measured.

An instrument of this description, which I procured in the year 1801, during my stay in England, is at present in the physical cabinet of the Royal Institution at London. Two similar ones, fitted up in Bavaria, are still kept in my cabinet at Munich. I have described this instrument thus minutely, simply because I am convinced that it is of very great service in experiments on the calorific and frigorific radiations from various bodies, and because it has been my earnest desire to induce natural philosophers to devote their attention to this subject, so worthy of investigation.

It only remains for me to say a few words in regard to the experiments which I have described very fully in the memoir read before the Royal Society on the 3d of February, 1804, which has been translated into French by Professor Pictet.*

I performed these experiments in Munich, in 1803, during the months of January, February, and March. According as the results seemed of importance, I immediately acquainted my friends in England and France with them. Among others, I communicated to Sir Joseph Banks, then President of the Royal Society of London, the very striking results of an experiment

* See page 23.

which I made, on the 11th of March, with two metallic vessels, both of which — one being naked, and the other having a covering of linen — I allowed to cool, exposed to the air, after having first filled them with warm water. In addition to this, I wrote him that I had made several experiments with various vessels *blackened* and *covered with repeated coatings of varnish*, and I announced the results obtained. I also informed him of the discovery which I made, with the help of my *thermoscope*, that different bodies of the same temperature give out very different quantities of calorific rays, and that frigorific rays have just as real an existence as the calorific rays from warm bodies.

Since Sir Joseph showed my letters to various persons, and since I did not keep my experiments or their results a secret from him or from any one else, my discovery was publicly mentioned in London even as early as the spring of the past year. As an incontrovertible proof of this fact, I can bring forward a letter from a friend of mine (to whom I had not mentioned my new discovery in any way), in which he congratulates me on the success of my researches, and informed me at the same time that he had learned what he knew with regard to my discoveries from Mr. Davy, a Professor in the Royal Institution, who had spoken publicly of them in his lectures on chemistry.

The memoir in which I gave an account of my investigations was finished early in May (1803); in the early part of June I left Munich for a journey into Switzerland. As I intended to proceed from Geneva to Paris, I took with me my memoir and some of my newly invented instruments, and among others the thermoscope.

On reaching Geneva, in August, I read my memoir in the presence of Professor Pictet, De Saussure, and various other persons, and at the same time repeated some of my experiments with the thermoscope.

As soon as I reached Paris, in the latter part of October, I had my memoir copied (by Mr. Cadel, of Glasgow, who was then in Paris), and sent it, in the middle of December, to London by the younger Mr. Livingston, who was kind enough to deliver it in person to Sir Joseph Banks on the 23d of December. As the Christmas recess of the Royal Society begins just after this time, my memoir could not be read in a public meeting of the Society until the 3d of February.

The 6th of June there were sent to me from London (through the elder Mr. Livingston, Minister Plenipotentiary of the United States of North America at Paris) two copies of my memoir, published by order of the Royal Society. At the same time I received a letter from Mr. Davy, Professor of Chemistry at the Royal Institution, in which he informed me that Mr. Leslie had, a short time previously, published a memoir on heat, and that in it he had described various experiments which bore a resemblance to some which I had performed.

The 2d of June I received at Paris, from M. Bertholet, Mr. Leslie's book, which was sent to me by Sir Joseph Banks. M. Bertholet had at the same time received from England a copy of the work, which was sent to him by one of his friends there.

As I had, only a short time before, occupied the attention of the National Institute with an account of my recent researches and discoveries,* the appear-

* Between the 19th of March and the 7th of May, 1804, I presented to the Na-

ance of a book coming from England, and containing a description of a number of experiments and discoveries in many respects not dissimilar to my own, could not fail to create a certain feeling of surprise among the philosophers of Paris, as I could plainly enough perceive. I find myself, therefore, compelled, although against my will, to explain as far as possible an occurrence which it is highly important for me should appear in its true light.

I am far from intending to assert that Mr. Leslie had any knowledge of those experiments of mine which bore a resemblance to those which he announced publicly in print. It is, however, equally certain that I did not know, and could not have known, the least thing about his. It will not be difficult for me to prove this.

It might, perhaps, be just as easy for Mr. Leslie to bring forward proofs that he knew absolutely nothing about my experiments. This would be all the more readily believed as he (in the course of certain remarks made in a note with regard to the observations which I offered in explanation of the propagation of heat in liquids) speaks of me as of a man *already dead* * at the time when he made these remarks.

It is certain that we are perfect strangers to each other, that we do not know each other even by sight, and that we never had any sort of correspondence with each other.

As regards the *priority of the public announcement* of our discoveries, this point can be easily made clear by

tional Institute five different memoirs on this subject. They will probably be printed in the "Mémoires de l'Institut."

* See the thirty-ninth note at the end of his work, beginning with the following words: "*A late* ingenious experimenter."

the statement of certain facts which do not admit of doubt.

It is true that I cannot determine with any great accuracy the time when Mr. Leslie's book first saw the light; it cannot, however, possibly have been published before the middle of May of this year, for the dedication is dated at Largo, in Fifeshire (Scotland), the 20th of May, 1804. This would be, consequently, nearly a year after the time when the most remarkable results of my investigations were known in London; it would be nine months from the time when, in Geneva, I read the memoir containing the circumstantial and detailed account of these investigations in the presence of a number of celebrated philosophers; it would be five months later than the time at which this memoir was placed in the hands of the President of the Royal Society of London; and it would be more than a quarter of a year from the time at which it was read publicly before this Society.

Still the *priority* in question, considered in and by itself, is of such slight importance that I should not have mentioned it at all, were it not that the facts which go to establish it tend at the same time to strengthen a far more important assertion, namely, that I am actually the *discoverer* of what I announced as discoveries.

If Mr. Leslie and myself, the one in Scotland, the other in Bavaria, each for himself and at about the same time, did actually make the same discoveries, this is a condition of things which has already happened more than once before our time; and then, as far as the interpretation of these phenomena is concerned, we differ from each other in our mode of explanation to such an extent that there can no question arise between us in regard to the ownership of our opinions. Nothing is more certain than that, in this respect, we have not borrowed one from the other in the slightest degree.

Besides, I have every reason for believing that even if I had not described so particularly the facts which I have brought forward, still all those who will take the trouble to consider impartially the numerous experiments on the subject of heat which I have made during more than twenty years, will be convinced that I must have been led to the investigations and discoveries in question by the entirely natural connection of ideas caused by my opinions on the subject, without needing to borrow, in the slightest degree, from any person whomsoever.

To close this historical review of my various researches on the subject of heat, I will give a very brief account of my labours in this connection, from my arrival in Paris, until the close of the month of October of the last year (1803).

As I had brought with me two thermoscopes, I had them adjusted, by Dumontier, with all possible care; I also sent to Munich for several other instruments which I had used, the year before, in my experiments on heat.

I also procured several new instruments, in order to make new experiments; among others, an apparatus which I intended to use to determine the progress of heat in a massive bar of metal, in glass, and in other solid substances. All these instruments I showed to several members of the National Institute, namely, to MM. Laplace, Delambre, Prony, and Biot.

To these philosophers, and at the same time to M. Bertholet as well, I proposed to perform the now well-known experiment of the cold body and the speaking-tube (and this was before the instrument necessary for the purpose had been invented), and thus to terminate our (in all respects very friendly) controversy on the reality of caloric.

This experiment was afterwards performed in the physical cabinet of the National Institute in the presence of Laplace, Bertholet, and Charles. The result was precisely as I had predicted.

On the 28th Ventose of the year 12 (the 19th of March, 1804) I presented to the Mathematical and Physical Class of the National Institute my first memoir, in which I described my thermoscope and a few of the discoveries that I had made with the help of this instrument.

On the 5th Germinal (26th of March, 1804) I presented to the same Class a second memoir, in which I sought to develop my ideas on the nature of heat, as well as on the manner in which it is excited and communicated. At the same time I gave the results of certain experiments which I had made on the cooling of warm bodies in the air.

On the 19th Germinal (9th of April, 1804) I presented to the Class a third memoir, which treated of an experiment, which I had recently made in Paris, on the nature of heat; by this experiment the influence of the rays emanating from cooling bodies was rendered manifest in a manner entirely new.

On the 10th Floréal (30th of April, 1804) my fourth memoir was presented to the Class. In this memoir I described an experiment which I performed with two flasks of equal size. One was made of glass, the other of tinned iron. Both were filled with boiling water, and exposed at the same time to the air, in which they were allowed to cool. The water contained in the glass flask cooled twice as fast as that in the one made of tinned iron, although the walls of the latter were much thinner than those of the glass flask. This memoir ends with some considerations on the comparison which has been instituted between a warm body and a sponge filled with water, and on the influence of radiation during the warming and cooling of bodies.

On the 17th Floréal (7th of May, 1804) I laid before the Class my fifth memoir, in which I gave an account of an entirely new series of experiments, which I had made in Paris, on the manner in which heat is propagated in a massive bar of metal, six inches long ana an inch and a half in diameter. This bar was heated at one end by boiling water, and cooled at the other end sometimes with a mixture of pounded ice and water, and sometimes simply with water of the temperature of the air.

M. Biot, member of the Institute, made, at about the same time with myself, several successive experiments on the propagation of heat in metallic bars and other solid bodies. He, however, used for this purpose bars of a different length from mine, and higher temperatures. Otherwise we obtained the same results from our experiments.

He hit upon the fortunate idea of employing similar experiments for measuring very high degrees of temperature; such, for example, as is necessary in the preparation of porcelain, or for melting metals not readily fusible.

As I was invited to prepare a condensed description of my recent experiments on heat, to be read at the public sitting of the National Institute on the 6th Messidor (June 26, 1804), I presented the memoir which follows.* As it has already been printed (in the *Moniteur* of the 9th Messidor, or the 29th of June, 1804), I may be allowed to introduce it into this collection. The case is otherwise with the five other memoirs which I presented to the first class of the Institute; for as they will be embodied in the Mémoires of the Class it would not be proper for me to publish them earlier.

To complete this historical review, I must, in addition, say a word or two on my attempts to perfect the application of heat to the arts and to all sorts of domestic purposes. Among the fifteen Essays which I have published in three octavo volumes are no less than eight which treat of the use of heat. They are as follows: —

Essay IV. Of Chimney Fireplaces; VI. On the Management of Fire and Economy of Fuel; X. Of Kitchen Fireplaces; XII. Of the Salubrity of Warm Rooms in Winter; XIII. Of the Salubrity of Warm Baths, and the Mode of their Preparation; XIV. Of the Management of Fire in Closed Fireplaces; XV. Of the Use of Steam as a Vehicle for transporting Heat.

* See page 166.

[This paper is translated from the German, as it appears in Vol. IV. of Rumford's Kleine Schriften.]

EXPERIMENTS AND OBSERVATIONS

ON THE

COOLING OF LIQUIDS IN VESSELS OF PORCELAIN, GILDED AND NOT GILDED.

NOTHING affords more entertainment than to compare the processes of the common arts of life and the ordinary habits of the people in their household operations with the principles of the physical and mathematical sciences. This comparison often presents very curious points of resemblance, and leads sometimes to very important improvements.

In all countries where the daily use of tea has become common among the rich, teapots of silver are preferred to those of porcelain or earthenware, and the reason given for this preference is that the beverage when prepared in the former is of a better quality than when prepared in the latter. I was, for a long time, of the opinion that this idea was owing simply to prejudice, and without foundation; but, having discovered some years since that metallic vessels, when clean and bright on the outside, possess the property of causing warm liquids which are put into them to retain their heat for a very long time, I began to see that the preference in question might be the legitimate result of long experience, as is almost always the case with those preferences which in the end are universally adopted.

In order to throw light on this subject, which had several points of interest for me, I made the following experiment. I procured (from M. Nast, a celebrated porcelain manufacturer, of Paris) two vessels of porcelain, of the same shape and of the same dimensions, the one white, the other completely covered on the outside with gilding; into these vessels I put equal quantities (250 grammes, or a quarter of a litre) of warm water, and then allowed them to cool gradually in a large room free from currents of air, having placed them three feet apart on a table in the middle of the room.

Each of the vessels was closed with a cork stopper, and by means of a mercurial thermometer with a cylindrical bulb, fixed in the axis of the vessel in such a way that while the thermometer was inserted in the cork the scale remained on the outside of the vessel, I noted very conveniently the progress of the cooling without touching the vessel, and without even approaching it sufficiently near for the heat of my body to interfere sensibly with the operation of cooling.

The result of this experiment was as I had expected. The gilded vessel cooled much more slowly than the plain one. Starting at the same time with both vessels at the same temperature, if it took *half an hour* for the plain vessel to cool down through a certain number of degrees, *three quarters of an hour* were necessary for the gilded one to cool down to the same point.

This comparative experiment was repeated several times, and invariably with the same result; the gilded vessel always cooled more slowly than the plain one in about the proportion of 3 to 2.

The advantage that can be gained from this remarkable property, possessed by metallic surfaces, of resisting the cooling (or heating) action of surrounding bodies, is too evident to need much explanation. Since, in household economy, use is often made of porcelain vessels for holding warm liquids, which it is desired to keep warm for a long time, — as, for example, tea, coffee, etc., — in all such cases it would be of advantage to use vessels gilded on the outside; or, if gilding be found too expensive, it is possible to use, and with equal advantage as regards retaining the heat, vessels which are silvered or covered with a layer, no matter how thin, of any other metal not liable to be readily oxidized in the air.

As to gilding the vessels on the inside, it would be to no purpose, for it would add nothing to the effect in question, as I have learned from the results of several experiments. This, however, applies only to simple vessels; for in case a double vessel were employed in order to retain more effectually the heat of any substance, the outside vessel must be gilded on the inside as well as on the outside; in no case is it necessary for the inner vessel to be gilded on the inside.

If it is a question of preserving the low temperature of liquids or other cold substances, such as ice-creams, etc., in this case, also, vessels having externally a polished metallic surface should be used; for a surface of this description throws off by reflection a large portion of the calorific rays which reach it from surrounding objects, and consequently the vessel grows warm very slowly.

Everybody knows how much time it takes to bring water to boiling in a silver coffee-pot which is clean and bright on the outside, especially before an open fire, or on glowing coals which burn without smoke. It is, however, very easy to hasten materially the heating of the liquid in this case; all that is necessary is to begin by blackening the outside of the coffee-pot over the flame of a candle or of a lamp, or to destroy or conceal in some other way the metallic lustre.

All the facts which I have just detailed are easily explained, and, to my mind, satisfactorily, by the theory of heat developed in the various memoirs on this subject which I have had the honour of presenting to this Assembly at different times.

If heat is nothing but a vibratory motion of the particles of a body, — a motion which always exists in all bodies, but which has greater or less rapidity or intensity according to the temperature of those bodies, — and if a body which is warmer than those which surround it is cooled on being exposed to their influence, not because it has transferred to them something material, to which the name of *caloric* has been given, but because of the effect of the action upon it of those bodies by means of their frigorific rays, that is to say, by the undulations caused in the surrounding mass of the fluid ether, — under these circumstances it is evident that the nature of the exterior surface of the warm body, which renders it more or less capable of reflecting the rays or undulations which reach it from surrounding objects colder than itself, ought to influence to a considerable extent the rapidity of the cooling process.

Now, we know that, of all the substances with which we are acquainted, the metals are the most impervious to light, and, at the same time, and perhaps as a necessary consequence, have for it the greatest reflecting power; moreover, the results of a large number of experiments have shown that they also possess in an eminent degree the power of reflecting the invisible rays or undulations which all objects in nature send off continually and in all directions from their surfaces in consequence of that peculiar motion of their particles which constitutes their temperature.

Hence it appears that vessels having a metallic surface on the outside must be well adapted for preserving the temperature of the substances which they contain, whether that temperature be high or low, warm or cold.

I am far from maintaining that the sort of material of which the vessel is made, and the thickness of its walls, are matters entirely indifferent, provided that the outer surface be covered with a thin metallic layer which is clean and bright. I am aware that neither heat nor cold can be communicated or propagated through the walls of a vessel, or of any other solid body, instantaneously, and that this communication takes place more quickly in some substances than in others, more quickly through a thin wall than through a thick wall of the same material; and it is evident that this difference must necessarily exert an influence on the rapidity of the change of temperature of the vessel and of the liquid it contains, whatever be the nature of the external surface of the vessel.

For example, as porcelain is a worse conductor of heat than gold or silver, a vessel of given form and dimensions, made of porcelain and well gilded on the outside, if filled with warm water, would cool rather more slowly in the air, or even in a Torricellian vacuum, than another vessel of the same dimensions made of gold or silver, and filled with water of the same temperature; but if the vessels were exposed at the same time to a strong and very cold current of air, or were plunged into cold water, the difference in the rate of cooling would be much greater.

Hence we may conclude that teapots and coffee-pots made of porcelain or earthenware and well gilded on the outside would be not only as good, but even better for common use than teapots and coffee-pots made of silver.

If equal quantities of warm water are placed in two porcelain vessels of the same form and dimensions, and with walls of the same thickness, the one gilded on the outside, the other plain, and these vessels are allowed at the same time to cool in still air, the gilded vessel is found to cool more slowly than the plain one in the proportion of 3 to 2, as has already been remarked; but if, instead of allowing the vessels to cool in air which is undisturbed, they are exposed to the action of a strong and cold current of air, the difference in the rapidity of cooling will be much less, as 6 to 5, for example; and if the current of air is very strong, and at the same time very cold, this difference will be still smaller.

If, instead of exposing the vessels in the air, they are plunged into cold water, the difference in the rapidity with which they cool will be reduced to almost nothing.

In the cases last mentioned we can say that the exterior surfaces of both vessels, although of different natures, yet, on being exposed to so great a degree of cold, are cooled to such an extent as to be in a condition to transmit the heat coming from the interior of the vessel as fast as it can reach them after making its way through the thickness of the walls, which offer all the while a certain amount of resistance to its passage.

To use another form of expression which I regard as more exact, and consequently more suitable, especially before this illustrious Assembly, it might be said that in the case in question the exterior surfaces (the one of white porcelain, the other of gold) of the two vessels being intimately exposed to the violent action of a rapid succession of the very cold particles of the surrounding fluid, became, both of them, cooled to such an extent that they were reduced to about the same temperature in spite of the continual heating action of the walls of the vessels in contact with them on the opposite side; and that, as a consequence, since these surfaces exercised on the walls of the vessels which they covered cooling actions which were sensibly equal, the two vessels were of necessity cooled with the same rapidity.

I will conclude this memoir with some observations which may serve to throw light on a point in the theory of heat which is of very great importance.

The great rapidity with which heat is communicated from one body to another, when two bodies of different temperatures are in contact, compared with the slowness of communication which takes place when the bodies are separated, however little, one from the other, has had a considerable tendency to give authority to the opinion quite generally adopted by chemists, that there are two modes by which heat can be transmitted from one body to another; that is, at a distance, by radiant *caloric*, and, on contact, by an actual transfusion of the same substance. If, however, attention be paid to a fact which no one up to this time has called into question, the phenomenon under consideration can, as it seems to me, be explained in a perfectly clear and satisfactory manner, without having recourse to such an extraordinary supposition as that there are two different modes by which heat is communicated.

It is generally recognized (I might say that it is proved) that the intensity of the action of calorific or frigorific rays is inversely proportional to the squares of the distances from the body from which they proceed; now, if this relation is constant, since the effect produced by these rays in a given time must necessarily be in proportion to the intensity of their action, it is evident that at the point of contact (if, indeed, there can be an actual contact between two bodies) the rapidity of the calorific action between two particles of different temperatures, and which are in contact, must be infinite.

But the time necessary to establish an equality of temperature throughout the entire masses of two bodies in contact, which are of sensible size and of different temperatures, will depend not only on the size of the bodies and on the extent of the surfaces by which they are in contact, but also, and above all, on the greater or less rapidity with which is propagated among the particles of the bodies that peculiar motion of those particles which constitutes their temperature.

I will observe here, in passing, that if in the communication of heat between two bodies in contact, it were only a question of the transfer from one to the other of the excess of a fluid as rare and as elastic as *caloric* is supposed to be, one would expect, it seems to me, an action as instantaneous as the discharge of a Leyden jar.

It cannot be said, in objection, that the warm body does not offer avenues enough for the escape of the caloric, for it is proved that the pores of all bodies, even of the most solid, are so considerable in comparison with the space occupied by the particles of those bodies, that a fluid as rare as caloric is supposed to be would be able to move about therein with great freedom. Besides, it often happens that a very large surface of the warm body is in contact with the cold body; but, even in this case, there is nothing in the action taking place in the communication of the heat which resembles in any way the sudden explosion which takes place on the restoration of the equilibrium among the particles of an elastic fluid; on the contrary, the slow and measured progress of this communication, as well as all the other phenomena that it presents, denote rather a gradual operation, like that which takes place when the motion of a body is accelerated or retarded.

The following experiment may serve to explain and confirm this important truth. If a ball of iron, three or four inches in diameter, fastened to a long handle of the same metal, be heated strongly in a forge until it is of a whitish-red heat, and then taken from the fire and plunged suddenly into cold water, the communication of the heat to the water will be so far from being instantaneous that a considerable time will pass before the ball ceases to be red and luminous at its surface; and even after the surface of the ball has cooled so far as no longer to give off visible light, the interior will still be incandescent. It is easy to establish this last fact; for if at this point the ball be taken from the water and held in the air for a few moments, the surface of the ball will again become red and luminous.

I confess frankly that I have never been able to reconcile these phenomena with that hypothesis which supposes that the increase of temperature of a body is due to the accumulation within it of a very rare and extremely mobile substance, especially when I have considered the great ease with which such a fluid ought to pass through the pores of all known bodies.

But whatever be the explanation given to the phenomena which present themselves in the heating and cooling of bodies, it is certain that every new fact relating to these actions which is discovered must tend towards perfecting the science of heat as well as the arts which depend upon it.

I flatter myself that this Assembly will find the results of the experiments which I have detailed sufficiently curious and interesting to deserve its attention.

AN ACCOUNT

OF A

CURIOUS PHENOMENON OBSERVED ON THE GLACIERS OF CHAMOUNY;

TOGETHER WITH

SOME OCCASIONAL OBSERVATIONS CONCERNING THE PROPAGATION OF HEAT IN FLUIDS.

IN an excursion which I made the last summer, in the month of August, to the glaciers of Chamouny, in company with Professor Pictet of Geneva, I had an opportunity of observing, on what is called the Sea of Ice (*Mer de Glace*), a phenomenon very common, as I was told, in those high and cold regions, but which was perfectly new to me, and engaged all my attention. At the surface of a solid mass of ice, of vast thickness and extent, we discovered a pit perfectly cylindrical, about seven inches in diameter and more than four feet deep, quite full of water. On examining it on the inside with a pole, I found that its sides were polished, and that its bottom was hemispherical and well defined.

This pit was not quite perpendicular to the plane of the horizon, but inclined a little towards the south as it descended; and in consequence of this inclination, its mouth, or opening at the surface of the ice, was not circular, but elliptical.

From our guides I learned that these cylindrical holes are frequently found on the level parts of the ice; that they are formed during the summer, increasing gradually in depth, as long as the hot weather continues; but that they are frozen up and disappear on the return of winter.

I would ask those who maintain that water is a conductor of heat, how these pits are formed. On a supposition that there is no direct communication of heat between neighbouring particles of that fluid which happen to be at different degrees of temperature, the phenomenon may easily be explained; but it appears to me to be inexplicable on any other supposition.

The quiescent mass of water by which the pit remains constantly filled must necessarily be at the temperature of freezing, for it is surrounded on every side by ice; but the pit goes on to increase in depth during the whole summer. From whence comes the heat that melts the ice continually at the bottom of the pit? and how does it happen that this heat acts on the *bottom* of the pit only, and not on its sides?

These curious phenomena may, I think, be explained in the following manner. The warm winds which in summer blow over the surface of this column of ice-cold water must undoubtedly communicate some small degree of heat to those particles of the fluid with which this warm air comes into immediate contact; and the particles of the water at the surface so heated, being rendered specifically heavier than they were before by this small increase of temperature, sink slowly to the bottom of the pit, where they come into contact with the ice, and communicate to it the heat by which the depth of the pit is continually increased.

This operation is exactly similar to that which took place in one of my experiments (see my Essay on the Propagation of Heat in Fluids, *Experiment* 17), the results of which no person to my knowledge has yet explained.

There is another very curious natural phenomenon which I could wish to see explained in a satisfactory manner by those who still refuse their assent to the opinions I have been led to adopt, respecting the manner in which heat is propagated in fluids. The water at the bottoms of all deep lakes is constantly at the same temperature (that of 41° Fahrenheit), summer and winter, without any sensible variation. This fact alone appears to me to be quite sufficient to prove that, if there be any immediate communication of heat between neighbouring particles or molecules of water, *de proche en proche*, or from one of them to the other, that communication must be so extremely slow that we may with safety consider it as having no existence; and it is with this limitation that I beg to be understood when I speak of fluids as being non-conductors of heat.

In treating of the propagation of heat in fluids, I have hitherto confined myself to the investigation of the simple matter of fact, without venturing to offer any conjectures relative to the causes of the phenomena observed. But the results of recent experiments on the calorific and frigorific radiations of hot and of cold bodies (an account of which I shall have the honour of laying before the Royal Society in a short time) have given me some new light respecting the nature of heat and the mode of its communication; and I have hopes of being able to show *why* all changes of temperature in *transparent* liquids must necessarily take place at their surfaces.

I have seen, with real pleasure, that several ingenious gentlemen in London and in Edinburgh have undertaken the investigation of the phenomena of the propagation of heat in fluids, and that they have made a number of new and ingenious experiments, with a view to the further elucidation of that most interesting subject. If I have hitherto abstained from taking public notice of their observations on the opinion I have advanced on that subject in my different publications, it was not from any want of respect for those gentlemen that I remained silent, but because I still found it to be quite impossible to explain the results of my own experiments on any other principles than those which, on the most mature and dispassionate deliberation I had been induced to adopt; and because my own experiments appeared to me to be quite as conclusive (to say no more of them) as those which were opposed to them; and, lastly, because I considered the principal point in dispute, relative to the passage of heat in fluids, as being so clearly established by the circumstances attending several great operations of nature, that this evidence did not appear to me to be in danger of being invalidated by conclusions drawn from partial and imperfect experiments, and particularly from such as are allowed on all hands to be extremely delicate.

In all our attempts to cause heat to descend in liquids, the heat unavoidably communicated to the sides of the containing vessel must occasion great uncertainty with respect to the results of the experiment; and when that vessel is constructed of ice, the flowing down of the water resulting from the thawing of that ice will cause motions in the liquid, and consequently inaccuracies of still greater moment, as I have found from my own experience; and when thermometers immersed in a liquid at a small distance below its surface acquire heat in consequence of a hot body being applied to the surface of the liquid, that event is no decisive proof that the heat acquired by the thermometer is communicated by the fluid, from above, downwards, from molecule to molecule, *de proche en proche;* so far from being so, it is not even a proof that it is from the fluid that the thermometer receives the heat which it acquires; for it is possible, for aught we know to the contrary, that it may be occasioned by the radiation of the hot body placed at the surface of the fluid.

In the experiments of which I have given an account in my Essay on the Propagation of Heat in Fluids, great masses, many pounds in weight, of boiling-hot water, were made to repose for a long time (three hours) on a cake of ice, without melting but a very small portion of it; and on repeating the experiment with an equal quantity of very cold water (namely, at the temperature of 41° Fahrenheit), nearly twice as much ice was melted in the same time. In these experiments the causes of uncertainty above mentioned did not exist, and the results of them were certainly most striking.

The conclusions which naturally flow from those results have always appeared to me to be so perfectly evident and indisputable as to stand in no need either of elucidation or of further proof.

If water be a conductor of heat, how did it happen that the heat in the boiling water did not, in three hours, find its way downwards to the cake of ice on which it reposed, and from which it was separated only by a stratum of cold water half an inch in thickness?

I wish that gentlemen who refuse their assent to the opinions I have advanced respecting the causes of this curious phenomenon would give a better explanation of it than that which I have ventured to offer. I could likewise wish that they would inform us how it happens that the water at the bottoms of all deep lakes remains constantly at the same temperature; and above all, how the cylindrical pits above described are formed in the immense masses of solid and compact ice which compose the glaciers of Chamouny.

A remark, which surprised me not a little, has been made by a gentleman of Edinburgh (Dr. Thomson), on the experiments I contrived to render visible the currents into which liquids are thrown on a sudden application of heat or of cold. He conceives that the motions observed in my experiments, among the small pieces of amber which were suspended in a weak solution of potash in water, were no proof of currents existing in that liquid; as they might, in his opinion, have been occasioned by a change of specific gravity in the amber, or by air attached to it. I am sorry that so mean an opinion of my accuracy as an observer should have been entertained, as to imagine that I could have been so easily deceived. For nothing, surely, is easier than to distinguish the motion of a solid suspended in a liquid of the same specific gravity, which is carried along by a current in the liquid, from that of a body which descends, or ascends, in the liquid in consequence of its relative weight or levity. In the one case the motion is uniform; in the other, it is accelerated. In a current the body may be carried forward in all directions, and even in curved lines; but when it falls in a quiescent fluid by the action of gravity, or rises in consequence of its being specifically lighter than the fluid, it must necessarily move in a vertical direction.

The fact is, that I very often observed, in the course of my numerous experiments, the motions of small particles of matter of different kinds in water, which Dr. Thomson describes; but so far from inferring *from them* the existence of currents in that fluid, their cause was so perfectly evident that I did not even think it necessary to make any mention of them.

I cannot conclude this paper without requesting that the Royal Society would excuse the liberty I have taken in troubling them with these remarks. Very desirous of avoiding every species of altercation, I have hitherto cautiously abstained from engaging in literary disputes; and I shall most certainly endeavour to avoid them in future.

I am responsible to the public for the accuracy of the accounts which I have published of my experiments; but it cannot reasonably be expected that I should answer all the objections that may be made to the conclusions which I have drawn from them. It will, however, at all times, afford me real satisfaction to see my opinions examined and my mistakes corrected; for my first and most earnest wish is, to contribute to the advancement of useful knowledge.

[This paper is printed from the Philosophical Transactions of the Royal Society, XCIV. (1804), pp. 23 – 29.]

AN ACCOUNT

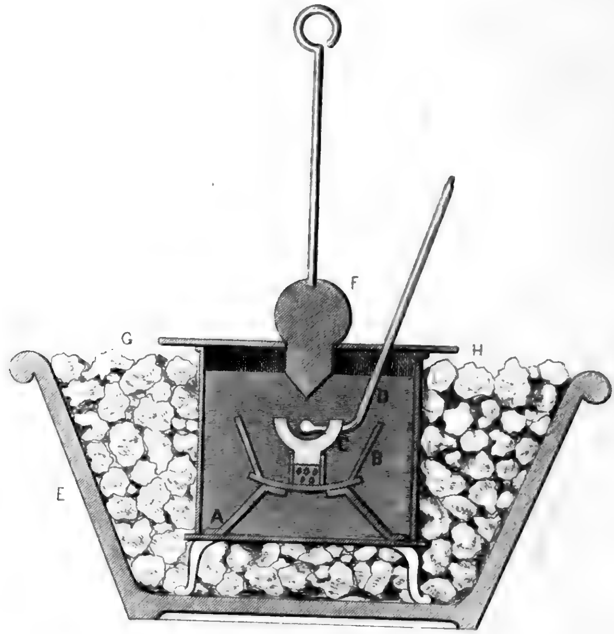
OF

SOME NEW EXPERIMENTS ON THE TEMPERATURE OF WATER AT ITS MAXIMUM DENSITY.

IN my seventh Essay on the Propagation of Heat in Fluids, and in a paper published in the Philosophical Transactions for the year 1804, in which I have given an account of a curious phenomenon frequently observed on the glaciers of Chamouny, I have ascribed the melting of the ice below the surface of the ice-cold water to currents of water slightly warmer, and consequently slightly heavier, which descend from the surface to the bottom of the ice-cold water; but the principal fact on which this supposition is founded having been called in question by various persons, I have endeavoured to establish it by new and decisive experiments.

If it is true that the temperature of water at its maximum density is considerably higher than the freezing-point of that liquid (as was announced many years ago by M. de Luc), and that the communication of heat in liquids is brought about by a movement of circulation caused by a change of density in the particles of the fluid resulting from a change of temperature, the explanation that I have given of the phenomenon of the melting of ice covered with a layer of ice-cold water by heat applied to the surface of the water, would seem

PLATE VI



natural and admissible; but if the density of water is greater at the temperature of melting ice than at any other more elevated temperature, as some philosophers assert, it is evident that the vertical descending currents of warm water which I have described cannot exist, and my explanation must be rejected.

This inquiry interested me all the more, because the fact in question had served as the foundation of the theory which I gave in my seventh Essay on the periodical winds of the polar regions, and as the basis of my conjectures on the existence of currents of cold water in the depths of the sea coming from the polar regions to the equator, and on the cause of the great difference which is found in the temperature of different countries situated in the same latitude and at the same height above the level of the sea.

After meditating on the means which I should employ to establish this important fact beyond doubt, I thought of the experiment which I am about to describe, and which is all the more interesting, since it not only demonstrates the existence in a mass of water which is warmed or cooled, of the currents assumed by my theory, but proves at the same time that the temperature at which the density of water is at a maximum is actually some degrees above that of melting ice.

Having provided a cylindrical vessel (A, Plate VI.), open above, made of thin sheet brass, $5\frac{1}{2}$ inches in diameter and 4 inches deep, supported on three strong legs $1\frac{1}{4}$ inches high, I placed in it a thin brass cup (B) 2 inches in diameter at its bottom (which is a little convex downwards), $2\frac{8}{10}$ inches wide at its brim, and $1\frac{3}{10}$ inches deep; this cup stands on three spreading legs made of strong brass wire, and of such form and length that when the cup is introduced into the cylindrical vessel, it remains firmly fixed in the axis of it, and in such a situation that the bottom of the cup is elevated just $1\frac{1}{4}$ inches above the bottom of the cylindrical vessel.

In the middle of this cup there stands a vertical tube of thin sheet brass $\frac{1}{2}$ of an inch in diameter and $\frac{6}{10}$ of an inch in length, open above, which serves as a support for another smaller cup (C), which is made of cork, the brim of which is on the same horizontal level with the brim of the larger brass cup in which it is placed.

This cork cup, which is spherical, being something less than half of an hollow sphere, is 1 inch in diameter at its brim, measured within, $\frac{4}{10}$ of an inch deep, and $\frac{1}{4}$ of an inch in thickness. It is firmly attached to the vertical tube on which it stands, by means of a cylindrical foot $\frac{1}{2}$ of an inch in diameter and $\frac{1}{4}$ of an inch high, which enters with friction into the opening of the vertical tube.

On one side of this cork cup there is a small opening, which receives and in which is confined the lower extremity of the tube of a small mercurial thermometer (D). The bulb of this thermometer, which is spherical, is $\frac{3}{10}$ of an inch in diameter, and it is so fixed in the middle of the cup, that its centre is $\frac{1}{4}$ of an inch above the bottom of the cup; consequently it does not touch the cup anywhere, nor does any part of it project above the level of its brim.

The tube of this thermometer, which is 6 inches in length, has an elbow near its lower end at the distance of 1 inch from its bulb, which elbow forms an angle of about 110 degrees, and the thermometer is so fixed in the cork cup, that the short branch of its tube, namely, that to the end of which the bulb is attached, lies in an horizontal position, while the longer branch (to which a scale made of ivory and graduated according to Fahrenheit is affixed) projects obliquely upwards and outwards in such a manner that the freezing-point of the scale lies just above the level of the top of the cylindrical vessel in which the cups are placed.

The cork cup, which was turned in the lathe, is neatly formed, and in order to close the pores of the cork, it was covered within and without with a thin coating of melted wax, which was polished after the wax was cold.

The thermometer was fixed to the cork cup by means of wax, and in doing this care was taken to preserve the regular form of the cup, both within and without.

The vertical brass tube which supports this cup in the axis of the brass cup is pierced with several small holes, in order to allow the water employed in the experiments to pass freely into and through it.

Having attached about 6 ounces of lead to each of the legs of the brass cup, in order to render it the more steady in its place, it was now introduced with its contents into the cylindrical vessel, and the vessel was placed in an earthen basin (E), and surrounded on all sides with pounded ice. This basin was 11 inches in diameter at its brim, 7 inches in diameter at the bottom, and 5 inches deep, and was placed on a firm table in a quiet room.

Several cakes of ice were then placed under the bottom of the brass cup, and the cup was surrounded on all sides by a circular row of other long pieces of ice fixed in a vertical position between the outer walls of the cup and the walls of the cylindrical vessel. These pieces were about 4 inches long, and extended from the bottom of the vessel to within a very short distance of the top.

All these pieces of ice having been fixed firmly in their places by means of some little wooden wedges, ice-cold water was poured into the cylindrical vessel until the surface of this liquid was an inch above the upper edge of the cork cup.

In this state of things it is evident that the two cups were filled with and surrounded on all sides by water at the temperature of melting ice, and that this temperature was maintained constant by the pieces of ice with which the water was in contact.

After having left the apparatus in this situation for about an hour, in order to satisfy myself that the temperature of the cold water was constant and uniform throughout its entire mass, I made the following experiment.

Experiment No. 1. — A solid ball of tin (F) having been provided, 2 inches in diameter, with a cylindrical projection on the lower side of it, 1 inch in diameter and ½ of an inch long ending in a conical point which projected (downwards) ½ of an inch farther, and having on the other side a strong iron wire 6 inches long, which served as a handle, — this ball, after having been immersed for half an hour in a considerable quantity of water at the temperature of 42° F., was withdrawn from the water, wiped dry with a handkerchief of the same temperature, placed without loss of time above the cylindrical vessel, and fixed in such a position that the entire conical point of the tin ball (½ of an inch in length) was submerged in the cold water contained in the vessel.

To fix and keep the metallic ball in its place, I used a strong slip of tin (G H), 6 inches long and 2½ inches wide, with a circular hole in the middle of it 1 inch in diameter. This slip of tin being laid horizontally on the top or brim of the cylindrical vessel in such a manner that the centre of the circular hole coincided with the axis of the cylindrical vessel, when the short cylindrical projection belonging to the ball was introduced into that hole, the ball was firmly supported in its proper place.

The ball was placed in such a position that the end of the conical projection was immediately over the cork cup, at the distance of $1\frac{1}{2}$ inches above the level of its brim and consequently $\frac{1}{2}$ of an inch above the upper part of the bulb of the small thermometer which lay in this cup.

The quantity of cold water in the cylindrical vessel had been so regulated beforehand that when the conical point was entirely submerged, the surface of the water was on a level with the base of this inverted cone, so that the whole of the cylindrical part of the projection was out of the water.

I knew that the particles of ice-cold water which were thus brought into contact with the conical point could not fail to acquire some small degree of heat from that relatively warm metal, and I concluded that if the particles of water so warmed should in fact become *heavier* than they were before, in consequence of this small increase of temperature, they must necessarily *descend* in the surrounding lighter ice-cold liquid, and as the heated metallic point was placed directly over the cork cup, and fixed immovably in that situation, I foresaw that the descending current of warm water must necessarily fall into that cup and at length fill it, and that the presence of this warm water in the cup would be announced by the rising of the thermometer.

The result of this very interesting experiment was just what I expected: the conical metallic point had not been in contact with the ice-cold water more than 20 seconds when the mercury in the thermometer began to rise, and in 3 minutes it had risen three degrees and a half, namely, from 32° to 35½°; when 5 minutes had elapsed it had risen to 36°.

Another small thermometer placed just below the surface of the ice-cold water, and only $\frac{2}{10}$ of an inch from the upper part of the conical point and on one side of it, did not appear to be sensibly affected by the vicinity of that warm body.

A third thermometer, the bulb of which was placed in the brass cup just on the outside of the cork cup and on a level with its brim, showed that the water which immediately surrounded the cork cup remained constantly at the temperature of freezing during the whole time that the experiment lasted.

As I well knew from the results of the experiments on the propagation of heat in a solid bar of metal,* that no one of the particles of cold water in contact with the surface of the conical projection, in the experiment which I have just described, could acquire by this momentary contact a temperature as high as that of the warm metal, I was by no means surprised to find that the thermometer belonging to the cork cup rose no higher than 36°.

In order to see if it could not be made to rise not only higher, but also more rapidly, by employing the metallic ball heated to such a temperature as it might be supposed would be sufficient to heat those particles of ice-cold water which should come into contact with its conical point, to the temperature at which the density of water is supposed to be a maximum, I made the following experiment.

* An account of these experiments has been given in a memoir presented to the Mathematical and Physical Class of the National Institute of France, on the 7th of May, 1804. See also p. 144.

Experiment No. 2. — Having removed the ball, I gently brushed away the warm water which in the last experiment had been lodged in the cavity of the cork cup, and which still remained there, as was evident from the indication of the thermometer belonging to the cup; I then placed several small cakes of ice in the cylindrical vessel, which ice, floating on the surface of the water in the vessel, prevented the water from receiving heat from the surrounding air, which at that time was at the temperature of 70° F. As the cork cup had been a little heated by the warm water in the foregoing experiment, time was now given it to cool.

As soon as the cup and the whole mass of the water in the cylindrical vessel appeared to have acquired the temperature of freezing, I carefully removed the cakes of ice which floated on the surface of the water, and introduced once more the projecting conical point belonging to the metallic ball into the ice-cold water in the vessel, placing it exactly in the same place which it had occupied in the foregoing experiment; but this ball, instead of being at the temperature of 42° F., as before, was now at the temperature of 60° F.

The results of this experiment were very striking, and, if I am not much mistaken, afford a direct, unexceptionable, and demonstrative proof, not only that the maximum of the density of water is in fact at a temperature which is several degrees above the point of freezing, but also that warm currents do actually set downwards in ice-cold water, whenever a certain small degree of heat is applied to the particles of that fluid which are at its surface, as I have already announced in my Essay on the Propagation of Heat in Fluids.

The conical metallic point had been in its place no more than 10 seconds when I distinctly saw that the mercury in the thermometer belonging to the cork cup was in motion, and, when 50 seconds had elapsed, it had risen four degrees, viz. from 32° to 36°.

When 2 minutes and 30 seconds had elapsed, reckoning from the moment when the metallic point was introduced into the cold water, the thermometer had risen to 39°, and at the end of 6 minutes to $39\frac{7}{8}$°, when it began to fall; but very slowly, however, for at the end of 8 minutes and 30 seconds it was at $39\frac{3}{4}$°.

A small mercurial thermometer, the bulb of which was placed on one side of the cork cup at the distance of about $\frac{2}{10}$ of an inch from it, showed no signs of being in the least affected by the vertical current of warm water which descended from the conical point into the cup in this experiment.

This experiment was repeated four times the same day (the 13th of June, 1805), and always with nearly the same results. The mean results of these four experiments were as follows: —

Time elapsed, reckoned from the beginning of the experiment.				Temperature of the water in the cork cup, as shown by the thermometer.
	m.	s.		Degrees.
	0	0		32
At	0	10	began to rise	32+
At	0	23	had risen to	33
	0	28	" " "	34
	0	35	" " "	35
	0	48	" " "	36
	1	3	" " "	37
	1	35	" " "	38
	2	32	" " "	39
	3	41	" " "	$39\frac{1}{2}$
	4	48	" " "	$39\frac{3}{4}$
	6	5	" " "	$39\frac{7}{8}$

As I had found by some of my experiments made in the year 1797 (of which an account is given in my seventh Essay, Part I.) that water at the temperature of about 42° F., and consequently what we should call very cold, melted considerably more ice, when standing on it, than an equal quantity of boiling-hot water in the same situation, I was very curious to see whether the thermometer, the bulb of which lay in the cork cup, would not also be less heated by the ball when it should be applied *very hot* to the surface of the water, than when its temperature was much lower.

Seeing that this research ought to throw great light on the mysterious operations of the distribution of heat in liquids, I hastened to make the following experiment.

Experiment No. 3. — The cylindrical vessel with its contents having been once more reduced to the uniform temperature of freezing water, the metallic ball was heated in boiling water, and being as expeditiously as possible taken out of that hot liquid, its projecting conical point was suddenly submerged in the ice-cold water, as in the former experiments.

The result of this experiment was very interesting. It was not till 50 seconds had elapsed that the thermometer began to show any signs of rising, and at the end of 1 minute and 7 seconds it had risen only 2 degrees.

In the foregoing experiment, when the metallic ball was so much colder, the thermometer began to rise in 10 seconds, and at the end of 1 minute and 3 seconds it had risen 5 degrees.

This difference is very remarkable, and if it does not prove the existence and great efficacy of currents in conveying heat in fluids, I must confess that I do not see how the existence of any invisible mechanical operation, the progress of which does not immediately fall under the cognizance of our senses, can ever be demonstrated.

As the experiment made with the ball heated in boiling water appeared to me to be very interesting, I repeated it twice, and its results were always nearly the same. The mean results of these three experiments were as follows: —

Time elapsed, reckoned from the beginning of the experiment.				Temperature of the water in the cork cup, as shown by the thermometer.
	m.	s.		Degrees.
	0	0	. . . .	32
At	0	50	the thermometer began to rise . .	32+
At	1	2	had risen to	33
	1	7	" " "	34
	1	18	" " "	35
	2	2	" " "	36
	3	2	" " "	36½
	4	17	" " "	37
	6	12	" " "	38
	7	17	" " "	38⅛
	9	0	" " "	38¼
	12	0	" " "	38¼
	14	0	" " "	38¼

By comparing the mean results of these experiments with the mean results of those in which the ball was at the temperature of 60° or less, we may see how much more rapid the communication of heat in the cold water from above downwards was when the metallic ball was *relatively cold* than when it was much warmer; but we must not consider of too much importance the determination of the relative rapidity thus made, because it is more than probable that it was not till after the conical metallic point had been considerably cooled by contact with the cold water that the vertical descending currents could exist by which the thermometer was at length heated. At the beginning of the experiment made with the tin ball warmed in boiling water, the particles of water which were in immediate contact with the conical point while it was still very warm, were heated to a temperature higher than that at which the density of water is at a maximum, and the density of these particles being diminished by this high degree of heat, the vertical currents in the cold water were at the beginning ascending currents, as I satisfied myself by means of a small thermometer placed by the side of the conical point at a distance of $\frac{2}{10}$ of an inch from its base, and immediately below the surface of the cold water: this thermometer began to rise very rapidly as soon as the warm metallic point was plunged into the cold water.

Another small thermometer, the bulb of which was situated at about the same distance from the axis of the conical projection, but $\frac{1}{2}$ of an inch below the surface of the cold water, preserved throughout the entire experiment the appearance of perfect rest.

The results of this last experiment are all the more interesting because they afford a demonstrative proof that it was neither by a direct communication of heat in the water, which was at rest, from molecule to molecule, *de proche en proche*, nor by calorific radiations passing through the water, that heat was communicated from the metallic point to the bulb of the thermometer, but actually by a descending current of warm water; for it is perfectly evident that if this heat had been communicated either by a direct transfer in the water from molecule to molecule or by calorific radiations passing from the surface of the metal through the water, which remained at rest, this communication would naturally have been the most rapid when the metallic point was the warmest. What did take place was exactly contrary to this, as we have just seen. Moreover, the small thermometer, which was placed close to the metallic body on one side, and which in this experiment was in no degree affected by the heat of this body, would not have failed to acquire as much heat at least as that placed in the cork cup, which was situated below the metallic body and at a greater distance from it.

The considerable amount of time which elapsed in the experiments performed with the tin ball heated in boiling water before the thermometer in the cork cup began to be so sensibly affected, and the rapidity with which it was then warmed through several degrees as soon as it began to rise, indicate a fact which it is important to notice. In order to throw light upon this fact, we must consider carefully the operation of the heating of cold water by the warm metallic surface with which it was in contact, and examine it in its progress and in all its details.

Let us begin by supposing that the conical point of the ball, at the temperature of boiling water, has just been submerged vertically up to the level of its base in a mass of undisturbed water at the temperature of melting ice. As the particles of water, which in this case are in contact with the warm metallic surface, cannot pass, all of a sudden, from the temperature of melting ice to that of boiling water without passing through all the intermediate degrees, and since these particles at the temperature of melting ice cannot become warmer without becoming more dense, it is evident that they must have a tendency to descend, and consequently to leave the surface of the metal, as soon as they begin to acquire heat; but experiment showed that, instead of descending, they were actually pushed upwards: this proves that they were heated so rapidly that, before they had time to leave the surface of the metal and to escape from its calorific influence, they had acquired a temperature so elevated that their density, after having passed rapidly the point of its maximum, became even less than it was at the temperature of melting ice. But after some moments, the metallic body having cooled somewhat, and the communication of heat to the particles of water taking place more slowly, these particles, having become more dense on account of a slight increase of temperature, had time to escape before becoming warmer, and at that time the descending current suddenly began.

This fact interests me the more, as it may serve in some sort to explain a phenomenon which I observed in an experiment made eight years ago, an account of which I gave in my Essay on the Propagation of Heat in Fluids.*

The phenomenon to which I have alluded was this: Having poured some mercury into a small cylindrical glass vessel 2 inches in diameter and $3\frac{1}{2}$ inches deep, until this fluid filled the vessel to the height of an inch, I poured on to the mercury twice as much water (that is, 2 inches), and, plunging the vessel up to the level of the upper surface of the mercury into a freezing mixture of pounded ice and sea-salt, the temperature of the air being 60° F., I allowed the whole to cool quietly, in order to see in what part of the water the ice would first

* See Vol. I. p. 357.

appear. It was at the bottom of the water, where this liquid was in contact with the mercury, that the ice formed.

The layer of water which rested immediately on the surface of the mercury having been cooled to about the temperature of 41° F., where the density of water is at its maximum, the particles of this water, which were then in immediate contact with the mercury, losing still more of their heat, became of necessity less dense, and had consequently a tendency to leave the bottom of the water and to ascend upwards; but the rapidity with which they were cooled by the mercury was so great that they were frozen before they could escape from the cooling influence of this cold body.

After all that I have said about the warm and cold currents which take place in a liquid which is warmed or cooled, it might perhaps be thought that I regard these currents as composed of single particles of the liquid, which, having been in immediate contact with the body which gives or which receives the heat, are all of the same temperature. I am all the farther from holding this opinion, since I know from the results of several experiments made expressly for elucidating this point, (and which I shall have the honor of presenting to the Class on another occasion,) that a liquid current cannot pass through another liquid mass which is at rest, and which is of the same kind and of about the same specific gravity, without producing a perceptible mixture of the two liquids; much less, therefore, can a small current of warm water pass without mixing through a mass of cold water; and the farther it advances the more it will be mixed, and the more, in consequence, will its temperature be found to be lowered.

For example, in the experiments of which I have just given an account, the cork cup, which received the current of warm water descending from the metallic point of the tin ball, was only $\frac{1}{2}$ of an inch below the extremity of this point; if this distance had been greater, the thermometer in the cup would certainly have risen to a less height: for this reason these experiments ought not to be regarded as suitable for determining with great exactness the temperature at which the density of water is at a maximum, but rather as proving that this temperature is really several degrees of the thermometric scale above that of melting ice; and this is all that I am particularly interested in showing at the present time.

Judging from the constant temperature which is found at all seasons at the bottom of deep lakes and from the results of several direct experiments, we may conclude that water is at its *maximum* density when it is at the temperature of about 41° of Fahrenheit's scale, which corresponds to 4° on that of Reaumur, and to 5° of the Centigrade scale.

[This paper is translated from the Mémoires de l'Institut, etc., VII. (1806), pp. 78–97. The greater part of the translation is taken from Nicholson's Journal, XI. (1805), pp. 225–235.]

INQUIRIES

CONCERNING

THE MODE OF THE PROPAGATION OF HEAT IN LIQUIDS.

THE motions in fluids which result from a change in their temperature give rise to so great a number of phenomena, that philosophers cannot bestow too much pains in investigating that interesting branch of knowledge.

When heat is propagated in solid bodies, it passes from particle to particle, *de proche en proche*, and apparently with the same celerity in every direction; but it is certain that heat is not transmitted in the same manner in fluids.

When a solid body is heated and plunged in a cold liquid, the particles of the liquid in contact with the body, being rarefied by the heat that they receive from it, and being rendered specifically lighter than the surrounding particles, are forced to give place to these last and to rise to the surface of the liquid; and the cold particles that replace them at the surface of the hot body, being in their turn heated, rarefied, and forced up, — all the particles thus heated by a successive contact with the hot body form a continued ascending current, which carries the whole of the heat immediately towards the surface of the liquid, so that the strata of the liquid situated at a small distance under the hot body are not sensibly heated by it.

When a solid body is plunged in a liquid which is hotter than the body, the particles of the liquid in contact with the body, being condensed by the cooling they undergo, descend, in consequence of the increase of their specific gravity, and fall to the bottom of the liquid; and the strata situated above the level of the cold body are not cooled by it immediately.

It is true that the viscosity of liquids, even of those which possess the highest known degree of fluidity, is still much too great to allow one of their particles individually being moved out of its place by any change of specific gravity occasioned by heat or cold; yet this does not prevent currents from being formed, in the manner above described, by small masses of the liquid composed of a great number of such particles.

The existence of currents in the ordinary cases of the heating and cooling of liquids cannot any longer be called in question; but philosophers are not yet agreed with respect to the extent of the effects produced by those currents.

In treating of abstruse subjects, it is indispensably necessary to fix with precision the exact meaning of the words we employ. The distinction established between *conductors* and *non-conductors* of heat is too vague not to stand in need of explanation. An example will show the ambiguity of these expressions.

If two equal cubes of any solid matter, — copper, for instance, — of two inches in diameter, the one at the temperature of 60°, the other at 100°, be placed one above the other, the cold cube will be heated by the hot one, and this last will be cooled.

If the cold cube be placed upon a table and its upper surface covered by a large plate of metal, — of silver, for instance,—a quarter of an inch thick, and if the hot cube be placed upon this plate immediately above the cold cube, the heat will descend through the metallic plate with a certain degree of facility, and will heat the cold cube.

If a dry board of the same thickness with the metallic plate be substituted in its place, the heat will descend through the wood, but with much less celerity than through the plate of silver.

But if a stratum of water or of any other liquid be substituted in place of the metallic plate or of the board, the result will be very different. If, for instance, the cold cube being placed in a large tub resting on the middle of its bottom, the hot cube be suspended over it by cords, or in any other manner so that the lower surface of the hot cube be immediately above the upper surface of the cold cube, at the distance of a quarter of an inch, and the tub be then filled with water at the same temperature as that of the cold cube, the heat will not descend from the hot cube to the cold one through the stratum of water of a quarter of an inch in thickness that separates them.

We may with propriety call silver a *good conductor* of heat, and dry wood a *bad conductor;* but what shall we say of water? I have called it a *non-conductor* for want of a more suitable term, but I always felt that that word expresses but imperfectly the quality that was meant to be designated.

In the experiment of the two cubes plunged in water, if the hot cube be placed below and the cold cube above it, the heat will not only be communicated from the hot to the cold cube, but it will pass even more rapidly than when the two cubes are separated by a plate of silver. But in this case it is evident that the heat is *transported* by the ascending currents which are formed in the liquid in consequence of the heat which it receives from the hot body.

The existence of these currents in certain cases has been known a long time, but philosophers have not been sufficiently attentive to the many curious phenomena that depend upon them. It has not even been suspected with what extreme slowness heat passes in fluids, from particle to particle, *de proche en proche*, in cases where the effects of such communication become sensible.

For some time after I had engaged in this interesting inquiry, I conceived that this kind of communication was absolutely impossible in all cases; but a more attentive examination of the phenomena has convinced me that this conclusion was too hasty. As early as the beginning of 1800, in a note published in the third edition of my Seventh Essay, I announced a conjecture that the non-conducting power of fluids might perhaps depend solely on the extreme mobility of their particles; and it is certain, if this conjecture is well founded, liquids must necessarily become conductors of heat (though very imperfect ones) in all cases where this mobility of their particles is destroyed, as well as in these rare but yet possible cases, where a change of temperature can take place in a liquid without giving its particles any tendency to move, or to be moved out of their places.

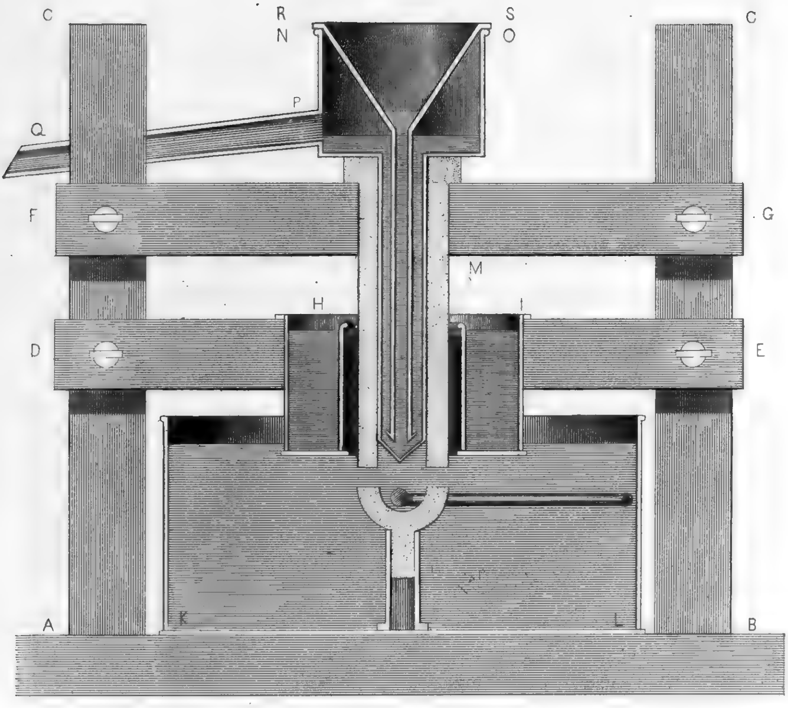
The unequivocal results of a great many experiments have shown, that in ordinary cases, and perhaps in all cases where heat is propagated in considerable masses of fluids, its distribution is accomplished precisely in the manner that the new theory supposes, that is to say, by currents. And it is certain that the knowledge of that fact has enabled us to explain in a satisfactory manner several interesting phenomena of nature, which before were enveloped in much obscurity.

When a hot solid body is plunged in a cold liquid, there can be no doubt concerning the existence of the vertical ascending currents which are formed in the liquid, and which convey to the surface the heat which its particles have received; but with respect to the strata of liquid situated under the hot body, *are they or are they not heated by this body by means of a direct communication of heat from above downwards, from particle to particle, these particles remaining in their places?* This is a question on which philosophers are not yet agreed. As it is a question of great importance, I have long meditated on the means of deciding it; and after several unsuccessful attempts, I have at last succeeded in making an experiment which I think is decisive.

As the apparatus which I used for this experiment, and which I have the honour of laying before the assembly, is somewhat complicated; and as it is indispensably necessary to be intimately acquainted with it, in order to form a judgment concerning the degree of confidence which the results of the experiment may deserve, — it is necessary to give a detailed description of this machinery. The annexed figure gives a distinct representation of its principal parts. It is drawn on a scale of a quarter of an inch to the inch, English measure.

A B (Plate VII.) is a board, of oak, seen in profile; it is 1½ inches thick, 18 inches long, and 11 inches in breadth. It serves to support two square upright pillars, C C, 18½ inches in height and 1½ inches square. They are firmly fixed in the board at the distance of 11 inches asunder, and serve to support the two cross-pieces, D E, F G, at different heights.

PLATE VII



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These cross-pieces are each pierced with two square holes, at the distance of 11 inches one from the other, into which the upright pillars C C enter, and the cross-pieces are supported at any height that is required, by means of a screw of compression. These screws are represented in the figure.

The cross-piece F G, which is represented in profile, is 17 inches in length, and $1\frac{1}{2}$ inches thick, and 3 inches in breadth. It is pierced in the middle by a cylindrical hole of 2 inches in diameter.

The cross-piece D E is 17 inches in length by $1\frac{1}{2}$ inches in thickness. It is 3 inches wide at each end and 6 inches in the middle, where it is pierced by a circular hole 5 inches in diameter.

The cross-piece D E serves to support the annular vessel H I, of which a vertical section passing through its axis is seen in the figure. This vessel, formed of thin brass plates, is 5 inches in diameter without, 3 inches in diameter within, and $27\frac{1}{8}$ inches in depth. This vessel is filled with water during the experiments to the height of $2\frac{1}{2}$ inches; and its form is such, that, if the water that it contains were frozen into a solid mass of ice, this piece of ice would have the form of a tube or perforated cylinder of 1 inch in thickness and $2\frac{1}{2}$ inches high by 5 inches in diameter without. Its cylindrical cavity would be precisely 3 inches in diameter.

K L is a vertical and central section of a cylindrical vessel of tin of 10 inches in diameter by $4\frac{1}{2}$ inches in depth. It is filled with water to the height of 4 inches, as it is seen in the figure.

The cross-piece D E is placed at such a height that the bottom of the annular vessel H I is plunged a quarter of an inch under the surface of the water contained in the great cylindrical vessel K L.

In the axis of this last vessel is placed a small hemispherical cup of wood 2 inches in diameter without and $\frac{1}{2}$ of an inch thick. It is kept in its place by a short vertical tube of tin, soldered to the bottom of the cylindrical vessel K L, into which the stalk of the cup fits tightly.

The middle of the cavity of this cup is occupied by the bulb of a small mercurial thermometer of great sensibility. Its tube, which has an ivory scale, is laid down horizontally, and fixed in one side of the cup, through which the tube passes, in such a manner that the lowest part of the bulb is elevated $\frac{1}{10}$ of an inch above the bottom of the cup. The diameter of the bulb being $\frac{8}{10}$ of an inch, and the hemispherical cup having $\frac{1}{2}$ inch of radius within, it is evident that the upper part of the bulb is $\frac{1}{10}$ of an inch below the level of the brim of the cup that contains it. To avoid charging the figure with too many details, the scale of the thermometer is not drawn, but the tube is distinctly represented.

The horizontal cross-piece F G serves to support a very essential part of the apparatus, which remains to be described.

This cross-piece supports, in the first place, a vertical tube of wood, M, $6\frac{6}{10}$ inches in length and 2 inches in diameter without. Its interior diameter is $1\frac{1}{20}$ inch. This tube is supported by a projecting collar (represented in the figure), $2\frac{1}{2}$ inches in diameter, which rests on the cross-piece F G. It is a vertical and central section of this tube that is represented in the figure, and it is dotted in order to distinguish it from the surrounding parts of the apparatus.

The lower part of this tube is plunged $\frac{6}{10}$ of an inch under the surface of the water in the large cylindrical vessel K L; and it is placed precisely above the wooden cup in the prolongation of its axis, the lower extremity of the tube being at the distance of $\frac{8}{10}$ of an inch above the horizontal level of the brim of the cup.

On the top of the tube of wood is placed a cylindrical vessel N O, of sheet brass, 3 inches in diameter, $2\frac{3}{4}$ inches high, which has a lateral spout, P Q, placed a little above the level of its bottom.

From the middle of the bottom of this vessel, there descends a cylindrical tube of brass, 6 inches in length and 1 inch in diameter, which ends below in a hollow conical point, as represented in the figure.

R S is a vertical and central section of a funnel of brass, which ends below in a cylindrical tube of $\frac{3}{10}$ of an inch in diameter and $6\frac{6}{10}$ inches long. This funnel is kept in its place in the axis of the cylindrical vessel N O by the exact fitting of its upper edge upon that of the vessel into which it is adjusted.

The lower end of the tube of this funnel is surrounded by a projecting edge or flange in the form of a hollow inverted cone. The diameter of this conical projecting brim above, at its base, is $\frac{7}{10}$ of an inch, and it is soldered below to the end of the tube.

When hot water is poured into the funnel, this liquid, descending by the tube of the funnel, strikes against the inner surface of the hollow inverted cone which terminates the vertical tube that belongs to the vessel N O, and then, rising up through this last tube into that vessel, it runs off by its spout. It was with a view to force this water to come into more intimate contact with the hollow cone that the projecting edge, in form of an inverted cone, was added to the lower end of the tube of the funnel.

The object chiefly in view in the arrangement of this apparatus was to give to the conical point which terminates the vertical tube of the vessel N O, an elevated temperature, which should remain constant during some time, for the purpose of observing if the heat, which must necessarily be communicated by this metallic point to the small quantity of water with which it is in contact, and which is confined in the lower part of the wooden tube M, would descend, or not, to the thermometer which was placed in the wooden cup.

There was still one source of error and uncertainty against which it was necessary to guard. The heat communicated through the sides of the wooden tube to the water contained in the great cylindrical vessel K L might be transported to the sides of that vessel, and, being then communicated from above downwards through these sides, might heat successively the lower strata of the liquid, and at last that stratum in which the thermometer was.

It was to prevent this that the annular vessel H I was used, and it performed its office in the following manner: The particles of water contained in the great vessel K L, which, being in contact with the exterior surface of the wooden tube, were heated by that tube, could not fail to rise to the surface, and there they necessarily came into contact with the interior sides of the annular vessel, to which they communicated the excess of heat they had received from the wooden tube.

This heat, passing readily through the thin metallic sides of that vessel, was given off as fast as it was received to the particles of cold water contained in the vessel which were in contact with its sides, and these particles, rising to the surface of the water contained in the annular vessel in consequence of their acquired heat and levity, the progress of the heat from the wooden tube to the sides of the large vessel K L, was interrupted, and all the heat that passed through the sides of the wooden tube was by these means turned aside in such a manner that it could no longer disturb the progress of the experiment, nor affect the certainty of its results.

Before I proceed to give an account of the result of this inquiry, I shall take the liberty to recall the attention of the Assembly to the most important circumstances of the experiment.

On pouring boiling water in a small uninterrupted stream into the funnel, the hollow conical point which terminates the vertical tube belonging to the vessel N O was heated, and kept at a constant temperature little under that of boiling water.

This point was surrounded by a small quantity of water contained in the cavity of the lower part of the wooden tube, and as this water could not change its place nor be displaced by the surrounding cold water, being enclosed and protected by the sides of the wooden tube, it would necessarily become very hot in a short time.

But this small quantity of hot water lay immediately upon a stratum of cold water, which separated it from the bulb of the thermometer, placed directly under it at the distance of only half an inch.

If heat could pass in the water from above downwards, it would no doubt pass from the lower stratum of hot water contained in the open end of the wooden tube to the bulb of the thermometer, which lay immediately below it and at so small a distance.

Three experiments were made with this apparatus, and always with exactly the same results. In the first, a stream of boiling water was poured into the funnel during 10 minutes; in the second, during 12 minutes; and in the third, during 15 minutes.

The thermometer, whose bulb was in the wooden cup, remained *at perfect rest* from the beginning of the experiment to the end of it without showing the slightest sign of being in any way affected by the hot water which was so near it.

These experiments were made at Munich in the month of July, 1805; the temperature of the air and of the water contained in the vessel K L being 70° Fahrenheit.

A small thermometer placed in the water contained in the annular vessel H I, in such a manner that its bulb was scarcely submerged, marked that this water had received a little heat in each of the three experiments.

Another similar thermometer placed in the water contained in the large vessel K L, immediately under its surface and near one side of the vessel, showed that this water had not acquired any sensible increase of temperature during the experiments.

From the results of these experiments we are authorized to conclude, that heat does not descend in water to a sensible distance, in cases where the particles of the liquid which receive heat are exposed to be displaced and forced upwards by the surrounding colder and denser particles, that is to say, in all the cases (and they are the most common) where heat is applied to the strata of the liquid situated under its surface.

But the results of the experiments in question do not prove that heat cannot in any case descend in water; and still less can it be inferred from them, that all direct communication of heat in this liquid, from particle to particle, *de proche en proche*, is impossible. They do not even prove that heat did not descend, *to a small distance*, below the level of the end of the wooden tube in these experiments; for it is certain that that event could take place without the thermometer, which was situated a little lower, being in any way affected by that heat.

The particles of water situated at a very small distance below the level of the lower end of the wooden tube, being heated by the stratum of hot water which rested immediately on them, might have been displaced by the surrounding colder and denser particles, and forced to rise to the surface; and these last being in their turn heated, forced upwards and replaced by other cold particles, it is evident that the heat could not make its way downwards so far as to arrive at the thermometer through a stratum of liquid, which, though apparently at rest, was nevertheless in part composed of particles which were continually changing.

I have long suspected that the apparent impossibility of a direct communication of heat between neighbouring particles of fluids depends solely on the great mobility of those particles (see note, p. 202, Vol. II. of my Essays, 3d edition, London, 1800); and if this suspicion be well founded, it is certain that when this mobility ceases, the effect which depends on it must cease likewise.

When I speak of the mobility of the particles of a liquid amongst each other, I am very far, as I have already observed, from supposing that individually they can enjoy a free motion. I was formerly of that opinion, but a more attentive investigation of the phenomena has convinced me that I was mistaken. But although one individual particle of a liquid can never be put in motion in consequence of a change of its specific gravity occasioned by a change of temperature, yet what cannot happen to a single particle may easily and must necessarily happen to small masses of the liquid consisting of a great number of these particles united, as is abundantly proved by the currents which are so easily excited by the contact of a hot or cold body plunged in a liquid.

The force by which the particles of liquids adhere together is very great, and it is more than probable that it is the cause of many very interesting phenomena, and amongst others of the suspension of the heavy bodies which much lighter liquids so frequently hold in solution.

From the result of an experiment which I made some years ago in order to determine the measure of the viscosity or the want of perfect fluidity in water at the temperature of 64° F., I found reason to conclude that a solid body, having a surface equal to 368 square inches, which should weigh only one grain Troy more than an equal volume of water, would remain suspended in that liquid; and from this datum it is easy to find by calculation what ought to be the diameter of a small solid spherule of the heaviest matter, — of gold, for instance, — in order to its remaining suspended in water in consequence of the viscosity of that liquid.

Having made this calculation in order to satisfy my curiosity, I found that a solid spherule of pure gold, of the diameter of $\frac{1}{300000}$ (or exactly $\frac{1}{298719}$) of an inch, ought to remain suspended in water in consequence of the adhesion of the particles of that liquid to each other. But I shall return to this subject on a future occasion.

[The preceding paper is printed from Nicholson's Journal, XIV. (1806), pp. 355–363. The paper, in a somewhat modified form, appears in the Bibliothèque Britannique (Science et Arts), XXXII. (1806), pp. 123–141, to which periodical it was contributed by Count Rumford in manuscript, which was translated by the French editor. In this version the beginning of the paper is much abridged; but in the latter part Rumford elaborated his speculations in regard to the effects produced by viscosity on the propagation of heat in liquids more at length than in Nicholson's Journal. In order to give fully his views on the subject, this portion of the French paper is here appended.

"If there is no direct communication of heat between contiguous particles of water at different temperatures, then the *apparent* mean temperature, which results so quickly when cold water is poured into a mass of warm water, must be produced by currents caused by differences in the specific gravity of the masses of the liquid at different temperatures. And if it is asked why the hot and cold particles, thus mixed together, do not separate again, on account of the difference in their specific gravity, we must seek for the reason in the imperfect fluidity of the water. This cause may keep the particles of water suspended, out of their natural position, just as it keeps in suspension, as well in other liquids as in water, particles of foreign substances, which, although specifically heavier or specifically lighter than the medium, are so small that the amount by which they are heavier or lighter than the surrounding liquid is not sufficient to overcome its viscosity.

"This want of perfect fluidity, a condition common to all liquids in different degrees, gives rise to a great number of very interesting phenomena, and it is a subject worthy of the close attention of philosophers.

"From the result of an experiment which I made a long time ago in order to determine the measure of the viscosity of pure water at the temperature of 64° F., that is the force necessary to separate contiguous particles of that liquid, I found reason to conclude that a solid body, having a surface equal to 368 square inches, which should weigh only one grain Troy more than an equal volume of water, would remain suspended in that liquid; and from this datum it is easy to find, by calculation, what ought to be the diameter of a small solid spherule of the heaviest matter, — of gold, for instance, — in order to its remaining suspended in water in consequence of the viscosity of that liquid: and it is also easy to prove, from the inflection which light experiences in passing over the surface of an opaque solid body, that a considerable quantity of opaque solid matter could be held suspended in water without sensibly diminishing its transparency, and without changing its colour; that is to say, without giving any indication of its presence.*

"I have long suspected that the suspension of solid substances which are held in solution by liquids is due solely to the imperfect fluidity of the solvents, and the results of a great number of experiments which I have made to elucidate this important subject have always confirmed this opinion. Since, then, bodies specifically heavier than water can nevertheless remain suspended in that liquid, there can be no difficulty in admitting that isolated particles of cold water can equally remain motionless in the warm water with which they find themselves accidentally mixed. But although this may be true of the individual particles, the same principle cannot apply to masses of sensible size made up of a great number of these particles. These masses must yield to the natural effect of the differences in their specific gravity, and form currents which will be ascending or descending according as the masses in question are warmer or colder than the surrounding liquid; and these currents must contribute very largely to the intimate mixture of the particles at different temperatures, and must soon bring about a certain uniform temperature throughout the entire mass of the liquid.

"I said this uniformity of temperature may be only *apparent;* because, if water is really a perfect non-conductor of heat, the particles of cold water, at least those which have not been warmed by contact with the walls of the vessel in which they are contained, ought to remain cold in spite of their more or less intimate mixture with the warm particles; but, notwithstanding this fact, the mean temperature of the liquid, as shown by the thermometer, will be precisely the same as if there had been an actual communication of heat among the particles.

"Long after I had had reason to persuade myself that all the heat acquired by liquids, when they are warmed, is communicated by the vessel containing them to the individual particles, which are successively

* In order to satisfy my curiosity, I found, by calculation, the diameter which a small solid spherule of gold ought to have in order to its remaining suspended in water in consequence of the viscosity of that liquid. I found this diameter to be $\frac{1}{298719}$, or, in round numbers, $\frac{1}{300000}$ of an inch; that is to say, about two hundred times smaller than the diameter of a single fibre of raw silk, as spun by the worm,—an object which is so fine as scarcely to be visible.

brought into contact with its sides by the currents formed in the mass of the liquid, — a long time, I say, after I had adopted this opinion, I continued to doubt whether single particles of warm water, when completely surrounded by particles of cold water, and remaining undisturbed in the midst of them, might not be able to communicate to these neighbouring particles that excess of heat which the shortness of the time of contact when the particles are in motion does not allow them to impart to each other. Indeed, if the property of water by which it is an apparent non-conductor of heat depends solely upon the extreme mobility of its particles, it is evident that the communication of heat, under the circumstances just supposed, must necessarily take place; and it must be remembered that in this case the fluidity of the liquid, as far as the particles in question are concerned, is as truly destroyed as if the entire mass were converted into ice.

"The inquiry as to the non-conducting power of fluids — an inquiry to which my experiments and observations have given rise — is, no doubt, of great interest to science; and, whatever may be the final result of its investigation, I shall regard myself fortunate in having drawn the attention of a great number of enlightened philosophers towards an object which was long neglected, and which was so worthy of being studied."]

EXPERIMENTS AND OBSERVATIONS

ON THE

ADHESION OF THE PARTICLES OF WATER TO EACH OTHER.

WE often see small bodies of a specific gravity much exceeding that of water float upon the surface of that fluid. Such, for example, are very small grains of sand, fine filings of the metals, and even small sewing-needles.

So extraordinary a phenomenon has not failed to excite the attention of philosophers. It formed a subject of discussion at the last sitting of the Class, and as this remarkable fact is intimately connected with a subject of research upon which I have been long employed, I shall here give an account of some experiments I have made to elucidate the same, which have afforded results of considerable interest.

Suspecting that the presence of air adhering to these small floating bodies, which is generally considered as the cause of their supension, is not indispensably necessary for the success of the experiment, I made the following experiments.

Experiment No. 1. — Having half filled with water a wine-glass one inch and a half in diameter at its edge, I poured on the surface of the water a stratum of sulphuric ether, one inch and a half in thickness; and when the whole was perfectly still, I took a very small sewing-needle with a pair of pincers, which I introduced below the ether, where, holding it horizontally at a small distance from the surface of the water, I let it fall. The needle descended to the water, and there floated on its surface.

Experiment No. 2. — Having melted some tin, I poured it into a spherical wooden box, and, shaking it strongly, the metal in cooling was reduced to powder, which was then sifted.

On examining this powder with a magnifier, it appeared composed of small spherules of different sizes; but these spherules were too small to be distinguished by the naked eye.

I took up on the point of a spatula a very small quantity of this metallic powder, and poured it gently from the height of a quarter of an inch on to the surface of the ether which rested upon the water in the glass.

The powder descended wholly through the ether, and when it arrived at the surface of the water, it remained floating.

Experiment No. 3. — Having poured a large drop of mercury into a china plate, I broke it into a great number of small spherules.

In order to take up and convey these small spherules one by one, I made a small tool or shovel out of a piece of brass wire, five inches long, and about one twentieth of an inch in diameter, bent to a right angle at one of its extremities. This bent part was about a quarter of an inch long, and was hammered flat, sharpened, and made a little concave.

By means of this tool I took up a small spherule of mercury, about one sixtieth of an inch in diameter, which I carefully conveyed into the stratum of ether to the distance of about one twentieth of an inch from the surface of the water beneath; and there, by a little inclination of the instrument, I caused the spherule of mercury to roll gently on to the surface of the water.

The spherule descended to that surface, and there remained floating.

When the eye was placed lower than the surface of the water, and the spherule was observed by looking upwards through the glass, it appeared suspended in a kind of bag, a little below the level of the surface.

Having placed a second spherule of mercury on the surface of the water, it immediately moved towards the former, and, approaching it with an accelerated motion, fell down into the same cavity, which then became longer; but the two spherules did not unite.

Having placed a third spherule on the surface of the water, it joined the two others; but the weight of these three spherules together being too great to be supported by the kind of pellicle which is formed at the surface of the water, the bag was broken, and the spherules descended through the water to the bottom of the vessel.

When the experiment was made with a spherule of mercury a little larger, namely, about the fortieth or fiftieth of an inch, it never failed to break the pellicle of the water, and to descend through that liquid to the bottom of the glass. But when the viscidity of the water was increased by dissolving a small quantity of gum-arabic in it, still larger spherules of mercury were supported at the surface of the liquid.

A spherule of mercury of a proper size to be supported by water at its surface, if placed gently there, would not fail to make its way through the pellicle of the water, if let fall from too great an height.

All the preceding experiments were repeated with a stratum of essential oil of turpentine, and afterwards with one of oil of olives, placed on the water contained in the glass instead of the ether, and the results were in all respects similar. I thought, however, that the spherules of mercury which were suspended upon the water were rather larger when the surface of the water was covered with oil than with ether; and in the experiments made with the powder of tin poured on the oil, the finest parts of the powder in very small quantity floated on the surface of the oil.

Experiment No. 4. — Having found means to place a stratum of alcohol on the water contained in the glass, so that the two liquids appeared as distinct from each other as when the upper stratum was oil, I poured from a very small height a small quantity of the very fine powder of tin upon the alcohol.

This powder totally descended through the alcohol and the water, without giving the smallest indication of its having been subjected to any resistance at the surface of the latter fluid.

Though this last surface appeared very distinctly to the eye, yet, judging from the manner in which the metallic powder descended to the bottom of the glass, I am disposed to think that it had no existence; and, in fact, it is probable that it was destroyed by the chemical action of the alcohol in contact with the water.

In order to examine more accurately the kind of film which is formed at the surface of the water, I made the following experiment.

Experiment No. 5. — In a cylindrical glass with a solid foot, the diameter of which was fourteen lines, or about an inch and a half English, and ten inches in height, I poured very limpid water to the height of nine inches, and on the water I placed a stratum of ether, three lines or twelfths of an inch in thickness. I then placed on the surface of the water a number of small solid bodies, which remained suspended, such as a small spherule of mercury, some pieces of extremely fine silver wire, two or three lines in length, and a little of the powder of tin. When the whole was perfectly tranquil, I took the glass in both hands, and carefully raising it, I turned it three or four times round its axis with considerable rapidity, keeping it in a vertical position. All the small bodies suspended at the surface of the water turned round along with the glass and stopped when it was stopped; but the liquid water below the surface did not at first begin to turn along with the glass, and its motion of rotation did not cease all at once upon stopping that of the vessel. In fact, all the appearances showed that there was a real pellicle at the surface of the water, and that this pellicle was strongly attached to the sides of the glass so as to move along with it.

Upon examining with a good magnifier, through the stratum of ether, the small bodies which were supported at the surface of the water, the existence of this pellicle could no longer be doubted; more particularly when it was touched with the point of a needle. For in this case all the small bodies were observed to tremble at the same time.

Having left this small apparatus at repose in a quiet chamber until the stratum of ether was entirely evaporated, I examined it again with a magnifier. The surface of the water was precisely in the same state; the small solid bodies were still there, in the same situation, and at the same distances from each other.

When this experiment was made with a cylindrical glass of much larger diameter, the effects of the adhesion of the pellicle of the water to the sides of the vessel were much less sensible with regard to those parts of the same which were situated near the axis. It was dif ficult to prevent the small bodies which floated on the surface of the water from uniting, and when united they often formed masses too heavy to continue to be supported; and, having broken the pellicle of the water, they fell to the bottom of the vessel.

If the particles of water adhere strongly to each other, it appears to me to be a necessary consequence that a kind of pellicle will be formed at the upper surface of the liquid, and even at all its surfaces, whatever may be in other respects the mobility of these particles, or rather of the small liquid masses composed of a great number of them, when they are remote from the surface and possess their fluidity without impediment.

When a small solid body, placed on the surface of water, becomes wetted, it immediately descends beneath the pellicle, which no longer opposes its resistance. At this period the viscidity of the water begins to manifest itself in a very different manner, but with infinitely less effect than when it acts at the confines of the liquid. But it is not yet time to inquire into this part of our subject.

With a view to render sensible the resistance which the pellicle of the inferior surface of a stratum of water opposes to a solid body which passes through that stratum by falling freely downwards, I made the following experiment.

Experiment No. 6. — Having filled a small wine-glass to about half its height with very pure mercury, I poured a stratum of water of three lines in thickness upon the mercury, and upon that a stratum of ether of two lines.

When the whole was at rest, I took with the small tool before described a spherule of mercury of about one third of a line in diameter, and let it fall through the stratum of ether.

This spherule, being too heavy to be supported by the pellicle at the superior surface of the water, broke it, and descended through that fluid; but upon its arrival at the inferior surface it was stopped, and remained there, preserving its spherical form.

I moved this spherule with the extremity of a feather, and even compressed it; but it always preserved its form without mixing with the mass of mercury on which it appeared to rest.

It was no doubt the pellicle of the inferior surface of the stratum of water which prevented this contact, and as this pellicle was supported by the mercury on which it rested, I was not at all surprised to find that it could support, without being broken, a spherule of mercury much larger than the pellicle of the superior surface could support.

In order to satisfy myself that the viscidity of the water was the cause of the suspension of this mercurial globule at the bottom of that fluid, I repeated the experiment and varied it by substituting water containing a certain quantity of gum-arabic, in solution, in the place of pure water; and I found, in fact, that much larger spherules were supported when the viscidity of the water was thus augmented.

To prove this fact in another manner, I again varied the experiment, by placing a stratum of ether immediately upon the mercury. The particles of this liquid appear to have very little adhesion to each other; for which reason I imagined that the kind of film that would be formed at its surface must have very little force. The results of my experiment fully confirmed this conjecture.

The very smallest spherules of mercury which I let fall through this liquid seldom failed to mix immediately with the mass of mercury on arriving at its surface, where they entirely disappeared; and I have never succeeded in causing either a spherule of mercury, or the smallest metallic particle, or any other body of greater specific gravity than ether, to swim upon its surface.

The results of the experiment were not perceptibly different when alcohol was substituted in the place of ether.

It is known that ether evaporates very rapidly. Is not this another proof that the particles of this liquid adhere to each other with much less force than those of water? But the following experiment proves this fact in a decisive manner.

Experiment No. 7. — Having half filled a small cylindrical glass with mercury, I placed on the mercury a stratum of ether four lines in thickness, and blew upon the ether with a pair of common bellows.

In less than one minute the ether had disappeared.

The same experiment being made with water, no sensible quantity of this fluid had disappeared in one minute.

The objects which are before our eyes from the earliest periods of our lives seldom employ our meditation, and not often our attention. We see, without surprise, immense masses of dust raised by the winds and carried to great distances; and at the same time we know that every particle of this powder is really a stone, almost three times as heavy as water, and of a size so considerable that its form may be perfectly seen by means of a good microscope.

And we see also, without surprise, that water, which is much lighter than dust, and is composed of particles incomparably smaller, is not carried off by the wind in the same manner.

In order to convince ourselves that the particles of water do strongly adhere to each other, and that they require to do so in order to prevent the greatest confusion in the universe, we need only figure to ourselves the inevitable consequences that would result from the want of such an adhesion.

The particles of water would be raised and carried off by the winds with infinitely more facility than the finest and lightest dust. Every strong breeze setting in from the ocean would bring with it a great inundation. Navigation would be impossible, and the banks of all the seas, lakes, and large rivers would be uninhabitable.

The adhesion of the particles of water to each other is the cause of the preservation of that liquid in masses. It covers the surface with a very strong pellicle, which defends and prevents it from being dispersed by the winds. Without this adhesion, water would be more volatile than ether, and more fugitive than dust.

But the adhesion is also the cause of other phenomena, which are of the greatest importance in the phenomena of nature.

The viscidity which results from the mutual adhesion of the particles of water renders this fluid proper to hold all kinds of bodies in solution, as well the most heavy as the lightest, provided always that they be reduced to very minute particles.

I have found, by a calculation founded on facts which appear to me to be decisive, that a solid spherule of pure gold, of the diameter of $\frac{1}{300,000}$ of an inch, would be suspended in water by the effect of its viscidity, even though this small body should be completely wetted and submerged in a tranquil mass of the fluid.

This viscidity, or want of perfect fluidity, which causes it to hold every kind of substance in solution, renders it eminently proper to become the vehicle of nourishment to plants and animals; and we accordingly see that it is exclusively employed in this office.

If the adhesion of the particles of water to each other were to cease, and the fluidity of this body were to become perfect, every living being would perish by inanition.

May I be permitted to remark the simplicity of the means employed by Nature in all her operations!

May I be permitted to express my profound admiration and adoration of the Author of so many wonders!

[This paper is printed from Nicholson's Journal, XV. (1806), pp. 52–56, 157–159, 173–175.]

CONTINUATION

OF

EXPERIMENTS AND OBSERVATIONS

ON THE

ADHESION OF THE PARTICLES OF LIQUIDS TO EACH OTHER.

BEFORE proceeding with the account of my experiments, I shall take the liberty of going back to a distant period, and of describing to the Class an occurrence which first fixed my attention on this subject and led me to engage in these researches.

Being occupied in the year 1786 with a series of experiments on the oxygen gas which is disengaged from water when this liquid mixed with various solid substances is exposed to the action of the sun's rays, among the substances employed in my investigation was a quantity of raw silk, wound from the cocoon on purpose for this experiment, in a single thread, just as it is produced by the silkworm.

It being necessary for completing my calculations that I should determine with precision the amount of the surface of this thread, which was almost two leagues in length, and which weighed in the air only about 20 grains Troy, and having no means of measuring directly the exact diameter of the thread, I undertook to calculate it from the known length of the thread and the specific gravity of the substance.

It was in weighing this substance in water to ascertain its specific gravity, that I encountered difficulties which for a long time seemed to me insurmountable, but by the exercise of patience and due precaution, I succeeded, after somewhat long and difficult labour, in accomplishing my object.

Those who are in the habit of making delicate experiments with the hydrostatic balance will conjecture immediately, before I have time to say it, that it was the air which remained obstinately attached to the surface of the silk when I weighed it in water, which rendered this operation so difficult.

I do not wish to abuse the patience of the Class by giving it a detailed account of all the means I was obliged to try before finding an efficient remedy for this inconvenience; it will suffice to say, that the silk was weighed finally in water, and with precision, and I will here add in passing, that the specific gravity of this substance was found to be to that of water as 1734 is to 1000. The following phenomenon, however, which I noticed while weighing the silk in water, struck me forcibly.

The silk being in the form of a skein about 6 inches long, and tied loosely in order to allow the water readily to enter among all the threads, it was hung from one of the arms of an excellent hydrostatic balance in a large mass of distilled water which had previously been freed from air by long boiling.

The weight of the silk in this situation having been determined, it was then placed, by means of silver pincers, and without taking it from the water, into a small glass vessel of oval form, about 2 inches in diameter and 3 inches long, and weighed again.

The weight of the silk when weighed in the small glass vessel was sensibly greater than when it was weighed out of the vessel in the same large amount of water, and on repeating the experiment several times, the result was always the same.

The following appeared to me to be a satisfactory explanation of this phenomenon.

As silk is one of those substances which can be wet by water, it is evident that the particles of the liquid which were in immediate contact with the surface of the thread must have remained attached to it. These particles, having become thus fixed and immovable, were in contact with other particles which still enjoyed their freedom of motion, and these particles again were in contact with others farther from the silk, and so on. Now, as the fluidity of various liquids is evidently very different, it is more than probable that no liquid possesses perfect fluidity; consequently water does not: and if any force whatever is needed to separate its particles and make them move on each other, it is evident that, in this case, if a solid body specifically heavier than water were plunged into a quiet mass of this liquid, there should be an apparent loss of weight on account of the viscosity of the liquid, and this loss of weight would be in proportion to the extent of the surface of the body.

If, for example, the body is suspended by a thread, the thread will not support all the excess of the weight of the body over the weight of a mass of the liquid equal to the volume of the solid body; for a part of this excess would be supported by the adhesion which exists among those particles of the liquid which are in contact with the particles attached to the surface of the body.

This appeared to me too evident to need demonstration or even further explanation.

In one of the experiments in question, the silk being suspended freely in the water, it was in contact with this liquid by a very great surface (about 550 square inches), and its loss of weight on account of the viscosity of the liquid was very sensible; but when it was weighed in a small vessel which had been previously counterpoised very exactly in the water, the arm of the balance supported all the excess of the weight of the silk over that of an equal bulk of water without any diminution.

The results of these experiments have furnished data for calculating, with sufficient exactness, the degree of force with which particles of water adhere to each other, when it is a question of causing them to move one upon the other at a temperature of about 60° F., and I found it to be such that a solid body specifically heavier than water, having a surface equal to 368 square inches (English), when submerged in water, ought to lose in weight, on account of the viscosity of the water, an amount equal to 1 grain Troy.

The discovery of this fact has put me in position, not only to prove that all bodies in nature, the heaviest as well as the lightest, can be suspended and supported in still water, on account of its viscosity, provided they are reduced to a sufficiently small size, but also to determine by calculation that a solid spherule of gold about $\frac{1}{300,000}$ of an inch in diameter would remain suspended in this manner, as I have already announced to the Class in the memoir read at the session held on the 16th of June, the past year (1806).*

Having announced facts as remarkable as these, I refrained from entering into more minute details. I did not even think it necessary to observe that, even if I should have deceived myself somewhat in my estimation of the force of cohesion of the particles of water, still, if

* This calculation will be found in a note at the end of this paper (page 315).

it be only granted (and this cannot be called into question) that the fluidity of this liquid is not absolutely perfect, but that a certain amount of force is necessary, no matter how small it may be, to separate the particles from each other, this alone will be sufficient to establish all that I have asserted with regard to the necessary consequences of the adhesion of the particles of liquids to each other.

It would only be a question in each case of supposing that a solid body immersed in any liquid be reduced to a sufficiently small size, and it could be proved that it must necessarily remain suspended there. But it is easy to see that the greater the force of cohesion between the particles of a liquid, the more capable this liquid becomes of holding in suspension foreign bodies of all sorts.

Water appeared to me to possess this quality to a remarkable degree; and it is certain that if there had been need of a vehicle for the nourishment of plants and animals, one capable of holding in suspension and of transporting from one place to another all sorts of substances, very different in weight and size, *without affecting them chemically*, it would never have been possible to find one more fitted for this purpose than water.

Is it not probable that this is one of the principal designs of the existence of this liquid in the economy of Nature?

Being accustomed to see traces of great wisdom and of admirable simplicity in all those dispositions of Nature which I have been able to comprehend, I have been perhaps too much inclined, in my ordinary meditations, to admit this conclusion. I must, however, confess that the facts which have seemed to me to render it probable have made a deep impression upon me.

Having found that the adhesion of the particles of water to each other is so considerable, I was not slow to perceive that this adhesion ought to manifest itself in a very peculiar and sensible manner at the surface of the liquid; and it was then that I saw clearly that it might be possible to explain in a satisfactory manner several phenomena which have always been regarded as difficult of explanation; as, for example, the suspension of heavy bodies of small size which appear to float on the surface of the water; the concave form taken on by the surface of water when confined in a small vessel; the change of this form into convex when, the vessel having been filled to the brim, more liquid is added; the suspension of liquids in capillary tubes, etc.

I wrote, in the winter of 1800, a memoir on this subject, which I afterwards showed to several persons, among others to Professor Pictet, of Geneva, when he was in London in 1801; also to Sir Charles Blagden. The reason for not publishing it at that time was that I needed the assistance of profound analysis in order to finish it.

When I arrived at Paris in the spring of 1802, I took advantage of this occasion to consult the greatest geometers of the century on the embarrassing question which stood in my way. Four persons now present in this Assembly can remember the circumstance. I desired to know the form which the vertical middle section of a drop of water, or other liquid substance, would take if placed on a plane horizontal surface, supposing that the liquid was restrained solely by the resistance of a pellicle exerting a given force on its surface.

The problem appeared very simple, but its solution is extremely difficult. I did not know at that time that Segner had attempted to solve it. I had no knowledge of the memoir which he published on this subject more than forty years ago in the first volume of the memoirs of the Royal Society of Göttingen.

If I recall these facts, it is simply to prove that I have not taken the liberty of occupying the attention of the Class with a subject as difficult as a research on the adhesion of the particles of liquids, and the various phenomena dependent upon it, without previous meditation; and to prove that the opinions which I have ventured to bring before it were adopted a long time since, and have been often examined before being announced.

I have most certainly nothing more at heart than to preserve the esteem and deserve the confidence of every member of this illustrious Assembly. The favour which they have shown me in giving me the right to sit among them, which I regard in the light of a very distinguished honour, as well as my respect for their talents, makes me hold it as a sacred duty never to abuse their attention with trifles, or crude ideas, or opinions formed in haste and ill-digested.

If I ventured to speak of the *pellicle* of the water, it is because I really believed in its existence; and I believe in it still, and more firmly than ever.

Allow me to recall to the Class the phenomena which have seemed to me to indicate its existence.

When I have seen little steel needles float on the surface of this liquid without sinking into it, and even without being wet; when I have seen little globules of mercury roll about on the surface of the water, then, coming to rest, and sinking to a certain depth in the liquid without, however, being wet by it, remain as though suspended in a small pocket; when I have seen diminutive spider-like insects, with long legs, run about over the water without their feet sinking into the liquid, or even being wet by it; when I have seen several minute bodies at a distance from each other, resting upon the surface of the water contained in a small vessel, tremble every time that the surface of the water was touched with the point of a needle, — I have been unable to doubt the existence of a resisting surface, a sort of pellicle on the surface of the liquid.

There is another phenomenon which seems to me to furnish a demonstrative proof of the existence of this resisting surface. When water is heated in any vessel, as soon as the liquid begins to become warm a considerable quantity of air is disengaged in the form of spherical bubbles, larger or smaller, which, passing through the liquid from below upwards, escape into the air. Now it very often happens that these little bubbles, after having traversed the liquid with great rapidity, are stopped all of a sudden when they have nearly reached the surface.

What is it that stops these bubbles if not a resisting pellicle at the surface of the liquid?

I endeavoured, but in vain, to explain these facts, by calling to my aid the atmospheric air. I saw clearly, as I observed the little globule of mercury situated in its little pocket, which sank sensibly lower than the level surface of the water, and which was scarcely large enough to hold the globule, — I saw, I say, that the film of air, which we might suppose still attached to the surface of the globule (if such a film really existed), could not be thick enough to buoy up this heavy body and make it float, hydrostatically, on the surface of the water. But when to the testimony of these experiments and to that of several others of the same sort, which can readily be performed, is added the evidence furnished by the certain knowledge which we have of a strong adhesion which exists among the particles of water, and of the effect which this adhesion must necessarily produce at the surface of the liquid, it seems to me impossible to call into doubt the existence of a resisting layer extremely thin at the surface of the water.

In announcing the existence of a sort of pellicle at the surface of liquids, I was far from thinking that it was a new idea. I am aware that several philosophers, and among others one of our celebrated colleagues, M. Monge, had suspected it before I did, but I think that I was the first to devise and perform decisive experiments which have established the fact beyond doubt; and it is certain that the observations that I have published on the effect which the adhesion of the particles of liquids to each other must have in the economy of Nature, have been borrowed from no one.

If the existence of a resisting film at the surfaces of liquids has just been confirmed by the results of the learned analytical researches of one of our celebrated colleagues, I ought, without doubt, to regard this event as a proof very flattering to me, that my conjectures on this subject were not ill founded.

I know that there are some persons who imagine that the results of the calculations of the illustrious author of the *Mécanique Céleste* on the rising of liquids in capillary tubes are opposed to the opinions which I have published on the adhesion of the particles of liquids to each other; but, as far as I have been able to understand the data on which these calculations are founded, it seems evident to me that the *attraction* with which M. La Place supposes the particles of the liquid to be endowed, does not differ essentially from the force which I have designated by the name *adhesion;* and with regard to the pellicle, of which I have often spoken, since the calculation of this learned geometrician and philosopher is founded on the supposition that the mutual attractions of the particles of the liquid situated a certain distance below the surface of the liquid do not contribute in any way to the rising of the liquid in a capillary tube, nor to any other similar effects which he has considered, it seems to me that the calculations of M. La Place simply relate to the force of cohesion of the layer of particles at the surface, or, in other words, to the pellicle in question.

I must, however, confess that I am not sufficiently well versed in the higher geometry to understand fully the calculations of M. La Place on this subject; and I shall take good care not to pass judgment on them. One must have, without doubt, a very profound acquaintance with analytical methods to feel the force of his demonstrations; but I have such a high opinion of the talents of this man, learned and worthy of esteem both as a geometrician and as a natural-philosopher, that I am always inclined to receive his opinions in matters of science (as well as on every other subject) with the greatest deference.

The researches to which I have sought to call the attention of philosophers would be, no doubt, of less importance if it was merely a question of the explanation of a few facts, isolated and of little utility in their applications; but the adhesion of the particles of liquids to each other is probably the cause of a great variety of phenomena which affect us intimately; and for this reason the subject must be regarded as very interesting. It seems to me that it is to this adhesion, and to the changes of its intensity, arising from different circumstances, that we must look for the proximate cause of the growth of plants and of animals.

I have already observed that the strong force of adhesion existing among the particles of water renders this liquid peculiarly fitted to serve as the vehicle for conveying nourishment to all living beings; and I think that I can show that this force of adhesion can be very much decreased, that this actually happens very often, and that one of the necessary consequences of such a diminution would be the deposition or precipitation of foreign matters which this liquid holds in suspension on account of its viscosity.

If water ascends as sap in the capillary tubes of trees as far as the leaves, it is possible that it there undergoes some change, or that it there receives some addition, which diminishes its viscosity, and disposes it in this way to deposit matters which it holds in suspension and which contribute to the growth of the plant.

If, during the digestion of food which takes place in the stomach, water, aided perhaps by the gastric juice, seizes at first upon nutritive particles of every sort which are there found and holds them in suspension, is it not possible that this liquid thus loaded, being mixed subsequently with a portion of bile, at its entrance into the intestinal canal, is by this means rendered less viscous and consequently better fitted to pass easily through the lacteal veins, and more disposed to yield up the nutritive particles as it enters into circulation?

In case this conjecture be well founded, we ought, undoubtedly, to find that a mixture of bile with water would diminish, to a sensible extent, the viscosity of this latter liquid; and I actually found by experiments which I shall have the honour of laying before the Class at some future time in detail, that mixing 1 part of bile with 1000 parts of water diminishes the adhesion of the particles of water to each other nearly one third, that is to say, in the ratio of 23 to 16; and that if 1 part only of bile be mixed with 30,000 parts of water, the diminution is still very apparent. In a mixture of 1 part of bile with 300 parts of water, the adhesion in question is reduced almost one half.

Milk is a liquid which seems to be already elaborated and fitted to serve as nourishment for animals; now I have found, by decisive experiments, that the adhesion among the particles of this animal fluid is less than that among the particles of water in the proportion of 13 to 19¾.

The adhesion of the particles of urine to each other varies considerably. I have found it from 13¼ to 16, that of water being 19¾.

Many persons have endeavoured to discover the nature of diseases by the examination of the urine; no one, however, as far as I know, has ever proposed to measure the force of adhesion of its particles to each other, a thing as easy to determine as it is useful to ascertain.

How interesting it would be to know the force of adhesion of the particles of the gastric juice, of the pancreatic juice, of the lymph, and of the blood, both in health and in the various diseases! Of how great importance would a knowledge of these facts be to the physiologist and to the physician!

How useful it would be for those who study vegetable physiology to know the adhesive force of the particles of the sap when rising and when descending, and that too in the various seasons!

How much light would be thrown on all chemical operations taking place in the wet way, if we could estimate exactly the force of adhesion existing among the particles of the various liquid agents which there come into play!

How many wonderful reactions there are which seem to depend on such a simple thing as the imperfect fluidity of liquids!

It seems to me that the facts which I have just announced are of such a character as to excite all our curiosity, and I hasten to make them public in order to induce all those who cultivate the sciences to assist me in these interesting researches.

I feel deeply that all that a single individual can effect by his own labours during the course of his short life, in extending the vast domain of science, is unfortunately a very small matter. It is only by the simultaneous efforts of a large number of men with good heads and skilled hands that we can hope to see a sensible advance of this great enterprise, of which men will never see the completion; and for this reason those who with true love for science take more delight in seeing its progress than in obtaining the pleasures of gratified vanity, ought rather to seek to associate with themselves a great number of zealous and skilful co-labourers than to endeavour to do everything themselves.

Happily for the progress of this new branch of research, the apparatus to be employed is portable, and of great simplicity, and the experiments are as easy to perform as their results are decisive and satisfactory. In general, the only thing needed will be an inverted siphon, with one of its capillary arms provided with a scale for measuring the height of the liquid in this arm above the level of the top of the column in the large arm; for it is now well established, by the results of conclusive experiments, that the heights to which various liquids rise in the same capillary tube are in proportion to the degrees of force with which the particles of the several liquids adhere to each other.

The experiments for determining the diminution produced in this force by a given increase of temperature demand more complicated apparatus and special care. The apparatus which I have used in this research is before the Class. Since, in the present state of the physical sciences, we can hardly flatter ourselves that we are able to take a single step in advance, except with the aid of instruments devised with care and executed with the utmost precision, I always regard it as a duty to afford the Class an opportunity of judging, by its own observation, of the excellence of those used by me in such new experiments as I have the honour of describing to the Class.

I will show, presently, the way in which this apparatus is used, and I will give to the Class, at a subsequent sitting, the account of the results of the experiments in which it has been employed.

I will conclude this memoir with some observations on a very important point, which should, perhaps, be still further elucidated.

I have shown how I have proceeded in measuring that sort of adhesion of the particles of water to each other which produces the viscosity of this liquid, that is, the force which must be exerted in order to cause those particles to move on each other; but we must by no means suppose that the same force will suffice to separate these same particles from each other, when two of them, which are in contact, are drawn in opposite directions along the line passing through their centres.

The very considerable weight of a drop of water which remains suspended from a solid body shows evidently that this latter force is incomparably greater than I have found the former to be. Now, when a solid body rests on the surface of a liquid, it cannot penetrate into it without breaking the layer of particles which are at this surface, and which may be considered as forming a sort of pellicle; in order to break this pellicle, it is evidently necessary to separate the particles which compose it, by compelling them to withdraw from each other directly or nearly in lines passing through their centres, and it is for this reason that small solid bodies specifically heavier than water remain on the surface of this liquid without penetrating into it.

Likewise, when water issuing from the upper extremity of the shortened capillary tube of an inverted siphon forms a small hemispherical mass, resting on the end of the tube and attached to its walls, the convex surface of this small mass of liquid is formed by a layer of particles which resist, with all the force of their attraction for each other, every effort tending to separate them; and it is the resistance of this single layer of particles, or of several layers resting immediately one upon another, and together forming a sort of very thin pellicle, which sustains the entire weight of the column of water in the other arm of the siphon, which is situated above the level of the surface of this small mass of liquid.

I have recently established this fact by means of an experiment, which I regard as decisive.

Having found a way of placing in the middle of this small hemispherical mass of water little isolated solid bodies which displaced a great part of the liquid without being wet by that which remained, this arrangement produced no change, either in the exterior form or in the dimensions of the little hemisphere, or in the force displayed in resisting the pressure of the more elevated column of water in the other arm of the siphon.

NOTE.

(See page 303.) The following calculation, which is neither long nor difficult to follow, may be of service in understanding what has just been advanced.

A cubic inch of water, English measure, weighs 253.175 grains Troy; consequently a spherical mass of this liquid 10.8233 inches in diameter, and which would have a surface of 368 square inches, would weigh 168,060 grains. And since the specific gravity of gold is to that of water as 192,581 is to 10,000, a sphere of gold of the same diameter would weigh 3,236,525 grains in vacuo.

Now, a similar sphere weighed in water would lose of its weight in vacuo an amount equal to the weight of a mass of water of a volume equal to that of the sphere. It would weigh, therefore, 3,236,525 — 168,060 = 3,068,465 grains, a deduction being made for the slight amount of its weight which it would lose on account of the viscosity of the liquid.

Since the surface of the globe is equal to 368 square inches, we see, from the result of the experiment of which we have just given an account, that this decrease of weight must be exactly one grain. Consequently the sphere suspended in water will weigh on the beam of the balance only 3,068,464 grains, and it will lose $\frac{1}{3,068,465}$ of its weight on account of the viscosity of the liquid.

Let us suppose, now, that the diameter of this sphere were 10 times as small, or 1.08233 inches, and let us see according to what law the effect produced on the viscosity of the liquid will be increased by this diminution of volume.

The volumes and consequently the weights of spheres of different diameters being as the cubes of those diameters, while their surfaces are as the squares of the same lines, it is evident that the weight of the small sphere mentioned above must be 1000 times less than the weight of the large sphere (the cube of 10 being 1000 and its square 100), consequently the smaller sphere ought to weigh in water only 3068.465 grains (deduction being made for the effect of the viscosity of the liquid), and its surface would be 3.68 square inches.

Since the diminution of weight which was due to the viscosity of the liquid was only 1 grain when the surface of the sphere was 368 square inches, it is evident that this diminution ought to be 100 times smaller, or $\frac{1}{100}$ of a grain, in the case of the smaller globe which has 100 times less surface; now $\frac{1}{100}$ of a grain in the case of a body which weighs only 3068.465 grains is $\frac{1}{306,846.5}$ of the real weight of the body in water, and by this amount the weight will be diminished on account of the viscosity of the liquid. This quantity is precisely 100 times more considerable, relatively to the weight of the body in water, than we have found it to be in the case of a body 10 times as large.

Hence we may conclude (and this can also easily be shown by a rigorous demonstration) that when a solid sphere heavier than water is submerged in this liquid, the decrease of weight due to the viscosity of the liquid is inversely proportional to the diameter of the sphere.

For example, if, when the diameter of the sphere was 10.8233 inches, the decrease of its weight in water due to the viscosity of that liquid is to its weight in the same liquid in the ratio of 1 to 3068465;—

When the diameter is reduced to	The diminution of weight due to the viscosity of the liquid will be to the weight of the body in water
1.08233 of an inch	as 1 to 306846.5
0.108233	" 1 " 30684.65
0.0108233	" 1 " 3068.465
0.00108233	" 1 " 306.8465
0.000108233	" 1 " 30.68465
0.0000108233	" 1 " 3.068465
0.00000108233	" 1 " 0.3068465

And in this last case it is evident that the minute body must of necessity remain suspended in the liquid.

I know very well that these long numerical calculations must seem superfluous to geometers accustomed to algebraic calculation; but many persons who are unacquainted with algebra desire to have brought within their reach satisfactory proofs of the truth of a conclusion which is given out to them as certain, especially when it is to serve as the foundation of a theory which is applied to very interesting phenomena.

As soon as it is shown that the diminution of weight which a sphere plunged into water experiences as a result of the viscosity of this liquid is inversely proportional to the diameter of the sphere, and when we know the amount of this diminution in a particular case, it is easy to determine, by a very simple calculation, what will be the diminution taking place in another case.

For example, we can determine what will be the diameter of the largest sphere of gold which will remain suspended in water on account of the viscosity of this liquid. Proceeding in this manner, I found this diameter equal to $\frac{1}{283,505}$ of an inch.

[This paper is translated from the French as it appears in the Bibliothèque Britannique (Science et Arts), XXXIV. (1807), pp. 301 – 313, and XXXV. pp. 3 – 16.]

OF THE SLOW PROGRESS

OF THE

SPONTANEOUS MIXTURE OF LIQUIDS

DISPOSED TO UNITE CHEMICALLY WITH EACH OTHER.

IN order to obtain the most exact knowledge of the nature of the forces which act in the chemical combination of various bodies, one must study the phenomena of these operations, not only in their results, but more especially in their progress.

When we mix together two liquids which we wish to have unite, we take care to shake them violently, in order to facilitate their union; it might, however, be very interesting to know what would happen, if, instead of mixing them, they were simply brought into contact by placing one upon the other in the same vessel, taking care to cause the lighter to rest upon the heavier.

Will the mixture take place under such circumstances? and with what degree of rapidity? These are questions interesting alike to the chemist and to the natural-philosopher.

The result would depend, without doubt, on several circumstances which we might be able to anticipate, and the effects of which we might perhaps estimate *à priori*. But since the results of experiments, when they are well made, are incomparably more satisfactory than conclusions drawn from any course of reasoning, especially in the case of the mysterious operations of Nature, I propose to speak before this illustrious Assembly simply of experiments that I have performed.

Having procured a cylindrical vessel of clear white glass 1 inch 8 lines in diameter, and 8 inches high, provided with a scale divided from the bottom upwards into inches and lines, I put it on a firm table in the middle of a cellar, where the temperature, which seemed to be tolerably constant, was 64 degrees of Fahrenheit's scale.

I then poured into this vessel, with due precautions, a layer of a saturated aqueous solution of muriate of soda, 3 inches in thickness, and on to this a layer of the same thickness of distilled water. This operation was performed in such a way that the two liquids lay one upon the other without being mixed, and when everything was at rest I let a large drop of the essential oil of cloves fall into the vessel. This oil being specifically heavier than water, and lighter than the solution of muriate of soda on which the water rested, the drop descended through the layer of water; when, however, it reached the neighbourhood of the surface of the saline solution it remained there, forming a little spherical ball, which maintained its position at rest, as though it were suspended, near the axis of the vessel.

I then poured, with proper precautions, a layer of olive oil four lines in thickness on to the surface of the water, to prevent the contact of the air with the liquid, and having observed, by means of the scale attached to the vessel, and noted down in a register, the height at which the little ball was suspended, I withdrew, and, locking the door, I left the apparatus to itself for twenty-four hours.

In a preliminary experiment, made to determine in what proportions the saturated solution should be mixed with distilled water, that the mixture might have the same specific gravity as the oil of cloves, I found that a mixture composed of 1 measure of the solution and 9 measures of distilled water had a slightly higher specific gravity than the oil; but with 10 measures of distilled water the oil sank in the mixture.

As the little ball of oil, designed to serve me as an index, was suspended a very little above the upper surface of the layer of the saturated solution, this showed me that the precautions which I had taken were sufficient to prevent the mixing of the distilled water and the saline solution when I put one upon the other, and I knew that this mixture could not take place subsequently without causing at the same time my little *sentinel*, which was there to warn me of this event, to ascend.

There was, however, a single source of error which I was obliged to guard against. I had observed, in other experiments of this kind, that the air which was disseminated through or dissolved in water containing in solution a small quantity of muriate of soda left the liquid, and attached itself to the little ball of oil of cloves which I had introduced into it, and, having formed on top of it a little bubble scarcely visible, caused it to ascend in the liquid, even when the density of the liquid had not changed at all.

To prevent this accident, I boiled for some time both the saturated solution and the distilled water employed in the experiment, in order to free them from air, and, for the same reason, I subsequently covered the water with a layer of olive oil to prevent the contact of this water with the atmospheric air.

After the little apparatus mentioned above had been left to itself for twenty-four hours, I entered the cellar, taking a light in order to note the progress of the experiment, and I found that the little ball had risen 3 lines.

The next day, at the same hour, I observed the ball again, and I found that it had risen about 3 lines more; and thus it continued to ascend about 3 lines a day for six days, when I put an end to the experiment.

I afterwards made nearly similar experiments with saturated aqueous solutions of nitrate of potash, carbonate of potash, and carbonate of soda. In each of these experiments the surface of the saturated solution was covered with a layer of distilled water 3 inches in thickness, but the surface of this layer of water was not covered by a layer of olive oil; it was exposed to the air, and this circumstance was, without doubt, the reason that the daily results of a single experiment were not always the same two days in succession.

The little ball of oil of cloves, which served as an index to mark the progress of the mixture of the saturated solution with the distilled water resting upon it, ascended usually 2 or 3 lines in twenty-four hours, but sometimes I found that it had left its position and had risen to the very surface of the water.

In such cases it was, without doubt, borne upwards by the air which it had attracted from the liquid; for when I allowed a fresh drop of the same oil to fall into the water, I found that it never failed to descend immediately in the liquid, and to take up its position 2 or 3 lines above the level at which, the day before, I had found the ball which had now left its place.

In the experiments made with solutions of carbonate of soda and carbonate of potash, the balls of oil changed in appearance by the end of two or three days; from being transparent, they became semi-opaque and of a whitish color; they changed at the same time with regard to their specific gravity as well, and became a little lighter. These changes were evidently due to the beginning of saponification.

This accidental circumstance made it necessary for me to renew each day the drop of oil which served as the index, allowing the others to pursue their way to the surface of the liquid without paying any further attention to them.

By using as indices little glass balloons of proper size and thickness, instead of the drops of oil, the inconveniences arising from the saponification of the oil might be avoided.

But without spending more time on the details of these experiments, I hasten to return to their results. They showed that the mixture went on continually, but very slowly, between the various aqueous solutions employed and the distilled water resting upon them.

There is nothing in this result to excite the surprise of any one, especially of chemists, unless it is the extreme slowness of the progress of the mixture in question. The fact, however, gives occasion for an inquiry of the greatest importance, which is far from being easy to solve.

Does this mixture depend upon a peculiar force of attraction different from the attraction of universal gravitation, a force which has been designated by the name of chemical affinity? Or is it simply a result of motions in the liquids in contact, caused by changes in their temperatures? Or is it, perhaps, the result of a peculiar and continual motion common to all liquids, caused by the instability of the equilibrium existing among their molecules?

I am very far from assuming to be able to solve this great problem, but it has often been the subject of my thoughts, and I have made at different times a considerable number of experiments with a view of throwing light into the profound darkness with which the subject is shrouded on every side.

At a subsequent sitting of the Class, I shall have the honour of giving an account of the continuation of my researches on this interesting subject.

[This paper is translated from the Mémoires de l'Institut, etc., VIII., II., pp. 100–115.]

OF THE USE OF STEAM

AS A

VEHICLE FOR TRANSPORTING HEAT.

MANY attempts have been made, at different periods, to heat liquids by means of steam introduced into them; but most of these have failed; and, indeed, until it was known that fluids are non-conductors of heat, and, consequently, that heat cannot be made to *descend* in them (which is a recent discovery), these attempts could hardly succeed; for, in order to their being successful, it is absolutely necessary that the tube which conveys the hot steam should open into the *lowest part* of the vessel which contains the liquid to be heated, or nearly on a level with its bottom; but as long as the erroneous opinion obtained, that heat could pass in fluids *in all directions*, there did not appear to be any reason for placing the opening of the steam-tube *at the bottom of the vessel*, while many were at hand which pointed out other places as being more convenient for it.

But to succeed in heating liquids by steam, it is necessary, not only that the steam should enter the liquid at the bottom of the vessel which contains it, but also that it should enter it *coming from above*.

The steam-tube should be in a vertical position, and the steam should descend through it previous to its entering the vessel, and mixing with the liquid which it is to heat; otherwise this liquid will be in danger of being forced back by this opening into the steam-boiler: for, as the hot steam is suddenly condensed on coming into contact with the cold liquid, a vacuum is necessarily formed in the end of the tube; into which vacuum the liquid in the vessel, pressed by the whole weight of the incumbent atmosphere, will rush with great force and with a loud noise; but if this tube be placed in a vertical position, and if it be made to rise to the height of six or seven feet above the level of the surface of the liquid which is to be heated, the portion of the liquid which is thus forced into the lower end of the tube will not have time to rise to that height before it will be met by steam, and obliged to return back into the vessel.

There will be no difficulty in arranging the apparatus in such a manner as effectually to prevent the liquid to be heated from being forced backwards into the steam-boiler; and when this is done, and some other necessary precautions to prevent accidents are taken, steam may be employed, with great advantage, for heating liquids, and for keeping them hot in a variety of cases in which fire, applied immediately to the bottoms of the containing vessels, is now used.

In dyeing, for instance, in bleaching, and in brewing, and in the processes of many other arts and manufactures, the adoption of this method of applying heat would be attended not only with a great saving of labour and of fuel, but also of a considerable saving of expense in the purchase and repairs of boilers, and of other expensive machinery: for, when steam is used instead of fire, for heating their contents, boilers may be made extremely thin and light; and as they may easily be supported and strengthened by hoops and braces of iron, and other cheap materials, they will cost but little, and seldom stand in need of repairs.

To these advantages we may add others of still greater importance. Boilers intended to be heated in this manner may, without the smallest difficulty, be placed in any part of a room, at any distance from the fire, and in situations in which they may be approached freely on every side. They may, moreover, easily be so surrounded with wood, or with other cheap substances which form warm covering, as most completely to confine the heat within them and prevent its escape. The tubes by which the steam is brought from the principal boiler (which tubes may conveniently be suspended just below the ceiling of the room) may, in like manner, be covered so as almost entirely to prevent all loss of heat by the surfaces of them, and this to whatever distances they may be made to extend.

In suspending these steam-tubes, care must, however, be taken to lay them in a situation *not perfectly horizontal*, under the ceiling, but to incline them at a small angle, making them rise gradually from their junction with the top of a large vertical steam-tube, which connects them with the steam-boiler, quite to their farthest extremities; for, when these tubes are so placed, it is evident that all the water formed in them, in consequence of the condensation of the steam in its passage through them, will run backwards, and fall into the boiler, instead of accumulating in them and obstructing the passage of the steam (which it would not fail to do were there any considerable bends or wavings, upwards and downwards, in these tubes), or of running forward and descending with the steam into the vessels containing the liquids to be heated, — which would happen if these tubes inclined *downwards*, instead of inclining upwards, as they recede from the boiler.

In order that clear and distinct ideas may be formed of the various parts of this apparatus, even without figures, I shall distinguish each part of it by a specific name. The vessel in which water is boiled in order to generate steam — and which, in its construction, may be made to resemble the boiler of a steam-engine — I shall call the *steam-boiler;* the vertical tube which, rising up from the top of the boiler, conveys the steam into the tubes (nearly horizontal) which are suspended from the ceiling of the room, I shall call the *steam-reservoir.* To the horizontal tubes I shall give the name of *conductors of steam;* and to the (smaller) tubes which, descending perpendicularly from these *horizontal conductors*, convey the steam to the liquids which are to be heated, I shall exclusively appropriate the appellation of *steam-tubes.*

The vessels in which the liquids that are to be heated are put, I shall call the *containing vessels.* These vessels may be made of any form; and, in many cases, they may, without any inconvenience, be constructed of wood, or of other cheap materials, instead of being made of costly metals, by which means a very heavy expense may be avoided; or they may be merely pits sunk in the ground, and lined with stone or with bricks.

Each *steam-tube* must descend *perpendicularly* from the *horizontal conductor* with which it is connected, to the level of the bottom of the *containing vessel* to which it belongs; and, moreover, must be furnished with a good cock, perfectly steam-tight, which may best be placed at the height of about six feet above the level of the floor of the room.

This *steam-tube* may either descend *within the vessel* to which it belongs, or *on the outside of it*, as shall be found most convenient. If it comes down on the outside of the vessel, it must enter it at its bottom by a short horizontal bend; and its junction with the bottom of the vessel must be well secured, to prevent leakage. If it comes down into the vessel on the inside of it, it must descend to the bottom of it, or at least to within a very few inches of the bottom of it; otherwise the liquid in the vessel will not be uniformly or equally heated.

When the steam-tube is brought down on the inside of the containing vessel, it may either come down perpendicularly and without touching the sides of it, or it may come down on one side of the vessel and in contact with it.

When several steam-tubes belonging to different containing-vessels are connected with one and the same horizontal steam-conductor, the upper end of each of these tubes, instead of being simply attached by solder or by rivets to the under side of the conductor, must *enter* at least one inch *within the cavity of it;* otherwise the water resulting from a condensation of a part of the steam in the conductor by the cold air which surrounds it, instead of finding its way back into the steam-boiler, will descend through the steam-tubes, and mix with the liquids in the vessels below; but when the open ends of these tubes *project upwards within the steam-conductor*, though it be but to a small height above the level of its under side, it is evident that this accident cannot happen.

It is not necessary to observe here, that, in order that the ends of the steam-tubes may project *within* the *horizontal conductor*, the diameters of the former must be considerably less than the diameter of the latter.

To prevent the loss of heat arising from the cooling of the different tubes through which the steam must pass in coming from the boiler, all those tubes should be well defended from the cold air of the atmosphere, by means of warm covering; but this may easily be done, and at a very trifling expense. The horizontal conductors may be enclosed within square wooden tubes, and surrounded on every side by charcoal dust, fine sawdust, or even by wool; and the steam-tubes, as well as the reservoir of steam, may be surrounded, first by three or four coatings of strong paper, firmly attached to them by paste or glue, and covered with a coating of varnish, and then by a covering of thick coarse cloth. It will likewise be advisable to cover the horizontal conductors with several coatings of paper; for, if the paper be put on to them while it is wet with the paste or glue, and if care be taken to put it on in long slips or bands, wound regularly round the tube in a spiral line from one end of it to the other, this covering will be useful, not only by confining more effectually the heat, but also by adding very much to the strength of the tube, and rendering it unnecessary to employ thick and strong sheets of metal in the construction of it.

However extraordinary and incredible it may appear, I can assert it as a fact, which I have proved by repeated experiments, that if a hollow tube, constructed of sheet copper $\frac{1}{20}$ of an inch in thickness, be covered by a coating only twice as thick, or $\frac{1}{10}$ of an inch in thickness, formed of layers of strong paper, firmly attached to it by good glue, the strength of the tube will be *more than doubled* by this covering.

I found by experiments, the most unexceptionable and decisive, — of which I intend at some future period to give to the public a full and detailed account, — that the strength of paper is such, when several sheets of it are firmly attached together with glue, that a solid cylinder of this substance, the transverse section of which should amount to only one superficial inch, would sustain a weight of 30,000 pounds avoirdupois, or above 13 tons, suspended to it, without being pulled asunder or broken.

The strength of hemp is still much greater, when it is pulled equally in the direction of the length of its fibres. I found, from the results of my experiments with this substance, that a cylinder of the size above mentioned, composed of the straight fibres of hemp glued together, would sustain 92,000 pounds without being pulled asunder.

A cylinder of equal dimensions, composed of the strongest iron I could ever meet with, would not sustain more than 66,000 pounds weight; and the iron must be very good not to be pulled asunder with a weight equal to 55,000 pounds avoirdupois.

I shall not, in this place, enlarge on the many advantages that may be derived from a knowledge of these curious facts. I have mentioned them now, in order that they may be known to the public; and that ingenious men, who have leisure for these researches, may be induced to turn their attention to a subject, not only very interesting on many accounts, but which promises to lead to most important improvements in mechanics.

I cannot return from this digression without just mentioning one or two results of my experimental investigations relative to the force of cohesion, or strength of bodies, which certainly are well calculated to excite the curiosity of men of science.

The strength of bodies of different sizes, *similar in form* and composed of the *same substance,* — or the forces by which they resist being pulled asunder by weight suspended to them, and acting in the direction of their lengths, — *is not in the simple ratio of the areas of their transverse sections,* or of their *fractures,* but in a higher ratio; and this ratio is different in different substances.

The *form* of a body has a considerable influence on its strength, *even when it is pulled in the direction of its length.*

All bodies, even the most brittle, appear to be *torn asunder,* or their particles separated, or fibres broken, *one after the other;* and hence it is evident that that *form* must be most favourable to the strength of any given body, pulled in the direction of its length, which enables the greatest number of its particles, or longitudinal fibres, to be separated to the greatest possible distance short of that at which the force of cohesion is overcome, before *any of them* have been forced *beyond* that limit.

It is more than probable that the apparent strength of different substances depends much more on the number of their particles that come into action before any of them are forced beyond the limits of the attraction of cohesion, than on any specific difference in the intensity of that force in those substances.

But to return to the subject more immediately under consideration. As it is essential that the steam employed in heating liquids, in the manner before described, should enter the containing vessel at or very near its bottom, it is evident that this steam must be sufficiently strong or elastic to overcome not only the pressure of the atmosphere, but also the additional pressure of the superincumbent liquid in the vessel; the steam-boiler must therefore be made strong enough to confine the steam, when its elasticity is so much increased, by means of additional heat, as to enable it to overcome that resistance. This increase of the elastic force of the steam need not, however, in any case, exceed a pressure of five or six pounds upon a square inch of the boiler, or *one third part*, or *one half*, of an atmosphere.

It is not necessary for me to observe here, that in this and also in all other cases where steam is used as a vehicle for conveying heat from one place to another, it is indispensably necessary to provide *safety-valves* of two kinds, — the one for letting a part of the steam escape, when, on the fire being suddenly increased, the steam becomes so strong as to expose the boiler to the danger of being burst by it; * the other for admitting air into the boiler, when, in consequence of the diminution of the heat, the steam in the boiler is condensed, and a vacuum is formed in it; and when, without this valve, there would be danger, either of the sides of the boiler being crushed, and forced inwards by the pressure of the atmosphere from without, or of the liquid in the containing vessels being forced upwards into the horizontal steam-conductors, and from thence into the steam-boiler. The last-mentioned accident, however, cannot happen, unless the cocks in some of the steam-tubes are left open. The two valves effectually prevent all accidents.

* The steam which escapes out of the boiler through the safety-valve may very easily be made to pass into the reservoir of water which feeds the boiler, and be condensed there; which will warm that water, and by that means save a quantity of heat which otherwise would escape into the atmosphere and be lost.

The reader will, no doubt, be more disposed to pay attention to what has here been advanced on this interesting subject, when he is informed that the proposed scheme has already been executed on a very large scale, and with complete success; and that the above details are little more than exact descriptions of what actually exists.

A great mercantile and manufacturing house at Leeds, that of Messrs. Gott and Company, had the courage, notwithstanding the mortifying prediction of all their neighbours, and the ridicule with which the scheme was attempted to be treated, to erect a *dyeing-house*, on a very large scale indeed, on the principles here described and recommended.

On my visit to Leeds in the summer of the year 1800, I waited on Mr. Gott, who was then mayor of the town, and who received me with great politeness, and showed me the cloth-halls and other curiosities of the place; but nothing he showed me interested me half so much as his own truly noble manufactory of superfine woollen cloths.

I had seen few manufactories so extensive, and none so complete in all its parts. It was burnt to the ground the year before, and had just been rebuilt on a larger scale, and with great improvements in almost every one of its details.

The reader may easily conceive that I felt no small degree of satisfaction, on going into the dyeing-house, to find it fitted up on principles which I had some share in bringing into repute, and which Mr. Gott told me he had adopted in consequence of the information he had acquired in the perusal of my *Seventh* Essay.

He assured me that the experiment had answered, even far beyond his most sanguine expectations; and, as a strong proof of the utility of the plan, he informed me that his next-door neighbour, who is a dyer by profession, and who at first was strongly prejudiced against these innovations, had adopted them, and is now convinced that they are real improvements.

Mr. Gott assured me that he had no doubt but they would be adopted by every dyer in Great Britain in the course of a very few years.

The dyeing-house of Messrs. Gott and Company, which is situated on the ground floor of the principal building of the manufactory, is very spacious, and contains a great number of coppers, of different sizes; and as these vessels, some of which are very large, are distributed about promiscuously, and apparently without any order in their arrangement, in two spacious rooms, — each copper appearing to be insulated, and to have no connection whatever with the others, — all of them together form a very singular appearance.

The rooms are paved with flat stones, and the brims of all the coppers, great and small, are placed at the same height (about three feet) above the pavement. Some of these coppers contain upwards of 1800 gallons; and they are all heated by steam from *one steam-boiler*, which is situated in a corner of one of the rooms, almost out of sight.

The horizontal tubes, which serve to conduct the steam from the boiler to the coppers, are suspended just below the ceiling of the rooms: they are made, some of lead and some of cast-iron, and are from four to five inches in diameter; but when I saw them, they were naked, or without any covering to confine the heat. On my observing to Mr. Gott that coverings for them would be useful, he told me that it was intended that they should be covered, and that coverings would be provided for them.

The vertical *steam-tubes*, by which the steam passes down from the horizontal *steam-conductors* into the coppers, are all constructed of lead; and are from $\frac{3}{4}$ of an inch to $2\frac{1}{2}$ inches in diameter, being made larger or smaller according to the sizes of the coppers to which they belong. These steam-tubes all pass down on the *outsides* of their coppers, and enter them horizontally at the level of their bottoms. Each copper is furnished with a brass cock, for letting off its contents; and it is filled with water from a cistern at a distance, which is brought to it by a leaden pipe. The coppers are all surrounded by thin circular brick walls, which serve not only to support the coppers, but also to confine the heat.

The rapidity with which these coppers are heated by means of steam is truly astonishing. Mr. Gott assured me that one of the largest of them, containing upwards of 1800 gallons, when filled with cold water from the cistern, requires no more than *half an hour* to heat it till it actually boils! By the greatest fire that could be made under such a copper, it would hardly be possible to make it boil in less than an hour.

It is easy to perceive that the *saving of time* which will result from the adoption of this new mode of applying heat will be very great; and it is likewise evident that it may be increased almost without limitation, merely by augmenting the diameter of the steam-tube. Care must, however, be taken, that the boiler be sufficiently large to furnish the quantities of steam required. The *saving of fuel* will also be very considerable. Mr. Gott informed me that, from the best calculation he had been able to make, it would amount to near two thirds of the quantity formerly expended, when each copper was heated by a separate fire.

But these savings are far from being the only advantages that will be derived from the introduction of these improvements in the management of heat. There is one, of great importance indeed, not yet mentioned, which alone would be sufficient to recommend the very general adoption of them. As the heat communicated by steam can never exceed the mean temperature of boiling water by more than a very few degrees, the substances exposed to it can never be injured by it.

In many arts and manufactures this circumstance will be productive of great advantages, but in none will its utility be more *apparent* than in cookery, and especially in public kitchens, where great quantities of food are prepared in large boilers; for, when the heat is conveyed in this manner, all the labour now employed in stirring about the contents of those boilers, to prevent the victuals from being spoiled by burning to the bottoms of them, will be unnecessary, and the loss of heat occasioned by this stirring prevented; and, instead of expensive coppers or metallic boilers, which are sometimes unwholesome, and always difficult to be kept clean, and often stand in need of repairs, common wooden tubs may, with great advantage, be used as culinary vessels; and their contents may be heated by *portable fireplaces*, by means of steam-boilers attached to them.

As these portable fireplaces and their steam-boilers may, without the smallest inconvenience, be made of such weight, form, and dimensions, as to be easily transported from one place to another by two men, and be carried through a doorway of the common width, with this machinery, and the steam-tubes belonging to it, and a few wooden tubs, a complete public kitchen, for supplying the poor and others with soups and also with puddings, vegetables, meat, and all other kinds of food prepared by *boiling*, might be established in half an hour in any room in which there is a chimney (by which the smoke from the portable fireplace can be carried off); and when the room should be no longer wanted as a kitchen, it might, in a few minutes, be cleared of all this culinary apparatus, and made ready to be used for any other purpose.

This method of conveying heat is peculiarly well adapted for heating baths. It is likewise highly probable that it would be found useful in the bleaching business and in washing linen. It would also be very useful in all cases where it is required to keep any liquid at about the boiling-point for a long time without making it boil; for the quantity of heat admitted may be very nicely regulated by means of the brass cock belonging to the steam-tube. Mr. Gott showed me a boiler in which shreds of skins were digesting in order to make glue, which was heated in this manner; and in which the heat was so regulated that, although the liquid never actually boiled, it always appeared to be upon the very point of beginning to boil.

This temperature had been found to be best calculated for making good glue. Had any other *lower* temperature been found to answer better, it might have been kept up with the same ease, and with equal precision, by regulating properly the quantity of steam admitted.

I need not say how much this country is obliged to Mr. Gott and his worthy colleagues. To the spirited exertions of such men, who abound in no other country, we owe one of the proudest distinctions of our national character, that of being *an enlightened and an enterprising people.*

In fitting up the great kitchen at the house of the Royal Institution, I availed myself of that opportunity to show, in a variety of different ways, how steam may be usefully employed in heating liquids.

On one side of the room, opposite to the fireplace, and where there is no appearance of any chimney, I fitted up a steam-boiler, of cast-iron, which, to confine the heat, is so completely covered up by the brickwork in which it is set, that no part of it is seen. This boiler is supplied with water from a reservoir at a distance (which is not seen), and by means of a cock, which is regulated by an hollow floating ball of thin copper, the water in the boiler always stands at the same height or level.

The steam from this boiler rises up perpendicularly in a tin tube, which is concealed in a square wooden tube, by the side of the wall of the room, and enters an horizontal tin tube (concealed in the same manner) which lies against the wall and just under the ceiling.

From this horizontal steam-*conductor* three tubes descend perpendicularly (concealed in three square wooden tubes), and enter three different kitchen boilers (on a level with their bottoms), which are set in brickwork against the same side of the room where the steam-boiler is situated.

As each of these boilers has its separate fireplace, properly furnished with a good double door and register ash-pit door, and also with a canal, furnished with a damper, for carrying off the smoke, either of these three boilers may be used for cooking, either with a fire made under it, or with steam brought into it from the neighbouring steam-boiler.

The object I had principally in view in this arrangement was to show, in the most striking and convincing manner, that all the different processes of cookery which are performed by boiling, such as boiling meat and vegetables *in boiling water*, making soups, stewing, etc., may in all cases be performed quite as well, and in many much better, by heating the liquid which is to be boiled, and keeping it boiling, by admitting hot steam *into it*, than by making a fire *under it*.

By using one of these boilers *alternately* in these two ways, on different days, in preparing the same kind of food, I concluded that all doubts on this subject would be most effectually removed.

To exhibit in a manner still more striking the application of steam to the boiling of liquids for culinary purposes, the following arrangement has been made and completed. A horizontal steam-conductor (concealed in a square wooden tube), communicating at right angles with the steam-conductor before described, passes, just below the ceiling, from the middle of one side of the room to the middle of the ceiling, and ends in a vessel in the form of a flat drum, about 10 inches in diameter and 5 inches high, which is attached to the ceiling perpendicularly over the centre of a large table which is placed in the middle of the room.

On the outside of this drum, or short hollow cylinder (which is made of tin and covered with wood, to confine the heat), there are, at equal distances, four projecting horizontal tubes, each about 1 inch in diameter and 2 inches long, which communicate with the inside of the drum. These tubes all point to the same centre, namely, to the centre of the drum.

To each of these short horizontal tubes there is fixed one end of a steam-tube composed of three pieces, fixed to each other, and movable, by means of joints, which are all steam-tight.

The end of this compound flexible steam-tube is united to the end of the short tube which projects from the side of the drum, by means of a steam-joint, in such a manner that the steam-tube attached to the drum, and communicating with it, may either be folded up in joints or lengths just under the ceiling, or it may be made to hang down from the end of the short tube to which it is attached. The lower joint, or rather division, of this flexible steam-tube, which reaches nearly to the top of the table, is furnished with a brass cock, by which it is occasionally closed, or, rather, by which it is always kept closed when it is not in actual use.

I might perhaps spare myself the trouble of describing the manner in which this culinary steam-apparatus is used, as the imagination of the reader will most probably have run before me. I shall, however, just mention a very striking and pleasing manner of making the experiment, in which the action of this machinery will be exhibited to great advantage.

If the cold water which is to be heated and made to boil by the steam is put into a large glass bowl or jar, on plunging the lower end of one of the flexible steam-tubes into the water, and then opening the steam-cock, the agitation into which the water in the glass vessel will be thrown will be visible through the glass; and the passage of the steam, in its elastic form, upwards through the water into the air, *after the water has become boiling hot* and not before, will be an instructive, as well as an amusing experiment.

Those of the flexible steam-tubes which are not in actual use are kept so folded up (in order to their being out of the way) that their two upper divisions, lying by the side of each other in a horizontal position, are just under the ceiling of the room; while their lower divisions hang vertically downwards, pointing towards the table.

In order that the kitchen may not be filled with steam when any of the boilers on the side of the room are used, their covers are all furnished with steam-tubes, which, communicating by a particular contrivance with a horizontal steam-tube which lies immediately over these boilers just under the ceiling, and which, by passing through the wall of the building, opens into the external air, all the waste steam from these boilers is carried out of the kitchen.

Before I conclude this Essay, I shall add a few observations concerning an application of steam which has not yet, to my knowledge, been made, but which there is much reason to think would turn out to be of very great importance indeed in many cases. This is the employing of it for communicating *degrees of heat above that of boiling water.*

I was led to meditate on this subject by an account I received, not long ago, of some very surprising effects which were produced in bleaching, by using the steam of a very strong solution of potash for boiling the linen, instead of water; as I was confident that no part of the alkali could possibly be evaporated in this process, I could not account in any other way for the effects produced, but by supposing them to have been owing to the *high temperature* of the steam which rose from this strong lixivium; and as steam, at a high temperature, might easily be procured and applied to the linen without the use of the alkali, I thought it would be worth while to try the experiment with hot steam produced from pure water. I mentioned this idea to Mr. Duffin, Secretary of the Linen Board in Ireland, who is himself concerned, in an extensive way, in the bleaching business, who has promised to make some experiments on this subject, which I took the liberty to point out and to recommend to him as being likely to lead to interesting results.

Meditating on the various uses to which *hot* or (which is the same thing) *strong steam* might be applied, it occurred to me that it would probably be found to be extremely useful in *alum works*, for concentrating the liquor from which alum is crystallized. There are, as is well known, many difficulties attending the evaporation and concentration of that liquid; and it is never done without occasioning a very considerable expense, as well for fuel, of which large quantities are consumed, as also on account of the frequent repairs of the pans, which are found to be necessary.

Most, if not all these difficulties might, I think, be avoided by introducing strong steam into this liquor, instead of concentrating it over a fire. This concentration might certainly be effected as well, and probably better and more expeditiously, by using hot steam, than by the immediate use of the heat of a fire, and the expense occasioned by the wear and tear of the apparatus would, no doubt, be much less in the former case than in the latter; and if it should be found (which is not unlikely) that *some certain temperature* is more advantageous in this process than any other, *that temperature*, when once discovered, may be preserved, with very little variation, when steam is used (by placing a valve, loaded with a proper weight, in the steam-tube, and obliging the steam to lift that valve, in order to pass through the tube); but there is no possibility of regulating, with any precision, the degrees of heat employed when liquids are evaporated in boilers over a fire.

I would just point out one more application of steam, which, if I am not much mistaken, will turn out to be very advantageous indeed in many respects; — it may be employed in heating the fermented liquor from which ardent spirits are distilled.

A proposal for introducing watery vapour into a liquor from which pure ardent spirits are to be distilled, or forced away by heat, will, no doubt, be thought very extraordinary by those who have never meditated on the subject; but when they shall have considered it with attention, they will find reason to conclude that this method of distilling bids fair to be very useful. The saving of expense for coppers and other costly utensils and machinery would be very considerable, and the danger of the flavour of the spirits being injured by the burning of the liquor to the sides of the copper would be entirely removed.

Steam has already been introduced, in several great manufactories in this country, into *drying-houses*, and employed with the best effects for heating and drying linen, cotton, and woollen goods, after they have been washed; it has also been used in the *drying-rooms* of several paper-manufactories. When it is used for any of these purposes, it should be introduced into tubes of large diameter, or into several smaller tubes, constructed of very thin sheet copper (or into any other metallic tubes, *having a large surface*, that would be cheaper); and these tubes should be placed nearly in a horizontal position in the *lower part* of the drying-room and *under* the goods that are to be dried; and (in order to economize the heat as much as possible) the water resulting from the condensation of the steam in the steam-tubes should be conducted by small tubes, well covered with warm covering, into the reservoir which feeds the steam-boiler.

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OBSERVATIONS

RELATIVE TO

THE MEANS OF INCREASING THE QUANTITIES OF HEAT OBTAINED IN THE COMBUSTION OF FUEL.

IT is a fact which has been long known, that clays, and several other incombustible substances, when mixed with sea coal in certain proportions, cause the coal to give out more heat in its combustion than it can be made to produce when it is burned pure or unmixed; but the cause of this increase of heat does not appear to have been yet investigated with that attention which so extraordinary and important a circumstance seems to demand.

Daily experience teaches us that all bodies — those which are incombustible, as well as those which are combustible and actually burning — throw off in all directions heat, or rather calorific (heat-making) rays, which generate heat wherever they are stopped or absorbed; but common observation was hardly sufficient to show any perceptible difference between the quantities of calorific rays thrown off by different bodies, when heated to the same temperature or exposed in the same fire, although the quantities so thrown off might be, and probably are, very different.

It has lately been ascertained, that, when the sides and back of an open chimney fireplace in which coals are burned are composed of firebricks, and heated red-hot, they throw off into the room incomparably more heat than all the coals that could possibly be put into the grate, even supposing them to burn with the greatest possible degree of brightness. Hence it appears that a red-hot burning coal does not send off near so many calorific rays as a piece of red-hot brick or stone of the same form and dimensions; and this interesting discovery will enable us to make very important improvements in the construction of our fireplaces, and also in the management of our fires.

The fuel, instead of being employed to heat the room *directly* or by the direct rays from the fire, should be so disposed or placed as *to heat the back and sides of the grate,* which must always be constructed of firebrick or firestone, and *never of iron or of any other metal.* Few coals, therefore, when properly placed, make a much better fire than a larger quantity, and shallow grates, when they are constructed of proper materials, throw more heat into a room, and with a much less consumption of fuel, than deep grates; for a large mass of coals in the grate arrests the rays which proceed from the back and sides of the grate, and prevents their coming into the room; or, as fires are generally managed, it prevents the back and sides of the grate from ever being sufficiently heated to assist much in heating the room, even though they be constructed of good materials and large quantities of coals be consumed in them.

It is possible, however, by a simple contrivance, to make a good and an economical fire in almost any grate, though it would always be advisable to construct fireplaces on good principles, or to improve them by judicious alterations, rather than to depend on the use of additional inventions for correcting their defects.

To make a good fire in a bad grate, the bottom of the grate must be first covered with a single layer of balls, made of good firebricks or artificial firestone, well burned, each ball being perfectly globular, and about $2\frac{1}{2}$ or $2\frac{3}{4}$ inches in diameter. On this layer of balls the fire is to be kindled, and, in filling the grate, more balls are to be added with the coals that are laid on; care must, however, be taken in this operation to mix the coals and the balls well together, otherwise, if a number of the balls should get together in a heap, they will cool, not being kept red-hot by the combustion of the surrounding fuel, and the fire will appear dull in that part; but if no more than a due proportion of the balls are used, and if they are properly mixed with the coals, they will all, except it be those perhaps at the bottom of the grate, become red-hot, and the fire will not only be very beautiful, but it will send off a vast quantity of radiant heat into the room, and will continue to give out heat for a great length of time. It is the opinion of several persons who have for a considerable time practised this method of making their fires, that more than one third of the fuel usually consumed may be saved by this simple contrivance. It is very probable that, with careful and judicious management, the saving would amount to one half, or fifty per cent.

As these balls, made in moulds and burnt in a kiln, would cost very little, and as a set of them would last a long time, — probably several years, — the saving of expense in heating rooms by chimney fires with bad grates, in this way, is obvious; but still, it should be remembered that a saving quite as great may be made by altering the grate, and making it a good fireplace.

In using these balls, care must be taken to prevent their accumulating at the bottom of the grate. As the coals go on to consume, the balls mixed with them will naturally settle down towards the bottom of the grate, and the tongs must be used occasionally to lift them up; and as the fire grows low, it will be proper to remove a part of them, and not to replace them in the grate till more coals are introduced. A little experience will show how a fire made in this manner can be managed to the greatest advantage and with the least trouble.

Balls made of pieces of any kind of well-burned hard brick, though not equally durable with firebrick, will answer very well, provided they be made perfectly round; but if they are not quite globular, their flat sides will get together, and by obstructing the free passage of the air amongst them and amongst the coals will prevent the fire from burning clear and bright.

The best composition for making these balls, when they are formed in moulds and afterwards dried and burned in a kiln, is pounded crucibles mixed up with moistened Sturbridge clay; but good balls may be made with any very hard burned common bricks, reduced to a coarse powder, and mixed with Sturbridge clay, or even with common clay. The balls should always be made so large as not to pass through between the front bars of a grate.

These balls have one advantage, which is peculiar to them, and which might perhaps recommend the use of them to the curious, even in fireplaces constructed on the best principles: they cause the cinders to be consumed almost entirely; and even the very ashes may be burned, or made to disappear, if care be taken to throw them repeatedly upon the fire when it burns with an intense heat. It is not difficult to account for this effect in a satisfactory manner, and in accounting for it we shall explain a circumstance on which it is probable that the great increase of the heat of an open fire where these balls are used may in some measure depend. The small particles of coal and of cinder which in a common fire fall through the bottom of the grate and escape combustion, when these balls are used can hardly fail to fall and lodge on some of them; and as they are intensely hot, these small bodies which alight upon them in their fall are soon heated red-hot, and disposed to take fire and burn; and as fresh air from below the grate is continually making its way upwards amongst the balls, every circumstance is highly favourable to the rapid and complete combustion of these small inflammable bodies. But if these small pieces of coal and cinder should, in their fall, happen to alight upon the metallic bars which form the bottom of the grate, as these bars are conductors of heat, and, on account of that circumstance, as well as of their situation, — *below* the fire, — never can be made very hot, any small particle of fuel that happens to come into contact with them not only cannot take fire, but would cease to burn, should it arrive in a state of actual combustion.

These facts are very important, and well deserving of the attention of those who may derive advantage from the improvement of fireplaces and the economy of fuel.

There are some circumstances which strongly indicate that an admixture of incombustible bodies with fuel, and especially with coal, causes an increase of the heat, even when the fuel is burned in a closed fireplace. No fireplace can well be contrived more completely closed than those of the iron stoves in common use in the Netherlands; but in these stoves, which are heated by coal fires, a large proportion of wet clay is always coarsely mixed with the coals before they are introduced into the fireplace. If this practice had not been found to be useful, it would certainly never have obtained generally, nor would it have been continued, as it has been, for more than two hundred years.

The combination of different substances, combustible and incombustible, to form, artificially, various kinds of cheap and pleasant fuel, particularly adapted for the different processes in which the fuel is employed, is a subject well worthy of the attention of enterprising and ingenious men. How much excellent fuel, for instance, might be made with proper additions and proper management, of the mountains of refuse coal-dust that lie useless at the mouths of coal-pits; and how much would it contribute to cleanliness and elegance if the use of improved coke, or of hard and light fire-balls, could be generally introduced in our houses and kitchens, instead of crude, black, powdery, dirty sea coal! Of the great economy that would result from such a change there cannot be the smallest doubt.

It is a melancholy truth, but at the same time a most indisputable fact, that, while the industry and ingenuity of millions are employed, with unceasing activity, in inventing, improving, and varying those superfluities which wealth and luxury introduce into society, no attention whatever is paid to the improvement of those common necessaries of life on which the subsistence of all, and the comforts and enjoyments of the great majority of mankind, absolutely depend.

Much will be done for the benefit of society, if means can be devised to call the attention of the active and benevolent to this long-neglected, but most interesting subject.

The Royal Institution seems to be well calculated to facilitate and expedite the accomplishment of this important object. Indeed, it is more than probable that this, precisely, is the object which was principally had in view in the foundation and arrangement of that establishment.

[This paper is printed from the Journals of the Royal Institution of Great Britain, I. (1802), pp. 28–33.]

DESCRIPTION OF A NEW BOILER,

CONSTRUCTED

WITH A VIEW TO THE SAVING OF FUEL.

IT is well known that much is gained in the saving of fuel, when an extensive surface is given to that part of the boiler against which the flame strikes; but this advantage is often counterbalanced by great inconveniences. For a boiler of the form usually employed, having the bottom very much extended in proportion to its capacity, must necessarily present a great surface to the atmosphere, and the loss of heat, occasioned by the cold air coming in contact with this surface, may be more than sufficient to compensate the advantage derived from the extended surface of the bottom. And where the boiler is employed for producing steam, as it is indispensably necessary that it should be of a thickness sufficient to resist the expansive force of the steam, it is evident that, if the diameter be augmented (with a view to increase the surface of the bottom), a considerable expense is incurred on account of the additional strength that must be given to the sides.

Having been engaged in the year 1796 in a set of experiments in which I employed the steam of boiling water as a vehicle of heat, I had a boiler made for this purpose, on a new construction, which answered well, and even beyond my expectations; and as this boiler might be used with advantage in many cases, even where it is only required to heat liquids in an open boiler, this, and another motive, which it would be useless to mention in this place, have lately induced me to construct one here (at Paris) and to present it to the Institute.

The object chiefly had in view in the construction of this boiler was to give it such a form, that the surface exposed to the fire should be great in comparison with its diameter and capacity; and this without having a great surface exposed to the cold air of the atmosphere.

The body of the boiler is in the shape of a drum. It is a vertical cylinder of copper 12 inches in diameter and 12 inches high, closed at top and at bottom by circular plates.

In the centre of the upper plate there is a cylindrical neck 6 inches in diameter and 3 inches high, shut at top by a plate of copper 3 inches in diameter and 3 lines in thickness, fastened down by screws.

This last plate is pierced by three holes, each about 5 lines in diameter. The first, which is in the centre of the plate, receives a vertical tube, which conveys water to the boiler from a reservoir, which is placed above. This tube, which descends in the inside of the boiler to within an inch above the circular plate which forms its bottom, has a cock near its lower end. This cock is alternately opened and shut, by means of a floater which swims on the surface of the water contained in the body of the boiler.

The second of the holes in the plate that closes the neck of the boiler receives the lower end of another vertical tube, which serves to convey the steam from the boiler to the place where it is to be used.

The third hole is occupied by a safety-valve.

This description shows that there is nothing new in the construction or arrangement of the upper part of this boiler. In its lower part there is a contrivance for increasing its surface, which has been found very useful.

The flat circular bottom of the body of the boiler, which, as I said before, is 12 inches in diameter, being pierced by seven holes, each 3 inches in diameter, seven cylindrical tubes of thin sheet-copper, 3 inches in diameter and 9 inches long, closed below by circular plates, are fixed in these holes, and firmly riveted, and then soldered to the flat bottom of the boiler.

On opening the communication between the boiler and its reservoir, the water first fills the seven tubes, and then rises to the cylindrical body of the boiler; but it can never rise above 6 inches in the body of the boiler, for when it has got to that height, the floater is lifted to the height necessary for shutting the cock that admits the water.

When the height of the water in the boiler is diminished a few lines by the evaporation, the floater descends a little, the cock is again opened, and the water flows in again from the reservoir.

As the seven tubes that descend from the flat bottom of the body of this boiler into the fireplace are surrounded on all sides by the flame, the liquid contained in the boiler is heated, and made to boil in a short time, and with the consumption of a relatively small quantity of fuel; and when the vertical sides of the body of the boiler and its upper part are suitably enveloped, in order to prevent the loss of heat by these surfaces, this apparatus may be employed with much advantage in all cases where it is required to boil water for procuring steam.

And as in the case where the boiler is constructed on a great scale, the seven tubes that descend from the bottom of the boiler into the fire may be made of cast-iron, whilst the body of the boiler is composed of sheet-iron or sheet-copper, it is certain that a boiler of this kind, sufficiently large for a steam-engine, a dyeing-house, or a spirit-distillery, would cost much less than a boiler of the usual form, of equal surface and power.

But in all cases where it is required to produce a great quantity of steam, it will be always preferable to employ several boilers of a middling size, placed beside each other, and heated each by a separate fire, instead of using one large boiler heated by one fire.

I have shown in my Sixth Essay, on the management of fire and the economy of fuel, that beyond a certain limit there is no advantage derived from augmenting the capacity of a boiler.

It will be perceived that the boiler which I have the honour of presenting to this Society is of a form fit for being placed in a portative furnace, and it was actually intended for that purpose.

Its furnace, which is made of bricks, with a circular iron grate of 6 inches in diameter, is built in the inside of a cylinder of sheet-iron, 17 inches in diameter and 3 feet high, and can be easily transported from place to place by two men.

This cylinder of sheet-iron, which is divided into two parts, in order to facilitate the construction of the masonry, weighs only forty-six pounds. The masonry weighs about a hundred and fifty pounds, and the boiler twenty-two pounds.

In order to form an estimate of the advantage which the particular form of this boiler gives it in accelerating its heating, we may compare the extent of surface that it presents to the action of the fire with that of the flat bottom of a common boiler.

The diameter of the bottom of a cylindrical boiler being 12 inches, the surface is 113.88 square inches; but the surface of the sides of the seven tubes that descend from the flat bottom of our boiler (which is likewise 12 inches in diameter) is 593.76 square inches. Therefore the new boiler has a surface exposed to the direct action of the fire, more than five times greater than that of a boiler of equal diameter and of the ordinary form; how much this difference must affect the celerity of heating is easy to conceive.

In the manner in which boilers are usually set, their vertical sides are but little struck by the flame, and on that account I have not taken the effect of the sides into consideration in my estimate; but even taking them into account, the new boiler will always have a surface exposed to the fire at least twice as great as that of a common cylindrical boiler of the same diameter, as can easily be shown.

The new boiler being 12 inches in diameter and 12 inches high, and each of its seven tubes being 3 inches in diameter and 9 inches high, its surface is 1160.44 square inches, without reckoning the circular plate that closes its top, nor its neck.

The surface of the bottom and sides of a cylindrical boiler of 12 inches in diameter and 12 inches high will be 566.68 square inches.

As the quantity of heat that enters a boiler in a given time is in proportion to the extent of surface that the boiler presents to the fire, it is evident that, other circumstances being the same, a boiler with tubes descending from its bottom will be heated at least twice as soon as a cylindrical boiler of the same diameter with a flat bottom.

In order that a cylindrical boiler with flat bottom, surrounded by flame on all sides, might have the same extent of surface exposed to the fire as a boiler with tubes, it would be necessary to give it a diameter greater than that of the boiler with tubes in the proportion of the square root of 1160.44 to the square root of 566.68, that is, of 17.171 to 12.

Therefore, in order that a cylindrical boiler with a flat bottom might have the same extent of surface exposed to the fire as our boiler with tubes of 12 inches in diameter, it would be necessary to give it a diameter of 17.171 inches.

But if the diameter of a boiler intended for producing steam be increased, it is necessary, at the same time, to increase its thickness, in order to increase its strength.

The necessary increase of thickness, and the expense that it will occasion, can be easily calculated.

The effort that an elastic fluid exerts against the sides of the containing vessel is in proportion to the surface of a longitudinal and central section of the vessel, and consequently in proportion to the square of its diameter, the form remaining the same. Hence we may conclude, that a steam-boiler of a cylindrical form with a flat bottom, which has the same extent of surface exposed to the fire as a boiler of 12 inches in diameter with tubes, should be at least twice as thick as this last, in order to have an equal degree of strength for resisting the expansive power of the steam.

The boiler which I have the honour of presenting to the Society is particularly intended to serve as a steam-boiler, but it may undoubtedly be applied to other purposes. Having shown it to M. Auzilly, son of a considerable soap-manufacturer of Marseilles, he thought that it might be employed with advantage in the making of soap; and from what he told me of the process, and of the boilers employed in that art, I am persuaded that the experiment would succeed perfectly.

But, after all, it remains to be determined whether it would not be still more advantageous to employ steam as a vehicle of heat in the making of soap, instead of lighting the fire under the bottom of the vessel in which the soap is made.

The result of an experiment which we are to make, M. Auzilly and myself, will probably throw some light upon this question.

[This paper is printed from Nicholson's Journal, XVII. (1807), pp. 5-10.]

EXPERIMENT

ON THE

USE OF THE HEAT OF STEAM, IN PLACE OF THAT OF AN OPEN FIRE, IN THE MAKING OF SOAP.

I HAD the honour of announcing to this Assembly, at the last meeting but one, that M. Auzilly and myself were to make an experiment on the use of steam in the making of soap. This experiment we have made, and with perfect success.

I have the honour to lay before the Society a piece of soap of about ten cubic inches, made in my laboratory by this new process, which required only six hours of boiling, whereas sixty hours and more are necessary in the ordinary method of making soap.

From all the appearances that we observed in the course of this experiment, and from its results, we think ourselves authorized to conclude that this new method of making soap cannot fail to be advantageous in every respect, and that it will soon be generally adopted.

We propose to repeat the experiment on a larger scale, as soon as we shall be able to procure the necessary utensils, and we beg the Society to appoint commissioners to be present during its execution.

As I intend to communicate to the Institute, upon a future occasion, all the details of our experiment, with an account of the apparatus we employed in it, I shall for the present make only one observation on the probable cause of the acceleration of the formation of soap, which we observed. I believe that this acceleration is due in great measure, if not entirely, to a motion of a peculiar kind in the mixture of oil and lye, occasioned by the sudden condensation of the steam introduced into the liquor. It is a sharp stroke, like that of a hammer, which made the whole apparatus tremble.

These strokes, which succeeded rapidly in certain circumstances, and which were violent enough to be heard at a considerable distance, must necessarily have forced the particles of oil and alkali to approach each other, and consequently to unite.

As the violence of these strokes diminished greatly as soon as the liquid had acquired nearly the temperature of the steam, I propose to supply this defect by a particular arrangement of the apparatus in the experiment we are going to make. I shall divide the vessel into two parts, by a horizontal diaphragm of thin sheet copper, and, causing a slow current of cold water to pass through the lower division or compartment of the vessel, I shall introduce steam into it, through a particular tube destined for that purpose, as soon as the mixture of oil and alkali which occupies the upper division of the vessel is become too hot for condensing the steam.

The steam which enters the water (always kept cold) that fills the lower compartment of the vessel will be condensed suddenly, and the sharp strokes which result will be communicated through the thin diaphragm to the hot liquid contained in the upper division of the vessel, and will, I expect, accelerate the union of the oil with the alkali. I shall then shut almost entirely the cock which admits steam into the upper division of the vessel, in order to prevent a useless consumption of steam and heat.

I shall not fail to give an account of the results of this new experiment to this Assembly; and I shall rejoice if by any researches I shall be so happy as to contribute to the improvement of an art which is undoubtedly of great importance to society.

[This paper is printed from Nicholson's Journal, XVII. (1807), pp. 10–12.]

ACCOUNT

OF SOME

NEW EXPERIMENTS ON WOOD AND CHARCOAL.

HAVING had occasion to dry several kinds of wood, to ascertain how much water was contained in them, I procured a piece of each kind six inches long and half an inch thick, and planed off some pretty thin shavings, which I kept to dry for eight days in a room, the temperature of which was constantly about 60° F. The wood had been previously drying two or three years in a joiner's workshop.

Of each kind of shavings I took 10 grammes (154.5 grains), which I placed on a china plate in a kind of stove made of sheet-iron, and heated them moderately by a small fire under the stove for twelve hours, after which they were suffered to cool gradually during twelve hours more. The stove, being surrounded with brickwork, was still hot twelve hours after the fire had been extinguished.

On taking out the china plates in succession and weighing the shavings anew, their weight was found to be diminished about one tenth, some a little more, others a little less. When the shavings were put into the stove, their weight was 10 grammes; when taken out, it was about 9. Their colour was not perceptibly altered, and they had no appearance of having been exposed to a strong heat.

Desirous of knowing how far the drying of wood might be carried, I replaced them all in the stove, which I heated as before, neither more nor less, for twelve hours, and afterward left to cool slowly for twelve hours.

On taking out the shavings the next day, they had all changed colour more or less; from a yellowish-white they had become light brown, dark brown, more or less yellow, and some of a fine purple.

Their weight, which was at first 10 grammes, was now found to be

Oak	7.16	Cherry	8.60
Elm	8.18	Linden	7.86
Beech	8.59	——— (after having been in the open air twenty-four hours)	8.06
Maple	8.41	Male fir	8.46
Ash	8.40	Female fir	8.66
Birch	7.40		
Service	8.46		

Wishing to know whether the wood might not be reduced to charcoal by continuing the moderate heat of the stove a long time, I took half the linden shavings, which weighed 4.03 grammes, placed them in a china saucer supported by a cylindrical earthen vessel 3 inches in diameter and 4 inches high, put this on an earthen plate, and covered it by a glass jar 6 inches in diameter and 8 inches high. On the earthen plate was a layer of ashes about an inch deep, serving to close the mouth of the jar slightly.

This little apparatus being placed in the stove, it was heated a third time for twelve hours, and then left twelve hours without fire, to cool gradually.

On taking out the apparatus, I found that the wood was become perfectly black, and that the glass jar was yellowish, and its transparency diminished.

On weighing the shavings, which retained their original figure completely, I was surprised to find that they weighed only 2.21 grammes. As they were the remains of 5 grammes of wood, and as, from the experiments of Messrs. Gay-Lussac and Thénard, I had expected to find in this wood at least fifty per cent of charcoal, I did not think it possible to reduce the weight of the shavings to less than 2.5 grammes, particularly with the moderate heat I employed.

To clear up my doubts, I replaced the apparatus in the stove, and heated it again as before for twelve hours, and afterwards left it in the stove twelve hours to cool.

On taking out the apparatus, I found that the shavings weighed only 1.5 grammes. The jar was less transparent, and of a blackish-yellow colour throughout, but particularly in its upper part, above the level of the brim of the saucer in which the shavings were. These shavings were still of a perfect black.

Having heated the apparatus again for twelve hours, and then left it to cool, I was surprised, on taking it out of the stove the next day, to find that the jar had again become clear and transparent. Not the least trace of the yellow coating with which its inner surface had been covered now remained.

On examining the wood, I found that this also had changed its colour. It had assumed a bluish hue, pretty deep, but very different from the decided black it had before. Its weight was 1.02 grammes.

I put it twice more into the stove, and each time its weight was diminished, so that the 5 grammes of wood were reduced at last to 0.27 of a gramme, or about a twentieth of the original weight.

I am persuaded that I should have diminished it still more, if I had continued the experiment longer; but it has been tried long enough to establish this remarkable fact, *that charcoal can be dissipated by a heat much less than has been considered necessary to burn it.*

It may be supposed that I was very desirous of knowing whether the same thing would occur to charcoal already formed by the usual process. Accordingly I took a piece of charcoal from my kitchen, heated it to a strong red heat, and, while it was still red, put it into a marble mortar, and powdered it. Having passed it through a sieve, I took 4.03 grammes of the powder, placed it in the saucer, heated it in the stove twelve hours, and then left it twelve hours to cool. On taking it out, it weighed but 3.81 grammes.

As this powdered charcoal was nothing but a collection of small bits of charcoal, which were in contact with the air only by a very small surface compared with that of the shavings, I made another experiment, the result of which was more striking and more satisfactory.

Having enclosed in a cloth a quantity of powdered charcoal, that had been passed through a sieve, I beat it strongly in a place where the air was still; and when the air appeared to be well loaded with the fine dust of the charcoal, I placed on the ground a white china saucer, quitted the place, and left the dust to settle.

The saucer was covered with it, so as to appear of a very dark gray.

Before all the dust had settled, I wrote some letters on the saucer with the point of my finger, and these letters were afterward covered with a still finer dust.

I imagined it possible that the part covered by a very fine dust might be found whitened, while that covered with a stratum of coarser charcoal powder would be found perhaps still black.

The result of the experiment showed that this precaution was not necessary. All the charcoal powder disappeared completely in the stove, and the saucer came out perfectly white.

Another saucer, which had been blackened a little by rubbing it with lampblack, and placed in the stove by the side of that blackened with charcoal dust, came out of the stove as black as it went in. As soon as I saw that the linden shavings converted into charcoal might be dissipated by the moderate heat of the stove, I suspected that they had been consumed slowly by a silent and invisible combustion, and that the product of this combustion could be nothing but carbonic acid gas.

To clear up this point I made the following experiment.

Having procured a stock of very dry birch shavings in ribands about a twentieth of a line thick, near half an inch broad, and six inches long, I dried them for eight days in a room heated by a stove, where the temperature was about 60° F., the shavings being laid on a table at a distance from the stove. Of these shavings thus dried, I took 10 grammes, which I placed on a china plate, and heated in the stove, in the manner already described, for twenty-four hours. When taken out of the stove, they weighed but 7.7 grammes, and had acquired a deep brown colour inclining to purple. They were still wood, however; for, though deeply browned, they burned with a very fine flame.

Of these brown shavings I made three parcels, each weighing 2.3 grammes. The first was placed in the stove on a white china plate, supported by a tile, but not covered. The second was put into it in a similar manner, except that it was covered with a glass jar, 6 inches in diameter and 6 inches high.

The third parcel was put into a glass vessel, 6 inches high, but only an inch and a quarter in diameter. This narrow vessel was put into a glass jar 3 inches in diameter and 7 inches high, which, being slightly closed with its glass cover, was also placed in the stove on a tile.

As the door of the stove (which is double, the better to confine the heat) does not shut so close as to prevent the free passage of air, and as the china plates on which two of the parcels were placed were flat, every circumstance was favourable for the free transmission of the carbonic acid gas arising from the decomposition of these two parcels by slow combustion, and there was nothing to prevent the progress of this operation. But the third parcel being enclosed in a narrow vessel, as this gas is much heavier than atmospheric air, the first portion of this gas arising from a commencement of combustion of the wood could not fail to descend in the vessel toward its bottom, gradually expel the air, and at length fill the vessel completely; and as this sort of inundation by carbonic acid gas could not fail to stop the combustion, I expected to find that this parcel of shavings would be preserved, at least in part, even though both the others should be entirely consumed.

The stove having been heated in the usual manner, I found the next day that the results of the experiment had been such as I anticipated. The two parcels of shavings placed on the china plates had disappeared entirely, nothing at all remaining except a very small quantity of ashes, of a white colour inclining a little to yellow.

The yellow ashes in the plate that was not covered with a glass jar were deranged and dispersed by the wind occasioned by opening the door of the stove too suddenly; but those in the other plate, being protected by the glass, were found all together. As they still retained their original figure of shavings, though reduced to a very small bulk, this appeared to me a demonstrative proof that the shavings, whence they arose, had not been burned by a common fire. For this reason, and also on account of their extraordinary colour, approaching very near that of the wood in its natural state, I preserved them, to show them to the Class. They weighed only 0.04 of a gramme; and as the shavings, of which they were the remains, weighed 2.987 grammes on coming out of the hands of the joiner, these ashes make only one and one third per cent of the weight of the wood.

The third parcel of shavings, which had been placed in a narrow glass vessel, had not disappeared, but the wood was converted into perfect charcoal. I have the honour to present it to the Class, in the same vessel in which it was charred.

As the three parcels of shavings were of the same wood, and equal in weight; as they were exposed together to the same degree of heat, and for the same time; and as the two portions that were placed so as to facilitate the escape of the carbonic acid gas arising from their decomposition, disappeared entirely, while the third, which was so circumstanced that the escape of this gas was impossible, did not disappear; — it seems to me that there can be no doubt of the cause of the phenomena that presented themselves; and it is certainly a curious fact, that charcoal, which has hitherto been considered as one of the most fixed substances known, can unite itself to oxygen, and form with it carbonic acid gas, at a temperature much below that at which it burns visibly.

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RESEARCHES

UPON THE

HEAT DEVELOPED IN COMBUSTION AND IN THE CONDENSATION OF VAPOURS.

SECTION I. — *Description of a new Calorimeter.*

ATTEMPTS have been long ago made to measure the heat that is developed in the combustion of inflammable substances; but the results of the experiments have been so contradictory, and the methods employed so little calculated to inspire confidence, that the undertaking is justly considered as very little advanced.

I had attempted it at three different times within these twenty years, but without success. After having made a great number of experiments with the most scrupulous care, with apparatus on which I had long reflected, and afterward caused to be executed by skilful workmen, I had found nothing, however, that appeared to me sufficiently decisive to deserve to be made public. A large apparatus in copper more than twelve feet long, which I had made at Munich fifteen years ago, and another, scarcely less expensive, made at Paris four years ago, which I have still in my laboratory, attest the desire I have long entertained of finding the means of elucidating a question that has always appeared to me of great importance, both with regard to the sciences and to the arts.

At length, however, I have the satisfaction of announcing to the Class, that, after all my fruitless attempts, I have discovered a very simple method of measuring the heat manifested in combustion, and this even with such precision as leaves nothing to be desired.

That the Class may be the better able to judge of my method of operating, and the reliance that may be placed on the results of my experiments, I place my apparatus before it.

The principal part of this apparatus is a kind of prismatic receiver, eight inches long, four inches and a half broad, and four inches three quarters high,* formed of very thin sheets of copper. This receiver, which well deserves the name, already celebrated, of *calorimeter*, is furnished with a long neck, near one of its extremities, three quarters of an inch in diameter, and three inches high, intended to receive and support a mercurial thermometer of a particular shape. The receiver has also another neck, an inch in diameter and the same in height, situate in the centre of its upper part, and closed by a cork.

Within this receiver, two lines above its flat bottom, is a particular kind of worm, receiving all the products of the combustion of the inflammable substances burned in the experiments, and transmitting the heat manifested in this combustion to a considerable body of water, which is in the receiver.

This worm, which is made of thin copper, occupies and covers the whole bottom of the receiver, yet without touching either its bottom or its sides. It is a flat tube, an inch and a half broad at one end and an inch at the other, and half an inch thick throughout. It is

* French measure.

bent horizontally, so as to pass three times from one end of the receiver to the other, and is supported in its place, two lines above the bottom of the receiver, by several little feet.

The aperture that forms the mouth of the worm is a circular hole in its bottom, near its broadest end. Into this hole is soldered a perpendicular tube, an inch in length and an inch in diameter, reaching within the worm to the height of a quarter of an inch above its bottom.

This tube passes through a circular hole in the bottom of the receiver, to which also it is soldered. Its lower aperture is seven lines below the bottom of the receiver; and through this the products of the combustion enter into the worm.

The other extremity of the worm passes horizontally through the perpendicular end of the receiver, opposite to that near which the products of the combustion enter the worm.

The worm, before it passes through the end of the receiver, is fashioned into the shape of a round pipe, half an inch in diameter; and an inch in length of this pipe is seen without the receiver. This piece is made to fit tight into another similar tube, belonging to the worm of another receiver, which I call the *secondary receiver;* the purpose of which is to receive the heat that might still be found in the products of combustion, after they have passed through the worm of the principal receiver.

To support these two receivers in the air, so as not to touch the table that supports them, each of them is fixed in a frame of dry linden wood, made of rods an inch square. Round the bottom of each receiver is a copper rim, three lines deep, which is fastened by a row of very small nails to the wooden frame. The body of the receiver itself enters about a line into the frame, to which it is very accurately fitted.

The flat form of the worm is essential to the perfection of the apparatus, as is evident when its purpose is considered.

All the products of the combustion being elastic fluids, and consequently substances incapable of communicating their heat but by proceeding particle after particle to deposit it on the surface of the cold and fixed body intended to receive it, it was indispensable so to construct the apparatus that the hot fluids should of necessity be spread *beneath* and *against* a large flat surface, placed horizontally, and always cold.

Before I employed horizontal worms made of flat tubes, I had more than once tried those of the common form; but they never answered my purpose otherwise than so imperfectly that I could never make any account of the experiments in which they were employed. There is no doubt but the shape I have adopted for the worm of my calorimeter would be very advantageous for every kind of apparatus for distillation.

One thing very important in the construction of my apparatus is the shape of the thermometer which I employ to measure the temperature of the water in the receiver. This thermometer — which I made myself, and which, after having undergone every kind of trial, has always appeared good — is a mercurial thermometer, divided according to Fahrenheit's scale. It is one of four, all similar, that I employed at Munich, in the winter of 1802, in my experiments on the refrigeration of liquids enclosed in vessels.

The reservoir of this thermometer is cylindrical, about two lines in diameter only, and four inches high; and as the water in my calorimeter is four inches deep, this thermometer always indicates the mean temperature of the fluid, whatever may be the temperature of its different strata.

In my various inquiries concerning heat, I have had frequent opportunities of seeing the importance of this precaution; and I cannot conceive how any one can expect to avoid great mistakes in measuring the temperature of liquids heated or cooled, if we do not attend to this. For my own part, I confess I pay little regard to the experiments of which I am told, when I know they are so negligently made; and assuredly I shall never waste my time in attempting to build theories on their results.

In using the apparatus I have described, several precautions are necessary. In the first place, it is obvious that when the object is to ascertain the quantity of heat developed in the combustion of any inflammable substance, it is indispensably necessary so to arrange matters that *the combustion shall be complete.* I have thought that it might be so considered, whenever the substance burned leaves no residuum, and burns with a clear flame, without smoke or smell.

The least smell, particularly that peculiar to the inflammable substance burned, is a certain indication that the combustion is imperfect.

I had long sought, before I was able to find, to my satisfaction, a mode of burning very volatile liquids, such as alcohol and ether; but I have at length discovered it, as will soon appear. I have frequently succeeded in burning highly rectified sulphuric ether, without the least smell of ether being diffused through the room; and it was in these instances alone that I considered the experiments as accurate.

As to wood, I have found a very simple method of burning it completely, without the least appearance of smoke or smell. I got a joiner to plane me shavings about half an inch wide, a tenth of a line thick, and six inches long; and holding these in the hand or with pliers, elevated at an angle of 45° or thereabout, and with the edges perpendicular, they burned like a match, with a very clear flame.

The slip of wood that burns being very thin, and placed between two flat flames which press on it closely, it is exposed to the action of so strong a heat that it burns perfectly and entirely.

If the shavings employed be too thick, a portion of the charcoal of the wood remains, particularly if it be oak, or any other wood of slow and difficult combustion; and in this case the experiments are defective. But if the shavings be sufficiently thin, and well dried, I have found that any kind of wood may be burned completely.

In burning candles, wax tapers, or fat oils in lamps, the only precautions necessary are so to arrange the wick as to yield no smoke; to place the flame properly in the aperture of the worm; and to surround the apparatus on all sides by screens, to prevent the flame from being deranged by the wind.

In these experiments there is one source of error, too obvious to escape the most superficial observer, and to which it was important to attend. While the calorimeter is warmed by the heat developed in the combustion of the inflammable substance which is burning at the aperture of the worm, it is continually cooled by the ambient air that surrounds it on all sides. It would be possible, no doubt, by calculations founded on a knowledge of the law of refrigeration of the receiver, which might be found by separate experiments, to ascertain the quantity of the effect produced by the refrigeration in question; and this even with a certain degree of precision: but it would have been impossible by this method, or by any other known, to calculate the effects of another cause of error, less obvious perhaps, but certainly more weighty, than that of the refrigeration of the external surface of the receiver.

The nitrogen which is mixed with the oxygen of the atmospheric air is necessarily carried into the worm with the proper products of the combustion; and without a precaution, which it occurred to me to employ to prevent the effects of this cause of error by making a compensation for them, all the experiments would have been of no value.

Fortunately the method I employed to obviate the effects of this cause of error was sufficient to prevent at the same time those that might have arisen from the cooling of the outer surface of the receiver.

As the receiver is cooled, whether by the atmospheric air in contact with its external surface or by the nitrogen and other gases traversing the worm with the products of combustion, only so far as the worm is hotter than the surrounding air, while, on the contrary, it is heated by these elastic fluids whenever it is at a lower temperature than they are, — by arranging matters so that the temperature of the water in the receiver shall be a certain number of degrees, 5° for instance, below the temperature of the air at the beginning of the experiment, and putting an end to the experiment as soon as the water in the receiver has acquired a temperature precisely the same number of degrees higher than the air, the receiver will be heated by the air during half the time of continuance of the experiment, and cooled by it during the other half; so that the calorific and frigorific effects of the air on the apparatus will counterbalance each other, and produce no perceptible effect on the results of the experiments; consequently they will require no correction.

When we are making experiments to elucidate natural phenomena, it is always more satisfactory to avoid errors, or to compensate them, than to trust to calculation for appreciating their effects.

As the law of the variation of the specific heat of water at different temperatures is not known, and as we have but an imperfect knowledge of the true measure of the intervals of temperature marked by the divisions of our thermometers, to prevent the effects that our uncertainty on these points would have on the subject of inquiry, I took care to make my experiments in a room where the temperature varied very little, and to confine them to a few degrees of elevation of the temperature of the water in the receiver.

It is true, I made some experiments in a room where the air was much colder, and in which I employed ice instead of water to fill the receiver; but these experiments were for a particular purpose, and are not classed with the others. Besides, they never afforded such uniform and satisfactory results as those made under other circumstances.

It has been fully proved, not only by the results of my experiments, but by the experiments of others also, that the vapour of water in contact with ice frequently freezes, while this same ice is melting by the heat, or that its thaw appears fully established.

To give an idea of the reliance that may be placed on the results of the experiments made with the new apparatus I have just described, I will introduce here the particulars of an experiment made purposely to discover its degree of perfection.

Having filled two receivers, properly connected with each other, with water at the temperature of the air of the room, 55° F., I burned a wax taper under the mouth of the principal receiver, so that all the products of the combustion passed through the worm of the secondary receiver, after having traversed that of the principal. Each of the receivers contained 2371 grammes [36621.5 grains] of water.

The following are the results of the experiment: —

TIME OF THE OBSERVATION.			TEMPERATURE OF THE WATER	
Hours.	Min.	Sec.	in the principal receiver.	in the secondary receiver.
9	37		55°	55°
	49	42	65	55
	56	15	70	55
10	2	52	75	55⅛
	9	32	80	55⅜
	16	34	85	55⅝
	23	54	90	55¾
	27			56
	31	40	95	56⅛
	39	35	100	56⅜
	47	40	105	56¾

From the results of this experiment it appears that the water in the secondary receiver did not begin to be heated perceptibly till that in the principal receiver had been heated 15° or 20°; and, as I had intended from the beginning never to continue an experiment longer than was necessary to raise the temperature of the water in the principal receiver 10° or 12° F., it may be supposed that, as soon as I found by this experiment how little heat remained in the products of combustion after they had passed through the worm of the principal receiver, I relinquished my original design of operating with the two receivers joined together. As it was evident, from the above results, that the second receiver could never be sensibly affected, or indicate anything except the confidence I might place in the indications of the first, I resolved to dispense with the trouble of using it.

It may be seen by the description I have given of this apparatus, that it may be used very conveniently for ascertaining the specific heat of gases, as well as that made apparent in the condensation of vapours, and generally in all researches where the quantity of heat communicated by an elastic fluid in cooling is to be measured. And as it would be extremely easy, by very simple means, to separate completely the products of the vapours condensed in the worm from the gases that pass through it without being condensed, I cannot avoid hoping that this apparatus will become useful as an instrument to be employed in chemical analyses. This, however, would only be an extension of the method already employed with so much success by M. de Saussure, and by Messrs. Gay Lussac and Thénard.

As soon as my apparatus was finished, I was eager to see what quantity of heat I should find in the combustion of wax and in that of olive oil, that I might afterward compare the results of my experiments with those of M. Lavoisier's; and, as I have the most implicit reliance on everything published by that excellent man, I sincerely wished to find in this comparison a proof of the accuracy of my method, and at the same time a confirmation of the estimates of M. Lavoisier.

SECTION II. — *Experiments made with white Wax.*

The air of the room being at the temperature of 61° F., 2781 grammes of water, of the temperature of 56° F., were put into the receiver of the calorimeter (including the quantity of this liquor that represents the specific heat of the instrument), and, a lighted wax taper having been properly placed at the entrance of the worm, the calorimeter was heated for 13 minutes and 26 seconds, when, the thermometer announcing that the water had acquired the temperature of 66° F., the taper was extinguished.

As I took care to weigh the taper before it was lighted, I found, by weighing it at the end of the experiment, that 1.63 grammes of wax had been burned.

To express the results of this experiment so as to render them obvious, and at the same time easy to be compared with the results of other similar experiments, we will see how much water of the temperature of melting ice would have been made to boil, at the mean pressure of the atmosphere, by the heat made apparent in the combustion of the 1.63 grammes of wax burned.

The distance on Fahrenheit's scale between the temperature of melting ice and boiling water being 180°, if the burning of 1.63 grammes of wax were requisite to raise the temperature of the water in the calorimeter 10°, the burning of 29.34 grammes would have been necessary to raise it 180°; and, if 29.34 grammes of wax could furnish by combustion sufficient heat to raise the temperature of 2781 grammes 180°, a gramme of this inflammable substance must furnish enough to heat 94.785 grammes of water to the same point.

Consequently, one pound of white wax, or wax taper, should furnish, in burning, sufficient heat to raise 94.785 pounds of water from the temperature of melting ice to the boiling point.

To find how many pounds of ice this quantity of heat would melt, we have only to add to the number of pounds of water at the temperature of melting ice it would cause to boil the third part of this number, and the sum would express the weight of the ice in pounds.

This, then, for white wax is: —

$$\begin{array}{r} 94.785 \\ +\ 31.595 \\ \hline \end{array}$$

$=$ 126.380 lbs. of ice melted for 1 lb. of the wax burned.

Before I compare the result of this experiment with that of an experiment made with the same substance by M. Lavoisier, I will give an account of two other experiments I made with wax, as the reader will undoubtedly be struck with the uniformity of their results. This is so remarkable that I should scarcely venture to publish them had I not proofs that all my experiments were actually made and minuted down before I began my calculation of their results, and were I not assured that any person who will follow my method, using the same apparatus, will find the same results on repeating my experiments.

As the mode of operating in making these experiments must now be well known, I may suppress the particulars in what follows without inconvenience, and give only the results of the experiments.

I will begin with three experiments made with white wax; and to render them more easy to compare, I will give them together in a tabular form.

Results of three Experiments on the Burning of white Wax, showing the Quantity of Water that would be heated 180°, or of Ice that would be melted, by one pound Weight of it.

No. of the Exp.	Quantity of wax burned.	Time employed in burning.	Quantity of water heated.	Elevation of its temperature.	Temp. of the water at the beginning of the exp.	Temp. of the water at the end of the exp.	Temperature of the air.	Results. Pounds of water heated 180°	Results. Pounds of ice melted.
	Grms.	m. s.	Grms.	Degrees.	Degrees.	Degrees.	Degrees.	lbs.	lbs.
1	1.63	13 24	2781	10° F.	56° F.	66° F.	61° F.	94.785	126.38
2	2.36	19 30		14½	51	65½	58	94.926	126.608
3	2.17	18 15		13¼	51¾	6	58	94.337	125.783

If we take the mean term between the results of these experiments, we shall find that the quantity of heat developed in the combustion of wax is such that one pound of this substance is sufficient to raise 94.682 pounds of water from the temperature of melting ice to the boiling point, and consequently that it should melt 126.242 pounds of ice.

According to the experiments of M. Lavoisier, the heat developed in the combustion of one pound of white wax was sufficient to melt 133.166 pounds of ice.

The difference between the results of our experiments with this substance is not very great; and if those of M. Lavoisier were made at a time when the temperature of the air was only a few degrees higher than that of melting ice (which I have no means of ascertaining), the quantity of nitrogen that must have entered into the calorimeter, with the oxygen employed to support the combustion, would have been so great as to account sufficiently for the difference. But the very great difference between the results of our experiments made with olive oil proves that one or other of our processes must have been defective.

The mean result of several experiments made with olive oil gave me for the measure of the quantity of heat developed in the combustion of one pound of this substance 90.439 pounds of water heated 180° F., or 120 pounds of ice melted, neglecting the fraction.

In the experiments of M. Lavoisier, more than 148 pounds of ice were melted by the heat that appeared to result from the combustion of one pound of this oil.

It is true that this result was considered by that eminent philosopher himself as too great to be capable of explanation; and he added, with that modesty which rendered him so engaging and so respectable: "We shall probably find ourselves under the necessity of making corrections, perhaps pretty considerable ones, in most of the results I have given; but I did not think this a sufficient reason to delay affording their assistance to those who might intend to pursue the same object."

As it appears very probable that all the fat oils, when perfectly pure, are composed of the same principles, I was curious to see whether rape oil, purified by sulphuric acid, would not afford more heat in its combustion than olive oil, when burned in its natural state. The result of three experiments showed me that rape oil, thus purified, does, in fact, yield more heat than olive oil. The difference is, indeed, pretty considerable, and more than I could have suspected.

The combustion of 1 lb. of purified rape oil gave . . .	93.073 lbs. of water heated 180°.
1 lb. of olive oil gave . .	90.439 " " " "

Chemists may tell us whether the quantity of incombustible matter separated from rape oil in purifying it be sufficient, or not, to account for this difference.

On comparing the results of the experiments made with white wax and those with the purified oil, it appears that equal weights of these substances afford nearly equal quantities of heat in their combustion; and as, in fact, this ought to be the case, from the quantities of combustible matter they contain, the result tends to strengthen our confidence in this method of measuring the heat developed in combustion.

The combustion of				
1 lb. of white wax gave .	94.682 lbs. of water heated 180°.			
1 lb. of purified oil . .	93.073 "	"	"	"

As the object I had chiefly in view in this series of experiments was to ascertain the quantities of heat developed in the combustion of pure hydrogen and carbon, in order to render this method useful in some chemical analyses, I examined particularly those inflammable substances that had been analyzed with most care.

Several attempts have been made to ascertain these interesting questions by direct experiments, in burning pure hydrogen, or pure hydrogen and carbon; but the results of these researches have varied so much that they cannot be relied on.

According to Crawford, the heat developed in the combustion of one pound of hydrogen gas is sufficient to raise the temperature of 410 pounds of water 180° F. But the estimation of M. Lavoisier is much lower. According to him, this heat would raise only 221.69 pounds of water the same number of degrees.

On the other hand, M. Lavoisier estimates the quantity of heat developed in the combustion of charcoal much higher than Dr. Crawford. I have many reasons to believe that they both estimate it too high; and, if this opinion be confirmed, we must estimate the heat developed in the combustion of hydrogen a little higher even than Crawford has done, to be able to account for that manifested in my experiments.

From several experiments, which I made five years ago, it appeared to me that one pound of charcoal, dried as much as possible before it was weighed by heating it red-hot in a crucible, was not capable of raising more than from 52 to 54 pounds of water from the temperature of melting ice to a boiling heat.

According to Crawford, this heat should suffice to boil 57.606 pounds, and according to Lavoisier, 72.375 pounds.

We shall see how these estimates agree with the results of my experiments.

As the experiments made with wax yielded very uniform results, and as the analysis of this substance has been made with great care, I shall examine how the quantities of hydrogen and carbon in this substance agree with the quantity of heat that it afforded me in combustion.

According to the analysis of Messrs. Gay-Lussac and Thénard, a pound of this substance contains

Carbon	0.8179 lb.
Free hydrogen	0.1191

If we adopt the calculations of Dr. Crawford, both for the heat furnished by the hydrogen and that furnished by the carbon, we shall have for the heat that should be furnished by the combustion

	Pounds of water raised from 32° to 212°.
Of 0.1191 lb. of hydrogen, after the ratio of 410 lbs. of water raised from 32° to 212° by burning 1 lb. of hydrogen	48.831 lbs.
Of 0.8179 lb. of carbon, after the ratio of 57.606 lbs. of water raised from 32° to 212° by burning 1 lb. of carbon	47.116 "
Total of the heat that ought to be furnished by the quantity of combustible matter (hydrogen and carbon) in 1 lb. of white wax	95.947 "
Quantity of heat furnished by 1 lb. of white wax, during its combustion, according to my experiments	94.682 "

If we adopt the calculations of M. Lavoisier for the heat furnished by carbon and hydrogen in their combustion, we shall have for the heat that ought to be furnished by the burning

Of 0.8179 lb. of carbon, after the ratio of 72.375 lbs. of water heated 180° by 1 lb.	59.195 lbs.
Of 0.1191 lb. of hydrogen, after the ratio of 221.69 lbs. of water heated 180° by 1 lb.	26.403 "
Total of the heat that ought to be furnished by the combustible matter in 1 lb. of white wax .	85.598 "

From the results of these calculations it appears that the estimations of Dr. Crawford agree much better with the experiments than those of M. Lavoisier.

Let us see how the results of the experiments made with fat oils agree with the estimations of these gentlemen.

According to the analysis of Messrs. Gay-Lussac and Thénard, a pound of olive oil contains

Carbon	0.7721 lb.
Free hydrogen	0.1208

According to the calculations of M. Lavoisier, we have,

For 0.7721 lb. of carbon .	55.881 lbs. of water heated 180°.	
" 0.1208 lb. of hydrogen .	26.780 " " " "	
Total	82.661 " " " "	

According to the calculations of Dr. Crawford, it is,

For 0.7721 lb. of carbon .	44.478 lbs. of water heated 180°.
" 0.1208 lb. of hydrogen .	49.528 " " " "
Total	94.006 " " " "

According to the experiments, 1 pound of purified rape oil furnished heat sufficient to raise 93.073 pounds of water 180°; and 1 pound of olive oil enough to heat 90.439 pounds.

From all these comparisons it follows that the estimations of Dr. Crawford agree much better than those of M. Lavoisier with the results of my experiments.

SECTION III. — *Experiments made with Spirit of Wine, Alcohol, and Sulphuric Ether.*

As the component parts of these inflammable liquids may be considered as well ascertained by the results of the excellent investigation of M. de Saussure, I undertook to examine them for the second time, in order to discover what quantities of heat are developed in their combustion. I had begun this undertaking five years ago; but, after having made a considerable number of experiments, I desisted from it on account of the great difficulties that occurred. As soon, however, as I had found means of rendering my apparatus more perfect, I formed the project of recommencing it.

Before I enter into the particulars of my experiments, I must say a few words respecting the difficulties that occurred to me, even after I had my new apparatus, and of the means I employed to surmount them. I even found myself exposed to dangers, which it is necessary for me to mention as a caution to those who may undertake the same inquiry.

When I made the experiments with highly rectified alcohol, and more particularly with ether, I found it very difficult to prevent a portion of these volatile liquids from escaping in the state of vapour from the bulk of them remaining in the lamp. I procured a small lamp, resembling in shape a small round snuff-box, with a nozzle rising from the centre of the circular plate, which closed it atop; and on this plate was fixed a small pan, to hold cold water, for keeping the nozzle cool and preventing the heat from being communicated to the body of the lamp. But this precaution was not sufficient, when I burned ether, as I found to my cost; for, though the pan was twice the diameter of the lamp, and filled with very cold water, the water was so heated in a few minutes that an explosion took place from vapour of ether kindling in the air with a flame that rose to the ceiling. Indeed it was near setting the house on fire.

Warned by this accident, I procured a new lamp, much smaller than the former, being only an inch in diameter and three quarters of an inch deep; and its nozzle, which was only two lines in diameter, was three quarters of an inch high. To keep this small lamp cool while burning, it was placed in a small pan, and kept constantly immersed in a mixture of water and pounded ice to within a quarter of an inch of the extremity of the nozzle. These precautions were sufficient to prevent any explosion, though not the evaporation either of the ether or of the alcohol. This fact I learned from observing that, as often as I made two consecutive experiments without filling the lamp afresh, the alcohol constantly appeared weaker in the second experiment than in the first.

The cause of this phenomenon was not difficult to discover. The most volatile and consequently the most combustible parts of this liquid, being diffused in vapour in the interior of the lamp, found means of escaping through the nozzle with the part of the liquid that traversed the match, leaving the alcohol that remained in the lamp perceptibly weakened.

To remedy this imperfection, I constructed a third lamp, which I now submit to the inspection of the Class. It is made of copper, and has the shape of a small cylindrical vase, an inch and a half in diameter, and three inches high, swelling out a little atop, and closed hermetically by a copper stopple, which, being ground with emery, fits tight into the neck of the vase. Through the centre of this stopple passes a small perpendicular hole, which can be shut completely or left a little open, as may be required, by means of a small screw carrying a copper collar.

A small tube, about an eighth of an inch in diameter and two inches and a half long, proceeds horizontally from the side of the vase very near the bottom. At the distance of an inch and four lines from the vase this tube is bent at a right angle, rising upwards perpendicularly to form the nozzle of the lamp.

This little tube is everywhere very thin, except at its upper extremity, where it is made thicker, to admit of being shaped so as to fit tight into a very small cylindrical extinguisher, five lines high by three and a half in diameter, intended to close the nozzle hermetically without touching or deranging the wick, the moment the lamp ceases to burn, and to keep it constantly closed when the lamp is not lighted.

Without this precaution, in experiments made with ether, so large a quantity of this volatile liquid would evaporate through the nozzle of the lamp while weighing, that it would be impossible to ascertain the quantity burned.

The nozzle of the lamp is steadied by two pieces of wire, proceeding from it horizontally, and soldered to the body of the lamp.

To keep this lamp constantly cold, as well as the liquid it contains, it is placed in a small pan, and covered completely, except the extremity of its nozzle and that of its neck, with a mixture of pounded ice and water.

When the lamp is weighed, it is taken out of the pan, and well wiped with a dry cloth before it is put into the scale.

When the lamp is kindled, the operator must not forget, after it has burned two or three minutes, to open the screw that closes its stopple a little, though but *very little*, otherwise it might go out.

As the little horizontal tube, by which the liquid that is burned passes from the reservoir of the lamp to its nozzle, is always filled with liquid, so that it can have no communication with the vapour diffused in the upper part of the reservoir, this vapour cannot escape by the nozzle of the lamp, as it did before I thought of this method of preventing it.

If I have been very minute in my description of this lamp, it is because I thought it necessary to spare those who might be disposed to repeat my experiments, or make similar ones, all the difficulties I had to surmount before I found the means of having under command the combustion of very volatile inflammable liquids.

As the apparatus I have employed has now been described, it will be easy to follow the steps of my experiments, and to appreciate their results. I will endeavour to describe them clearly, but also as briefly as possible.

Having procured a stock of spirit of wine of the shops, and of alcohol of different degrees of purity, I ascertained with the greatest care their specific gravities at the temperature of 60° F., taking that of water at the same temperature as 1000000. I chose this temperature that I might afterward the more easily ascertain the quantities of water that each ought to contain, according to the tables constructed from the experiments of M. Lowitz.

The following table will show the specific gravity of each, and the quantity of pure alcohol of Lowitz and of water contained in it.

Liquid.	Specific gravity at 60° F.	Composition.	
		Pure alcohol of Lowitz	Water.
Alcohol of 42°	817624	0.9179	0.0821
Alcohol of the shops	847140	0.8057	0.1943
Spirit of wine of 33°	853240	0.7788	0.2212

The following are the results of the experiments made to ascertain the quantities of heat which these liquids furnished in burning.

In three experiments made with the spirit of wine the quantities of heat manifested were, —

In the 1st, 53.260 lbs. of water raised from the temperature of
" 2d, 51.727 " melting ice to that of ebullition.
" 3d, 52.855 "
The mean result is . . . . 52.614 lbs.

As a pound of this liquid contained but 0.7788 of the alcohol considered by Lowitz as pure, the other part

(=0.2212) being only water, which does not burn, to find how much water would be raised from the temperature of melting ice to that of ebullition by a pound of the pure alcohol of Lowitz, we have only to divide the quantity, that is, the measure, of the mean heat developed in the experiments with the spirit of wine by the fraction that expresses the quantity of alcohol in a pound of this liquid.

Thus, we have $\frac{52.614}{0.7788} = 67.558$ pounds, the measure of the heat developed in the combustion of one pound of pure alcohol of Lowitz, according to the mean result of the experiments made with spirit of wine.

In two experiments made with the alcohol of the shops, the mean result was 54.218 pounds; and, as this contained 0.8057 pound of pure alcohol, we have for the measure of the heat developed in the combustion of 1 pound of pure alcohol $\frac{54.218}{0.8057} = 67.293$ pounds of water heated 180° F.

Of three experiments made with the alcohol at 42°, the mean result was 61.952 pounds of water heated 180° F. by the heat developed in the combustion of one pound of this liquid.

Hence, 1 pound of pure alcohol should furnish heat enough in burning to raise 67.57 pounds of water 180° F.; for $\frac{61.952}{0.9179} = 67.101$.

Taking the mean between the results of these eight experiments with three alcoholic liquors, we shall have for the measure of the heat developed in the combustion of one pound of pure alcohol of Lowitz 67.317 pounds of water raised from the temperature of melting ice to that of ebullition.

It will be extremely interesting, no doubt, to know whether this quantity of heat agree with the quantities of combustible matter (carbon and hydrogen) in alcohol. We will see.

According to the analysis of M. de Saussure, 1 pound of the alcohol of Lowitz contains

Carbon	0.4282 lb.
Free hydrogen	0.1018
Water	0.4700
	1.0000

Now, according to the calculations of Dr. Crawford, we shall have for the measure of the heat developed in the combustion of

0.4282 lb. of carbon	24.667 lbs. of water heated 180° F.
0.1018 lb. of hydrogen	41.738 " " " "
Total	66.405 " " " "
The experiments gave us	67.317 " " " "

It is rare in a research of such delicacy to find the results of experiment agree so perfectly with those of calculation.

Section IV. — *Heat developed in the Combustion of Sulphuric Ether.*

I have already mentioned the difficulties which I overcame before being able to regulate the combustion of this substance in such a way as to render the results of my experiments regular and satisfactory; but I met with still further difficulties in the course of this delicate inquiry.

As alcohol is necessarily employed in making sulphuric ether, and as these two liquids may be united in any proportions, it is extremely difficult, if not impossible, to separate them entirely ; and as both are colourless and limpid, either when mixed or separate, we can scarcely judge of the degree of purity of the ether, except by its specific gravity, and even in this way but very imperfectly.

The most highly rectified sulphuric ether which I could procure, and which I employed in my experiments, was prepared in M. Vauquelin's laboratory. Its specific gravity is 728.34 at the temperature of 16° Reaumur. As that which was employed by M. de Saussure in his analysis was only of the specific gravity of 717 at the same temperature, by regarding the ether which I employed as being a mixture of the same degree of purity with that of M. de Saussure, and the pure alcohol of Lowitz having a specific gravity of 792, we shall find, upon making a calculation, that the ether which I employed was a mixture of 85 parts of ether of the specific gravity of 717, and 15 parts of pure alcohol of Lowitz of the specific gravity of 792.

On burning this mixture under my calorimeter, after having brought my apparatus to the highest degree of perfection, I obtained the following results : —

Duration of the experiment.		Ether burned.	Quantity of water heated.	Temperature of water in the calorimeter		Elevation of the temperature of the calorimeter.	Temperature of the air.	Result.
				At the commencement.	At the end of the experiment.			Quantity of water heated 180° of Fahrenheit with the heat developed in the combustion of one pound of the combustible.
m.	s.	Grms.	Grms.	Degrees	Degrees.	Degrees.	Degrees	
11		1.96	2781	55½° F.	65⅝° F.	10⅛° F.	60° F.	79.996
11	15	2.01		54¼	64¾	10½	60	80.710
9		2.		58¾	69⅞	10⅜	60	80.146
20		3.29		56¾	73¾	17	61	79.884
22		3.06		56½	72½	16	64½	80.784
Mean result of the five experiments								80.304

Continuing to make use of the estimates of Crawford, for the quantities of heat developed in the combustion of hydrogen and carbon, we shall see if these estimates are sufficient to account for the heat manifested in these five experiments.

As the ether employed was a mixture of 15 parts of pure alcohol of Lowitz, and 85 parts of ether of the specific gravity of 717 at the temperature of 16° Reaumur, and consequently similar to the ether analyzed by M. de Saussure, we shall begin by determining the quantity of heat which ought to be developed in the combustion of these fifteen parts of alcohol.

As M. de Saussure has shown that in one pound of Lowitz's alcohol (of the specific gravity of 792) there are 0.4282 pound of carbon and 0.1068 pound of free hydrogen, we ought to find in 0.15 pound of this same liquid 0.06423 pound of carbon, and 0.01527 of free hydrogen.

According to the estimate of Crawford, 0.06423 pound of carbon ought to furnish a sufficiency of heat in its combustion to raise the temperature of 3.7002 pounds of water 180° F.; and 0.01527 pound of hydrogen ought to furnish enough to raise 6.2607 pounds of water the same number of degrees; and these two quantities of water, making together 9.9609 pounds, afford a measure of the quantity of heat which must be developed in the combustion of the 15 parts of alcohol, which are found mixed with 85 parts of ether, in order to form the combustible liquid employed under the name of sulphuric ether in my experiments.

Now, as one pound of this mixed liquid has furnished in its combustion enough of heat to raise 80.304 pounds of water 180° F., if we deduct from this mass the quantity of water which the 15 per cent of alcohol must heat (= 9.909), that which remains (= 70.3431 pounds of water) will be the measure of the quantity of heat developed in the combustion of 85 per cent of ether of the gravity of 717, which exists in this combustible liquid.

According to the analysis of sulphuric ether made by M. de Saussure, we ought to find in one pound of this liquid (of the specific gravity of 717)

Carbon	0.590 lb.
Free and combustible hydrogen	0.194
Oxygen and hydrogen in the proportions necessary to form water	0.216
	1.000

Consequently, we ought to find in 0.85 pound of the same kind of ether the following quantities of combustible substances, viz.: —

Carbon	0.5015 lb.
Free and combustible hydrogen	0.1651

We shall now see if these quantities of combustible substances are sufficient to account for the heat which is manifested in our experiments.

The 0.5015 pound of carbon ought to furnish sufficient heat to raise 28.89 pounds of water 180° F.; and the 0.1651 pound of hydrogen sufficient to heat 67.64 pounds the same number of degrees.

These two masses of water form together 96.53 pounds; but we shall see that the quantity of heat furnished by the 85 parts of ether in the experiments cannot be greater than that which is necessary to heat 70.3431 pounds of water 180° F.

As the experiments have been made with the greatest care, and frequently repeated, and always with very uniform results; and as the estimates which we have adopted, with respect to the quantities of heat which are developed in the combustion of hydrogen and in that of carbon, have been confirmed so as to leave little doubt upon this subject,—upon investigating the cause of the great difference between the quantity of heat actually developed in the combustion of the 85 parts of sulphuric ether burned in the experiments which we have examined, and the quantity given by calculation, we are compelled, in my opinion, to admit that there is an error in the analysis of this liquid, and that it does not contain so much free and inflammable combustible matter as M. de Saussure ascribes to it.

As it seems to me to be much more probable that an error has been committed in determining the quantity of free hydrogen in this substance than in determining the quantity of carbon, I shall suppose with M. de Saussure that there is really in one pound of sulphuric ether (of the specific gravity of 717) 0.59 of carbon; but instead of estimating the quantity of free hydrogen in this liquid according to the results of M. de Saussure, I shall adopt the estimate of Mr. Cruickshanks.

This excellent chemist concluded, from his experiments, that in the vapour of sulphuric ether the carbon is to the hydrogen as 5 to 1.

In the 0.85 pound of sulphuric ether (specific gravity 717) which were mixed with the 0.15 pound of alcohol, in order to form one pound of the mixed liquid employed in my experiments, there were 0.5015 pound of carbon; and dividing this number by 5, we shall see that this carbon ought to be united with 0.1003 pound of free hydrogen, instead of being united with 0.1651 pound, as we shall suppose according to M. de Saussure.

Let us now see if, by adopting the analysis of Mr. Cruickshanks with respect to the hydrogen, instead of that of M. de Saussure, the calculation will agree better with the experiment.

We have seen that the quantity of water heated 180° Fahrenheit, which represents the quantity of heat which must be developed in the combustion of the 0.15 lb. of alcohol, was . . .	9.9609 lbs.
And that the quantity answering to 0.5015 lb. of carbon, which exists in the 0.85 of ether, was .	28.89
We shall for the present add that which answers to the combustion of 0.1003 lb. of free combustible hydrogen, which, according to Mr. Cruickshanks, ought to be found united to this quantity of carbon in order to form the ether . .	41.123
These three quantities of water together are the measure of the heat which must be developed in the combustion of one pound of sulphuric ether of the kind employed in my experiments . . .	79.9739
The mean result of five experiments was . .	80.304.

This coincidence between the calculation and the experiment is, doubtless, too remarkable to be owing to chance; but I am ready to prove that it occurred without being foreseen or expected.

From all these results we may conclude, that one pound of sulphuric ether, of the specific gravity 717 at the temperature of 16° Reaumur, or of the same species with that employed by M. de Saussure, should have furnished in combustion enough of heat to raise 82.369 pounds of water 180° F., viz.: —

That furnished by 0.59 lb. of carbon . . .	33.989 lbs.
And that furnished by 0.118 lb. of hydrogen .	48.380
	82.369

If the proportion of free hydrogen in the ether analyzed by M. de Saussure was really such as he has determined it to be, one pound of this liquid ought to furnish a sufficiency of heat in its combustion to raise 113.566 pounds of water 180° F., viz.: —

That furnished by 0.59 lb. of carbon . . .	33.989 lbs.
And that which was furnished by 0.194091 of hydrogen	79.577
	113.566

But I can the less persuade myself that this liquid can furnish in its combustion so much heat, because one pound of white wax furnished no more than what was sufficient to heat 94.682 pounds of water the same number of degrees.

According to the analysis of M. de Saussure, 100 parts of sulphuric ether, of the specific gravity of 717, at 16° Reaumur, are composed of

Carbon	59 parts
Hydrogen	22
Oxygen	19
	100

Supposing that the 19 parts of oxygen are combined with 3.6 parts hydrogen, so as to form with them 21.6 parts water, 100 parts of this kind of ether ought to be composed of

Carbon	59
Free and combustible hydrogen	19.4
Consequently, inflammable substances	78.4
Water	21.6
	100

From the result of my experiments, 100 parts of this kind of ether ought to be composed of

Carbon		59
Free or combustible hydrogen		11.8
Consequently, combustible substances		70.8
Water		29.2
		100

Or, reducing the water to its elements, —

Carbon		59
Hydrogen, free or combustible	11.8	
Ditto, non-combustible	3.5	
		15.3
Oxygen		25.7
		100

According to M. de Saussure's analysis, as well as from the results of my experiments, 100 parts of pure alcohol of Lowitz, of the specific gravity of 792, at the temperature of 16° Reaumur, are composed of

Carbon	42.82
Free or combustible hydrogen	10.18
Consequently, combustible substances	53
Water	47
	100

Or, reducing the water to its elements, 100 parts of this alcohol are composed of

Carbon		42.82
Hydrogen, combined and non-combustible	5.64	
Hydrogen, combustible	10.18	
		15.82
Oxygen		41.36
		100

By supposing that water exists completely formed both in alcohol and ether, the constituent parts of these two liquids would be, according to the results of our inquiries,

	Alcohol.	Ether.
Carbon	42.82	59
Combustible hydrogen	10.18	11.8
Water	47	29.2
	100	100

The elements of water exist most assuredly both in alcohol and ether; but there is good reason to believe that water does not exist in its natural state of condensation in these two substances, neither when they are in a state of liquidity, nor when, being sufficiently heated, they are transformed into elastic fluids.

When we mix water with alcohol, there is a considerable change both in temperature and volume, which indicates a new arrangement of elements, or a chemical action; and what proves in a still more certain manner that this action has taken place, the liquid which results from this mixture may be distilled, i. e. *vaporized* by heat, and afterwards *condensed*, without being decomposed: but it is, above all, in the little heat which is developed in the condensation of the vapour of alcohol and ether that we discover certain proofs that the oxygen and hydrogen which exist as elements in these liquids do not exist in the state of water. I shall recur to this subject again.

SECTION V. — *On the Quantity of Heat developed in the Combustion of Naphtha.*

The naphtha which I made use of in my experiments was supplied by M. Vauquelin: it had been purified by distillation, and its specific gravity at the temperature of 56° F. was 827.31.

The following are the details and results of two experiments made with this liquid on the 29th of January, 1812.

The capacity of the calorimeter for heat was equal to that of 2781 grammes of water.

	Duration of the experiment.	Quantity of naphtha burned.	Elevation of the temperature of the calorimeter in degrees of Fahrenheit.	Result. Pounds of water heated 180° with 1 lb. of this substance.
	m.	Grammes.	Degrees.	lbs.
1st Experiment	32	4.45	16°F.	73.881
2d Experiment	36	2.77	12¾	72.771
Mean result				73.376

The naphtha was burned in the same small lamp which I had employed in my experiments made with alcohol and sulphuric ether; but as I had not been able to succeed in burning the naphtha without smoke, I cannot rely implicitly upon the results of these experiments. Perhaps with pure oxygen gas we might succeed in burning it entirely.

I have met with the same difficulty in burning oil of turpentine and colophon; and for this reason I thought it would be useless to detail my experiments with these two substances.

Section VI. — *On the Quantity of Heat developed in the Combustion of Tallow.*

Having procured tallow candles of a good quality, those which are called *six in the pound*, I burned one under the calorimeter, taking care to keep it well snuffed, in order to avoid smoke.

The following are the details and results of two experiments made on the same day (16th of November, 1811) with one of these candles.

The capacity of the calorimeter for heat was equal to that of 2371 grammes of water.

	Time while the candle was burning under the calorimeter.	Quantity of tallow burned.	Elevation of the temperature of the water in the calorimeter.	Result. Quantity of water heated 180° with the heat developed in the combustion of 1 lb. of tallow.
	m. s.	Grammes.	Degrees.	lbs.
1st Experiment	16 2	1.6	$10\frac{1}{4}$°F.	84.385
2d Experiment	16 50	1.7	$10\frac{1}{2}$	82.991
Mean result				83.688
We have seen that with white wax the result was .				94.682
With purified rape oil				93.073
And with olive oil				90.439

Section VII. — *Quantity of Heat developed in the Combustion of Charcoal.*

If we could burn under the calorimeter some pieces of wood made into charcoal with the same facility that we burn thin pieces of dry wood, the investigation in question would not be attended with difficulty; but the charcoal cannot be burned in this manner. We can light a piece of charcoal very well, and if it be very thin it continues to burn until it is entirely consumed; but the combustion is so slow, and furnishes so little heat, that it would require several hours to heat the calorimeter sufficiently to give an appreciable result; and for this single reason the result could not but be extremely uncertain.

I have long endeavoured, but without success, to find a method, by steeping thin chips of wood in some inflammable liquid, to burn the charcoal more rapidly.

Some chips of wood of a known weight, perfectly dried and strongly heated, were plunged into white wax, melted and very hot, and the chips, when taken out and cooled, were again weighed.

Their augmentation in weight gave me the quantity of wax which they had imbibed; and as I knew accurately how much heat this quantity of wax should have given in its combustion, if the chips thus prepared had been burned properly under the calorimeter, I should certainly have discovered how much heat the charcoal would have furnished; but the experiment did not succeed.

The wax was entirely burned, and the chip of wood became very red; but it was not burned, at least not entirely, nor in such a way as to give me the least hope of being able to derive any advantage from my experiment; and I did not succeed any better by steeping my chips of charcoal in melted tallow, in oil, alcohol, sulphuric ether, naphtha, essential oil of turpentine, in a solution of gum-arabic, and in that of sugar. I have also tried colophon, but without more success.

I have made several experiments in order to determine directly the quantity of heat which is developed in the combustion of considerable masses of charcoal (80 grammes) burned in a small stove, under a calorimeter of a large size, which I procured at Paris four years ago, and which I have still in my laboratory; but the results of these experiments have been too variable to satisfy myself.

After all the care which I took, I found that the experiments of Crawford were better than mine; and as they furnished more heat than I could find, I have not hesitated to adopt their results instead of relying upon my own.

Section VIII. — *Quantities of Heat developed in the Combustion of Wood.*

In a memoir which I had the honour to present to the Class on the 9th of September, 1812, I gave an account of a considerable number of experiments (upwards of fifty) which I made in order to determine the quantities of heat which are developed in the combustion of different kinds of wood.

From the results of these experiments, it appears that, at equal weights, the light and soft woods give out a little more heat than the compact and heavy woods; but as the difference is very small, we may rather ascribe it to a greater degree of humidity in the latter.

It is certain that the compact retain humidity with more tenacity than the light woods, and a small difference in the dryness of a wood ought to produce a sensible effect on its apparent weight, and consequently upon the result of the calculations which we employ in order to determine the heat which it furnishes.

In physical and chemical researches, it is always satisfactory to be able to compare the results of new experiments with those of more ancient date, particularly when the latter have been made by persons remarkable for their accuracy.

M. Lavoisier has shown that equal quantities of heat are produced in the combustion of 1089 parts in weight of oak, and 600 parts of charcoal; consequently equal quantities of heat ought to be furnished in the combustion of one pound of oak and 0.55 of a pound of charcoal.

According to the experiments of Dr. Crawford, one pound of charcoal furnishes in its combustion enough of heat to raise the temperature of 57.608 pounds of water 180° of F.

Consequently the temperature of 31.684 pounds of water would be raised the same number of degrees by the heat furnished in the combustion of 0.55 pound of charcoal.

According to the result of the experiments of M. Lavoisier, this same quantity of heat ought to be furnished in the combustion of one pound of oak.

Having made four consecutive experiments with very good dry oak wood, and in very thin slips, burned so as to give out neither smoke nor smell, and which left but an inappreciable quantity of ashes and no charcoal, I obtained the following results: —

Number of experiments.	Quantity of wood burned.	Elevation of the temperature of the calorimeter.	Result. Pounds of water heated 180° with one pound of combustible.
	Grammes.	Degrees.	lbs.
1	5.10	$10\frac{1}{4}$° F.	31.051
2	5.13	$10\frac{1}{2}$	31.623
3	5.12	$10\frac{3}{8}$	31.941
4	4.95	10	31.212
Mean result			31.457
Result according to Lavoisier and Crawford's experiments			31.684

It is rare to find experiments made by different persons at distant periods, and with very different apparatus, which agree better together.

But experiments which are well made can never fail in agreeing in their results, whatever be the difference of the methods employed: it is, nevertheless, necessary to remark, that the coincidence in question could not be so perfect as it appears, for everything depends upon the equality of the humidity which may exist in the wood and charcoal employed, — a circumstance which it is impossible to establish.

Section IX. — *On the greatest Intensity of Heat which it is possible to produce by the Combustion of inflammable Substances.*

It is well known that the heat of a small fire seems to be less intense than that of a large fire, even when the same species of combustible is employed; but I do not know that it has been attempted to determine the limits of the intensity of a fire, or the greatest degree of heat which it is possible to produce by means of combustion.

In order to elucidate this subject, it is necessary to consider attentively what passes in the chemical operation which we call *combustion*.

In all known cases where two elementary substances unite together so as to form a new substance, there is a change of temperature, so that the new substance *at the moment of its formation* has a temperature differing strongly from that of the surrounding bodies. Consequently, the surrounding bodies are always either heated or cooled more or less by the new body which has been formed.

But in order that this effect may be sensible to our organs, or capable of acting in a sensible manner upon our apparatus, it is necessary that the quantity of the new substance formed should be considerable; for it is certain that the most intense heat, if it be developed in a very small particle of matter, may exist without producing any sensible effect which could give us any indications of its existence.

It is not less true that the chemical union of two atoms, two different elementary substances, ought always, under every circumstance, to be accompanied with one and the same change of temperature; for this union takes effect in a place so distant, relative to all the other bodies (if, in every case, all the interstices are not filled with particles of an ethereal fluid), that we cannot conceive how the change of temperature in question may be either augmented or diminished by the effect of the action of these surrounding bodies.

It is extremely probable, from what we have been able to remark in a great number of phenomena, that the approximation of the elementary particles of bodies is always accompanied by an elevation of their temperature; and as there cannot be new substances formed except in consequence of an approximation and the chemical union of elementary particles, we may conclude that there cannot be new chemical compositions without a development of heat.

We may form an idea of what passes in combustion, by considering the phenomena which take place when water freezes.

At a certain temperature, which is invariable, the molecules of the liquid are disposed to approximate in order to form a solid body, ice; and the first particle of ice which is formed is accompanied by a development of a certain quantity of heat, which quantity is invariable.

It is also very probable that it is at a temperature which is *invariable* that the oxygen and hydrogen are disposed to approximate and unite in order to form an atom of vapour, and that the intensity of the heat developed at the moment of this union is also invariable, and that it is always manifested in all its intensity in the atom of vapour which is formed.

But as the atom of vapour is extremely small, and surrounded by bodies relatively very cold, its heat is soon dissipated.

There is, however, a method, which appears certain, that we may employ in order to determine the temperature of an atom of vapour at the moment of its formation, and by this means we may know what is the highest temperature which it is possible to procure by means of combustion.

We have seen that, according to the results of the researches of Dr. Crawford, it seems that when 1 pound of hydrogen is burned, enough of heat is developed on this occasion to elevate the temperature of 410 pounds of water 180° F. (= 100 degrees centigrade).

Now as 1 pound of hydrogen perfectly dry is united by burning to 7.3333 pounds of oxygen, and forms with it 8.3333 pounds of steam, it is evident that the quantity of heat which exists in 8.3333 pounds of steam at the instant when this steam is formed, is equal to that which is necessary to raise the temperature of 410 pounds of water 180° F., or to elevate the temperature of 73,800 pounds of water one degree of the scale of Fahrenheit.

From this calculation we may conclude that the quantity of heat which exists in 1 pound of steam, at the instant when it is formed, is sufficient to raise the temperature of 1 pound of water 10,063 degrees.

If the capacity of the steam for heat was equal to that of liquid water, it is very certain that the temperature of the vapour *at the instant of its formation* would be that of 10,063° F.

In order to form an idea of this degree of intensity, we may compare it to an intensity of heat which is known.

A piece of iron heated until it becomes red even in daylight has then the temperature of 1000° F.; consequently the temperature of the steam at the instant of its formation would be ten times higher than that of red-hot iron: but as, according to Crawford, the capacity of the steam for heat is greater than that of water in the proportion of 1.55 to 1, the temperature in question will be less than that of 10,063° in the same proportion. It will therefore be equal to 8750° F.

Here, therefore, is the limit of the intensity of the heat in the midst of the greatest fire, in which pure hydrogen would be employed as a combustible, and in which the fire would be fed by pure oxygen. This is an intensity which we may approach more or less, but which we can never attain.

As Wedgwood's pyrometer indicates much higher temperatures, it seems demonstrated by the result of this calculation that the scale of this pyrometer is faulty. These doubts have been stated by other chemists.

But in order to decide definitively upon this interesting question, it would be indispensably necessary to know accurately the capacity of steam for heat *at different temperatures;* a thing unknown, and which is difficult to determine.

Upon examining the subject attentively, we shall find, however, reasons for thinking that the capacity of steam for heat ought necessarily to be diminished with the increase of its temperature. The following calculations may serve to elucidate this subject.

In order to determine the highest degree of temperature which can exist in the midst of the greatest fire when pure hydrogen is the only combustible employed and when the fire is fed by atmospheric air, it is necessary to remark that, as oxygen and nitrogen are intimately mixed in the atmosphere, the heat which results from the combustion of hydrogen ought to be immediately divided between the vapour which results from the union of the hydrogen with the oxygen, and the nitrogen which is found necessarily mixed with this vapour.

In order to simplify our inquiry, we shall commence by supposing that all the oxygen which exists in the atmospheric air is employed.

In this case, as it requires 7.3333 pounds of oxygen to be united to 1 pound of hydrogen in order to compose 8.3333 pounds of steam, and as the atmospheric air is composed of 21 pounds of oxygen gas mixed with 79 pounds of nitrogen, the 7.3333 pounds of oxygen which are united to 1 pound of hydrogen in order to form 8.3333 pounds of steam, ought to be found mixed with 27.587 pounds of nitrogen; consequently the heat developed in the combustion of 1 pound of hydrogen ought to be also divided between 8.3333 pounds of steam and 27.587 pounds of nitrogen; and this partition ought to take place in the direct ratio of the weights of these two fluids, and of their capacity for heat.

The capacity of the steam being to that of nitrogen as 1.55 to 0.7036 (according to Crawford), all the heat in question will be divided so that the steam shall retain the part of it represented by the number 9.5832 ($= 8.3333 \times 1.55$); and the nitrogen will receive the other part of it, $= 19.41$ (being the product of 27.587 multiplied by 0.7036).

Now, as the two numbers 9.5832 and 19.41 are both in the proportion of 1 to 2.0254, it is evident that the temperature will be the same which we should have if all the heat in question was equally divided between the steam which would result from the combustion of 3.0254 pounds of hydrogen, i. e. between 25.2113 pounds of steam.

And as we have seen that the heat manifested in the combustion of 1 pound of hydrogen, which is in the 8.3333 pounds of steam which are the products of this combustion, is sufficient for raising the temperature of this steam to that of 8750° F., it is evident that if this same quantity of heat is divided among 25.2113 pounds of steam, the temperature of this steam could not be higher than 2891° F.

This is, therefore, the highest temperature which we ought to find in the midst of a strong fire fed by the atmospheric air in which the combustible burned is pure hydrogen.

As this temperature is much lower than that which we can excite by combustion, even without employing pure hydrogen or pure oxygen, the result of this calculation furnishes a demonstrative proof that the capacity for heat of steam, or rather that of nitrogen, is diminished when its temperature is increased. In all probability, the capacities of both, and generally of all elastic fluids, are diminished when their temperature is increased.

We shall now see what is the highest temperature which it would be possible to attain by burning charcoal, and by blowing the fire with pure oxygen gas.

According to Crawford, 1 pound of charcoal gives heat sufficient in its combustion to raise the temperature of 57.608 pounds of water 180° F., or to raise the temperature of 9369.44 pounds of water 1 degree.

Now, as 1 pound of charcoal is united to 2.5714 pounds of oxygen in burning, and forms with it 3.5714 pounds of carbonic acid, the heat which is found in 3.5714 pounds of carbonic acid *at the instant of its formation* would be sufficient to raise the temperature of 9369.44 pounds of water 1 degree; consequently the heat which is in 1 pound of this acid at the moment of its formation would be sufficient to raise the temperature of 3643.6 pounds of water 1 degree.

Here we have the *quantity* of heat which exists in the carbonic acid at the instant of its formation. In order to know what is the *intensity* which it would indicate if we could measure it at this moment by means of a thermometer, it would be necessary to know precisely the *specific heat* of the carbonic acid. If, with Crawford, we take it at 1.0459 (that of water being taken = 1), we shall have 3811° F. for the measure of the intensity of the heat which exists in the carbonic acid at the moment of its formation, and consequently for the intensity of the greatest fire made with charcoal (without mixture of hydrogen), even in the case where the fire is fed by *pure oxygen*.

It remains to determine the temperature which we might hope to obtain by burning charcoal with *atmospheric air*.

As we have found that the temperature of the 3.5714 pounds of carbonic acid, which are the product of the combustion of 1 pound of charcoal, is that of 3811° F. at the moment of its formation, we have only to ascertain how much the temperature of this acid ought to be diminished by the mixture of the nitrogen which must necessarily be there when the oxygen employed in the combustion of the charcoal is furnished by the atmospheric air.

As, in the atmospheric air, every pound of oxygen is mixed with 3.7619 pounds of nitrogen, the 2.5714 pounds of oxygen employed in the combustion of 1 pound of charcoal ought to be mixed with 9.6735 pounds of nitrogen; consequently all the heat developed in the combustion of 1 pound of charcoal will be found divided between 3.5714 pounds of carbonic acid and 9.6735 pounds of nitrogen.

And as the specific heat of the carbonic acid is to that of nitrogen as 1.0459 to 0.7036, this heat will be divided between these two substances in the proportion of ($3.5714 \times 1.0459 =$) 3.7354 to ($96735 \times 0.7036 =$) 6.8062, which is in the proportion of 1 to 6.8221 or of 3.5714 to 6.5075; and thence we may conclude that the temperature of the mixture of 3.5714 pounds of carbonic acid and of 9.6735 of nitrogen would be the same as if we had mixed with the 3.5714 pounds of carbonic acid 6.5075 pounds more of this same acid, making together 10.0789 pounds of carbonic acid.

Now, as the heat developed in the combustion of 1 pound of charcoal was sufficient to raise the temperature of the 3.5714 pounds of carbonic acid coming from this combustion to that of 3811° F., this same quantity of heat ought to be sufficient to raise the temperature of 10.0789 pounds of carbonic acid to the temperature of 1350° F.

This is, according to the results of this calculation, the highest temperature which we ought to expect to find amid the strongest charcoal fire fed by atmospheric air.

But we are very certain that the intensity of the heat of the strongest charcoal fire is far superior to the above calculation; consequently we are authorized to conclude that the capacity for heat of the carbonic acid, and that of nitrogen gas, are *much diminished* when these elastic fluids are exposed to a *very high temperature.*

If, in endeavouring to discover the limit of intensity of a charcoal fire, I have supposed the fire to be *very large,* it is not because I suppose that the heat developed in combustion is more intense *at the primitive source* in a large than in a small fire; but as a small fire is always surrounded by bodies relatively very cold, such as the bars of the grate, etc., the products of the combustion (which are always at the instant of their formation at the same temperature) are so rapidly cooled when the fire is small, that the temperature which we may find in such a fire is necessarily lower than that which we find in the midst of a larger fire, where a greater quantity of the same kind of combustible is employed.

When a large charcoal fire is well lighted up in a close stove, constructed with bricks or fire-stones, all the interior surfaces become excessively hot, and the heat accumulates and becomes very intense throughout the whole interior of the stove, so that iron and even stones are melted in it, and flow like liquids; but when the fire-place is small, it is with difficulty that it can be heated so much as to make the sides red-hot; and if the fire-place be very small, a charcoal fire cannot be kept up at all, even with continual blowing. We may truly say that such a fire *dies of cold*, an expression which with as much force as justice describes the event as it really happens.

But if it be the cold communicated by the surrounding bodies which hinders a very small charcoal fire from burning, could we not make it burn by guarding it in a proper manner against the cold?

This is an experiment which I tried six years ago with the greatest success, and which ended in my causing to be made small portable cooking-stoves now in general use in Paris, and elsewhere for aught I know.

By surrounding the body of the stove with two strata of enclosed air, the cooling of the fireplace and the charcoal it contains is hindered; and in this way the charcoal burns perfectly well, and the fire is so well kept up that it obeys a small register, which regulates the quantity of air admitted into the body of the stove.

Some judgment may be formed of the advantages which ought to result from the use of these small portable furnaces in cooking, etc., arising from the saving of time and combustibles, when we are informed that the combustion may be regulated without any difficulty, so as to consume the charge of charcoal in 20 minutes with a brisk heat, or so as to keep up a moderate fire for three hours.

With these portable cooking-stoves it is indispensably necessary to use kettles or saucepans of a particular construction. They ought to be suspended by their rims, in large circles of wrought-iron or copper, the better to keep in the heat. The circle of a saucepan ought to be half an inch more in breadth than the saucepan is in depth.

But to return to the main branch of my subject. If the present state of our knowledge does not admit of our establishing with a rigorous precision the highest temperature which it is possible to excite by means of the combustion of inflammable bodies, the calculation which I have submitted to the Class may nevertheless serve to guide our conjectures on this interesting subject. They will at all events show what is wanting to enable us duly to appreciate the subject.

SECTION X. — *On the Quantity of Heat developed in the Condensation of the Vapour of Water.*

Having filled the calorimeter and placed it on its stand, a current of vapour was introduced into the worm through a cork placed in the lower aperture of the worm. This cork having been perforated with a hole two lines in diameter, in the direction of its axis, a small cork (two lines in diameter and two in height) was fitted into it, and four other holes about a line in diameter, pierced horizontally through the sides of the large cork at two lines below its upper extremity, and communicating with the hole two lines in diameter in the axis of this cork, afforded a passage to the vapour, to admit of its entering by four small channels horizontally into the worm.

As the apertures of these small channels were higher than the level of the flat bottom of the worm, the water which resulted from the condensation of this vapour did not prevent the vapour from continuing to flow through these passages.

This vapour came from a long-necked matrass containing distilled water, which was put on a portable stove placed in a chimney at some distance from the calorimeter; and in order to stop all direct communication of heat between the stove and the calorimeter, the former was masked by plates, and the tube which conducted the vapour to the calorimeter was well covered with flannel.

The cold water which filled the calorimeter was of a lower temperature than that of the chamber by 6° F., and when the thermometer of the calorimeter announced an augmentation of temperature by 12° F., an end was put to the experiment.

The water produced by the condensation of the vapour in the worm was carefully weighed; and from its quantity, as well as from the heat communicated to the calorimeter, the heat developed by the vapour in its condensation was determined.

As a small part of the heat communicated to the calorimeter proceeded from the cooling of the water condensed in the worm, after the vapour had been changed into water, an account was kept of this heat. It was supposed that the water at the moment of condensation was at the temperature of 212° F., being that of boiling water; and it was determined, by calculation, what part of the heat communicated to the calorimeter must have been owing to this boiling water.

In making this calculation, no account was taken of the difference in the capacity of water for heat which depends on its temperature; this is but imperfectly known, and besides, the correction which would have been the result could not but have been very small.

The following are the details and results of two experiments made on the 21st of January, 1812.

Number of exp.	Temperature of the room.	State of the calorimeter (equal in capacity for heat to 2,781 grammes of water).			Quantity of vapour condensed into water in the worm.	Result.
		Temperature at the beginning of the experiment.	Temperature at the end of the experiment.	Elevation of its temperature.		Quantity of water which may be heated 1° F. with the heat developed in the condensation of 1 lb. of vapour.
	Degr's.	Degrees.	Degrees.	Degrees.	Grammes.	lbs.
1	61	55	$67\frac{1}{2}$	$12\frac{1}{4}$	29.61	1029.3
2	$62\frac{1}{4}$	$57\frac{1}{4}$	$67\frac{1}{2}$	$10\frac{1}{2}$	24.4	1052.3
Mean result						1040.8

By expressing the mean result of these two experiments in the way employed by Mr. Watt and others, I shall say that 1040 degrees of heat (Fahrenheit) are liberated in the condensation of steam, and that consequently this very quantity of heat is employed and rendered latent when the water, already at the temperature of boiling water, is changed into steam.

The duration of each of these two experiments was from ten to eleven minutes, and I had boiled the water some time in the matrass (to drive out the air which it contained) before I directed the steam from it into the worm of the calorimeter.

As the results of these experiments have been very uniform, and as they agree very well with the later experiments made by Mr. Watt with a view to determine the same question, I have not thought it necessary to repeat them.

I have, besides, been very much occupied with the following branch of my inquiries.

Section XI. — *Of the Quantity of Heat developed in the Condensation of the Vapour of Alcohol.*

As chemists are not agreed as to the state of the elements of the water which exist in alcohol, I thought that, by determining with precision the quantity of heat which is developed, we should be better able to form conjectures as to the state of the water, if it be at all times found in this inflammable liquid.

The results of the experiments which I made with alcohol are less regular than those of the experiments made with water, as might have been expected ; but they have nevertheless been sufficiently uniform to establish a fact which will be regarded, without doubt, as very curious and important.

As the vapour which is extracted from spirit of wine when boiled, varies a little with the intensity of the fire used in boiling it, I took care to note the time which was taken in every experiment, in order to be able to judge, by comparing the quantity of vapour condensed with the time employed to form it, of the intensity of the heat employed to boil the liquid.

In the following table we shall see the details and results of five experiments made on the same day (January 21, 1812) with alcohol of different degrees of strength. The capacity of the calorimeter was always equal to that of 2781 grammes of water, and the thermometer employed was that of Fahrenheit.

Number of experiments.	Specific gravity of the alcohol employed.	Time employed in the experiment.	Temperature of the apartment.	State of the calorimeter.			Quantity of alcohol condensed in the calorimeter.	Result.
				Temperature at the beginning.	Temperature at the end	Elevation of its temperature.		Quantity of water which may be heated 1° of F. with the heat developed in the condensation of 1 lb. of vapour.
		Min.					Gram.	
1	85342	7	61°	54¼°	68½°	14¼°	69.86	499.54
2	85342	5	61	56	66¼	10¼	52.21	476.83
3	84714	8	60½	55½	65½	10	48.82	500.03
4	81763	4¼	61	56	66½	10½	56.61	479.92
5	85342	6½	64	57	71½	14½	71.31	499.65

On determining, by calculation, the quantity of water which may be heated *one degree*, by the heat developed in the combustion of one pound of this vapour, I took care to keep an account of the difference between the capacity of water for heat and that of alcohol, when I determined how much heat should have been communicated to the calorimeter by the alcohol, and produced by the condensation of the steam, by being cooled in the worm.

In order to prove the state of the elements of the water which exist in the steam of alcohol, it must be shown how much water these elements ought to form.

We shall select the experiment which was made with alcohol of the specific gravity of 81,763, and which contained the least water. The quantity of steam condensed in this experiment was 56.61 grammes.

In 100 parts of this alcohol there were

91.79 parts of pure alcohol of Lowitz, and
8.21 parts of water.

Consequently there were in the 56.61 grammes of alcohol condensed in the calorimeter,

51.962 grammes of alcohol of Lowitz, and
4.648 grammes of water.

Now, as M. de Saussure has shown that there are 47 parts of water in 100 parts of alcohol of Lowitz, there must have been 24.422 grammes of water in the 51.962 grammes of alcohol of Lowitz, which were condensed in the calorimeter.

If to this quantity of water (= 24.422 grammes) we add the 4.648 grammes which were found mixed with 51.962 grammes of alcohol of Lowitz, in order to compose the 56.61 grammes of alcohol employed in the experiment, we shall have 29.07 grammes of water which ought to have existed ready formed either in the common state of water or in some other state, in the 56.61 grammes of alcohol condensed in the calorimeter.

But the condensation of 29.07 grammes of steam into liquid water ought to have of themselves furnished more heat than we had, in the experiment in question, in the condensation of these 29.07 grammes of elements of water with 27.57 grammes of carbon and hydrogen, which concur with these elements in forming the steam of the alcohol which was condensed.

If we apply a similar calculation to the results of the experiments made with alcohol which contained more water, the result of the inquiry will be still more striking.

In the experiment No. 5 the alcohol employed was of the specific gravity of 85,342 ; consequently 100 parts of this alcohol were composed of

77.88 parts of alcohol of Lowitz, and
22.12 water.

And in the experiment 71.31 grammes of vapour of alcohol were condensed.

There were, therefore, in these 71.31 grammes of condensed alcohol,

55.688 grammes of alcohol of Lowitz, and
15.622 grammes of water.

In the 55.688 grammes of alcohol of Lowitz there were 26.102 grammes of water, according to the analysis of M. de Saussure; and this last quantity of water (= 26.012 grammes), added to the quantity found above, viz. 15.622 grammes, makes 41.727 grammes of water which ought to have existed, either as steam or otherwise, in the 71.31 grammes of alcoholic vapour condensed in the calorimeter, in the experiment in question.

In order to simplify our calculation, and to render our comparisons more striking, we shall show how much pure water, in vapour, ought to have been sufficient to furnish, in its condensation, the same quantity of heat which was furnished by the condensation of 71.31 grammes of alcoholic vapour, in the experiment in question.

In this experiment the temperature of the calorimeter was raised to $14\frac{1}{2}°$ of Fahrenheit.

In the second experiment, made with the steam of pure water, the temperature of the same calorimeter was raised $10\frac{1}{2}°$ of Fahrenheit, with the heat developed in the condensation of 24.4 grammes of this vapour.

Consequently the temperature of the calorimeter must have been elevated to $14\frac{1}{2}°$ of Fahrenheit, with the heat which must have been developed in the condensation of 33.695 grammes of steam from pure water.

Now, as the hydrogen and the oxygen forming the elements of 41.727 grammes of water, which are found to form constituent parts of the 71.31 grammes of vapour of alcohol condensed in the experiment in question, only furnished in their condensation the same quantity of heat as 33.695 grammes of steam of pure water should have furnished, it is clearly proved, in my opinion, that these elements are not so united as to form water, so long as they concur in the formation of alcohol.

I have discovered that the vapour of sulphuric ether furnishes about one half less of heat in its condensation than that of alcohol, and consequently one fourth only of what is furnished by the steam of water of equal weight; but, having been interrupted by an accident in the course of my experiments with ether, I am desirous of finishing them before I publish the results.

[The first three sections of this paper are printed from Nicholson's Journal, Vol. XXXII. (1813), pp. 105–125; the remainder from Tilloch's Philosophical Magazine, Vol. XLI. (1813), pp. 439–444, and Vol. XLII. pp. 296–307, and Vol. XLIII. (1814), pp. 64–69. On pages 392, 410, and 412 there are several numerical errors and inconsistencies; but, as the original French memoirs are not accessible, no attempt has been made to reconcile them.]

ON THE CAPACITY FOR HEAT

OR

CALORIFIC POWER OF VARIOUS LIQUIDS.

THIS subject is of rather an obscure nature, and it has been so little examined, that it will be useful to begin by elucidating it as well as I can.

Let us suppose two cylindrical vessels, with very thick sides, made of lead or any other metal, and perfectly equal in size, each being capable of containing a pint.

These two vessels being at the freezing-point, we shall pour into the one a pound of water at the temperature of 96° F. (= $28\frac{1}{2}$° R.), being that of the blood, and into the other a pound of olive oil at the same temperature.

Each of these liquids will heat the cold vessel in which it is placed, the vessel in its turn will cool the liquid, and both the liquid and the vessel will latterly be of the same temperature.

If water and oil of olives had the same calorific power, a pound of water at the temperature of 96° would heat its cold vessel precisely as much and not more than a pound of oil would heat its vessel, the two vessels being of the same weight and at the same temperature at the commencement of the experiment.

But experience shows that water heats its vessel much more than oil does; consequently the calorific power of water is greater than the calorific power of oil of olives, when the *quantities* of these two liquids are estimated by their weight; and, if we designate the calorific power of water by 1, the calorific power of oil of olives will be expressed by a fraction under 1.

The power with which any given body, solid or liquid, being at a given temperature, resists the calorific or frigorific action of bodies warmer or colder than itself, is in proportion to its calorific power; and the greater is this power, the longer it resists these actions of surrounding bodies.

If, under equal surfaces, a pound of water and a pound of oil of olives, both at the same temperature (96° F.), are placed at the same time in a place where the temperature is lower (that of freezing, for instance), the oil of olives will be cooled much more rapidly than the water.

If it be in a *warm* place that the two liquids are exposed, the oil of olives will still have its temperature most rapidly changed; it will be more heated than the water.

In two cylindrical glass vessels, of equal size and very thin, place equal quantities of water, and at the same temperature (96° F.).

A piece of lead weighing a pound, and a piece of copper of the same weight, having been cooled in a mixture of pounded ice and water, remove them from this cold mixture and plunge each of them suddenly into one of the vessels of water.

The two masses of water will be cooled, but that which contains the copper most, for the calorific power of copper is greater than the calorific power of lead.

We may also say that the *frigorific power* of copper is greater than the *frigorific power* of lead, and, in the case in question, the expression, perhaps, will be most suitable.

It is always the same power; it is that by means of which any body resists the action of surrounding bodies, and which tends to change its temperature either by increase or diminution.

Much obscurity has been introduced into the science by vague ideas being attached to the words *hot* and *cold;* but it will not suit my purpose to enlarge upon this subject at present. I have already delivered my opinion in a former paper.

The little heat which I discovered in the condensation of alcohol having induced me to think that the specific heat of this liquid had not been accurately determined, and wishing to know it precisely, in order to enable me to finish the calculations which were necessary for elucidating the results of some of my experiments, I constructed a small and very simple apparatus, by the help of which I could easily, and as I presume accurately, determine it.

This apparatus consists of a small bottle of a particular form, constructed of thin leaves of red copper, intended to contain the liquid which is to be the subject of the experiment; and a small cylindrical vase, also constructed of thin pieces of red copper, in which I place water at a certain temperature. Into this water I plunge the bottle of copper containing the liquid which is the subject of the experiment; this liquid being of a different temperature from that of the water in the outer vase.

As the capacity of the vase for heat, as well as that of the bottle, is known, I determine, by a very simple calculation, the capacity for heat of the liquid contained in the bottle. This calculation, which is well known, is founded in the changes which take place in the temperature of liquids, in the vase and in the bottle, by taking a uniform temperature, when the bottle is immersed in the water contained in the vessel.

In order that this equality of temperature may be speedily brought about, the form of the bottle is such that it has a very great surface relative to its small capacity, and in order to manage it without touching it, its neck, which is small, is closed by a long cork, which serves as a handle.

In order to diminish as much as possible the effect of the atmosphere and of surrounding bodies upon the apparatus, while the experiment is going on, the quantity of water in the vessel is regulated so as to keep the bottle wholly submerged in the liquid, and even the upper end of the neck covered, when the bottle is immersed. The vessel which contains this water is placed and suspended by a ring of cork in another vessel larger and higher, and the interval between the two is filled with eider-down.

The form of the bottle is such that its horizontal section presents the figure of a rectangular cross. Some idea may be conceived of its form and dimensions, if we suppose a square piece of stick, each facet of which is four lines broad by four inches three lines in length, upon the four faces of which we have fixed four sticks of the same length (i. e. four inches three lines), but each of them being four lines thick by eight broad.

The four sticks last described will exhibit the figure of the bottle; for the square piece of stick will be concealed by them from our view.

The neck of the bottle is in the prolongation of its axis; it is four lines diameter by four high; it ought to be circular; the cork should be an inch long, and the bottle weigh 76.07 grammes without its cork.

The cylindrical vase which contains the water is two inches diameter, and four inches nine lines high, and it weighs 74.65 grammes.

The exterior vessel, in which the latter is suspended by the cork ring, is five inches three lines high, and three inches diameter, so that the sides and bottom are everywhere separated by an interval of six lines; this interval is filled with eider-down, as already mentioned.

To prevent the water from touching the eider-down, the cork ring is covered with a thin coating of mastic.

In order to ascertain the temperature of the bottle, and of the liquid which it contains, without being obliged to plunge a thermometer into the bottle, which would in this case be inconvenient, I employed a very simple method.

I placed a large bucket filled with water in a room with a northern aspect. I allowed it to assume the temperature of the room, taking care to shut the door and windows day and night. I placed the small bottle on a stand in this bucket, keeping the upper part of the cork only out of the water. As the bottle is small and has a large surface, it speedily acquires the temperature of the bucket of water; but, in order to be well convinced that the bottle and the liquid which it contains have acquired the temperature in question, I leave the bottle a considerable time in the bucket, frequently half an hour and sometimes more.

In giving a detailed account of an experiment made with this apparatus, I shall have an opportunity of giving clear and precise ideas of the different parts of my apparatus, and of the particular objects which they are intended to attain.

Having found by various preliminary experiments made with water that the capacity for heat of the cylindrical vessel with that of the thermometer employed to determine the temperature of the water which it contained, was equal to that of 24.3 grammes of water, and that the specific heat of the bottle of copper was equal to that of 8.36 grammes of water, I made the following experiment with purified linseed-oil.

I put into the cylindrical vessel 180 grammes of water; the temperature of the room was $59\frac{1}{2}°$ F. I filled the copper bottle with the above oil, and corked it. I cooled it in a bucket of water at the temperature of $44\frac{1}{4}°$ F. The oil in the bottle weighed 82.55 grammes.

The bottle, having had time to acquire the temperature of $44\frac{1}{4}°$ F., was withdrawn from the bucket, and placed in a cylindrical vessel of tinned iron, of about four inches diameter and six high, filled to the height of four inches and a half with water at the temperature of $44\frac{1}{4}°$ F.

The bottle, being submerged in this vessel of cold water, was carried into the room where I had placed the small vessel of copper belonging to the apparatus; it was then taken out of the cold water, and plunged into the water contained in the small cylindrical vessel of copper, which contained 180 grammes of water at the temperature of $59\frac{1}{2}°$ F.

A thermometer having a cylindrical reservoir four inches long, which was placed in this vessel beside the copper bottle, soon fell, and in three or four minutes it marked $56\frac{1}{2}°$ of F., where it remained a long time stationary, and afterwards began to ascend slowly.

The capacities for heat of the warm bodies which were cooled in this experiment were equal to that of 204.3 grammes of water; viz., —

That of the water employed	180 grammes.
That of the vases and thermometer . . .	24.3
Total . . .	204.3

The capacity for heat of the bottle containing the oil was equal to that of	8.36 grammes of water.
And to this we must add the cold water adhering to the bottle, when it came out of the cold water, and was plunged into the water contained in the copper vessel. I found by a particular experiment that this quantity of water was	1.04
Total	9.40

Now, as the temperature of the warm water in the cylindrical vase of copper was that of $59\frac{1}{2}°$ before the mixture, and $56\frac{3}{4}°$ after the communication of the heat had been obtained, it is evident that this water was cooled $2\frac{3}{4}°$. But if we multiply the number of grammes of water which the specific heat of this water represents, and that of the vessel ($=204.3$ grammes), by the number of degrees which it has been cooled ($2\frac{3}{4}$) we shall have a product which will express the number of grammes of water which would have been cooled 1° F. by a loss of heat equal to that which the vessel and its contents supported in this experiment. It is $204.3 \times 2.75 = 561.84$ grammes.

We shall now see what part of this heat was communicated to the bottle and to the small portion of cold water attached to it, and what part to the oil contained in the bottle.

As the temperature of the bottle and its contents was $44\frac{1}{4}°$ F. before the mixture, and $65\frac{1}{2}°$ afterwards, it is evident that the bottle had acquired $12\frac{1}{4}°$ of heat; consequently, if we multiply 9.4 (the number which expresses the sum of the capacities for heat of the bottle, and of the cold water adhering to it) by $12\frac{1}{4}$ we shall have a product which will express the number of grammes of water which would have been heated one degree by the heat communicated during the experiment to the bottle, and to the small portion of water which adhered to it.

It is $9.4 \times 12.25 = 111.15$ grammes.

If from the heat lost by the vessel and the warm water, which we have found equal to that which is necessary for raising the temperature of 561.84 grammes of water one degree of F., . . .	561.84 grammes
we take the quantities which the bottle and the water adhering to the bottle have received . .	115.15
we shall have	446.69 grammes

of water heated one degree, expressing the quantity of heat employed for raising to $12\frac{1}{4}°$ F. the temperature of the 82.55 grammes of linseed oil which were put into the bottle.

On dividing this number (446.69) by $12\frac{1}{4}$, we shall see how many grammes of water would have been heated one degree by the quantity of heat in question.

It is therefore $\frac{446.69}{12.25} = 36.464$ grammes of water.

By the results of this calculation we find that the same quantity of heat which is necessary to raise the temperature of 36.464 grammes of water $12\frac{1}{4}$ degrees of Fahrenheit's thermometer is sufficient to raise the temperature of 82.55 grammes of oil the same number of degrees.

Consequently the capacity of water for heat is greater than that of oil of linseed in the proportion of 82.55 to

36.464; and if we express the capacity of the water by *unity*, as is usually done, the capacity of the above oil ought to be expressed by the fraction 0.44172.

These details must no doubt appear superfluous to those who are versed in the higher branches of knowledge, and who are accustomed to express the most complete relations by algebraical signs; but it must be recollected that the subject of which I treat is familiar to few, and that it is necessary to explain with rigorous accuracy the principles upon which the method employed is founded, as well as the manner of using the apparatus which I recommend.

On repeating twice the experiment made with pure linseed oil I had as a result in one of these experiments a capacity for heat equal to 0 44411
and in the other equal to 0.47193
If to these two results we add that of the first experiment, equal to 0.44172

we shall have as a mean result 0.45192

The following are the results of some experiments made with other liquids. Olive oil furnished: —

	Specific heat of olive oil.
1st experiment	0.45944
2d experiment	0.43422
3d experiment	0.42183
Mean result . . .	0.43849

Three experiments made with naphtha gave the following results: —

	Specific heat of naphtha.
1st experiment	0.43408
2d experiment	0.39234
3d experiment	0.41905
Mean result . . .	0.41519

Three experiments made with spirits of turpentine gave the following results: —

	Specific heat of turpentine.
1st experiment	0.29322
2d experiment	0.37031
3d experiment	0.34216
Mean result . . .	0.33856

Two experiments with alcohol, of the specific gravity of 817,624, gave the following results: —

	Specific heat of alcohol.
1st experiment	0.54924
2d experiment	0.55063
Mean result . . .	0.54993

Two experiments with spirit of wine, of the specific gravity of 85,324, gave the following results: —

	Specific heat of spirit of wine.
1st experiment	0.57840
2d experiment	0.58317
Mean result . .	0.58078

Two experiments with sulphuric ether, of the specific gravity of 72,880, gave as results: —

	Specific heat of sulphuric ether.
1st experiment	0.53711
2d experiment	0.54768
Mean result . . .	0.54329

I was at first much surprised to find so great a capacity for heat in sulphuric ether, but my astonishment was diminished when I recollected that this liquid can unite with alcohol in all proportions without exhibiting any symptoms of a chemical action; for this reason, therefore, we ought to expect to find the same capacity for heat in both liquids.

[This paper is printed from Tilloch's Philosophical Magazine, XLIII. (1814), pp. 212–218.]

INQUIRIES

RELATIVE TO THE STRUCTURE OF WOOD,

The specific Gravity of its solid Parts, and the Quantity of Liquids and elastic Fluids contained in it under various Circumstances; the Quantity of Charcoal to be obtained from it; and the Quantity of Heat produced by its Combustion.

SINCE the days of Grew and Malpighi, there have been but few regular inquiries into the structure of wood. The science of botany has, indeed, taken an excursive range; and the indefatigable zeal of modern naturalists, who have travelled over all the known world, has made us acquainted with an astonishing number of plants, unknown before in Europe, and therefore called *new*, by which our gardens and apartments are embellished with a profusion of gay flowers; but still the knowledge of the vegetable economy is scarcely at all advanced. The circulation of the sap in plants is still a subject of dispute, and the causes of its ascension are very imperfectly known. The specific gravity of the solid parts which form the wood of plants is unascertained, and, by consequence, the proportions of solids, of liquids, and of elastic fluids; the component parts of a plant, with the variations to which they are subject in different seasons, are matters of which we are still ignorant.

It is, indeed, known, that the wood of a tree remains and preserves its primitive form after it has been converted into charcoal; but no one has explained this extraordinary phenomenon, very little attention having been paid to it.

An earthen vessel becomes hard and brittle in the potter's furnace; the vessel shrinks during the operation of baking, but it undergoes no alteration of shape. This phenomenon is easily accounted for; the water, which distended the particles of the clay, kept them at a distance from each other, and rendered the mass soft and flexible, having been expelled by the power of the heat, the several particles contract themselves together, and form a hard brittle body, though the clay remains the same before and after the operation.

Is it not possible that wood is converted into charcoal by a similar process? For either the charcoal is already formed in the wood, or, the wood being decomposed, the charcoal is formed of its elements or a part of them. But is it not evidently impossible that the elements of a solid body should be so totally deranged as to separate them entirely from each other without destroying the form or figure of the body?

In the sequel of this paper it will be shown that the specific gravity of the solid parts of any kind of wood is very nearly the same as that of the charcoal obtained from it, — a circumstance that gives a great degree of probability to the hypothesis that the two substances are identically the same.

But I do not mean to amuse the Class with a detail of my own conjectures; it is to my experiments and their results that I now claim the honour of calling its attention.

I was by accident first induced to enter upon this examination and inquiry into the structure of wood. In the course of a long series of researches upon heat, I wished to determine the quantities of that element produced by the combustion of different kinds of wood; but I had scarcely begun the inquiry when I found that, in order to procure satisfactory results to my experiments, it was indispensably necessary to obtain a better knowledge of wood itself; and therefore I immediately devoted myself to the study.

My first aim was to determine the specific gravity of the solid parts which compose the fabric of the wood, in order afterwards to determine the quantities of sap or water contained in wood under various circumstances.

Having found that very thin shavings filled with sap, or even with water, could be thoroughly dried in less than an hour, without injury to the wood, in a stove kept at a higher temperature than that of boiling water, or at about 500° of Fahrenheit's scale (= 260° French), I determined on using shavings of this description in my experiments.

Section I. — *Of the specific Gravity of the solid Parts of Wood.*

I began with the wood of the lime-tree, of which the texture is very fine and regular. From a small board, five inches long and half an inch thick, very dry, I took a quantity of thin shavings with a very sharp plane. These were exposed for eight days in the month of January upon a table in a large room not otherwise occupied, in order that they might attract from the atmosphere all that moisture which, as an hygrometric body, they were capable of imbibing. The temperature of the room was about 46° F.

Ten grammes (154.5 grains) of those shavings, laid on a china plate, were placed in a large stove made of sheet-iron, and there exposed to a regular heat of about 245° F. for two hours, in the course of which time they were frequently taken out and weighed in order to observe the progress of their desiccation. When they ceased to lose weight, the operation was stopped; when perfectly dried, their weight was 8.121 grammes.

By previous trials with my apparatus, I had learned that if the stove was too much heated the shavings became discoloured, which is always indicated by the emission of a particular odour, very readily to be perceived; but, by a careful regulation of the fire, this accident may be avoided and the shavings be thoroughly dried without injury, or even subjecting them to any sensible alteration.

I concluded that they had not undergone any change, because, upon again exposing them to the atmosphere, they regained the same weight which they had, under similar circumstances, prior to their being dried in the stove.

Being thus possessed of the weight of my shavings, as well under exposure to the air as in a dried state, which latter I could not but look upon as being perfect, it only remained to ascertain their weight in water when all their vessels and pores were completely filled with that liquid, to enable me to determine the specific gravity of the solid parts of this wood, which was accomplished without difficulty by the following process: —

A cylindrical copper vessel, 10 inches in diameter and as many deep, was filled with water from the Seine, previously well filtered, and, being set upon a common chafing-dish, was made to boil for some time, to expel the air contained in the water. The shavings were then thrown into the boiling water, and kept in that state for an hour. The water was not long in filling the vessels and pores of the shavings, from which it dislodged the air contained in them; so that the wood, specifically heavier than the water, was precipitated to the bottom of the vessel, and there remained.

When the vessel was removed from the chafing-dish, the water was suffered to cool to the temperature of 60° F., and then, plunging in both hands, I placed (under the water) all the shavings in a cylindrical glass vase, whose weight I had previously ascertained, which was suspended in the water by a silken cord, fastened at its other extremity to the arm of an accurate hydrostatic balance.

On weighing the shavings in the glass case thus immersed, I found their weight equal to 2.651 grammes.

As the shavings, while dry, weighed 8.121 grammes in the air, and 2.651 grammes in the water, they must have lost 5.47 grammes of their weight in the latter; consequently they must have displaced 5.47 grammes of water; and the specific gravity of the solid parts of this wood must be to that of the water at the temperature of 60° F. as 8.121 to 5.47, or as 14,846 to 10,000.

It may perhaps excite some surprise that the solid parts of so light a wood as that of the lime-tree should be heavier, by nearly one half, than water, taken in equal bulks. But this surprise will, without doubt, be increased when I declare that the specific gravities of the solid parts of all kinds of wood are so nearly alike as almost to induce a belief that there is the same identity in the ligneous substance of all sorts of wood as in the osseous substance of all species of animals.

I procured, from a joiner's workshop, dried wood of the eight following species; viz., poplar, lime, birch, fir, maple, beech, elm, and oak; and had them cut into small boards, 5 inches in length and 6 inches broad, from each of which I planed off some thin shavings, and exposed them to the air for eight days, in the month of January, in a large room, where the temperature, which varied but little, was about 40° to 45° F.

When these shavings had acquired their ordinary degree of dryness under existing circumstances, 10 grammes of each sort were weighed off, and, being laid separately in china plates, were thoroughly dried in the stove.

On being taken out of the stove, they were again weighed, and then thrown into boiling water, to expel the air from their pores and to moisten them thoroughly. When they had boiled for an hour, they were suffered to remain in the liquor till it was sufficiently cool; and after they had been weighed in the water, the specific gravity of their solids was calculated in the usual way.

The following table gives the details and results of this inquiry: —

Species of wood.	Weight.			Specific gravity of the solid parts of the wood.	Weight of a cubic inch of the solid parts of the wood.
	Exposed to the air in a room in winter.	Thoroughly dried in a stove.	In the water at 60° F.		
	Grammes.	Grammes.	Grammes.		Grammes.
Poplar	10	8.045	2.629	14854	29.45
Lime	10	8.121	2.651	14846	29.40
Birch	10	8.062	2.632	14848	29.44
Fir	10	8.247	2.601	14621	28.96
Maple	10	8.137	2.563	14599	28.93
Beech	10	8.144	2.832	15284	30.30
Elm	10	8.180	2.793	15186	30.11
Oak	10	8.336	2.905	15344	30.42
			Water	10000	19.83

The specific weight of the solid matter which composes the fabric of these woods is so nearly alike in them all, that the small variations to be observed in the different experiments may perhaps be accounted for otherwise than by supposing the ligneous substance to be essentially different in the several species.

The charcoal obtained from the various kinds of wood, if carefully prepared, has no sensible difference; and all the seerwoods give nearly the same chemical results when treated in the same manner. Hence, without doubt, we have good reason to suspect that the ligneous substance of all woods is identical. But without stopping to discuss this question at present, I shall endeavour to elucidate another, no less interesting, and which yields results more satisfactory.

Section II. — *Of the Quantities of Sap and Air discovered in Trees and in Seerwoods.*

Grew and Malpighi discovered in plants certain vessels which they suspected to be destined for the reception of air; and many physiologists have supposed that the air found shut up in the vessels of plants (if it be really confined there) would necessarily cause a reaction upon the neighbouring vessels, with an elastic force as variable as the temperature to which this elastic fluid is exposed, and might probably contribute to the circulation of the sap.

It would, doubtless, be an interesting question to determine precisely the quantity of air contained in plants in different seasons and under various circumstances. By examining the variations to which this quantity of air is subjected, and combining them with other simultaneous phenomena, we might hope to make some discovery which may assist us a little to elucidate the profound obscurity that at present conceals this part of the vegetable economy.

The specific gravity of the solid parts of a plant being known, it becomes very easy to determine in every case the quantity of air contained in its vessels and pores.

The following example will render this position perfectly clear.

An oak in complete health, in a growing state, was cut down on the 6th of September, 1812. A cylindrical piece, 6 inches long and rather more than an inch in diameter, taken from the middle of the trunk of this young tree, 3 feet above the earth, weighed, when full of sap, 181.57 grammes.

Upon plunging this piece of wood into a cylindrical vessel about $1\frac{1}{2}$ inch in diameter and $6\frac{1}{4}$ inches in height, filled with water at the temperature of 62° F., it displaced 188.57 grammes of the water;* whence I conclude with certainty, that this piece of oak, filled with sap, possessed a bulk equal to 9.5093 cubic inches, that its

* In order to determine and keep an account of the quantity of water remaining on the surface of this piece of wood at the instant of withdrawing it from the vessel, it was weighed when taken out, whilst still quite wet. As its weight had been taken previously to the operation, the augmentation it had acquired from the water was ascertained to a nicety.

The vessel when empty weighed 188.22 grammes, and when filled with water at the temperature of 60° F., 474 9 grammes; so that it contained 286.68 grammes of water. When the piece of wood was plunged into the water, a small glass plate, about two inches in diameter and two lines in thickness, ground with emery to fit it to the edges of the vessel, so as to close it hermetically, was laid upon its mouth, to shut up the piece of wood with the water still remaining in the vessel, whilst its outside was wiped with a dry cloth.

When the exterior of the vessel had been thoroughly dried, the glass cover was carefully removed, and the piece of wood withdrawn; the vessel was then weighed again with its remaining contents of water; and from its weight the quantity of water displaced by the wood was calculated.

specific gravity was 96,515, and, consequently, that a cubic inch of it weighed 19.134 grammes.

When the piece of wood had been reduced to the shape of a small board, about half an inch in thickness, I took from it forty very thin shavings weighing 19.9 grammes, but when thoroughly dried in the stove, at a temperature of 262° F., they weighed only 12.45 grammes.

From this experiment, it is evident that the wood in question, being full of sap, was composed of 12.45 ligneous parts, and 7.45 parts of water, or of sap, whose specific gravity is nearly the same as that of water.

Now, as one cubic inch of this wood weighed 19.134 grammes, it is very certain that it was composed of 11.971 grammes of ligneous parts, which were consequently solids, and of 7.163 grammes of sap.

But we have already seen, from the results of the experiments detailed in the former part of this memoir,* that a cubic inch of the solid parts of the wood of the oak weighs 30.42 grammes: —

Consequently the 11.971 grammes of solid parts found in one cubic inch of this wood, when the tree was alive, could have no greater bulk than	0.39353 cubic inch.
As one cubic inch of water weighs 19.83 grammes, the 7.163 grammes of sap found in the cubic inch of this wood must have occupied a bulk equal to	0.36122
Consequently a cubic inch of the wood in question contained a quantity of air whose bulk was equal to	0.24525
Making together	1.00000

* See the table, page 440.

We conclude from these results, that a young oak, in a growing state, at the beginning of September, when the wood appears to be diffused with sap, contains, nevertheless, about a fourth of its bulk of air, and that its solid ligneous parts do not make quite $\frac{4}{10}$ of its bulk. But we shall presently see that the lighter woods contain still less of ligneous parts, and more of air, than the oak.

A young Italian poplar, 3 inches in diameter, measured at 2 feet above the earth, was cut down on the 6th of September, while the tree appeared to be in a growing state. The specific gravity of a piece taken from the middle of the trunk was found to be 57,946; consequently a cubic inch of this wood weighed 11.49 grammes.

From a piece of this wood, apparently full of sap, forty thin shavings were taken, 6 inches in length and half an inch broad. The wood from which these shavings were planed weighed 12.37 grammes; and the shavings, when thoroughly dried in the stove, weighed 7.5 grammes.*

We hence conclude that a cubic inch of this wood, in its original state, while the tree was still alive, contained 7.1531 grammes of ligneous parts which formed the fabric of the wood, and 4.3369 grammes of sap, differing in its specific gravity little or nothing from common water.

* As the heat excited by the plane in taking off these shavings was sufficient to evaporate a very sensible quantity of sap belonging to the wood from which they were cut, the shavings became perceptibly dry during the operation; for I found that forty thin shavings sometimes lost more than one gramme (about $\frac{1}{12}$ of their weight) in less than a minute. In order to obtain their true weight, whilst they still remained part of the wood, I adopted the precaution of weighing the piece of wood both the moment before and the moment after the operation of planing. The difference in the weight of the wood, under these two circumstances, indicates the weight necessary to be given to the shavings, and which is here always attributed to them.

As one cubic inch of the solid parts of this kind of wood weighs 29.45 grammes,* the 7.1531 grammes of ligneous parts found in a cubic inch of the trunk of the living tree, in September, could only have occupied the space of	0.24289 cubic inch.
And the 4.3369 grammes of sap, contained in it, only	0.21880
Consequently, in one cubic inch of this wood there was a bulk of air equal to	0.53831
Total	1.00000

The difference between the structure of the oak and of the poplar becomes very conspicuous on making a comparison, according to the subjoined method, between the constituent parts of these two kinds of wood, both in a growing state.

Thus, a cubic inch of wood is composed of: —

	Ligneous parts.	Sap.	Air.
The oak . . .	0.39353	0.36122	0.24525
The poplar . .	0.24289	0.21880	0.53831

This striking difference in the proportions of the ligneous substance, of sap, and of air, discovered in these two species, sufficiently explain the difference observable in their weight and hardness. This inquiry may probably lead to other discoveries of more general utility in the study of the vegetable economy.

Section III. — *Of the relative Quantities of Sap and Air found in the same Tree, in Winter and in Summer; and in different Portions of the same Tree, at the same Time.*

The following experiments were undertaken with a view to discover the difference between the quantities of

* See the table, page 440.

sap and air found in the wood composing the trunk of a large tree, in winter and in summer.

On the 20th of January, 1812, I had a lime-tree felled of about twenty-five or thirty years' growth, which had stood among several others of the same age in my garden at Auteuil. On taking a piece of wood from the middle of the trunk, at about 3 feet above the ground, it appeared to be filled and even drowned in sap. Its specific gravity was 76,617; consequently, one cubic inch of the wood weighed 15.788 grammes.

Having planed off 10 grammes of thin shavings from this piece, and dried them thoroughly in the stove, I found their weight reduced 4.72 grammes.

Thus in possession of the specific gravity of the solid part of this wood, it was easy to determine, with the aid of these data, the constituent parts of a cubic inch, which were as follows: —

Ligneous parts	0.25353 cubic inch.
Sap	0.44549
Air	0.30098
	1.00000

On the 8th of September, in the same year (1812), I had a piece of wood (= 5.84 cubic inches) cut from the trunk of another lime, of equal age with the former (from 25 to 30 years), at the height of 3 feet above the earth. This tree was in a growing state, and the piece taken from it, after it had been trimmed by the joiner, weighed 87.8 grammes, and displaced 115.8 grammes of water, at the temperature of 62° F.; consequently, its specific gravity was 75,820. In the month of January the specific gravity of this same species of wood had been found to be 79,617.

From the piece of wood taken from the tree on the 8th of September, I had 14.19 grammes of thin shavings planed off, which, after they had been thoroughly dried in the stove, weighed only 7.35 grammes. Hence we have, as the constituent parts of a cubic inch of this wood: —

Ligneous parts	0.26489 cubic inch.
Sap	0.36546
Air	0.36965
	1.00000

From the results of these two experiments, we may conclude that the body of the tree contains more sap in the winter than in summer, and more air in summer than in winter. But the following experiments demonstrate the sap to be very disproportionately distributed in the several parts of the same tree, at the same season.

On the 8th of September, I had a branch, about 3 inches in diameter, cut from the lime just spoken of, and which issued from the trunk at the height of 10 feet above the surface of the earth. From the lower end of this branch I took a piece of wood, and subjected it to the investigation requisite to ascertain its constituent parts.

Its specific gravity was 70,201. The same day I found the specific gravity of a piece of the trunk of the same tree to be 75,820.

Surprising as this difference appeared, my astonishment was still more excited, on finding that a piece of wood of three years' growth, cut from the upper end of the same branch, where it was but one inch in diameter, had a specific gravity of 85,240.

There was, therefore, much more sap and less air in the wood of the upper extremity of the branch than in the lower, which was nearer to the body of the tree.

I afterwards examined the young shoots of the current year, in the same tree, as well as in several other species of wood, and uniformly found that the specific gravity of the young wood, that is to say, of the current year, is always considerably greater than that of the same species of wood when grown older. Doubtlessly, because it contains more sap and less air than the old wood.

In the management of experiments for determining the specific gravity of wood of the current year, it is indispensably necessary to take an account of the space occupied by the pith, without which precaution we shall be led to false conclusions.

I found the specific gravity of the oak of the current year to be 116,530; that of the elm 110,540. Young shoots of these trees, deprived of their bark and pith, descend rapidly on being thrown into water; whilst species of the same tree, more advanced in age, swim on the surface, even when the wood is green, and more full of sap.

This fact is worthy the attention of persons occupied in the study of vegetable physiology.

I was next curious to examine the root of the lime from which I had already had one piece of wood from the trunk, and two pieces from one of its branches. With this view, on the 8th of September, 1812, I caused one of its roots, of about 2 inches diameter, to be taken up, and cut from it a piece weighing 93.25 grammes, which displaced 115.8 grammes of water. Its specific gravity was 80,527, and, consequently, greater than that of the wood extracted from the trunk of the same tree, but less than that cut from the upper end of one of its branches. 20.48 grammes of thin shavings, from this piece of the root of the lime, weighed only 10.85 grammes after being thoroughly dried in the stove.

From these data, I determined the constituent parts of a cubic inch of the root thus: —

Ligneous parts	0.28775 cubic inch.
Sap	0.37358
Air	0.33867
	1.00000

The constituent parts of a cubic inch of the body of the same tree were, as we have shown: —

Ligneous parts	0.26489 cubic inch.
Sap	0.36546
Air	0.36965
	1.00000

The constituent parts of a cubic inch of the wood of the same tree, taken the same day from the lower extremity of a branch, were: —

Ligneous parts	0.25713 cubic inch.
Sap	0.27513
Air	0.46774
	1.00000

Lastly, the constituent parts of a cubic inch of the wood, taken near the upper extremity of the same branch, were: —

Ligneous parts	0.25388 cubic inch.
Sap	0.47599
Air	0.27013
	1.00000

For the more easy comparison of the results of these four experiments upon the wood of the lime-tree, made on the same day, with different portions of the same tree, I have collected them together in the following table.

	A cubic inch of wood was composed of		
	Ligneous parts.	Sap.	Air.
The root	0.28775	0.37358	0.33867
The trunk	0.26489	0.36546	0.36965
The lower end of a branch .	0.25713	0.27513	0.46774
The upper end of " . .	0.25388	0.47599	0.27013
Wood taken from the trunk of a lime-tree of the same age, on the 20th of Jan. . .	0.25353	0.44549	0.30098

Being desirous to ascertain whether a difference considerable enough to be valued existed between the wood of the heart, or core, and the sap-wood found between the rind and the body of the same tree, I took, on the 11th of September, an elm fagot, 5 inches in diameter, lopped from a large tree, which had been felled on the 20th of the preceding April, and had two cylindrical pieces, each 6 inches in length, cut out of it. The thickest of these taken from the core weighed 191.05 grammes, and displaced 194.45 grammes of water; the other, consisting of the sap-wood, weighed 93.61 grammes, and displaced 111.45 grammes of water.

The specific gravity of the core was, therefore, 98,251; that of the sap-wood, 81,764. But as the fagot had lain exposed to all the summer rains, the wood was far from being dry. I was, however, much surprised at discovering that the core of the wood was more charged with sap or water than that of the same kind of wood when in a growing state, — a fact which induces a suspicion that the sap in trees is not enclosed in vessels or tubes apparently impervious to that liquid.

To obtain a better knowledge of the wood in question, I planed off forty shavings, 6 inches in length and half an inch in breadth, from a small board cut from the core; with an equal number of shavings, of similar dimensions, from another board cut from the sap-wood.

The forty shavings from the core, taken just as they were planed off, weighed 16.37 grammes, and 10.53 grammes after they had been thoroughly dried in the stove.

The forty shavings of sap-wood weighed 16.97 grammes before they were dried, and 11.99 grammes afterwards.

Thus possessed of the specific gravity of the solid parts of this kind of wood, it only remained to determine, from these data, the constituent parts of an inch of the wood, which was readily performed, as follows: —

	Ligneous parts.	Sap.	Air.
In the core of the elm . . .	0.41622	0.35055	0.23323
In the sap-wood	0.38934	0.23994	0.37072

It appears, from the results of these experiments, that the sap-wood of the elm contains rather more ligneous parts in its timber than the core of the same tree; and that it contains much less sap and more air. But as the tree had been felled nearly five months before it became the subject of investigation, it is very possible that the sap-wood had become much drier than the core of the tree.

I had purposed to repeat these experiments upon wood in a growing state and upon seerwood; but the interference of other occupations has prevented a continuance of the inquiry. It cannot, however, but lead to results curious in themselves; and I therefore recommend it to the notice of all students in vegetable economy, as well as to those who love that noble science, and feel a gratification in being able to remove the veil under which the mysterious operations of nature are concealed.

The particular object which I had in view in exploring the structure of wood has led me by a way by no means likely to be fertile in interesting discoveries; but I have begun the work, and feel myself bound to complete it, in preference to every other consideration. These fascinating researches, I am aware, have already carried me too far, and I must now resign them into the hands of others, in order to fulfil my engagements. This I do most cheerfully, and it will give me the greatest pleasure to behold a field, too long neglected, once more broken up.

SECTION IV. — *Of the Quantities of Water contained in Woods considered as dry, or Seerwoods.*

Wood is a hygrometric substance, and, when exposed to the atmospheric air, always imbibes a visible quantity of water, varying, however, with the temperature and humidity of the air.

If the moisture in the wood were confined in vessels so constructed as to be totally impervious to water, the fabric of the wood would be uniformly the same, with the exception only of the variations caused in its dimensions by change of temperature, in which case it would be very easy to determine the quantity of water contained in the wood, when the specific gravity of its solid parts was known. But, as the bulk of all woods is considerably diminished in drying, the experiment is rendered rather prolix, though by no means difficult, and its results are clear and satisfactory.

A few examples will suffice to point out the method to be pursued.

The composition of the oak, in a growing state, at the beginning of September, has been already given. In order to ascertain the change which this wood undergoes by the process of drying, I made the following experiment.

From a fagot of oak, $5\frac{1}{2}$ inches in diameter, which, covered with its bark, had been exposed to dry in the open air for eighteen months, I took a piece of rather more than an inch square and 6 inches in length; it was good firewood, and seemed very dry.

This piece, after being trimmed by the joiner, weighed 126.2 grammes, and displaced 157.05 grammes of water; its specific gravity was consequently 80,357, and a cubic inch weighed 15.939 grammes.

Forty-three shavings of this wood, 6 inches long and half an inch broad, weighed 17.9 grammes; but when thoroughly dried in the stove, they were reduced to 13.7 grammes. They were therefore, prior to being put into the stove, composed of 13.7 grammes of solid parts — that is to say, of dry or seerwood — and 4.2 grammes of water.

The results of this experiment indicate that 100 kilogrammes of this excellent firewood contained 76 kilogrammes of seerwood and 24 of water; which is, probably, the ordinary state of the best firewood sold in the timber-yards of Paris, and all other places.

Were the wood to be kept for several years in a dry place, secured from the rain, it is possible that it might become dry to such a degree as to contain only about 12 per cent of water, and 88 of seerwood. But it will appear in the sequel that wood of any kind, exposed to the atmosphere, could never become more dry, on account of its hygrometric quality, which it constantly preserves.

The following are the constituent parts of a cubic inch of firewood employed in this experiment: —

Ligneous parts, or seerwood . .	0.40166 cubic inch.
Sap, or water	0.18982
Air	0.40852
	1.00000

Thus we are enabled clearly to demonstrate the difference between the oak in a growing state, and the same kind of wood after it has been felled and dried in the air, secured from the rain, for eighteen months: —

	Dry wood.	Water.	Air.
In a cubic inch of oak, in a growing state	0.39353	0.36122	0.24525
In a cubic inch of the same kind of wood, after it had been felled and dried for 18 months	0.40166	0.18982	0.40852

By comparing the relative quantities of seerwood contained in a piece of timber while in a growing state, and in the same timber after it has been dried, we may ascertain how much its fabric has shrunk by desiccation.

It appears from these experiments, that the oak sold in the timber-yards of Paris for firewood contains rather more than one half of the sap which it formerly had in a growing state.

I have made several similar experiments upon other species of wood; but their results are better calculated for exhibition in a table than for circumstantial detail.

SECTION V. — *Of the Quantities of Water attracted from the Atmosphere by Woods of various Species, after being perfectly dried.*

It has been long known that charcoal imbibes the humidity of the atmosphere with considerable eagerness; but I have discovered that dry wood attracts it with still greater avidity. The following are the details and results of a series of experiments made last winter, with a view to elucidate this subject.

Having procured thin shavings, about 5 inches long and half an inch broad, of nine different species of the woods of our climate, in order more certainly to reduce them to an equal degree of dryness, I began my experiment by boiling them for two hours in water, that they might be thoroughly impregnated with that element.

I then dried them well in a stove, in which they were kept during 24 hours, exposed to a temperature higher than that of boiling water, at about 250° of Fahrenheit's scale.

On taking them out of the stove, they were carefully weighed, being still hot; they were then suffered to remain in the open air for 24 hours, in a large room, whose temperature was uniformly during the day and night at about 45° to 46° F. This was on the 1st of February, 1812.

The weight of the shavings, on being removed from the stove thoroughly dried, and after having been exposed to the air of the large room, was as follows: —

Species of wood.	Weight.	
	On being withdrawn from the stove.	After exposure for 24 hours, in a room at a temperature of 46° F.
	Grammes.	Grammes.
Italian poplar	3.58	4.45
Lime-tree, seasoned, and fit for the joiner's use	5.28	6.40
" green wood	5.39	6.47
Beech	7.02	8.62
Birch	4.41	5.47
Fir	5.41	6.56
Elm	5.87	7.16
Oak	6.46	7.93
Maple	4.76	5.85

Hence it appears that 100 parts of the wood, after 24 hours' exposure in the large room, were composed of dry wood and water in the following proportions : —

100 parts of	Seerwood.	Water.
Poplar	80.55	19.55
Lime-tree, seasoned	82.50	17.50
" green	83.31	16.69
Beech	81.44	18.56
Birch	80.62	19.38
Fir	82.4	7.53
Elm	81.80	17.20
Oak	83.36	16.64
Maple	81.37	18.63

I suffered all these woods to remain in the large room during eight days, but their weight was very little augmented ; and as often as the temperature of the air of the room was raised above 46° F. they lost weight. So that the above may be considered as their habitual state of dryness during the winter, in our climate.

To ascertain the quantity of moisture habitually retained by these woods in the summer, I made the following experiments.

Thin shavings of the species of woods below, half an inch broad, were thoroughly dried in the stove, and then exposed for 24 hours in a room with a northern aspect, whose temperature was tolerably uniform at 62° F. The following are the results : —

Species of wood.	Weight.		In 100 parts of wood were found	
	When dry.	At the accustomed state of humidity in the air at 62° F.	Seerwood.	Water.
	Grammes.	Grammes.	Parts	Parts.
Elm, the core	10.53	11.55	91.185	8.815
" the sap-wood	11.99	13.15	91.197	8.803
Oak, seasoned and fit for the joiner's use	13.70	15.05	91.030	8.970
Oak felled 6th Sept.	12.45	13.70	90.667	9.333
Lime, seasoned	7.27	7.80	93.205	6.795
" when growing	6.75	7.30	92.466	7.534
" the root	9.96	10.80	92.222	7.778
Elm, seasoned	9.25	10.80	91.133	8.867
Italian poplar	7.50	8.00	93.750	6.250

With a view to ascertain the habitual state of the dryness of woods in autumn, I carefully preserved these same shavings till the 3d of November, in a northern chamber, not inhabited; at which period its temperature had stood for several days at 52° F., with little variation. I then weighed the shavings, and from their weight calculated the quantity of water contained in them.

The following table, containing the results of all these experiments, displays, in a familiar and satisfactory manner, the customary state of the woods, in different seasons, in our climate.

Species of wood.	100 parts in weight of wood, cut into thin shavings and exposed to the air, contained water		
	In summer, at a temperature of 62° F.	In autumn, at a temperature of 52° F.	In winter, at a temperature of 45° F.
	Parts.	Parts.	Parts.
Poplar	6.25	11.35	19.55
Lime	7.78	11.74	17.50
Oak	8.97	12.46	16.64
Elm	8.86	11.12	17.20

From a comparison of these results it appears that these woods, when exposed to the air at a temperature of 45° F., contain twice the quantity of water that they do when the temperature of the air is at 60° F. But it is necessary that the wood be cut into very thin shavings, to enable it to become suddenly *in equilibrio* with the air, conformably to its quality of a hygrometric body; otherwise the state of the air may change, and that very frequently, before its humidity or dryness can have had sufficient opportunity to produce all its effect upon the wood.

To discover what is termed *the medium dryness* of any species of wood, in our climate, it is requisite that we be acquainted with the quantity of water contained in wood every day of the year, and even in every hour and every minute, which is obviously impossible; but there is another method to be pursued in this inquiry, much less laborious and which will lead to results as satisfactory as the nature of the subject will admit.

As a very large piece of wood, a large beam for instance, dries so very gradually in the air as not to attain a state of perfect dryness in less than 50 or 60 years, it is sufficient to examine the interior of such a beam, after having been sheltered for 80 or 100 years from the rain, to discover the state of such part of the wood as may still be considered sound.

In pulling down old houses, we meet with beams proper for the present inquiry.

An old castle in my neighbourhood being pulled down, I had an opportunity of examining the interior of a large oaken beam, which had, without doubt, been there more than 150 years, and as it formed part of the timbers of the edifice, had been secured from the rains.

A piece of this wood in a high state of preservation, after it had been planed by the workman, was accurately weighed, and then plunged into water, to ascertain its specific gravity. It weighed 75.05 grammes, and displaced 110 grammes of water, at the temperature of 61° F.; its specific gravity, therefore, was 68,227, and a cubic inch weighed 13.53 grammes.

Forty shavings of the wood weighed 11.4 grammes, which were reduced to 10.2 grammes when they had been thoroughly dried in the stove.

Hence we may conclude that a cubic inch of this old wood was composed of

Ligneous parts	0.39794 cubic inch.
Water	0.07186
Air	0.53020
	1.00000

We may also conclude from these results, that the wood of the centre of a large oaken post, though kept for ages out of the reach of the rain, can never contain, in our climate, less than 10 per cent of its weight in water; and that a cubic inch of such wood contains more than half a cubic inch of air.

The *yearly medium temperature* at Paris is about $54\frac{1}{2}$° F.; now, as we have just seen that the habitual state of dryness in woods at the temperature of 52° F. is such as to give about 11 per cent of water for 100 parts of wood, we must not be surprised at finding 10 per cent of water in the interior of a large beam, after it had been sheltered from the rain during 150 years.

To ascertain whether the property of wood to attract moisture from the atmosphere was augmented or diminished by the beginning of carbonization, I made the following experiments.

Fourteen grammes of ash-shavings, after being highly dried on a marble slab over a chafing-dish, were exposed to the air, in the month of February, in a large room whose temperature was about 20° F., and in 15 hours they had gained 1.65 grammes in weight.

Fourteen grammes of the same sort of shavings, having been first scorched in the stove till they had assumed a brown color, were at the same time dried over the chafing-dish, and exposed with the others to the cold air for the same length of time; but they gained in weight only 1.01 grammes, while those which had not been scorched, as already stated, had gained 1.65 grammes.

Fourteen grammes of the shavings of lime-wood, in their natural state, and fourteen grammes of the same kind of shavings, after they had been violently scorched in the stove, were dried together over the chafing-dish, and then exposed in the open air, at the temperature of 40° F. for 15 hours. The shavings in their natural state gained 1.33 grammes in weight; while those that had been scorched gained 0.7 grammes.

A similar experiment, upon shavings of the cherry-tree, some in their natural state, and others scorched, was productive of the same result.

Whence we conclude that wood in its natural state attracts the moisture of the air more copiously than it does after having been subjected to the first degree of carbonization.

From similar experiments upon wood and charcoal, I find that dry wood attracts humidity more powerfully than dry charcoal.

It would be worth ascertaining, whether wood is not also more powerfully attractive of gas than charcoal; but as I have not time to enter upon this particular inquiry, I can only recommend it to those whose inclinations may lead that way. Leaving, therefore, this subject untouched, I must, without any further circumerration, pursue the original object I had in view in these disquisitions upon wood, namely, to endeavour to become acquainted with those inflammable substances which burn on setting fire to a piece of wood under a calorimeter.

SECTION VI. — *Of the Quantities of Charcoal to be obtained from different Kinds of Wood.*

Having discovered that pieces of wood, more or less thick, may be perfectly carbonized in glass vases with thin tops, closely covered, and exposed for two or three days to a moderate heat in a stove, I adopted this method in all my experiments on the carbonization of wood.

The glass vases which I make use of are what the chemists call *proofs*, with feet: they are small cylindrical vessels, about $1\frac{1}{2}$ inch in diameter and 6 inches in height; the covers consist of glass plates about 2 inches in diameter and from 2 to 3 lines in thickness, neatly ground with very fine emery, well diluted with water, on a large glass slab; and, the edges of the vases being ground with the same exactness, they become hermetically closed by the covers, so as to preclude every access of the air, especially if the edges of the vases and the whole surface of the covers be well rubbed with black-lead.

The elastic fluids, in escaping from the interior of the vases, occasionally raise the cover for a moment, on one side, even when surmounted by a considerable weight; but as it is only raised a very little, and falls again immediately, the vase is never open more than an instant at a time, and then not so as to admit the obtrusion of any extraneous matter.

When one of these vases is put into the stove, it is placed upon a square tile, or half-brick, of burned earth, and another of the same kind is also laid upon the cover to keep it steady.

During the carbonization of the wood, the interior of the vase is always clouded, assuming a very deep blackish-yellow colour; and during the operation a strong smell of soot or of pyroligneous acid issues from the stove; which is even insupportable at the commencement, if it be too nearly approached, as well as on withdrawing the vases from the stove, if the covers be removed without due precaution.

There is, therefore, a *decomposition* during the carbonization of wood, and a formation of pyroligneous acid. This fact has been long known; but, in some of my experiments, and particularly in those made upon fir, with a very moderate fire, I obtained a product, which, upon a very exact scrutiny, appeared to me to be *bitumen.*

This product had been condensed upon the glass cover, whence it had afterwards run in large drops upon the vertical surface of the side of the vase. It was hard and brittle, of a dark yellow colour; it was not affected by boiling water, nor by boiling alcohol, but was gradually dissolved by sulphuric ether.

It would be superfluous here to enter upon the details of all my experiments relative to the carbonization of wood. As the process I have employed cannot now but be well known, after what I have said in this memoir and in the one that I had the honour to present to the Class on the 30th of December in last year, I shall here only give the result of those experiments.

The six following, made with different species of wood, were so uniformly alike in their results, that I was much surprised.

One hundred parts (10 grammes) of the six following kinds of wood, in thin shavings, and thoroughly dried, were carbonized at one time in the stove, in glass vases, well closed with flat glass covers. As the heat was managed with great care, in order to determine with precision, from the weight of the vases, the moment when the operation was finished, the experiment occupied four days and as many nights. When the vases with their contents ceased to lose weight, the process was stopped, and the charcoal was weighed while still hot.

The following were the results: —

100 parts in weight of dry wood gave in dry charcoal.	Poplar	43.57 parts.
	Lime	43.59 "
	Fir	44.18 "
	Maple	42.23 "
	Elm	43.27 "
	Oak	43.00 "

The medium term of the results of these six experiments gives 43.33 parts of charcoal in 100 parts of dry wood; and as they were made with woods differing considerably in their apparent weight, their hardness, and other distinctive physical characters, we may conclude, from the great similarity of the results of these experiments, that none of the circumstances from which the woods derive their particular characters have any material influence upon the quantities of charcoal they are capable of yielding; and hence we may deduce that the ligneous substance or seerwood, if not the same in all, is at least composed of identical substances.

There is still, however, a very interesting question remaining for discussion, namely, Is the seerwood charcoal?

To elucidate this question, I began by examining whether charcoal had the same specific gravity as seerwood.

I, therefore, reduced some common oak-charcoal, which appeared to be well manufactured, into pieces about the size of small peas, and then boiled them in a pretty good quantity of Seine water, previously well filtered; the pores of the charcoal were speedily so completely filled with this liquid, that, becoming heavier than the water, in equal bulk, it precipitated itself to the bottom of the vessel, and there remained.

On removing the vessel from the fire, the water was suffered to cool to the temperature of 60° F.; and then the charcoal, while still submerged, was put into the small glass vase of the hydrostatic balance, and weighed. Its weight in the water, at the temperature of 60° F., was 2.44 grammes.

When the charcoal was taken out of the water, it was put into a cylindrical glass vessel 1½ inch in diameter, and 6 inches in height, in which it was thoroughly dried in the stove at a temperature of about 265° F.

After it had been six hours in the stove, it was taken out and weighed while still hot, and found to be equal to 6.7 grammes; therefore its specific gravity was 157,273.

We have before shown that the specific gravity of the solid parts of oak, in the state of seerwood, is 153,440.

This is certainly very similar to that of charcoal made of the same kind of wood; but we have not yet proved seerwood to be charcoal; on the contrary, we have just seen that it requires 100 parts of seerwood to obtain 43.33 parts of dry charcoal.

Neither is seerwood simply a hydrure of dry wood, as we shall see in the sequel.

It should seem that the fabric of a plant, which may perhaps be nothing but pure charcoal, is always covered with a substance analogous to the flesh which conceals the skeleton of an animal. This vegetable flesh does not exist in considerable masses; for, as the plant is not under the necessity of moving from one place to another in search of nourishment, it has no need either of flexible joints in its skeleton, nor of muscles capable of exerting a great force; and it probably arises from the circumstances of the skeleton and the flesh being very intimately blended together, that they are not discriminated and distinguished from each other.

I consider seerwood as the skeleton of the plant, with the flesh, though quite dried, still adhering to it; and as we have seen that there are 43.33 parts of charcoal in 100 parts of seerwood, I should say that 100 parts of seerwood are composed of

Charcoal	43.33 parts.
Vegetable flesh, dried	56.67
Making together	100.00 parts.

The beautiful analyses of Messrs. Gay-Lussac and Thénard have shown us that seerwood is composed of carbon, hydrogen, and oxygen; and that two different species of wood analyzed by them (the beech and the oak) were composed of these three elements in nearly equal proportions. They also discovered that the oxygen and hydrogen in these woods are in the requisite proportions for the formation of water; wherefore they concluded that carbon was the only combustible substance contained in wood.

It will appear in the sequel, how well the results of these ingenious inquiries accord with those of my experiments.

But first, I shall examine what quantity of charcoal it is possible to obtain from different species of woods, under various degrees of dryness, pursuing the method already adopted in my experiments.

From the mode in which charcoal is ordinarily made, a very considerable portion is lost and improvidently burned during the operation.

As it appears to be clearly proved, by the results of the six experiments above related, that the quantity of charcoal to be obtained from any given quantity of wood is invariably in proportion to the quantity of dry ligneous substance contained in the wood, the inquiry into the quantities of charcoal to be produced from different species of woods, at various degrees of dryness, becomes limited to that of the quantities of wood absolutely dry, contained in the woods in question.

It has been shown that 100 parts in weight of oak, thoroughly dried, give 43 parts of charcoal.

We have likewise seen, that 100 parts of oak as dry as it can be made in summer, at the temperature of 62° F., contain only 91 parts of seerwood, and, consequently, that 100 parts of such wood would furnish only 39.13 parts of charcoal.

From the results of an experiment of which I have given an account in this memoir, it appears that 100 parts of oak, in the state wherein it is found when exposed to the winter's air, at the temperature of 46° F., contain only 83.36 parts of seerwood; consequently, 100 parts of such wood would yield no more than 35.84 parts of charcoal.

From the examination we have made of the oak in that state in which it is deemed fit for burning, we have found that 100 parts of this kind of wood contain only 76 parts of absolutely dry wood; whence we conclude that 100 parts of such wood would produce 32.68 parts of charcoal.

It has been shown that 100 parts of an oak felled on the 6th of September, while in a growing state, contained only 62.56 parts of seerwood, and that, consequently, 100 parts of such wood would yield only 26.9 parts of charcoal.

In making these calculations, no account has been taken of the quantity of wood, or other combustible, burned in order to heat the closed vessel in which the wood was carbonized, pursuant to the process here adopted. But it may be remarked that such quantity will be increased or diminished according to the construction of the furnace and the arrangement of the other parts of the apparatus; and it will always be too considerable to be omitted in the list of expenses.

As M. Proust obtained only 19 or 20 parts of charcoal in 100 of oak, it is probable that some waste occurred in the process; but as it is certain that in the carbonization of wood some loss will happen, so in the ordinary method of making charcoal there is always a considerable reduction of the quantity that ought to be produced, arising from the quantity of wood consumed, either wholly or in part, to obtain heat sufficient to char the portion of wood that is reduced to a coal.

Messrs. Gay-Lussac and Thénard found from 52 to

53 parts of carbon in 100 of seerwood, but 100 parts of seerwood yielded me only 43 parts of charcoal; this difference, however, it is easy to explain, as will be seen in the sequel.

Section VII. — *Of the Quantities of Heat developed in the Combustion of different Species of Wood.*

Many persons have already endeavoured to determine the relative quantities of heat furnished by wood and charcoal in their combustion; but the results of their inquiries have not been satisfactory. Their apparatus has been too imperfect, not to leave vast incertitude in the conclusions drawn from their investigations. Indeed, the subject is so intricate in itself, that with the best instruments the utmost care is requisite, lest, after much labour, the inquirer should be forced to content himself with approximations instead of accurate results and valuations strictly determined.

All woods contain much moisture, even when apparently very dry; and as the persons alluded to have neglected to determine the quantities of absolutely dry wood burned by them, much uncertainty prevails in the results of all their experiments.

Another source of uncertainty lies in the great quantity of heat suffered to escape with the smoke and other products of the combustion.

As the calorimeter used in my experiments has been described in a memoir which I had the honour to present to the Class on the 24th of February, 1812, it is unnecessary here to resume that subject; suffice it to explain, in a few words, the various precautions I adopted in burning wood under the calorimeter.

I picked out the woods intended for the experiment from a joiner's workshop, and they all appeared to be quite dry; I had them formed into small boards, 6 inches in length and $\frac{1}{2}$ an inch thick. From these boards I had some shavings planed off, about $\frac{1}{10}$ of a line thick, $\frac{1}{2}$ an inch broad, and 6 inches in length.

When these shavings were sufficiently dry, they were burned, one by one, under the mouth of the calorimeter; and I took care to hold them, by means of a small pair of nippers, so as to make them burn with a brisk flame, and without the least smoke or smell or calculable residuum in ashes.

The following is the method I pursued in making these experiments.

The calorimeter, filled with water at a temperature of about 5° of Fahrenheit's thermometer lower than that of the apartment in which the experiments were made, was placed upon its stand at the height of about 18 inches above the table on which the apparatus was laid.

The extremity of the calorimeter containing the opening, which I call its mouth, projects about 4 inches beyond the edge of the stand, so as easily to admit the point of the flame from the small piece of burning wood; and the height of the stand is so adjusted that the operator may rest both his elbows on the table, while his hands sustain the fragment of wood to be burned.

Near the calorimeter stands a small lamp, by which the pieces of wood, or rather shavings, may, without loss of time, be set on fire and burned in succession; and care is taken to have always in the hand a sufficient quantity of the shavings, of a known weight.

The very small portions of the shavings which remain between the nippers are carefully preserved, and weighed at the close of the experiments, to determine precisely how much of the wood has been consumed.

An assistant keeps his eye constantly on the thermometer attached to the apparatus, and announces the moment when the water in the calorimeter has attained a temperature as much higher than that of the room as it was below it at the beginning of the operation; and the flame from the piece of wood then burning is immediately blown out.

The remains of the shaving are laid aside, to be afterwards weighed with the other fragments.

The water in the calorimeter was then stirred, by shaking it, taking care to hold the instrument by its wooden frame, and the temperature of the water was minutely observed and set down in a register.

An experiment of this kind usually occupies about 10 or 12 minutes, according to the nature of the wood and the number of degrees to which the temperature of the calorimeter is raised.

I made choice of the birch for my first experiments, because the texture of its wood is very firm and even, and burns with a very regular flame.

To give the details and their results in few words, I have placed them together in the subjoined table.

The calorimeter, with the water it contained, was equal in capacity, as to heat, to 2781 grammes of water.

Heat developed in the Combustion of Birch Wood.

	No. of exp.	Quantity of wood consumed.	Heat communicated to the calorimeter.	Result. With the heat developed in combustion of 1 pound of combustible: Pounds of water heated 1° of Fahrenheit's thermometer.	Pounds of water at the temperature of melting ice, thrown into ebullition.
		Grms.	Degr's.		
Firewood, 2 years old	1	5.00	10½	5875	32.445
" " "	2	4.00	8½		32.841
Shavings dried in the air	3	4.55	10¼	6261	34.805
" " "	4	4.54	10¼		34.881
Shavings highly dried over a chafing-dish .	5	3.97	10	7002	38.916
Shavings highly dried over a chafing-dish .	6	2.58	6½		38.925
Shavings highly dried over a chafing-dish .	7	4.97	12½		38.858
Shavings highly dried and scorched in a stove	8	5.07	10¼	5614	31.325
Shavings highly dried and scorched in a stove	9	5.10	10¼		31.052
Shavings scorched, but not to so high a degree	10	4.89	10½	5971	33.174

On comparing the results of these six experiments, all made with the same kind of wood, in thin shavings, it will appear that the drier the wood, the greater was the quantity of heat produced from a given weight of shavings. But I found, in taking account of the quantities of moisture contained in the woods, the quantities of heat were always sensibly proportional to those of the dry wood burned, with the exception, however, of the three latter experiments, which were made with wood highly dried for 24 hours in a stove, and which gave several indications, by no means equivocal, of the beginning of a decomposition.

The shavings most scorched in the stove gave less heat than those which had been less scorched; the two sorts being taken in equal weights.

In all these experiments more or less water dripped from the worm, a certain proof that some hydrogen had been burned; this fact I was very desirous to verify, on account of its great importance to science.

It is not, therefore, mere carbon which furnished all the heat developed in the combustion of woods; of this important fact we shall shortly have an additional proof.

As the great quantity of nitrogen carried along with the products of the combustion, and which, after having passed through the worm, was lost in the atmosphere, also, without doubt, took with it a little more moisture than it had brought into the apparatus, a calculation of the quantity of water formed in the combustion of wood, grounded only on that found in the worm, would be erroneous, though there was always considerably more than necessary to demonstrate that water had been formed.

Before we close this paper, we shall point out a mode whereby the quantity of water thus formed may be estimated, even to such a degree of precision as to leave nothing more to be desired. But it is first necessary to determine the quantity of heat developed in the combustion of the carbon found in this wood, and which was totally consumed.

Although our experiments on the carbonization of wood, in close vessels, by a moderate fire, leave no doubt as to the quantities of charcoal which the woods therein employed were capable of producing, still the knowledge of this fact is not alone sufficient to enable us to determine the quantity of carbon contained in the wood.

As 100 parts of wood are required for 43 of charcoal, it is evident that the seerwood is at least partially decomposed when the charcoal is produced in the process of carbonization, that is to say, when the skeleton of the wood is deprived of its flesh, and left naked; and it is well known that a great quantity of pyroligneous acid is formed in the carbonization of wood, and this acid contains carbon.

From the process employed by Messrs. Gay-Lussac and Thénard, in their learned analysis, there can be no doubt that they discovered and kept an account of all the carbon found in the woods analyzed by them; and as there was no pyroligneous acid formed in my experiments when the wood was totally consumed without either smoke or smell, it is manifest that in this case all the carbon contained in the wood was burned.

According to the analyses of Messrs. Gay-Lussac and Thénard, 100 parts of oak, perfectly dry, contain 52.54 parts of carbon; and 100 parts of beech contain 51.45.

Now, as it seems to me extremely probable that the dry ligneous substance is palpably the same in all woods, I shall take the medium term of the results of these two analyses, and consider it as an indubitable fact, that 100 parts of perfectly dry wood contain 52 parts of carbon.

Therefore, as 100 parts of seerwood furnished me with only 43 of charcoal, we must conclude, if dry charcoal be considered as carbon, that of the 52 parts of carbon contained in 100 parts of seerwood, 9 are taken up in the composition of the pyroligneous acid formed in the carbonization of the wood, which 9 parts make more than 17 per cent of all the carbon contained in the wood.

Though charcoal should not be purely carbon, we must, nevertheless, admit that there is still a much greater proportion of carbon employed in the formation of that acid, or of other substances which fly off into the atmosphere during the process of the carbonization of the wood.

In pursuing inquiries in natural philosophy, the first object that demands attention is to keep an accurate account of weights; and so long as we proceed with the balance in hand, there is little hazard of being misled.

And here, before I proceed further in the inquiry into the sources of the heat developed during the combustion of wood, I shall exhibit a general table of the details and results of forty-three experiments made upon eleven different kinds of the woods of our climate. As I shall have occasion to refer to some of these experiments for the establishment of facts, it is requisite that they should first be known.

All these experiments having been made and registered long before I began the calculations ultimately adopted for the elucidation of their results, I have not hesitated to rely on them. And further, as they were made with all possible care, and with instruments to me apparently perfect, I can answer for their accuracy.

New experiments ever bear a certain value; all the knowledge which constitutes the imperishable riches of mankind consists only of accurate statements of well-conducted experiments. Happy they who have the good fortune of contributing something to the general stock!

Heat developed in the Combustion of various Species of Wood.

Species of wood.	Quality.	Number of the experiment.	Quantity of wood burned.	Heat communicated to the calorimeter whose capacity was equal to 2781 grammes of water.	Quantity of water at the thawing temperature, boiled by the heat developed in the combustion of one pound of combustible.
			Grms.	Degr's.	Pounds.
Lime	Joiner's dry wood, 4 years old	11	4.52	10⅛	34.609
		12	4.55	10¼	34.805
	Same kind highly dried over a chafing-dish	13	4.06	10¼	39.605
		14	3.80	10	40.658
	Same kind, rather less dried .	15	5.57	14	38.833
Beech	Joiner's dry wood, 4 or 5 years old	16	4.74	10⅜	33.817
		17	4.72	10¼	33.752
	Same kind, highly dried over a chafing-dish	18	5.07	12	36.334
		19	4.43	10⅜	36.184
Elm	Joiner's wood, rather moist .	20	6.34	11½	27.147
	Joiner's dry wood, 4 or 5 years old	21	5.28	10⅜	30.359
		22	5.45	10⅝	30.051
	Same kind, highly dried over a chafing-dish	23	4.70	10½	34.515
		24	5.28	11½	33.651
	Same kind, dried and scorched in the stove	25	4.00	8	30.900
Oak	Common firewood, in moderate shavings	26	4.83	8	25.590
	Same kind in thicker shavings; leaving a residuum of charcoal	27	6.40	10¼	24.748
	Same kind in thin shavings .	28	6.14	10½	26.272
	Same kind, thin shavings, well dried in the air	29	7.22	13	29.210
	Joiner's wood, very dry, in thin shavings	30	5.30	10¼	29.880
		31	5.33	10¼	19.796
	Thick shavings, leaving 0.92 gramme of charcoal . . .	32	6.48	11	26.227
Ash	Joiner's common dry wood .	33	5.29	10½	30.666
	Same kind, shavings dried in the air	34	3.78	8¼	33.720
	Same kind, highly dried over a chafing-dish	35	5.23	12	35.449

Heat developed in the Combustion of Wood. (Continued.)

Species of wood.	Quality.	Number of the experiment.	Quantity of wood burned.	Heat communicated to the calorimeter whose capacity was equal to 2781 grammes of water.	Quantity of water at the thawing temperature, boiled by the heat developed in the combustion of one pound of combustible.
			Grms.	Degr's.	Pounds.
Maple	Seasoned wood, highly dried over a chafing-dish . . .	36	3.85	9	36.117
Service	Same kind	37	4.49	10½	36.130
	Same kind, scorched in a stove	38	4.30	9	32.337
Cherry	Joiner's dry wood	39	4.75	10¼	33.339
	Same kind, highly dried over a chafing-dish	40	4.36	10½	36.904
	Same kind, scorched in a stove	41	5.00	11¼	34.763
Fir	Joiner's common dry wood .	42	5.35	10½	30.322
	Shavings, well dried in the air	43	4.09	9	34.000
	Highly dried over a chafing-dish	44	3.72	9	37.379
	Dried and scorched in a stove	45	4.40	9½	33.358
	Thick shavings, leaving much charcoal	46	4.51	6½	28.695
Poplar	Joiner's common dry wood .	47	4.13	9¼	34.601
	Same kind, highly dried over a chafing-dish	48	3.95	9½	37.161
Hornbeam	Joiner's dry wood	49	4.98	10¼	31.800
		50	5.01	10¼	31.609
Oak	Dried to {81.4 wood / 19.6 water} imperfectly burned, leaving a residuum of charcoal, in the combustion, of 0.81 gramme	51	6.14	10½	26.421
	0.73 "	52	4.83	8	25.591
	0.94 "	53	6.71	11	25.917

These experiments might lead to a great number of observations ; but I shall endeavour to reduce them to the exposition of a few simple facts which they present.

One fact, certainly very curious and of the first importance to the knowledge of the vegetable economy, appears to be well established; namely, that the skeleton of trees is pure charcoal, and that it exists in a perfect state in wood.

If this charcoal did not exist perfectly formed in wood, it could not possibly preserve its form, while its envelope of vegetable flesh is destroyed by the fire in the process of carbonization.

As the vegetable flesh contains hydrogen as well as carbon, it is more inflammable than charcoal, and is consumed at a lower temperature; and, by proper management of the fire, it may be totally destroyed without the enclosed skeleton of charcoal being injured.

Some months ago I presented the Class with a small sprig of charcoal produced from a piece of oak partially burned under my calorimeter. It was nearly all the charcoal contained in the piece. All the coal or flesh of the wood burned with a brisk flame, and the skeleton of the wood had got red, but the heat was not sufficient to consume it.

The charcoal-maker seldom does more than burn the flesh of the wood, and leaves the skeleton of charcoal naked.

The dry vegetable flesh produces more heat in its combustion than an equal weight of dry charcoal.

Shavings scorched in the stove by a great heat yield less heat in their combustion than shavings of the same kind of wood, whose vegetable flesh has not been touched. See experiments Nos. 5, 6, 7, 8, 9, 10, 25, 38, 41, 45.

In tables of experiments similar to those registered in the preceding table, it is scarcely possible to have errors on the greater side; but they may easily enough happen on the lesser. We may, therefore, place the more confidence in those wherein the quantities of heat manifested have been the greatest.

In the experiments Nos. 13 and 14, the wood of the lime-tree, dried over a chafing-dish, was productive of more heat than any other wood that I examined.

The result, it will be seen, was for 1 pound of this wood burned in experiment,

No. 13 . .	39.605	pounds of water heated 180° F., and in
No. 14 . .	40.658	" " "
Medium . .	40.1315	

In order accurately to ascertain how much water this wood contained, I dried thoroughly in the stove a parcel of shavings which had been previously dried over the chafing-dish, and found that it still retained 6.977 per cent of water.

Therefore we may conclude that 1 pound of this wood contains only 0.93023 pound of seerwood.

Now, if 0.93023 pound of seerwood will heat 40.1315 pounds of water 180° F., 1 pound of the same wood ought to heat 43.141 pounds; and I therefore take this quantity of water heated 180° F. as the standard of the heat developed in the combustion of 1 pound of wood perfectly dried.

Many persons have endeavoured to account for the heat manifested in the combustion of wood, by attributing it altogether to the charcoal contained in the wood burned.

This hypothesis we have now to examine.

It has been seen, that 100 parts of the wood of the lime-tree, perfectly dried, yielded 43.59 parts of charcoal; consequently 1 pound of this wood, thoroughly dried, can contain only 0.4359 pound of charcoal.

According to the results of Crawford's experiments, which we have found to be very accurate, 1 pound of charcoal furnishes in its combustion only the necessary heat for raising 57.608 pounds of water 180° F.; therefore the charcoal contained in 1 pound of dry lime-wood, equal to 0.4359 pound, can furnish in its combustion no more heat than is necessary to raise 25.111 pounds of water 180°; but as the experiment has given 43.141 pounds, there must certainly have been some other substance burned beside the charcoal, and which could have been none other than hydrogen.

Before we determine the quantity of hydrogen consumed, it is essential to ascertain how much heat has been furnished, not merely by the charcoal itself, but by the charcoal and the carbon contained in the wood; for it is very certain that all the carbon was burned, since no pyroligneous acid was formed.

According to the analyses of Messrs. Gay Lussac and Thénard, 1 pound of dry wood contains 0.52 pound of carbon.

If we adopt Crawford's estimate, we shall find that the combustion of 0.52 pound of carbon ought to furnish heat sufficient to raise 29.956 pounds of water 180° F.

Deducting this quantity of water from that given by the experiment, namely, 43.141 pounds, we shall have 13.185 pounds as the measure of the heat produced by the combustion of the hydrogen consumed in the experiment.

From the results of this inquiry we may conclude, that of the heat manifested in the combustion of wood, rather more than two thirds are produced by the combustion of the carbon, and a little less than one third by the hydrogen consumed.

These data supply us with an easy method of determining the quota of free and combustible hydrogen contained in seerwood.

According to Crawford's estimate, which we have followed all along, 1 pound of hydrogen yields in its combustion heat sufficient to raise 410 pounds of water 180° F.; therefore, the 13.185 pounds heated 180° in the experiment in question must have required 0.035158 pound of hydrogen, which is consequently the amount of free and combustible hydrogen contained in 1 pound of seerwood.

Assuming the medium term of the results of the two analyses of dry wood, made by Messrs. Gay Lussac and Thénard, 1 pound of seerwood would be composed of

Carbon	0.52 pound.
Hydrogen and oxygen, in the necessary proportions for forming water . .	0.48
	1.00

From the result of my experiments, 1 pound of seerwood is composed of two distinct substances; namely, —

A skeleton of charcoal weighing . . .	0.43 pound.
Vegetable flesh	0.57
	1.00

And these 0.57 pound of vegetable flesh are composed of

Carbon, free and combustible . . .	0.090 pound.
Hydrogen, free and combustible . .	0.035
Hydrogen and oxygen, in the necessary proportions for the formation of water . .	0.445
	0.570

In making these estimates, I have availed myself of the valuation of the total quantity of carbon contained in seerwood, given in the analysis of Messrs. Gay Lussac and Thénard; and I have supposed the 43 per cent of charcoal, which I found to be contained in seerwood, to be pure carbon.

Should it ultimately appear that charcoal is not pure carbon, which is extremely probable, numerous alterations in all these estimates must follow, though the experiments made upon the woods will always retain their value. And I cannot but hope that they will be frequently repeated, with such variations as may conduce to important discoveries.

It will be a satisfaction to me to know that I have put into the hands of more skilful workmen than myself some instruments of which they may advantageously avail themselves: and to have pointed out, as well as a little smoothed, a new path, wherein they may walk without danger of being lost.

SECTION VIII. — *Of the Quantity of Heat lost in the Carbonization of Wood.*

In making charcoal, a considerable quantity of heat is dissipated and lost in the air; whence it is evident that the same amount of heat cannot be obtained from burning a given quantity of charcoal as would be furnished by the combustion of the wood of which it is formed.

We can now determine, with great precision, the loss of heat which is inevitable in making charcoal, even when all possible precautions have been taken; as well as that which happens every day in the process employed by the charcoal-maker.

As the combustion of 1 pound of charcoal, perfectly dry, yields heat sufficient to boil 57.608 pounds of water, at the thawing temperature; and as 1 pound of wood, thoroughly dry, furnishes 0.4333 pound of dry charcoal, it follows, that the charcoal produced from 1 pound of dry wood should furnish in its combustion heat sufficient to boil 24.958 pounds of water, at the thawing temperature.

But we have already seen that the combustion of 1 pound of wood, thoroughly dry, should furnish sufficient heat to boil 43.143 pounds of water at the freezing temperature; or, which is the same thing, to raise it 180° of Fahrenheit's thermometer.

These two numbers (43.143 and 24.958), which express the quantities of heat in question, being in the proportion of 100 to 57.849, it is evident that the loss of heat *inevitable* in the carbonization of wood is upwards of 42 per cent, or exactly 42.151 per cent of the total quantity that the wood will furnish.

In order to determine the loss of heat which occurs in the forests, by the ordinary process of the charcoal-burner, it is requisite to ascertain the precise product of charcoal from a given quantity of wood, though it is probable that this product is very variable. M. Proust estimates it at 20 per cent in weight at the highest.

Adopting, for a moment, this estimate, and supposing the carbonized wood in the same state of dryness as what is usually sold for firewood; as 100 pounds of such wood contains only 0.76 pound of perfectly dry wood, this quantity would furnish in its combustion only the degree of heat necessary to raise 32.043 pounds of water 180° F.

But the 0.20 pound of charcoal produced by the carbonization of 1 pound of this wood, according to the usual process, can only furnish by combustion a sufficient quantity of heat to raise 11.521 pounds of water 180° F.; and as the numbers 32.043 and 11.521 are nearly in the proportion of 100 to 36, it should seem that the loss of heat in question is about 64 per cent.

One very important fact, which appears to be well ascertained by the results of this inquiry, is, that all the charcoal produced from the carbonization of 3 pounds of any kind of wood scarcely gives more heat in its combustion than would be furnished by 1 pound of the same sort of wood burned, and in its natural state.

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CHIMNEY FIREPLACES,

WITH

PROPOSALS FOR IMPROVING THEM TO SAVE FUEL; TO RENDER DWELLING-HOUSES MORE COMFORTABLE AND SALUBRIOUS, AND EFFECTUALLY TO PREVENT CHIMNEYS FROM SMOKING.

CHAPTER I.

Fireplaces for burning Coals, or Wood, in an open Chimney, are capable of great Improvement. — Smoking Chimneys may in all cases be completely cured. — The immoderate Size of the Throats of Chimneys the principal Cause of all their Imperfections. — Philosophical Investigation of the Subject. — Remedies proposed for all the Defects that have been discovered in Chimneys and their open Fireplaces. — These Remedies applicable to Chimneys destined for burning Wood, or Turf, as well as those constructed for burning Coals.

THE plague of a smoking chimney is proverbial; but there are many other very great defects in open fireplaces, as they are now commonly constructed in this country, and indeed throughout Europe, which, being less obvious, are seldom attended to; and there are some of them very fatal in their consequences to health; and, I am persuaded, cost the lives of thousands every year in this island.

Those cold and chilling draughts of air on one side of the body while the other side is scorched by a chimney fire, which every one who reads this must often have felt, cannot but be highly detrimental to health, and in weak and delicate constitutions must often produce the most fatal effects. I have not a doubt in my own mind that thousands die in this country every year of consumptions occasioned solely by this cause, — by a cause which might be so easily removed! — by a cause whose removal would tend to promote comfort and convenience in so many ways!

Strongly impressed as my mind is with the importance of this subject, it is not possible for me to remain silent. The subject is too nearly connected with many of the most essential enjoyments of life not to be highly interesting to all those who feel pleasure in promoting or in contemplating the comfort and happiness of mankind. And without suffering myself to be deterred either by the fear of being thought to give to the subject a degree of importance to which it is not entitled, or by the apprehension of being tiresome to my readers by the prolixity of my descriptions, I shall proceed to investigate the subject in all its parts and details with the utmost care and attention. And first with regard to smoking chimneys.

There are various causes by which chimneys may be prevented from carrying smoke, but there are none that may not easily be discovered and completely removed. This will doubtless be considered as a bold assertion; but I trust I shall be able to make it appear in a manner perfectly satisfactory to my readers that I have not ventured to give this opinion but upon good and sufficient grounds.

Those who will take the trouble to consider the nature and properties of elastic fluids, of air, smoke, and vapour, and to examine the laws of their motions, and the necessary consequences of their being rarified by heat, will perceive that it would be as much a miracle if smoke should not rise in a chimney, all hindrances to its ascent being removed, as that water should refuse to run in a syphon, or to descend in a river.

The whole mystery, therefore, of curing smoking chimneys, is comprised in this simple direction; *find out and remove those local hindrances which forcibly prevent the smoke from following its natural tendency to go up the chimney;* or rather, to speak more accurately, which prevent its being forced up the chimney by the pressure of the heavier air of the room.

Although the causes by which the ascent of smoke in a chimney *may be* obstructed are various, yet that cause which will most commonly, and I may say almost universally, be found to operate, is one which it is always very easy to discover, and as easy to remove, — the bad construction of the chimney *in the neighbourhood of the fireplace.*

In the course of all my experience and practice in curing smoking chimneys, — and I certainly have not had less than five hundred under my hands, and among them many which were thought to be quite incurable, — I never have been obliged, except in one single instance, to have recourse to any other method of cure than merely reducing the fireplace, and the throat of the chimney, or that part of it which lies immediately above the fireplace, to a proper form and just dimensions.

That my principles for constructing fireplaces are equally applicable to those which are designed for burning coal, as to those in which wood is burned, has lately been abundantly proved by experiments made here in

London; for of above a hundred and fifty fireplaces which have been altered in this city under my direction, within these last two months, there is not one which has not answered perfectly well.* And by several experiments which have been made with great care, and with the assistance of thermometers, it has been demonstrated, that the saving of fuel, arising from these improvements of fireplaces, amounts in all cases to more than *half*, and in many cases to more than *two thirds*, of the quantity formerly consumed. Now as the alterations in fireplaces which are necessary may be made at a very trifling expense,— as any kind of grate or stove may be made use of, and as no iron work but merely a few bricks and some mortar, or a few small pieces of fire-stone, are required, — the improvement in question is very important when considered merely with a view to economy; but it should be remembered, that not only a great saving is made of fuel by the alterations proposed, but that rooms are made much more comfortable,

* Eves and Sutton, bricklayers, Broad Sanctuary, Westminster, have alone altered above ninety chimneys. The experiment was first made in London at Lord Palmerston's house in Hanover Square; then two chimneys were altered in the house of Sir John Sinclair, Baronet, President of the Board of Agriculture; one in the room in in which the Board meets, and the other in the Secretary's room; which last being much frequented by persons from all parts of Great Britain, it was hoped that circumstance would tend much to expedite the introduction of these improvements in various parts of the kingdom. Several chimneys were then altered in the house of Sir Joseph Banks, Baronet, K. B., President of the Royal Society. Afterwards a number were altered in Devonshire House; in the house of Earl Besborough, in Cavendish Square, and at his seat at Rockhampton; at Holywell House, near St. Alban's, the seat of the Countess Dowager Spencer; at Melbourne House; at Lady Templeton's, in Portland Place; at Mrs. Montagu's, in Portman Square; at Lord Sudley's, in Dover Street; at the Marquis of Salisbury's seat, at Hatfield, and at his house in town; at Lord Palmerston's seat in Broadlands, near Southampton, and at several gentlemen's houses in that neighborhood; and a great many others; but it would be tiresome to enumerate them all, and even these are mentioned merely for the satisfaction of those who may wish to make inquiries respecting the success of the experiments.

and more salubrious; that they may be more equally warmed, and more easily kept at any required temperature; that all draughts of cold air from the doors and windows towards the fireplace, which are so fatal to delicate constitutions, will be completely prevented; that in consequence of the air being equally warm all over the room, or in all parts of it, it may be entirely changed with the greatest facility, and the room completely ventilated when this air is become unfit for respiration, and this merely by throwing open for a moment a door opening into some passage from whence fresh air may be had, and the upper part of a window; or by opening the upper part of one window and the lower part of another. And as the operation of ventilating the room, even when it is done in the most complete manner, will never require the door and window to be open more than one minute, in this short time the walls of the room will not be sensibly cooled, and the fresh air which comes into the room will, in a very few minutes, be so completely warmed by these walls, that the temperature of the room, though the air in it be perfectly changed, will be brought to be very nearly the same as it was before the ventilation.

Those who are acquainted with the principles of pneumatics, and know why the warm air in a room rushes out at an opening made for it at the top of a window when colder air from without is permitted to enter by the door or by any other opening situated lower than the first, will see that it would be quite impossible to ventilate a room in the complete and expeditious manner here described, where the air in a room is partially warmed, or hardly warmed at all, and where the walls of the room, remote from the fire, are constantly cold; which must always be the case where, in consequence of a strong current up the chimney, streams of cold air are continually coming in through all the crevices of the doors and windows, and flowing into the fireplace.

But although rooms furnished with fireplaces constructed upon the principles here recommended, may be easily and most effectually ventilated (and this is certainly a circumstance in favour of the proposed improvements), yet such total ventilations will very seldom, if ever, be necessary. As long as *any fire* is kept up in the room, there is so considerable a current of air up the chimney, notwithstanding all the reduction that can be made in the size of its throat, that the continual change of air in the room which this current occasions will, generally, be found to be quite sufficient for keeping the air in the room sweet and wholesome; and, indeed, in rooms in which there is no open fireplace, and consequently no current of air from the room setting up the chimney,— which is the case in Germany and all the northern parts of Europe, where rooms are heated by stoves, whose fireplaces, opening without, are not supplied with the air necessary for the combustion of the fuel from the room; and although in most of the rooms abroad, which are so heated, the windows and doors are double, and both are closed in the most exact manner possible, by slips of paper pasted over the crevices, or by slips of list or fur, yet when these rooms are tolerably large, and when they are not very much crowded by company, nor filled with a great many burning lamps or candles, the air in them is seldom so much injured as to become oppressive or unwholesome, and those who inhabit them show by their ruddy countenances, as well as by every other sign of perfect health, that they suffer no inconvenience whatever from their closeness. There is frequently, it is true, an oppressiveness in the air of a room heated by a German stove, of which those who are not so much accustomed to living in those rooms seldom fail to complain, and indeed with much reason; but this oppressiveness does not arise from the air of the room being injured by the respiration and perspiration of those who inhabit it; it arises from a very different cause, — from a fault in the construction of German stoves in general, but which may be easily and most completely remedied, as I shall show more fully in another place. In the mean time, I would just observe here with regard to these stoves, that as they are often made of iron, and as this metal is a very good conductor of heat, some part of the stove in contact with the air of the room becomes so hot as to calcine or rather to *roast* the dust which lights upon it; which never can fail to produce a very disagreeable effect on the air of the room. And even when the stove is constructed of pantiles or pottery-ware, if any part of it in contact with the air of the room is suffered to become very hot, which seldom fails to be the case in German stoves constructed on the common principles, nearly the same effects will be found to be produced on the air as when the stove is made of iron, as I have very frequently had occasion to observe.

Though a room be closed in the most perfect manner possible, yet, as the quantity of air injured and rendered unfit for further use by the respiration of two or three persons in a few hours is very small compared to the immense volume of air which a room of a moderate size contains; and as a large quantity of fresh air always enters the room, and an equal quantity of the warm air of the room is driven out of it every time the door is opened, there is much less danger of the air of a room becoming unwholesome for the want of ventilation than has been generally imagined; particularly in cold weather, when all the different causes which conspire to change the air of warmed rooms act with increased power and effect.

Those who have any doubts respecting the very great change of air or ventilation which takes place each time the door of a warm room is opened in cold weather, need only set the door of such a room wide open for a moment, and hold two lighted candles in the doorway, one near the top of the door and the other near the bottom of it: the violence with which the flame of that above will be driven outwards, and that below inwards, by the two strong currents of air which, passing in opposite directions, rush in and out of the room at the same time, will be convinced that the change of air which actually takes place must be very considerable indeed; and these currents will be stronger, and consequently the change of air greater, in proportion as the difference is greater between the temperatures of the air within the room and of that without. I have been more particular upon this subject, — the ventilation of warmed rooms which are constantly inhabited, — as I know that people in general in this country have great apprehensions of the bad consequences to health of living in rooms in which there is not a continual influx of cold air from without. I am as much an advocate for a *free circulation* of air as anybody, and always sleep in a bed without curtains on that account; but I am much inclined to think, that the currents of cold air which never fail to be produced in rooms heated by fireplaces constructed upon the common principle, — those partial heats on one side of the body, and cold blasts on the other, so often felt in houses in this country, — are infinitely more detrimental to health than the supposed closeness of the air in a room warmed more equally, and by a smaller fire.

All these advantages, attending the introduction of the improvements in fireplaces here recommended, are certainly important, and I do not know that they are counterbalanced by any one disadvantage whatsoever. The only complaint that I have ever heard made against them was that they made the rooms *too* warm; but the remedy to this evil is so perfectly simple and obvious, that I should be almost afraid to mention it, lest it might be considered as an insult to the understanding of the person to whom such information should be given; for nothing surely can be conceived more perfectly ridiculous than the embarrassment of a person on account of the too great heat of his room, when it is in his power to diminish *at pleasure* the fire by which it is warmed; and yet, strange as it may appear, this has sometimes happened!

Before I proceed to give directions for the construction of fireplaces, it will be proper to examine more carefully the fireplaces now in common use; to point out their faults; and to establish the principles upon which fireplaces ought to be constructed.

The great fault of all the open fireplaces, or chimneys, for burning wood or coals in an open fire, now in common use, is, that they are much too large; or, rather, it is *the throat of the chimney*, or the lower part of its open canal, in the neighborhood of the mantle and immediately over the fire, which is too large. This opening has hitherto been left larger than otherwise it probably would have been made, in order to give a passage to the chimney-sweeper; but I shall show hereafter how a passage for the chimney-sweeper may be contrived without leaving the throat of the chimney of such enormous dimensions as to swallow up and devour all the warm air of the room, instead of merely giving a passage to the smoke and heated vapour which rise from the fire, for which last purpose alone it ought to be destined.

Were it my intention to treat my subject in a formal scientific manner, it would doubtless be proper, and even necessary, to begin by explaining in the fullest manner, and upon the principles founded on the laws of nature, relative to the motions of elastic fluids, as far as they have been discovered and demonstrated, the causes of the ascent of smoke; and also to explain and illustrate upon the same principles, and even to measure or estimate by calculations, the precise effects of all those mechanical aids which may be proposed for assisting it in its ascent, or rather for removing those obstacles which hinder its motion upwards; but as it is my wish rather to write a useful practical treatise than a learned dissertation,—being more desirous to contribute in diffusing useful knowledge by which the comforts and enjoyments of mankind may be increased, than to acquire the reputation of a philosopher among learned men,—I shall endeavour to write in such a manner as to be easily understood *by those who are most likely to profit by the information I have to communicate*, and consequently most likely to assist in bringing into general use the improvements I recommend. This being premised, I shall proceed, without any further preface or introduction, to the investigation of the subject I have undertaken to treat.

As the immoderate size of the throats of chimneys is the great fault of their construction, it is this fault which ought always to be first attended to in every attempt which is made to improve them; for however perfect the construction of a fireplace may be in other respects, if the opening left for the passage of the smoke is larger than is necessary for that purpose, nothing can prevent the warm air of the room from escaping through it; and whenever this happens, there is not only an unnecessary loss of heat, but the warm air which leaves the room to go up the chimney being replaced by cold air from without, the draughts of cold air, so often mentioned, cannot fail to be produced in the room, to the great annoyance of those who inhabit it. But although both these evils may be effectually remedied by reducing the throat of the chimney to a proper size, yet in doing this several precautions will be necessary. And first of all, the throat of the chimney should be in its proper place: that is to say, in that place in which it ought to be, in order that the ascent of the smoke may be most facilitated; for every means which can be employed for facilitating the ascent of the smoke in the chimney must naturally tend to prevent the chimney from smoking; now as the smoke and hot vapour which rise from a fire naturally tend *upwards*, the proper place for the throat of the chimney is evidently perpendicularly *over the fire*.

But there is another circumstance to be attended to in determining the proper place for the throat of a chimney, and that is to ascertain its distance from the fire, or *how far* above the burning fuel it ought to be placed. In determining this point, there are many things to be considered, and several advantages and disadvantages to be weighed and balanced.

As the smoke and vapour which ascend from burning fuel rise in consequence of their being rarefied by heat, and made lighter than the air of the surrounding atmosphere; and as the degree of their rarefaction, and consequently their tendency to rise, is in proportion to the intensity of their heat; and further, as they are hotter near the fire than at a greater distance from it, it is clear that the nearer the throat of a chimney is to the fire, the stronger will be what is commonly called its *draught*, and the less danger there will be of its smoking. But on the other hand, when the draught of a chimney is very strong, and particularly when this strong draught is occasioned by the throat of the chimney being very near the fire, it may so happen that the draught of air into the fire may become so strong as to cause the fuel to be consumed too rapidly. There are likewise several other inconveniences which would attend the placing of the throat of a chimney *very near* the burning fuel.

In introducing the improvements proposed, in chimneys already built, there can be no question in regard to the height of the throat of the chimney, for its place will be determined by the height of the mantle. It can hardly be made lower than the mantle; and it ought always to be brought down as nearly upon the level with the bottom of it as possible. If the chimney is apt to smoke, it will sometimes be necessary either to lower the mantle or to diminish the height of the opening of the fireplace, by throwing over a flat arch, or putting in a straight piece of stone from one side of it to the other, or, which will be still more simple and easy in practice, building a wall of bricks, supported by a flat bar of iron, immediately under the mantle.

Nothing is so effectual to prevent chimneys from smoking as diminishing the opening of the fireplace in the manner here described, and lowering and diminishing the throat of the chimney; and I have always found, except in the single instance already mentioned, that a perfect cure may be effected by *these means alone*, even in the most desperate cases. It is true, that when the construction of the chimney is very bad indeed, or its situation very unfavourable to the ascent of the smoke, and especially when both these disadvantages exist at the same time, it may sometimes be necessary to diminish the opening of the fireplace, and particularly to lower it, and also to lower the throat of the chimney, more than might be wished; but still I think this can produce no inconveniences to be compared with that greatest of all plagues, a smoking chimney.

The position of the throat of a chimney being determined, the next points to be ascertained are its size and form, and the manner in which it ought to be connected with the fireplace below, and with the open canal of the chimney above.

But as these investigations are intimately connected with those which relate to the form proper to be given to the fireplace itself, we must consider them all together.

That these inquiries may be pursued with due method, and that the conclusions drawn from them may be clear and satisfactory, it will be necessary to consider, first, what the objects are which ought principally to be had in view in the construction of a fireplace; and secondly, to see how these objects can best be attained.

Now the design of a chimney fire being simply to warm a room, it is necessary, first of all, to contrive matters so that the room shall be actually warmed; secondly, that it be warmed with the smallest expense of fuel possible; and, thirdly, that, in warming it, the air of the room be preserved perfectly pure and fit for respiration, and free from smoke and all disagreeable smells.

In order to take measures with certainty for warming a room by means of an open chimney fire, it will be necessary to consider *how*, or *in what manner*, such a fire communicates heat to a room. This question may perhaps, at the first view of it, appear to be superfluous and trifling, but a more careful examination of the matter will show it to be highly deserving of the most attentive investigation.

To determine in what manner a room is heated by an open chimney fire, it will be necessary, first of all, to find out *under what form* the heat generated in the combustion of the fuel exists, and then to see how it is communicated to those bodies which are heated by it.

In regard to the first of these subjects of inquiry, it is quite certain that the heat which is generated in the combustion of the fuel exists under *two* perfectly distinct and very different forms. One part of it is *combined* with the smoke, vapour, and heated air, which rise from the burning fuel, and goes off with them into the upper regions of the atmosphere; while the other part, which appears to be *uncombined*, or, as some ingenious philosophers have supposed, combined only with light, is sent off from the fire in rays in all possible directions.

With respect to the second subject of inquiry, namely, — how this heat, existing under these two different forms, is communicated to other bodies; it is highly probable that the combined heat can only be communicated to other bodies by *actual contact* with the body with which it is combined; and with regard to the rays which are sent off by burning fuel, it is certain that *they* communicate or generate heat only *when* and *where* they are stopped or absorbed. In passing through air, which is transparent, they certainly do not communicate any heat to it; and it seems highly probable that they do not communicate heat to solid bodies by which they are reflected.

In these respects they seem to bear a great resemblance to the solar rays. But n order not to distract the attention of my reader or carry him too far away from the subject more immediately under consideration, I must not enter too deeply into these inquiries respecting the nature and properties of what has been called *radiant heat*. It is certainly a most curious subject of philosophical investigation, but more time would be required to do it justice than we now have to spare. We must, therefore, content ourselves with such a partial examination of it as will be sufficient for our present purpose.

A question which naturally presents itself here is, What proportion does the radiant heat bear to the combined heat? Though that point has not yet been determined with any considerable degree of precision, it is, however, quite certain, that the quantity of heat which goes off combined with the smoke, vapour, and heated air, is much more considerable, perhaps three or four times greater, at least, than that which is sent off from the fire in rays. And yet, small as the quantity is of this radiant heat, it is the only part of the heat generated in the combustion of fuel burned in an open fireplace, which is ever employed, or which can ever be employed, in heating a room.

The whole of the combined heat escapes by the chimney, and is totally lost; and, indeed, no part of it could ever be brought into a room from an open fireplace, without bringing along with it the smoke with which it is combined; which, of course, would render it impossible for the room to be inhabited. There is, however, one method by which combined heat, and even that which arises from an open fireplace, may be made to assist in warming a room; and that is by making it pass through something analogous to a German stove, placed in the chimney above the fire. But of this contrivance I shall take occasion to treat more fully hereafter; in the mean time I shall continue to investigate the properties of open chimney fireplaces, constructed upon the most simple principles, such as are now in common use; and shall endeavour to point out and explain all those improvements of which *they* appear to me to be capable. When fuel is burned in fireplaces upon this simple construction, where the smoke escapes immediately by the open canal of the chimney, it is quite evident that all the combined heat must of necessity be lost; and as it is the radiant heat alone which can be employed in heating a room, it becomes an object of much importance to determine how the greatest quantity of it may be generated in the combustion of the fuel, and how the greatest proportion possible of that generated may be brought into the room.

Now, the quantity of radiant heat generated in the combustion of a given quantity of any kind of fuel depends very much upon the management of the fire, or upon the manner in which the fuel is consumed. When the fire burns bright, much radiant heat will be sent off from it; but when it is *smothered up*, very little will be generated; and indeed very little combined heat, that can be employed to any useful purpose; most of the heat produced will be immediately *expended* in giving elasticity to a thick dense vapour or smoke which will be seen rising from the fire; and the combustion being very incomplete, a great part of the inflammable matter of the fuel being merely rarefied and driven up the chimney without being inflamed, the fuel will be wasted to little purpose. And hence it appears of how much importance it is, whether it be considered with a view to economy, or to cleanliness, comfort, and elegance, to pay due attention to the management of a chimney fire.

Nothing can be more perfectly void of common-sense, and wasteful and slovenly at the same time, than the manner in which chimney fires, and particularly where coals are burned, are commonly managed by servants. They throw on a load of coals at once, through which the flame is hours in making its way; and frequently it is not without much trouble that the fire is prevented from going quite out. During this time, no heat is communicated to the room; and what is still worse, the throat of the chimney, being occupied merely by a heavy dense vapour not possessed of any considerable degree of heat, and consequently not having much elasticity, the warm air of the room finds less difficulty in forcing its way up the chimney and escaping, than when the fire burns bright; and it happens not unfrequently, especially in chimneys and fireplaces ill constructed, that this current of warm air from the room, which presses into the chimney, crossing upon the current of heavy smoke which rises slowly from the fire, obstructs it in its ascent, and beats it back into the room; hence it is that chimneys so often smoke when too large a quantity of fresh coals is put upon the fire. So many coals should never be put on the fire at once, as to prevent the free passage of the flame between. In short, a fire should never be smothered; and when proper attention is paid to the quantity of coals put on, there will be very little use for the poker; and this circumstance will contribute very much to cleanliness and to the preservation of furniture.

Those who have feeling enough to be made miserable by anything careless, slovenly, and wasteful, which happens under their eyes, who know what comfort is, and consequently are worthy of the enjoyments of a *clean hearth* and *cheerful fire*, should really either take the trouble themselves to manage their fires (which, indeed, would rather be an amusement to them than a trouble), or they should instruct their servants to manage them better.

But to return to the subject more immediately under consideration. As we have seen what is necessary to the production or generation of radiant heat, it remains to determine how the greatest proportion of that generated and sent off from the fire in all directions may be made to enter the room, and assist in warming it. Now, as the rays which are thrown off from burning fuel have this property in common with light, that they generate heat only *when* and *where* they are stopped or absorbed, and also in being capable of being reflected *without generating heat* at the surfaces of various bodies, the knowledge of these properties will enable us to take measures, with the utmost certainty, for producing the effect required, — that is to say, for bringing as much radiant heat as possible into the room.

This must be done, first, by causing as many as possible of the rays, as they are sent off from the fire in straight lines, to come *directly* into the room; which can only be effected by bringing the fire as far forward as possible, and leaving the opening of the fireplace as wide and as high as can be done without inconvenience; and secondly, by making the sides and back of the fireplace of such form, and constructing them of such materials, as to cause the direct rays from the fire, which strike against them, to be sent into the room *by reflection* in the greatest abundance.

Now it will be found, upon examination, that the best form for the vertical sides of a fireplace, or the *covings* (as they are called), is that of an upright plane, making an angle with the plane of the back of the fireplace of about 135 degrees. According to the present construction of chimneys, this angle is 90 degrees, or forms a right angle; but as in this case the two sides or covings of the fireplace (A C, B D, Plate VIII., Fig. 1) are parallel to each other, it is evident that they are very ill contrived for throwing into the room by reflection the rays from the fire which fall on them.

To have a clear and perfect idea of the alterations I propose in the forms of fireplaces, the reader need only observe, that, whereas the backs of fireplaces, as they are now commonly constructed, are as wide as the opening of the fireplace in front, and the sides of it are of course perpendicular to it and parallel to each other, — in the fireplaces I recommend, the back (*i k*, Plate IX., Fig. 3) is only about one third of the width of the opening of the fireplace in front (*a b*), and consequently that the two sides or covings of the fireplace (*a i* and *b k*), instead of being perpendicular to the back, are inclined to it at an angle of about 135 degrees; and in consequence of this position, instead of being parallel to each other, each of them presents an oblique front towards the opening of the chimney, by means of which the rays which they reflect are thrown into the room. A bare inspection of the annexed drawings (Plate VIII., Fig. 1, and Plate IX., Fig. 3) will render this matter perfectly clear and intelligible.

In regard to the materials which it will be most advantageous to employ in the construction of fireplaces, so much light has, I flatter myself, already been thrown on the subject we are investigating, and the principles adopted have been established on such clear and obvious facts, that no great difficulty will attend the determination of that point. As the object in view is to bring radiant heat into the room, it is clear that that material is best for the construction of a fireplace, which reflects the most, or which *absorbs the least* of it; for that heat which is *absorbed* cannot be *reflected.* Now, as bodies which absorb radiant heat are necessarily heated in consequence of that absorption, to discover which of the various materials that can be employed for constructing fireplaces are best adapted for that purpose, we have only to find out by an experiment, very easy to be made, what bodies acquire *least heat* when exposed to the direct rays of a clear fire; for those which are least heated evidently absorb the least, and consequently reflect the most radiant heat. And hence it appears that iron, and, in general, metals of all kinds, which are well known to *grow very hot* when exposed to the rays projected by burning fuel, are to be reckoned among the *very worst* materials that it is possible to employ in the construction of fireplaces.

The best materials I have hitherto been able to discover are fire-stone, and common bricks and mortar. Both these materials are, fortunately, very cheap; and as to their comparative merits, I hardly know to which of them the preference ought to be given.

When bricks are used, they should be covered with a thin coating of plaster, which, when it is become perfectly dry, should be whitewashed. The fire-stone should likewise be whitewashed, when that is used; and every part of the fireplace, which is not exposed to being soiled and made black by the smoke, should be kept as white and clean as possible. As *white* reflects more heat, as well as more light, than any other colour, it ought always to be preferred for the inside of a chimney fireplace, and *black*, which reflects neither light nor heat, should be most avoided.

I am well aware how much the opinion I have here ventured to give, respecting the unfitness of iron and other metals to be employed in the construction of open fireplaces, differs from the opinion generally received upon that subject; and I even know that the very reason, which, according to my ideas of the matter, renders them totally unfit for the purpose, is commonly assigned for making use of them; namely,—that they soon grow very hot. But I would beg leave to ask what advantage is derived from heating them?

I have shown the disadvantage of it; namely,—that the quantity of radiant heat thrown into the room is diminished; and it is easy to show that almost the whole of that absorbed by the metal is ultimately carried up the chimney by the air, which, coming into contact with this hot metal, is heated and rarefied by it, and, forcing its way upwards, goes off with the smoke; and as no current of air ever sets from any part of the opening of a fireplace into the room, it is impossible to conceive how the heat existing in the metal composing any part of the apparatus of the fireplace, and situated within its cavity, can come, or be brought, into the room.

This difficulty may be in part removed, by supposing, what indeed seems to be true in a certain degree, that the heated metal sends off in rays the heat it acquires from the fire, even when it is not heated red-hot; but still, as it never can be admitted that the heat absorbed by the metal, and afterwards thrown off by it in rays, is *increased* by this operation, nothing can be gained by it; and as much must necessarily be lost in consequence of the great quantity of heat communicated by the hot metal to the air in contact with it, which, as has already been shown, always makes its way up the chimney, and flies off into the atmosphere, the loss of heat attending the use of it is too evident to require being further insisted on.

There is, however, in chimney fireplaces destined for burning coals, one essential part, the grate, which cannot well be made of anything else but iron; but there is no necessity whatever for that immense quantity of iron which surrounds grates as they are now commonly constructed and fitted up, and which not only renders them very expensive, but injures very essentially the fireplace. If it should be necessary to diminish the opening of a large chimney in order to prevent its smoking, it is much more simple, economical, and better in all respects, to do this with marble, fire-stone, or even with bricks and mortar, than to make use of iron, which, as has already been shown, is the very worst material that can possibly be employed for that purpose; and as to registers, they not only are quite unnecessary where the throat of a chimney is properly constructed, and of proper dimensions, but in that case would do much harm. If they act at all, it must be by opposing their flat surfaces to the current of rising smoke in a manner which cannot fail to embarrass and impede its motion. But we have shown that the passage of the smoke through the throat of a chimney ought to be facilitated as much as possible, in order that it may be enabled to pass by a small aperture.

Register stoves have often been found to be of use; but it is because, the great fault of all fireplaces constructed upon the common principles being the enormous dimensions of the throat of the chimney, this fault has been in some measure corrected by them; but I will venture to affirm that there never was a fireplace so corrected that would not have been much more improved, and with infinitely less expense, by the alterations here recommended, and which will be more particularly explained in the next chapter.

CHAPTER II.

Practical Directions designed for the Use of Workmen, showing how they are to proceed in making the Alterations necessary to improve Chimney Fireplaces, and effectually to cure smoking Chimneys.

ALL chimney fireplaces, without exception, whether they are designed for burning wood or coals, and even those which do not smoke, as well as those which do, may be greatly improved by making the alterations in them here recommended; for it is by no means *merely* to prevent chimneys from smoking that these improvements are recommended, but it is also to make them better in all other respects as fireplaces; and when the alterations proposed are properly executed, which may very easily be done with the assistance of the following plain and simple directions, the chimneys will never fail to answer, I will venture to say, even beyond expectation. The room will be heated much more equally and more pleasantly with *less than half the fuel* used before; the fire will be more cheerful and more agreeable, and the general appearance of the fireplace more neat and elegant; and the chimney *will never smoke.*

The advantages which are derived from mechanical inventions and contrivances are, I know, frequently accompanied by disadvantages which it is not always possible to avoid; but in the case in question, I can say with truth, that I know of no disadvantage whatever that attends the fireplaces constructed upon the principles here recommended. But to proceed in giving directions for the construction of these fireplaces.

That what I have to offer on this subject may be the more easily understood, it will be proper to begin by explaining the precise meaning of all those technical words and expressions which I may find it necessary or convenient to use.

By the *throat* of a chimney, I mean the lower extremity of its canal, where it unites with the upper part of its open fireplace. This throat is commonly found about a foot above the level of the lower part of the mantle, and it is sometimes contracted to a smaller size than the rest of the canal of the chimney, and sometimes not.

Plate X., Fig. 5, shows the section of a chimney on the common construction, in which *d e* is the throat.

Fig. 6 shows the section of the same chimney altered and improved, in which *d i* is the reduced throat.

The *breast* of a chimney is that part of it which is immediately behind the mantle. It is the wall which forms the entrance from below, into the throat of the chimney in front, or towards the room. It is opposite to the upper extremity of the back of the open fireplace, and parallel to it; in short, it may be said to be the back part of the mantle itself. In the figures 5 and 6, it is marked by the letter *d*. The *width* of the throat of the chimney (*d e*, Fig. 5, and *d i*, Fig. 6) is taken from the breast of the chimney to the back, and its *length* is taken at right angles to its width, or in a line parallel to the mantle (*a*, Figs. 5 and 6).

Before I proceed to give particular directions respecting the exact forms and dimensions of the different parts of a fireplace, it may be useful to make such general and practical observations upon the subject as can be clearly understood without the assistance of drawings; for the more complete the knowledge of any subject is, which can be acquired without drawings, the more easy will it be to understand the drawings when it becomes necessary to have recourse to them.

The bringing forward of the fire into the room, or rather bringing it nearer to the front of the opening of the fireplace, and the diminishing of the throat of the chimney, being two objects principally had in view in the alterations in fireplaces here recommended, it is evident that both these may be attained, merely by bringing forward the back of the chimney. The only question therefore is, how far it should be brought forward.

The answer is short, and easy to be understood, — bring it forward as far as possible, without diminishing too much the passage which must be left for the smoke. Now as this passage, which, in its narrowest part, I have called the *throat of the chimney*, ought, for reasons which are fully explained in the foregoing chapter, to be immediately, or perpendicularly, over the fire, it is evident that the back of the chimney must always be built perfectly upright. To determine therefore the place for the new back, or how far precisely it ought to be brought forward, nothing more is necessary than to ascertain how wide the throat of the chimney ought to be left, or what space must be left between the top of the breast of the chimney, where the upright canal of the chimney begins, and the new back of the fireplace carried up perpendicularly to that height.

In the course of my numerous experiments upon chimneys, I have taken much pains to determine the width proper to be given to this passage, and I have found, that, when the back of the fireplace is of a proper width, the best width for the throat of a chimney, when the chimney and the fireplace are at the usual form and size, is *four inches*. Three inches might sometimes answer, especially where the fireplace is very small, and the chimney good, and well situated; but as it is always of much importance to prevent those accidental puffs of smoke which are sometimes thrown into rooms by the carelessness of servants in putting on suddenly too many coals at once upon the fire, and as I found these accidents sometimes happened when the throats of chimneys were made very narrow, I found that, upon the whole, all circumstances being well considered, and advantages and disadvantages compared and balanced, *four inches* is the best width that can be given to the throat of a chimney; and this, whether the fireplace be destined to burn wood, coals, turf, or any other fuel commonly used for heating rooms by an open fire.

In fireplaces destined for heating very large halls, and where very great fires are kept up, the throat of the chimney, may, if it should be thought necessary, be made four inches and an half, or five inches wide; but I have frequently made fireplaces for halls, which have answered perfectly well, where the throats of the chimneys have not been wider than four inches.

It may perhaps appear extraordinary, upon the first view of the matter, that fireplaces of such different sizes should all require the throat of the chimney to be of the same width; but when it is considered that the *capacity* of the throat of a chimney does not depend on its width alone, but on its width and length taken together, and that in large fireplaces, the width of the back, and consequently the length of the throat of the chimney, is greater than in those which are smaller, this difficulty vanishes.

And this leads us to consider another important point respecting open fireplaces, and that is, the width which it will, in each case, be proper to give to the back. In fireplaces as they are now commonly constructed, the back is of equal width with the opening of the fireplace in front; but this construction is faulty on two accounts. First, in a fireplace so constructed, the sides of the fireplace — or *covings*, as they are called — are parallel to each other, and consequently ill contrived to throw out into the room the heat they receive from the fire in the form of rays; and secondly, the large open corners, which are formed by making the back as wide as the opening of the fireplace in front, occasion eddies of wind which frequently disturb the fire, and embarrass the smoke in its ascent in such a manner as often to bring it into the room. Both these defects may be entirely remedied by diminishing the width of the back of the fireplace. The width which, in most cases, it will be best to give it is *one third* of the width of the opening of the fireplace in front. But it is not absolutely necessary to conform rigorously to this decision, nor will it always be possible. It will frequently happen that the back of a chimney must be made wider than, according to the rule here given, it ought to be. This may be either to accommodate the fireplace to a stove, which, being already on hand, must, to avoid the expense of purchasing a new one, be employed; or for other reasons; and any small deviation from the general rule will be attended with no considerable inconvenience. It will always be best, however, to conform to it as far as circumstances will allow.

Where a chimney is designed for warming a room of a middling size, and where the thickness of the wall of the chimney in front, measured from the front of the mantle to the breast of the chimney, is nine inches, I should set off four inches more for the width of the throat of the chimney, which, supposing the back of the chimney to be built upright, as it always ought to be, will give thirteen inches for the depth of the fireplace, measured upon the hearth from the opening of the fireplace in front to the back. In this case, thirteen inches would be a good size for the width of the back; and three times thirteen inches, or thirty-nine inches, for the width of the opening of the fireplace in front; and the angle made by the back of the fireplace and the sides of it, or covings, would be just 135 degrees, which is the best position they can have for throwing heat into the room.

But I will suppose that in altering such a chimney it is found necessary, in order to accommodate the fireplace to a grate or stove already on hand, to make the fireplace sixteen inches wide. In that case, I should merely increase the width of the back to the dimensions required, without altering the depth of the chimney or increasing the width of the opening of the chimney in front. The covings, it is true, would be somewhat reduced in their width by this alteration; and their position with respect to the plane of the back of the chimney would be a little changed; but these alterations would produce no bad effects of any considerable consequence, and would be much less likely to injure the fireplace, than an attempt to bring the proportions of its parts nearer to the standard, by increasing the depth of the chimney, and the width of its opening in front; or than an attempt to preserve that particular obliquity of the covings which is recommended as the best (135 degrees), by increasing the width of the opening of the fireplace, without increasing its depth.

In order to illustrate this subject more fully, we will suppose one case more. We will suppose that in the chimney which is to be altered, the width of the fireplace in front is either wider or narrower than it ought to be, in order that the different parts of the fireplace, after it is altered, may be of the proper dimensions. In this case, I should determine the depth of the fireplace, and the width of the back of it, without any regard to the width of the opening of the fireplace in front; and when this is done, if the opening of the fireplace should be only two or three inches too wide, — that is to say, only two or three inches wider than is necessary in order that the covings may be brought into their proper position with respect to the back, — I should not alter the width of this opening, but should accommodate the covings to this width, by increasing their breadth, and increasing the angle they make with the back of the fireplace; but if the opening of the fireplace should be more than three inches too wide, I should reduce it to the proper width by slips of stone, or by bricks and mortar.

When the width of the opening of the fireplace in front is very great compared with the depth of the fireplace, and with the width of the back, the covings in that case being very wide and consequently very oblique, and the fireplace very shallow, any sudden motion of the air in front of the fireplace (that motion, for instance, which would be occasioned by the clothes of a woman passing hastily before the fire, and very near it) would be apt to cause eddies in the air, *within the opening of the fireplace*, by which puffs of smoke might easily be brought into the room.

Should the opening of the chimney be too narrow, which however will seldom be found to be the case, it will, in general, be advisable to let it remain as it is, and to accommodate the covings to it, rather than to attempt to increase its width, which would be attended with a good deal of trouble, and probably a considerable expense.

From all that has been said, it is evident that the points of the greatest importance, and which ought most particularly to be attended to in altering fireplaces upon the principles here recommended, are, the bringing forward the back to its proper place, and making it of a proper width. But it is time that I should mention another matter upon which it is probable that my reader is already impatient to receive information. Provision must be made for the passage of the chimney-sweeper up the chimney. This may easily be done in the following manner. In building up the new back of the fireplace, — when this wall (which need never be more than the width of a single brick in thickness) is brought up so high that there remains no more than about ten or eleven inches between what is then the top of it and the inside of the mantle, or lower extremity of the breast of the chimney, — an opening, or doorway, eleven or twelve inches wide, must be begun in the middle of the back, and continued quite to the top of it, which, according to the height to which it will commonly be necessary to carry up the back, will make the opening about twelve or fourteen inches high; which will be quite sufficient to allow the chimney-sweeper to pass. When the fireplace is finished, this doorway is to be closed by a few bricks, by a tile, or a fit piece of stone, placed in it, dry or without mortar, and confined in its place by means of a rabbet made for that purpose in the brick-work. As often as the chimney is swept, the chimney-sweeper takes down this temporary wall, which is very easily done, and when he has finished his work he puts it again into its place. The annexed drawing (Plate X., Fig. 6) will give a clear idea of this contrivance; and the experience I have had of it has proved that it answers perfectly well the purpose for which it is designed.

I observed above, that the new back, which it will always be found necessary to build in order to bring the fire sufficiently forward, in altering a chimney constructed on the common principles, need never be thicker than the width of a common brick. I may say the same of the thickness necessary to be given to the new sides, or covings, of the chimney; or if the new back and covings are constructed of stone, one inch and three quarters, or two inches, in thickness, will be sufficient. Care should be taken in building up these new walls to unite the back to the covings in a solid manner.

Whether the new back and covings are constructed of stone, or built of bricks, the space between them and the old back and covings of the chimney ought to be filled up, to give greater solidity to the structure. This may be done with loose rubbish, or pieces of broken bricks, or stones, provided the work be strengthened by a few layers or courses of bricks laid in mortar; but it will be indispensably necessary to finish the work, where these new walls end, that is to say, at the top of the throat of the chimney, where it ends abruptly in the open canal of the chimney, by a horizontal course of bricks well secured with mortar. This course of bricks will be upon a level with the top of the doorway left for the chimney-sweeper.

From these descriptions it is clear, that, where the throat of the chimney has an end, that is to say, where it enters into the lower part of the open canal of the chimney, there the three walls which form the two covings and the back of the fireplace all end abruptly. It is of much importance that they should end in this manner; for were they to be sloped outward and raised in such a manner as to swell out the upper extremity of the throat of the chimney in the form of a trumpet, and increase it by degrees to the size of the canal of the chimney, this manner of uniting the lower extremity of the canal of the chimney with the throat would tend to assist the winds which may attempt to blow down the chimney, in forcing their way through the throat, and throwing the smoke backward into the room; but when the throat of the chimney ends abruptly, and the ends of the new walls form a flat horizontal surface, it will be much more difficult for any wind from above to find and force its way through the narrow passage of the throat of the chimney.

As the two walls which form the new covings of the chimney are not parallel to each other, but inclined, presenting an oblique surface towards the front of the chimney, and as they are built perfectly upright and quite flat, from the hearth to the top of the throat, where they end, it is evident that a horizontal section of the throat will not be an oblong square; but its deviation from that form is a matter of no consequence; and no attempts should ever be made, by twisting the covings above, where they approach the breast of the chimney, to bring it to that form. All twists, bends, prominences, excavations, and other irregularities of form, in the covings of a chimney, never fail to produce eddies in the current of air which is continually passing into and through an open fireplace in which a fire is burning; and all such eddies disturb either the fire, or the ascending current of smoke, or both, and not unfrequently cause the smoke to be thrown back into the room. Hence it appears, that the covings of chimneys should never be made circular, or in the form of any other curve, but always quite flat.

For the same reason, that is to say, to prevent eddies, the breast of the chimney, which forms that side of the throat that is in front, or nearest to the room, should be neatly cleaned off, and its surface made quite regular and smooth.

This may easily be done by covering it with a coat of plaster, which may be made thicker or thinner in different parts as may be necessary in order to bring the breast of the chimney to be of the proper form.

With regard to the form of the breast of a chimney, this is a matter of very great importance, and which ought always to be particularly attended to. The worst form it can have is that of a vertical plane, or upright flat; and next to this, the worst form is an inclined plane. Both these forms cause the current of warm air from the room, which will, in spite of every precaution, sometimes find its way into the chimney, to cross upon the current of smoke, which rises from the fire, in a manner most likely to embarrass it in its ascent, and drive it back. The inclined plane which is formed by a flat register placed in the throat of a chimney produces the same effects; and this is one reason, among many others, which have induced me to disapprove of register stoves.

The current of air, which, passing under the mantle, gets into the chimney, should be made *gradually to bend its course upwards*, by which means it will unite *quietly* with the ascending current of smoke, and will be less likely to check it, or force it back into the room. Now this may be effected with the greatest ease and certainty, merely by *rounding off* the breast of the chimney or back part of the mantle, instead of leaving it flat, or full of holes and corners; and this, of course, ought always to be done.

I have hitherto given no precise directions in regard to the height to which the new back and covings ought to be carried. This will depend not only on the height of the mantle, but also, and more especially, on the height of the breast of the chimney, or of that part of the chimney where the breast ends and the upright canal begins. The back and covings must rise a few inches, 5 or 6 for instance, higher than this part, otherwise the throat of the chimney will not be properly formed; but I know of no advantages that would be gained by carrying them up still higher.

I mentioned above, that the space between the walls which form the new back and covings, and the old back and sides of the fireplace, should be filled up; but this must not be understood to apply to the space between the wall of dry bricks, or the tile which closes the passage for the chimney-sweeper, and the old back of the chimney; for that space must be left void, otherwise, though this tile (which at most will not be more than two inches in thickness) were taken away, there would not be room sufficient for him to pass.

In forming this doorway, the best method of proceeding is to place the tile or flat piece of stone destined for closing it in its proper place, and to build round it, or rather by the sides of it, taking care not to bring any mortar near it, in order that it may be easily removed when the doorway is finished. With regard to the rabbet which should be made in the doorway to receive it and fix it more firmly in its place, this may either be formed at the same time when the doorway is built, or it may be made after it is finished, by attaching to its bottom and sides, with strong mortar, pieces of thin roof tiles. Such as are about half an inch in thickness will be best for this use; if they are thicker, they will diminish too much the opening of the doorway, and will likewise be more liable to be torn away by the chimney-sweeper in passing up and down the chimney.

It will hardly be necessary for me to add, that the tile, or flat stone, or wall of dry bricks, which is used for closing up this doorway, must be of sufficient height to reach quite up to a level with the top of the walls which form the new back and covings of the chimneys.

I ought, perhaps, to apologize for having been so very particular in these descriptions and explanations; but it must be remembered that this chapter is written principally for the information of those who, having had few opportunities of employing their attention in abstruse philosophical researches, are not sufficiently practised in these intricate investigations to seize, with facility, new ideas, and consequently, that I have frequently been obliged to *labour* to make myself understood.

I have only to express my wishes that my reader may not be more *fatigued* with this labour than I have been; for we shall then most certainly be satisfied with each other. But to return once more to the charge.

There is one important circumstance respecting chimney fireplaces destined for burning coals, which still remains to be further examined; and that is the grate.

Although there are few grates that may not be used in chimneys constructed or altered upon the principles here recommended, yet they are not, by any means, all equally well adapted for that purpose. Those whose construction is the most simple, and which, of course, are the cheapest, are beyond comparison the best, *on all accounts*. Nothing being wanted in these chimneys but merely a grate for containing the coals, and in which they will burn with a clear fire, and all additional apparatus being not only useless, but very pernicious, all complicated and expensive grates should be laid aside, and such as are more simple substituted in the room of them. And in the choice of a grate, as in everything else, *beauty* and *elegance* may easily be united with the *most perfect simplicity*. Indeed, they are incompatible with everything else.

In placing the grate, the thing principally to be attended to is to make the back of it coincide with the back of the fireplace; but as many of the grates now in common use will be found to be too large, when the fireplaces are altered and improved, it will be necessary to diminish their capacities by filling them up at the back and sides with pieces of fire-stone. When this is done, it is the front of the flat piece of fire-stone which is made to form a new back to the grate, which must be made to coincide with and mark part of the back of the fireplace. But in diminishing the capacities of grates with pieces of fire-stone, care must be taken not to make them *too narrow*.

The proper width for grates destined for rooms of a middling size will be from 6 to 8 inches, and their length may be diminished more or less, according as the room is heated with more or less difficulty, or as the weather is more or less severe. But where the width of a grate is not more than 5 inches, it will be very difficult to prevent the fire from going out.

It goes out for the same reason that a live coal from the grate that falls upon the hearth soon ceases to be red-hot; it is cooled by the surrounding cold air of the atmosphere. The knowledge of the cause which produces this effect is important, as it indicates the means which may be used for preventing it. But of this subject I shall treat more fully hereafter.

It frequently happens that the iron backs of grates are not vertical, or upright, but inclined backwards. When these grates are so much too wide as to render it necessary to fill them up behind with fire-stone, the inclination of the back will be of little consequence; for by making the piece of stone with which the width of the grate is to be diminished in the form of a wedge, or thicker above than below, the front of this stone, which in effect will become the back of the grate, may be made perfectly vertical, and, the iron back of the grate being hid in the solid work of the back of the fireplace, will produce no effect whatever; but, if the grate be already so narrow as not to admit of any diminution of its width, in that case it will be best to take away the iron back of the grate entirely, and, fixing the grate firmly in the brickwork, cause the back of the fireplace to serve as a back to the grate. This I have very frequently done, and have always found it to answer perfectly well.

Where it is necessary that the fire in a grate should be very small, it will be best, in reducing the grate with fire-stone, to bring its cavity, destined for containing the fuel, to the form of one half of a hollow hemisphere; the two semicircular openings being one above, to receive the coals, and the other in front, or towards the bars of the grate; for when the coals are burned in such a confined space, and surrounded on all sides, except in the front and above, by fire-stone (a substance peculiarly well adapted for confining heat), the heat of the fire will be concentrated, and, the cold air of the atmosphere being kept at a distance, a much smaller quantity of coals will burn than could possibly be made to burn in a grate where they would be more exposed to be cooled by the surrounding air, or to have their heat carried off by being in contact with iron, or with any other substance through which heat passes with greater facility than through fire-stone.

Being persuaded that, if the improvements in chimney fireplaces here recommended should be generally adopted (which I cannot help flattering myself will be the case), it will become necessary to reduce, very considerably, the sizes of grates, I was desirous of showing how this may, with the greatest safety and facility, be done.

Where grates, which are designed for rooms of a middling size, are longer than 14 or 15 inches, it will always be best, not merely to diminish their lengths, by filling them up at their two ends with fire-stone, but, forming the back of the chimney of a proper width, without paying any regard to the length of the grate, to carry the covings through the two ends of the grate in such a manner as to conceal them, or at least to conceal the back corners of them in the walls of the covings.

I cannot help flattering myself that the directions here given in regard to the alterations which it may be necessary to make in fireplaces, in order to introduce the improvements proposed, will be found to be so perfectly plain and intelligible that no one who reads them will be at any loss respecting the manner in which the work is to be performed; but as order and arrangement tend much to facilitate all mechanical operations, I shall here give a few short directions respecting the manner of *laying out the work*, which may be found useful, and particularly to gentlemen who may undertake to be their own architects, in ordering and directing the alterations to be made for the improvement of their fireplaces.

Directions for laying out the Work.

If there be a grate in the chimney which is to be altered, it will always be best to take it away; and when this is done, the rubbish must be removed, and the hearth swept perfectly clean.

Suppose the annexed figure (Plate VIII., Fig. 1) to represent the ground plan of such a fireplace; A B being the opening of it in front, A C and B D the two sides or covings, and C D the back.

Figure 2 shows the elevation of this fireplace.

First, draw a straight line, with chalk or with a lead-pencil, upon the hearth, from one jamb to the other, even with the front of the jambs. The dotted line A B (Plate IX., Fig. 3) may represent this line.

From the middle C of this line (A B) another line *c d* is to be drawn perpendicular to it, across the hearth, to the middle *d* of the back of the chimney.

A person must now stand upright in the chimney, with his back to the back of the chimney, and hold a plumb-line to the middle of the upper part of the breast of the chimney (Plate X., Fig. 5, *d*), or where the canal of the chimney begins to rise perpendicularly; taking care to place the line above in such a manner that the plumb may fall on the line *c d*, drawn on the hearth from the middle of the opening of the chimney in front to the middle of the back, and an assistant must mark the precise place *e*, on that line where the plumb falls.

This being done, and the person in the chimney having quitted his station, 4 inches are to be set off on the line *c d*, from *e* towards *d*; and the point *f*, where these 4 inches end (which must be marked with chalk, or with a pencil), will show how far the new back is to be brought forward.

Through *f* draw the line *g h*, parallel to the line A B, and this line *g h* will show the direction of the new back, or the ground line upon which it is to be built.

The line *c f* will show the depth of the new fireplace; and if it should happen that *c f* is equal to about *one third* of the line A B, and if the grate can be accommodated to the fireplace instead of its being necessary to accommodate the fireplace to the grate, in that case half the length of the line *c f* is to be set off from *f* on the line *g f h*, on one side to *k*, and on the other to *i*, and the line *i k* will show the ground line of the forepart of the back of the chimney.

In all cases where the width of the opening of the fireplace in front (A B) happens to be not greater, or not more than two or three inches greater, than *three times* the width of the new back of the chimney (*i k*), this opening may be left, and lines drawn from *i* to A and from *k* to B will show the width and position of the front of the new covings; but when the opening of the fireplace in front is still wider, it must be reduced, which is to be done in the following manner.

From *c*, the middle of the line A B, *c a* and *c b* must be set off equal to the width of the back (*i k*), added to half its width (*f i*), and lines drawn from *i* to *a* and from *k* to *b* will show the ground plan of the fronts of the new covings.

When this is done, nothing more will be necessary than to build up the back and covings, and, if the fireplace is designed for burning coals, to fix the grate in its proper place, according to the directions already given.

When the width of the fireplace is reduced, the edges of the covings *a* A and *b* B are to make a finish with the front of the jambs. And in general it will be best, not only for the sake of the appearance of the chimney, but for other reasons also, to lower the height of the opening of the fireplace, whenever its width in front is diminished.

Fig. 4 (Plate IX.) shows a front view of the chimney after it has been altered according to the directions here given. By comparing it with Fig. 2 (which shows a front view of the same chimney before it was altered), the manner in which the opening of the fireplace in front is diminished may be seen. In Fig. 4, the under part of the doorway by which the chimney-sweeper gets up the chimney is represented by white dotted lines. The doorway is represented closed.

I shall finish this chapter with some general observations relative to the subject under consideration; with directions how to proceed where such local circumstances exist as render modifications of the general plan indispensably necessary.

Whether a chimney be designed for burning wood upon the hearth, or wood or coals in a grate, the form of the fireplace is, in my opinion, most perfect when *the width of the back* is equal to *the depth of the fireplace*, and the opening of the fireplace in front equal to *three times* the width of the back, or, which is the same thing, to *three times the depth of the fireplace*.

But if the chimney be designed for burning wood upon the hearth, upon handirons, or dogs, as they are called, it will sometimes be necessary to accommodate the width of the back to the length of the wood; and when this is the case, the covings must be accommodated to the width of the back and the opening of the chimney in front.

When the wall of the chimney in front, measured from the upper part of the breast of the chimney to the front of the mantle, is very thin, it may happen, and especially in chimneys designed for burning wood upon the hearth, or upon dogs, that the depth of the chimney, determining according to the directions here given, may be too small.

Thus, for example, supposing the wall of the chimney in front, from the upper part of the breast of the chimney to the front of the mantle, to be only 4 inches (which is sometimes the case, particularly in rooms situated near the top of a house), in this case, if we take 4 inches for the width of the throat, this will give 8 inches only for the depth of the fireplace, which would be too little, even were coals to be burned instead of wood. In this case I should increase the depth of the fireplace at the hearth to 12 or 13 inches, and should build the back perpendicular to the height of the top of the burning fuel (whether it be wood burned upon the hearth, or coals in a grate), and then, sloping the back by a gentle inclination forward, bring it to its proper place, that is to say, *perpendicularly under the back part of the throat of the chimney.* This slope (which will bring the back forward 4 or 5 inches, or just as much as the depth of the fireplace is increased), though it ought not to be too abrupt, yet it ought to be quite finished at the height of 8 or 10 inches above the fire, otherwise it may perhaps cause the chimney to smoke; but when it is very near the fire, the heat of the fire will enable the current of rising smoke to overcome the obstacle which this slope will oppose to its ascent, which it could not do so easily were the slope situated at a greater distance from the burning fuel.*

Figs. 7, 8, and 9 (Plate X.) show a plan, elevation,

* Having been obliged to carry backward the fireplace in the manner here described, in order to accommodate it to a chimney whose walls in front were remarkably thin, I was surprised to find, upon lighting the fire, that it appeared to give out more heat into the room than any fireplace I had ever constructed. This effect was quite unexpected; but the cause of it was too obvious not to be immediately discovered. The flame rising from the fire broke against the part of the back which sloped forward over the fire, and this part of the back being soon very much heated, and in consequence of its being very hot, (and when the fire burned bright it was frequently quite red-hot,) it threw off into the room a great deal of radiant heat. It is not possible that this oblique surface (the slope of the back of the fireplace) could have been heated red-hot *merely* by the radiant heat projected by the burning fuel; for other parts of the fireplace nearer the fire, and better situated for receiving radiant heat, were never found to be so much heated; and hence it appears that the combined heat in the current of smoke and hot vapour which rises from an open fire *may be*, at least *in part*, stopped in its passage up the chimney, changed into radiant heat, and afterwards thrown into the room. This opens a new and very interesting field for experiment, and bids fair to lead to important improvements in the construction of fireplaces. I have of late been much engaged in these investigations, and am now actually employed daily in making a variety of experiments with grates and fireplaces, upon different constructions, in the room I inhabit in the Royal Hotel in Pall Mall; and Mr. Hopkins, of Greek Street, Soho, Ironmonger to his Majesty, and Mrs. Hempel, at her Pottery at Chelsea, are both at work in their different lines of business, under my direction, in the construction of fireplaces upon a principle entirely new, and which, I flatter myself, will be found to be not only elegant and convenient, but very economical. But as I mean soon to publish a particular account of these fireplaces, with drawings and ample directions for constructing them, I shall not enlarge further on the subject in this place. It may, however, not be amiss just to mention here, that these new invented fireplaces not being fixed to the walls of the chimney, but merely set down upon the hearth, may be used in any open chimney; and that chimneys altered or constructed on the principles here recommended are particularly well adapted for receiving them.

The public in general, and more particularly those tradesmen and manufacturers whom it may concern, are requested to observe, that, as the author does not intend to take out himself, or to suffer others to take out, any patent for any invention of his which may be of public utility, all persons are at full liberty to imitate them, and vend them, for their own emolument, when and where and in any way they may think proper; and those who may wish for any further information respecting any of those inventions or improvements will receive (*gratis*) all the information they can require by applying to the author, who will take pleasure in giving them every assistance in his power.

and section of a fireplace constructed or altered upon this principle. The wall of the chimney in front at *a* (Fig. 9) being only 4 inches thick, 4 inches more added to it for the width of the throat would have left the depth of the fireplace measured upon the hearth *b c* only 8 inches, which would have been too little; a niche *c* and *e* was therefore made in the new back of the fireplace for receiving the grate, which niche was 6 inches deep in the centre of it, below 13 inches wide (or equal in width to the grate), and 23 inches high; finishing above with a semicircular arch, which, in its highest part, rose 7 inches above the upper part of the grate. The doorway for the chimney-sweeper, which begins just above the top of the niche, may be seen distinctly in both the Figs. 8 and 9. The space marked *g* (Fig. 9) behind this doorway may either be filled with loose bricks, or may be left void. The manner in which the piece of stone (*f*, Fig. 9) which is put under the mantle of the chimney to reduce the height of the opening of the fireplace, is rounded off on the inside, in order to give a fair run to the column of smoke in its ascent through the throat of the chimney, is clearly expressed in this figure.

The plan (Fig. 7) and elevation (Fig. 8) show how much the width of the opening of the fireplace in front is diminished, and how the covings in the new fireplace are formed.

A perfect idea of the form and dimension of the fireplace in its original state, as also after its alteration, may be had by a careful inspection of these figures.

I have added the drawing (Fig. 10, Plate XI.) merely to show how a fault, which I have found workmen in general whom I have employed in altering fireplaces are very apt to commit, is to be avoided. In chimneys like that represented in this figure, where the jambs A and B project far into the room, and where the front edge of the marble slab *o*, which forms the coving, does not come so far forward as the front of the jambs, the workmen in constructing the new covings are very apt to place them, not in the line *c* A, which they ought to do, but in the line *c o*, which is a great fault. The covings of a chimney should never range *behind* the front of the jambs, however those jambs may project into the room; but it is not absolutely necessary that the covings should *make a finish* with the internal front corners of the jambs, or that they should be continued from the back *c* quite to the front of the jambs at A. They may finish in front at *a* and *b*, and small corners, A, *o*, *a*, may be left for placing the shovels, tongs, etc.

Were the new coving to range with the front edge of the old coving *o*, the obliquity of the new coving would commonly be too great; or the angle *d c o* would exceed 135 degrees, *which it never should do*, or at least never by more than a very few degrees.

No inconvenience of any importance will arise from making the obliquity of the covings *less* than what is here recommended; but many cannot fail to be produced by making it much greater; and as I know from experience that workmen are very apt to do this, I have thought it necessary to warn them particularly against it.

Fig. 11 shows how the width and obliquity of the covings of a chimney are to be accommodated to the width of the back, and to the opening in front and depth of the fireplace, where the width of the opening of the fireplace is less than three times the width of the new back.

As all those who may be employed in altering chimneys may not perhaps know how to set off an angle of any certain number of degrees, or may not have at hand the instruments necessary for doing it, I shall here show how an instrument may be made which will be found to be very useful in laying out the work for the bricklayers.

Upon a board about 18 inches wide and 4 feet long, or upon the floor or a table, draw three equal squares (A, B, C, Fig. 12, Plate XIII.), of about 12 or 14 inches each side, placed in a straight line, and touching each other. From the back corner *c* of the centre square B draw a diagonal line across the square A, to its outward front corner *f*, and the adjoining angle formed by the lines *d c* and *c f* will be equal to 135 degrees, the angle which the plane of the back of a chimney fireplace ought to make with the plane of its covings. And a bevel *m n* being made to this angle with thin slips of hard wood, this little instrument will be found to be very useful in marking out on the hearth, with chalk, the plans of the walls which are to form the covings of fireplaces.

As chimneys which are apt to smoke will require the covings to be placed less obliquely in respect to the back than others which have not that defect, it would be convenient to be provided with several bevels, — three or four, for instance, forming different angles. That already described, which may be called No. 1, will measure the obliquity of the covings when the fireplace can be made of the most perfect form; another, No. 2, may be made to a smaller angle, *d c e;* and another, No. 3, for chimneys which are very apt to smoke, at the still smaller angle *d c i.* Or a bevel may be so contrived, by means of a joint, and an arch, properly graduated, as to serve for all the different degrees of obliquity which it may ever be necessary to give to the covings of fire-places.

Another point of much importance, and particularly in chimneys which are apt to smoke, is to form the throat of the chimney properly, by carrying up the back and covings to a proper height.

This workmen are apt to neglect to do, probably on account of the difficulty they find in working where the opening of the canal of the chimney is so much reduced. But it is absolutely necessary that these walls should be carried up 5 or 6 inches at least above the upper part of the breast of the chimney, or to that point where the wall which forms the front of the throat begins to rise perpendicularly. If the workman has intelligence enough to avail himself of the opening which is formed in the back of the fireplace to give a passage to the chimney-sweeper, he will find little difficulty in finishing his work in a proper manner.

In placing the plumb-line against the breast of the chimney, in order to ascertain how far the new back is to be brought forward, great care must be taken to place it at the very top of the breast, where the canal of the chimney *begins to rise perpendicularly;* otherwise, when the plumb-line is placed too low, or against the slope of the breast, when the new back comes to be raised to its proper height, the throat of the chimney will be found to be too narrow.

Sometimes, and indeed very often, the top of the breast of a chimney lies very high, or far above the fire (see Figs. 13 and 14, Plate XIII., where *d* shows the top of the breast of the chimney); when this is the case, it must be brought lower, otherwise the chimney will be very apt to smoke. So much has been said, in the first chapter of this essay, of the advantages to be derived from bringing the throat of a chimney near to the burning fuel, that I do not think it necessary to enlarge on them in this place, taking it for granted that the utility and necessity of that arrangement have already been made sufficiently evident; but a few directions for workmen, to show them how the breast (and consequently the throat) of a chimney can most readily be lowered, may not be superfluous.

Where the too great height of the breast of a chimney is owing to the great height of the mantle (see Fig. 13), or, which is the same thing, of the opening of the fireplace in front, which will commonly be found to be the case, the only remedy for the evil will be to bring down the mantle lower; or, rather, to make the opening of the fireplace in front lower, by throwing across the top of this opening, from one jamb to the other, and immediately under the mantle, a very flat arch, a wall of bricks and mortar, supported on straight bars of iron, or a piece of stone (*h*, Fig. 13). When this is done, the slope of the old throat of the chimney, or of the back side of the mantle, is to be filled up with plaster, so as to form one continued flat, vertical, or upright plane surface with the lower part of the wall of the canal of the chimney, and a new breast is to be formed lower down, care being taken to round it off properly, and make it finish at the lower surface of the new wall built under the mantle; which wall forms, in fact, a new mantle.

The annexed drawing (Fig. 13), which represents the section of a chimney in which the breast has been lowered according to the method here described, will show these various alterations in a clear and satisfactory manner. In this figure, as well as in most of the others in this essay, the old walls are distinguished from the new ones by the manner in which they are shaded; the old walls being shaded by diagonal lines, and the new ones by vertical lines. The additions, which are formed of plaster, are shaded by dots instead of lines.

Where the too great height of the breast of a chimney is occasioned, not by the height of the mantle, but by the too great width of the breast, in that case (which, however, will seldom be found to occur), this defect may be remedied by covering the lower part of the breast with a thick coating of plaster, supported, if necessary, by nails or studs driven into the wall which forms the breast, and properly rounded off at the lower part of the mantle. (See Fig. 14.)

CHAPTER III.

Of the Cause of the Ascent of Smoke. — Illustration of the Subject by familiar Comparisons and Experiments. — Of Chimneys which affect and cause each other to smoke. — Of Chimneys which smoke from Want of Air. — Of the Eddies of Wind which sometimes blow down Chimneys, and cause them to smoke.

THOUGH it was my wish to avoid all abstruse philosophical investigations in this essay, yet I feel that it is necessary to say a few words upon a subject generally considered as difficult to be explained, which is too intimately connected with the matter under consideration to be passed over in silence. A knowledge of the cause of the ascent of smoke being indispensably necessary to those who engage in the improvement of fireplaces, or who are desirous of forming just ideas relative to the operations of fire and the management of heat, I shall devote a few pages to the investigation of that curious and interesting subject. And as many of those who may derive advantage from these inquiries are not much accustomed to philosophical disquisitions, and would not readily comprehend either the language or the diagrams commonly used by scientific writers to explain the phenomena in question, I shall take pains to express myself in the most familiar manner, and to use such comparisons for illustration as may easily be understood.

If small leaden bullets, or large goose-shot, be mixed with peas, and the whole well shaken in a bushel, the shot will separate from the peas, and will take its place at the bottom of the bushel; forcing, by its greater weight, the peas, which are lighter, to move upwards, contrary to their natural tendency, and take their places above.

If water and linseed oil, which is lighter than water, be mixed in a vessel by shaking them together, upon suffering this mixture to remain quiet the water will descend and occupy the bottom of the vessel, and the oil, being forced out of its place by the greater pressure downwards of the heavier liquid, will be obliged to rise and swim on the surface of the water.

If a bottle containing linseed oil be plunged in water with its mouth upwards, and open, the oil will ascend out of the bottle, and, passing upwards through the mass of water, in a continued stream, will spread itself over its surface.

In like manner, when two fluids of any kind, of different densities, come into contact, or are mixed with each other, that which is the lightest will be forced upwards by that which is the heaviest.

And as heat rarefies all bodies, fluids as well as solids, air as well as water or mercury, it follows that two portions of the same fluid, at different temperatures, being brought into contact with each other, that portion which is the hottest, being more rarified, or specifically *lighter* than that which is colder, must be forced upwards by this last. And this is what always happens in fact.

When hot water and cold water are mixed, the hottest part of the mixture will be found to be at the surface above; and when cold air is admitted into a warmed room, it will always be found to take its place at the bottom of the room, the warmer air being in part expelled, and in part forced upwards to the top of the room.

Both air and water being transparent and colourless fluids, their internal motions are not easily discovered by the sight; and when these motions are very slow, they make no impression whatever on any of our senses, consequently they cannot be detected by us without the aid of some mechanical contrivance. But where we have reason to think that those motions exist, means should be sought, and may often be found, for rendering them perceptible.

If a bottle containing hot water tinged with logwood, or any other colouring drug, be immersed, with its mouth open, and upwards, into a deep glass jar filled with cold water, the ascent of the hot water from the bottle through the mass of cold water will be perfectly visible through the glass. Now, nothing can be more evident than that both of these fluids are forced or *pushed*, and not *drawn* upwards. Smoke is frequently said to be drawn up the chimney, and that a chimney draws well or ill; but these are careless expressions, and lead to very erroneous ideas respecting the cause of the ascent of smoke, and consequently tend to prevent the progress of improvements in the management of fires. The experiment just mentioned with the coloured water is very striking and beautiful, and it is well calculated to give a just idea of the cause of the ascent of smoke. The cold water in the jar, which, in consequence of its superior weight or density, forces the heated and rarefied water in the bottle to give place to it, and to move upwards out of its way, may represent the cold air of the atmosphere, while the rising column of coloured water will represent the column of smoke which ascends from a fire.

If smoke required a chimney to *draw* it upwards, how happens it that smoke rises from a fire which is made in the open air, where there is no chimney?

If a tube, open at both ends, and of such a length that its upper end be below the surface of the cold water in the jar, be held vertically over the mouth of the bottle which contains the hot coloured water, the hot water will rise up through it, just as smoke rises in a chimney.

If the tube be previously heated before it is plunged into the cold water, the ascent of the hot coloured water will be facilitated and accelerated, in like manner as smoke is known to rise with greater facility in a chimney which is hot, than in one in which no fire has been made for a long time. But in neither of these cases can it, with any propriety, be said that the hot water is *drawn* up the tube. The hotter the water in the bottle is, and the colder that in the jar, the greater will be the velocity with which the hot water will be forced up through the tube; and the same holds of the ascent of hot smoke in a chimney. When the fire is intense, and the weather very cold, the ascent of the smoke is very rapid; and under such circumstances chimneys seldom smoke.

As the cold water of the jar immediately surrounding the bottle which contains the hot water will be heated by the bottle, while the other parts of the water in the jar will remain cold, this water so heated, becoming specifically lighter than that which surrounds it, will be forced upwards; and if it finds its way into the tube will rise up through it with the coloured hot water. The warmed air of a room heated by an open chimney fireplace has always a tendency to rise (if I may use that inaccurate expression), and, finding its way into the chimney, frequently goes off with the smoke.

What has been said will, I flatter myself, be sufficient to explain and illustrate, in a clear and satisfactory manner, the cause of the ascent of smoke; and just ideas upon that subject are absolutely necessary in order to judge, with certainty, of the merit of any scheme proposed for the improvement of fireplaces, or to take effectual measures, in all cases, for curing smoking chimneys. For, though the perpetual changes and alterations which are produced by accident, whim, and caprice, do sometimes lead to useful discoveries, yet the progress of improvement under such guidance must be exceedingly slow, fluctuating, and uncertain.

As to the causes of the smoking of chimneys, they are very numerous and various; but as a general idea of them may be acquired from what has already been said upon that subject in various parts of this essay, and as they may, in all cases (a very few only excepted), be completely remedied by making the alterations in fireplaces here pointed out, I do not think it necessary to enumerate them all in this place, or to enter into those long details and investigations which would be required to show the precise manner in which each of them operates, either alone or in conjunction with others.

There is, however, one cause of smoking chimneys which I think it is necessary to mention more particularly. In modern-built houses, where the doors and windows are generally made to close with such accuracy that no crevice is left for the passage of the air from without, the chimneys in rooms adjoining to each other, or connected by close passages, are frequently found to affect each other; and this is easy to be accounted for. When there is a fire burning in one of the chimneys, as the air necessary to supply the current up the chimney where the fire burns cannot be had in sufficient quantities from without, through the very small crevices of the doors and windows, the air in the room becomes rarefied, not by heat, but by subtraction of that portion of air which is employed in keeping up the fire, or supporting the combustion of the fuel, and, in consequence of this rarefaction, its elasticity is diminished, and being at last overcome by the pressure of the external air of the atmosphere, this external air rushes into the room by the only passage left for it, namely, by the open chimney of the neighbouring room; and the flow of air into the fireplace, and up the chimney where the fire is burning, being constant, this expense of air is supplied by a continued current down the other chimney.

If an attempt be made to light fires in both chimneys at the same time, it will be found to be very difficult to get the fires to burn, and the rooms will both be filled with smoke.

One of the fires — that which is made in the chimney where the construction of the fireplace is best adapted to facilitate the ascent of the smoke; or, if both fireplaces are on the same construction, that which has the wind most favourable, or in which the fire happens to be soonest kindled — will overcome the other, and cause its smoke to be beat back into the room by the cold air which descends through the chimney. The most obvious remedy in this case is to provide for the supply of fresh air necessary for keeping up the fires by opening a passage for the external air into the room by a shorter road than down one of the chimneys; and when this is done, both chimneys will be found to be effectually cured.

But chimneys so circumstanced may very frequently be prevented from smoking, even without opening any new passage for the external air, merely by diminishing the draught (as it is called) up the chimneys; which can best be done by altering both fireplaces upon the principles recommended and fully explained in the foregoing chapters of this essay.

Should the doors and windows of a room be closed with so much nicety as to leave no crevices by which a supply of air can enter sufficient for maintaining the fire, *after the current of air up the chimney has been diminished as much as possible by diminishing the throat of the fireplace,* in that case there would be no other way of preventing the chimney from smoking but by opening a passage for the admission of fresh air from without; but this, I believe, will very seldom be found to be the case.

A case more frequently to be met with is, where currents of air set down chimneys in consequence of a diminution and rarefaction of the air in a room, occasioned by the doors of the room opening into passages or courts where the air is rarefied by the action of some particular winds. In such cases the evil may be remedied, either by causing the doors in question to close more accurately, or (which will be still more effectual) by giving a supply of air to the passage or court which wants it by some other way.

Where the top of a chimney is commanded by high buildings, by cliffs, or by high grounds, it will frequently happen, in windy weather, that the eddies formed in the atmosphere by these obstacles will blow down the chimney, and beat down the smoke into the room. This, it is true, will be much less likely to happen when the throat of the chimney is contracted and properly formed than when it is left quite open, and the fireplace badly constructed; but as it is *possible* that a chimney may be so much exposed to these eddies in very high winds as to be made to smoke sometimes when the wind blows with violence from a certain quarter, it is necessary to show how the effects of those eddies may be prevented.

Various mechanical contrivances have been imagined for preventing the wind from blowing down chimneys, and many of them have been found to be useful; there are, however, many of these inventions, which, though they prevent the wind from blowing down the chimney, are so ill-contrived on other accounts as to obstruct the ascent of the smoke, and do more harm than good.

Of this description are all those chimney-pots with flat horizontal plates or roofs placed upon supporters just above the opening of the pot; and most of the caps which turn with the wind are not much better. One of the most simple contrivances that can be made use of, and which in most cases will be found to answer the purpose intended as well or better than more complicated machinery, is to cover the top of the chimney with a hollow truncated pyramid or cone, the diameter of which above, or opening for the passage of the smoke, is about 10 or 11 inches. This pyramid, or cone (for either will answer), should be of earthenware or of cast-iron; its perpendicular height may be equal to the diameter of its opening above, and the diameter of its opening below equal to three times its height. It should be placed upon the top of the chimney, and it may be contrived so as to make a handsome finish to the brick-work. Where several flues come out near each other, or in the same stack of chimneys, the form of a pyramid will be better than that of a cone for these covers.

The intention of this contrivance is, that the winds and eddies which strike against the oblique surface of these covers may be reflected upwards, instead of blowing down the chimney. The invention is by no means new, but it has not hitherto been often put in practice. As often as I have seen it tried, it has been found to be of use; I cannot say, however, that I was ever obliged to have recourse to it, or to any similar contrivance; and if I forbear to enlarge upon the subject of these inventions, it is because I am persuaded that when chimneys are properly constructed *in the neighbourhood of the fireplace,* little more will be necessary to be done at the top of the chimney than to leave it open.

I cannot conclude this essay without again recommending, in the strongest manner, a careful attention to the management of fires in open chimneys; for not only the quantity of heat produced in the combustion of fuel depends much on the manner in which the fire is managed, but even of the heat actually generated a very small part only will be saved, or usefully employed, when the fire is made in a careless and slovenly manner.

In lighting a coal fire, more wood should be employed than is commonly used, and fewer coals; and as soon as the fire burns bright, and the coals are well lighted, and *not before*, more coals should be added to increase the fire to its proper size.*

The enormous waste of fuel in London may be estimated by the vast dark cloud which continually hangs over this great metropolis, and frequently overshadows

* *Kindling-balls*, composed of equal parts of coal, charcoal, and clay, the two former reduced to a fine powder, well mixed and kneaded together with the clay moistened with water, and then formed into balls of the size of hens' eggs, and thoroughly dried, might be used with great advantage instead of wood for kindling fires. These *kindling-balls* may be made so inflammable as to take fire in an instant, and with the smallest spark, by dipping them in a strong solution of nitre and then drying them again; and they would neither be expensive nor liable to be spoiled by long keeping. Perhaps a quantity of pure charcoal, reduced to a very fine powder and mixed with the solution of nitre in which they are dipped, would render them still more inflammable.

I have often wondered that no attempts should have been made to improve the fires which are made in the open chimneys of elegant apartments, by preparing the fuel; for nothing surely was ever more dirty, inelegant, and disgusting than a common coal fire.

Fire-balls, of the size of goose-eggs, composed of coal and charcoal in powder, mixed up with a due proportion of wet clay, and well dried, would make a much more cleanly, and in all respects a pleasanter, fire than can be made with crude coals; and I believe would not be more expensive fuel. In Flanders and in several parts of Germany, and particularly in the Duchies of Juliers and Bergen, where coals are used as fuel, the coals are always prepared before they are used, by pounding them to a powder, and mixing them up with an equal weight of clay, and a sufficient quantity of water to form the whole into a mass which is kneaded together and formed into cakes; which cakes are afterwards well dried and kept in a dry place for use. And the whole country, far and wide; for this dense cloud is certainly composed almost entirely of *unconsumed coal*, which, having stolen wings from the innumerable fires of this great city, has escaped by the chimneys, and continues to sail about in the air, till, having lost the heat which gave it volatility, it falls in a dry shower of extremely fine black dust to the ground, obscuring the atmosphere in its descent, and frequently changing the brightest day into more than Egyptian darkness.

I never view from a distance, as I come into town, this black cloud which hangs over London, without wishing to be able to compute the immense number of caldrons of coals of which it is composed; for, could this be ascertained, I am persuaded so striking a fact would awaken the curiosity and excite the astonishment of all ranks of the inhabitants, and *perhaps* turn their

it has been found by long experience, that the expense attending this preparation is amply repaid by the improvement of the fuel. The coals, thus mixed with clay, not only burn longer, but give much more heat than when they are burned in their crude state.

It will doubtless appear extraordinary to those who have not considered the subject with some attention, that the quantity of heat produced in the combustion of any given quantity of coals should be increased by mixing the coals with clay, which is certainly an incombustible body; but the phenomenon may, I think, be explained in a satisfactory manner.

The heat generated in the combustion of any small particle of coal existing under two distinct forms, namely, in that which is *combined* with the flame and smoke which rise from the fire, and which, if means are not found to stop it, goes off immediately by the chimney and is lost, and the *radiant heat* which is sent off from the fire, in all directions, in right lines; I think it reasonable to conclude, that the particles of clay, which are surrounded on all sides by the flame, arrest a part at least of the combined heat, and prevent its escape; and this combined heat so arrested, heating the clay red-hot, is retained in it, and, being changed by this operation to radiant heat, is afterwards emitted, and may be directed and employed to useful purposes.

In composing *fire-balls*, I think it probable that a certain proportion of chaff — of straw cut very fine, or even of saw-dust — might be employed with great advantage. I wish those who have leisure would turn their thoughts to this subject, for I am persuaded that very important improvements would result from a thorough investigation of it.

minds to an object of economy to which they have hitherto paid little attention.

Conclusion.

Though the saving of fuel which will result from the improvements in the forms of *chimney fireplaces*, here recommended, will be very considerable, yet I hope to be able to show in a future essay that still greater savings may be made, and more important advantages derived, from the introduction of improvements I shall propose in *kitchen fireplaces*.

I hope, likewise, to be able to show in an essay on *cottage fireplaces*, which I am now preparing for publication, that *three quarters*, at least, of the fuel which cottagers now consume in cooking their victuals and in warming their dwellings, may with great ease, and without any expensive apparatus, be saved.

[This paper is printed from the English edition of Rumford's Essays, Vol. I, pp. 305–387.]

EXPLANATION OF THE FIGURES.

PLATE VIII.

FIG. 1.

The plan of a fireplace on the common construction.
A B, the opening of the fireplace in front.
C D, the back of the fireplace.
A C and B D, the covings.
See page 523.

FIG. 2.

This figure shows the elevation, or front view, of a fireplace on the common construction. See page 523.

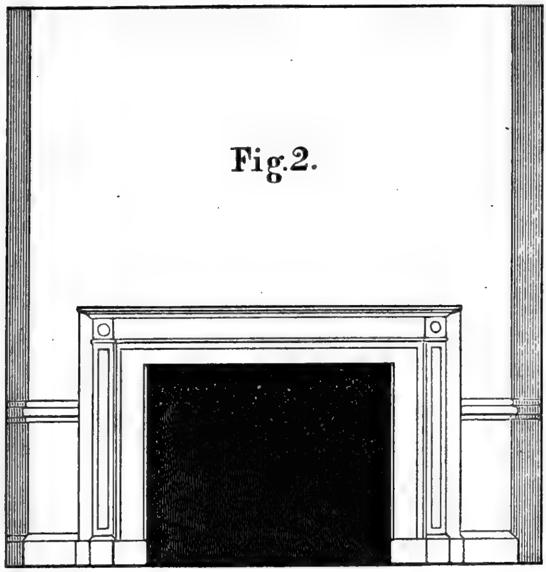


Fig. 2.

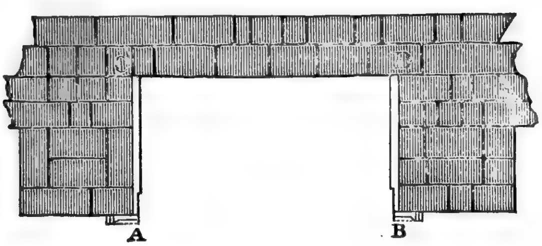


Fig. 1.

Scale of 1 2 3 4 5 *Feet.*

PLATE IX.

Fig. 3.

This figure shows how the fireplace represented by the Fig. 1 is to be altered, in order to its being improved.

A B is the opening in front, C D the back, and A C and B D the covings of the fireplace in its original state.

a b its opening in front, *i k* its back, and *a i* and *b k* its covings after it has been altered; *e* is a point upon the hearth upon which a plumb suspended from the middle of the upper part of the breast of the chimney falls. The situation for the new back is ascertained by taking the line *e f* equal to four inches. The new back and covings are represented as being built of bricks, and the space between these and the old back and covings as being filled up with rubbish. See page 523.

Fig. 4.

This figure represents the elevation or front view of the fireplace (Fig. 3) after it has been altered. The lower part of the doorway left for the chimney-sweeper is shown in this figure by white dotted lines. See page 525.

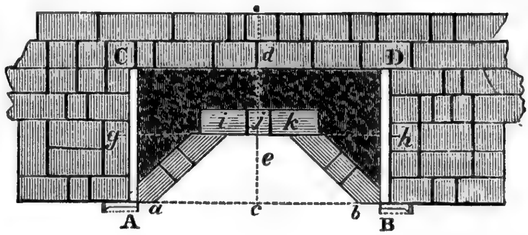


Fig. 3.

Scale of 1 2 3 4 5 Feet.

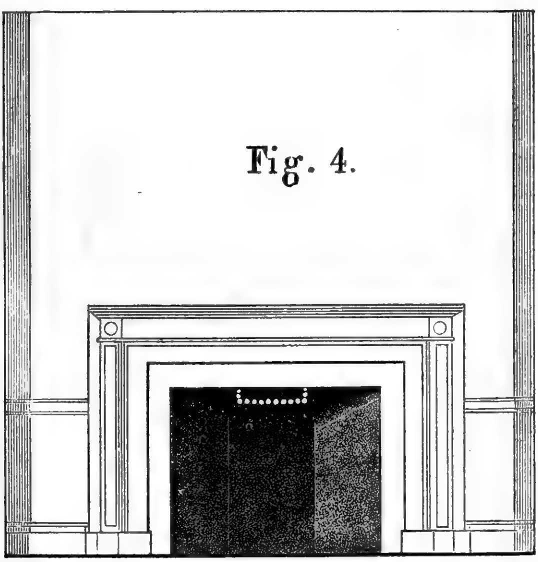


Fig. 4.

PLATE X.

Fig. 5.

This figure shows the section of a chimney fireplace and of a part of the canal of the chimney on the common construction.

a b is the opening in front; *b c* the depth of the fireplace at the hearth; *d* the breast of the chimney.

d e, the throat of the chimney, and *d f*, *g e*, a part of the open canal of the chimney.

Fig. 6.

Shows a section of the same chimney after it has been altered.

k l is the new back of the fireplace; *l i* the tile or stone which closes the doorway for the chimney-sweeper; *d i* the throat of the chimney, narrowed to four inches; *a*, the mantle, and *b*, the new wall made under the mantle, to diminish the height of the opening of the fireplace in front.

N. B.—These two figures are sections of the same chimney which is represented in each of the four preceding figures.

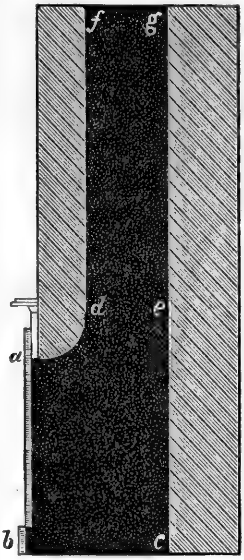


Fig. 5.

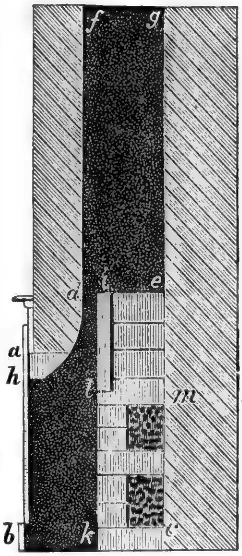


Fig. 6.

PLATE XI.

Fig. 7.

This figure represents the ground plan of a chimney fireplace in which the grate is placed in a niche, and in which the original width A B of the fireplace is considerably diminished.

a b is the opening of the fireplace in front after it has been altered, and *d* is the back of the niche in which the grate is placed. See page 527.

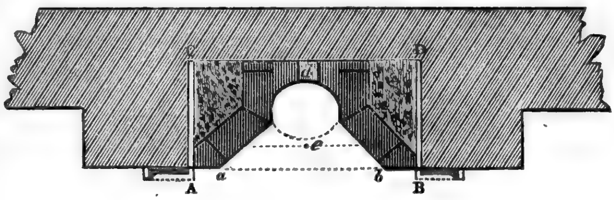
Fig. 8.

Shows a front view of the same fireplace after it has been altered; where may be seen the grate, and the doorway for the chimney-sweeper. See page 527.

Fig. 9.

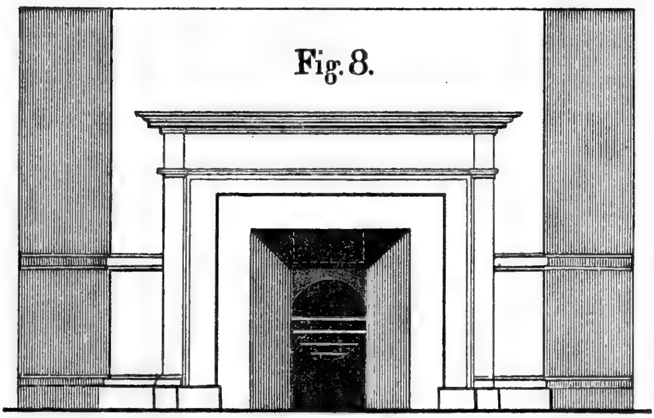
Shows a section of the same fireplace, *c d e* being a section of the niche, *g* the doorway for the chimney-sweeper, closed by a piece of firestone, and *f* the new wall under the mantle, by which the height of the opening of the fireplace in front is diminished. See page 527.

Fig. 7.



Scale of 1 2 3 4 5 Feet.

Fig. 8.



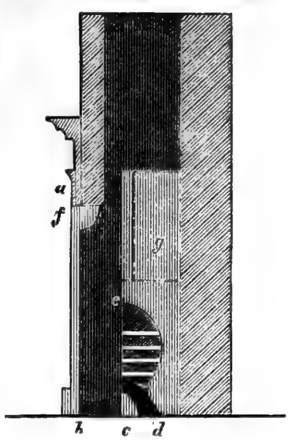


Fig. 9.

PLATE XII.

Fig. 10.

This figure shows how the covings are to be placed when the front of the covings (*a* and *b*) do not come so far forward as the front of the opening of the fireplace, or the jambs (A and B). See page 528.

Fig. 11.

This figure shows how the width and obliquity of the covings are to be accommodated to the width of the back of a fireplace, in cases where it is necessary to make the back very wide. See page 529.

Fig. 10.

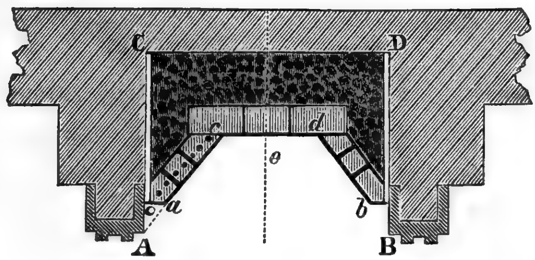
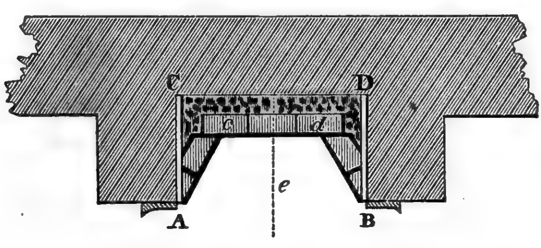


Fig. 11.



Scale of 1 2 3 4 5 *Feet.*

PLATE XIII.

Fig. 12.

This figure shows how an instrument called a bevel (*m n*), useful in laying out the work, in altering chimney fireplaces, may be constructed. See page 530.

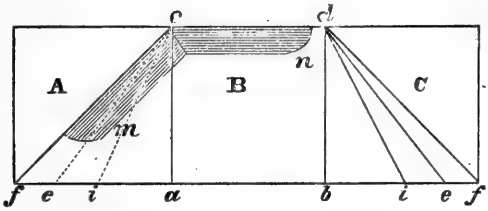
Fig. 13.

This shows how, when the breast of a chimney (*d*) is too high, it may be brought down by means of a wall (*h*) placed under the mantle, and a coating of plaster, which in this figure is represented by the part marked by dots. See page 532.

Fig. 14.

This shows how the breast of a chimney may be brought down merely by a coating of plaster. See page 533.

Fig. 12.



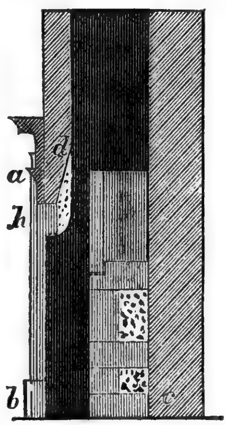


Fig. 13.

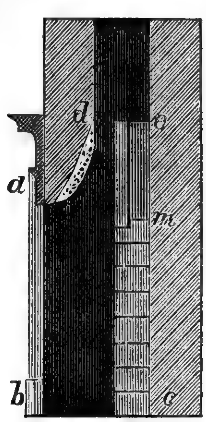


Fig. 14.

SUPPLEMENTARY OBSERVATIONS

CONCERNING

CHIMNEY FIREPLACES.

OBSERVATIONS CONCERNING OPEN CHIMNEY FIREPLACES.

An Account of various Faults that have been committed by Workmen, in England, who have been employed in altering Chimney Fireplaces, and fitting them up according to the Method recommended by the Author, in his Fourth Essay. — Consequences which have resulted from these Mistakes. — Necessity of adhering strictly, and without Deviation, to the Directions which have been given. — Those Particulars are pointed out in which Workmen are most liable to fail.

I WAS much flattered on my return to England, in September, 1798, after an absence of two years, to find that the improvements in the construction of chimney fireplaces, which I had recommended in my Fourth Essay, published in London in the beginning of the year 1796, were coming into use in various parts of the country; and I have since taken a good deal of pains to find out how they have answered, and what faults and imperfections have been discovered in them. And as the information I have obtained by these inquiries has enabled me to make several remarks and observations relative to the construction and management of these fireplaces, that may be of use to those who have introduced them, or may be desirous of introducing them, I feel it to be my duty to lay them before the public.

It has been objected to these fireplaces, that they sometimes occasion dust and ashes to come into the room when the fire is stirred. I have examined several fireplaces said to have been fitted up on my principles, that have certainly had that fault; but I have commonly, I might say invariably, found, that their imperfections have arisen from faults in their construction. Either the grate has been brought out *too far* into the room, or the opening of the fireplace in front has been left too wide or too high, or the workman has neglected to lower and to round off the breast of the chimney, or, what I have often found to be the case, several of these faults have existed together, in the same fireplace.

When the throat of a chimney is situated very high up above the mantle, and especially when the mantle and breast of the chimney, or the wall that reposes on the mantle, are very thin, workmen who are employed to alter chimneys, setting about the work with their minds strongly prepossessed with what they consider as the *leading principle* in the construction of these fireplaces, namely, that the throat of the chimney should not be more than four inches wide, they are very apt to bring the grate too far forward. In dropping their plumb-line from the breast of the chimney, they do not reach up high enough into the chimney, but take a part of the breast, where it still goes on to slope backwards, for the bottom of the perpendicular canal of the chimney. They also very often commit another fault, not less essential, and that has the same tendency, in neglecting to *bring down the throat of the chimney nearer to the fire*, when it happens to be situated too high.

This I have not only recommended in my Essay on Chimney Fireplaces, but have given the most particular directions how it is to be done (see page 531), and, to mark the importance of the object still more strongly, have accompanied those directions by an engraving.

It is indeed a very important point, that the throat of the chimney should be near the fire, and it should always be carefully attended to. It is likewise very important to "*round off the breast of the chimney*," though this, I find, is very often entirely neglected, even by workmen who have had much practice in the construction of the fireplaces I have recommended.

The breast of a chimney should always be rounded off in the neatest manner possible, beginning from the very front of the lower part of the mantle, and ending at the narrowest part of the throat of the chimney, where the breast ends in the front part of the perpendicular canal of the chimney. If the under surface of the mantle is flat and wide, it will be impossible to round off the breast properly; and that circumstance alone renders it indispensably necessary, in those cases, to alter the mantle, or to run under it a thinner piece of stone, or a thin wall of bricks, supported on an iron bar, in order that the breast of the chimney may be brought to be of the proper form, and the throat of the chimney may be brought into its proper situation.

If the under side of the mantle be left broad and flat, it is easy to perceive that the cloud of dust or light ashes that rises from a coal fire nearly burned out when it is violently stirred about with a poker, striking perpendicularly against this flat part of it, must unavoidably be beat back into the room; but when the breast of the chimney is properly rounded off, the ascending cloud of dust and smoke more easily finds its way into the throat of the chimney, and is even directed and assisted in some measure by the warm air of the room that gets under the mantle, and is going the same way.

Another very common fault that I have observed in chimney fireplaces, that have been altered on what have been called my principles, and which has a direct tendency to bring dust, and even smoke, into the room, is the sloping of the covings too much, and leaving the opening of the fireplace in front too wide. I have said, in my Essay on Chimney Fireplaces, that where chimneys are well constructed and well situated, and have never been apt to smoke, in altering them the covings may be placed at an angle of 135 degrees with the back; but I have expressly said that they should never exceed that angle, and have stated at large the bad consequences that must follow from making the opening of a fireplace very wide, when its depth is very shallow (see page 510). I have also expressly said (page 530), that, for chimneys that are apt to smoke, the covings should be placed *less obliquely*, in respect to the back, than in others that have not that fault. But most of the workmen who have altered chimneys seem to have paid little attention to these distinctions, and I have frequently found, and sometimes in fireplaces that have been remarkably shallow, that the covings have been placed at an angle even more oblique than that above mentioned.

Another cause that sometimes has considerable effect in bringing dust and smoke into rooms, from the fires that are made in them, is the great nicety with which the doors and windows are fitted in their frames, which prevents a sufficient quantity of fresh air from coming into the room to supply a brisk current up the chimney. It is, however, evident, that all the alterations in fireplaces on the common construction, that have been recommended in order to improve them, must tend directly and very powerfully to lessen this evil; but nothing will so completely remedy it as lowering the mantle, and diminishing the width of the fireplace.

How many fireplaces in close rooms have been cured completely of throwing puffs of smoke and dust into the room, merely by placing a register stove in them! But there is surely nothing peculiar to a register-stove that could enable it to perform such a cure, but merely as it serves to diminish the width and height of the opening of the fireplace; and how much easier could this be done with marble, or other stone, or with bricks and mortar, plastered over and incrusted in front with proper ornaments in stucco, or in artificial stone!

I am the more anxious that something of this sort should be introduced, as the openings of chimney fireplaces are in general certainly too wide and too high, and as I am convinced that there is no way of reducing them to a proper size, that would be so cheap, or more effectual, or that could be made more ornamental.

Those who are fond of the glitter of polished steel, and have no objection to the expense of it, or to the labour that is required to keep it bright, may surround their fireplaces *in front* with a border of it, for *there* it will do no harm, and may use grates and fenders of the most exquisite workmanship; but if they wish to have a pleasant, cheerful, and economical fire, the covings of their fireplaces must be placed obliquely, and they must not be constructed of metal; and if the sides and back of the grate be constructed of fire-bricks instead of iron, the fire will burn still brighter, and will send off considerably more radiant heat into the room.

I have abundant reason to think, that if, in constructing or altering chimney fireplaces, the rules laid down in my essay on that subject are *strictly* adhered to, chimneys so fitted up will very seldom be found either to smoke, or to throw out dust into the room; and should they be found to have either of these faults, there is a remedy for the evil, as effectual as it is simple and obvious: *Bring down the mantle and the throat of the chimney lower; and if it should be found necessary, reduce the width of the opening of the fireplace in front, and diminish obliquity of the covings.*

These alterations will certainly be effectual to prevent either smoke or dust from coming into the room *when there is a fire burning in the grate;* but it sometimes happens, and indeed not unfrequently, that dust and soot are drawn down a chimney in which there is no fire, to the great annoyance of those who are in the room, and to the great damage of the furniture. When this happens, it is commonly occasioned by a very strong draught up *another chimney,* in which there *is a fire,* in an adjoining room; and when that is the case, the most simple remedy is to alter that other chimney, and, constructing its fireplace on good principles, to reduce its throat to reasonable dimensions. But if the passage of the air down a chimney in which there is no fire is occasioned by strong eddies of wind, there is no remedy for that evil but placing a chimney-pot, of a peculiar construction, on the top of the chimney, which shall counteract the effect of those eddies; or by closing up the throat of the chimney occasionally, by a door made for that purpose of sheet-iron.

If the doorway that is left in the back of the fireplace for giving a passage to the chimney-sweeper, instead of being closed with a tile, or with a flat piece of stone, set in a groove made to receive it, according to the directions given in my Fourth Essay, be closed with a flat piece of cast-iron, or of plate-iron, fixed at its lower end, to the lower end of the doorway, by a hinge, or movable on two gudgeons, — this plate may easily be so contrived as to serve occasionally as a register or door for diminishing or closing the throat of the chimney.

As this plate, situated at the *back part* of the chimney, could not produce any of those bad effects that have with reason been attributed to the registers of common register-stoves (which are placed on the breast of the chimney), it appears to me to be very probable, that it would be found useful as a register for occasionally altering the size of the throat of the chimney, and regulating its draught, as well as for occasionally closing up that passage entirely. It would certainly be worth while to try the experiment.*

Before I quit this subject, I must mention another fault, which workmen employed in altering chimney fireplaces that are furnished with grates or stoves with sloping backs are very apt to make. They leave the back of the grate in its place, and instead of carrying up the back of the fireplace perpendicularly *from the bottom of the grate*, they first begin to carry it up perpendicularly from the top of the iron plate that forms the back of the grate; and as this plate not only slopes backwards considerably, but rises several inches above the

* Since the introduction of the cottage and gridiron grates, this contrivance has come into very general use, and experience has shown it to be extremely useful. I would strongly recommend it to those who fit up chimney fireplaces on these principles, never to omit this register; it costs a mere trifle, and is very useful on many accounts.

level of the upper bar of the grate, this necessarily throws the fire very far into the room. This tends to bring both smoke and dust into the room, not only because it brings the fire too far forward, but also because it occasions the air of the room, that slips in by the sides of the covings, to get behind the current of smoke that rises perpendicularly from the fire, which air frequently crowds the smoke forward, and causes it to strike against the mantle. This is a great fault, and I am sorry to say that I have found it very common in many parts of England, where attempts have been made to introduce the fireplaces I have recommended. Where grates *with sloping backs* are used in fitting up these fireplaces, these backs must either be taken quite away or bricked up, and the new back part, or back wall of the fireplace, must be made to serve as a back for the grate, against which the burning fuel is laid.

As I am giving an account of the mistakes that have been made by some of those who have been employed in fitting up chimney fireplaces on the principles I have publicly recommended, it will naturally be expected that I should take some notice of those numerous *improvements* that have been announced to the public, said to have been made in stoves, grates, etc., to which advertisers in the newspapers have thought proper to affix my name. As I am extremely anxious not to injure any man, either in his reputation for ingenuity, or in his trade, or in any other way, I shall not say one word more on this subject than what I feel it to be my duty to the public to declare, namely, that I am not the inventor of any of those stoves or grates that have been offered to the public for sale under my name.

Having mentioned the inconveniences that sometimes arise from doors and windows being fitted to their frames with so much nicety as not to give a sufficient passage to air from without to get into the room to supply the current up the chimney, which must always exist when a fire is burning in the room, I embrace this opportunity of mentioning a contrivance for remedying this defect, which I am persuaded would not only be found most effectual for that purpose, but would at the same time contribute very essentially to rendering dwelling-houses more salubrious and more comfortable, by facilitating the means of warming them more equally and ventilating them more easily and more effectually.

In building a house, an *air-canal*, about twelve or fifteen inches square, in the clear, and open at both ends, may be constructed in or near the centre of each stack of chimneys; and two branches from this air-canal, both furnished with registers, may open into each of the adjoining rooms, — one of these branches opening into the fireplace, just under the grate, and the other over the fireplace, and near the top of the room, or just under the ceiling. Each of these branches should be about four inches square, in the clear; and to prevent the uncouth appearance of the open mouth of that which opens into the room over the fireplace, it may be masked by a medallion, a picture, or any other piece of ornamental furniture proper for that use, placed before it at the distance of one or two inches from the side or wall of the room.

The bottom of this *air-tube* should reach to the ground, where it should communicate freely with the open air of the atmosphere; but it should not rise quite so high as the chimneys (or canals for carrying off the smoke) are carried up, but should end (by lateral openings, communicating with the air of the atmosphere) immediately above the roof of the house.

If this air-tube be situated in the middle of a building, it is evident that a horizontal canal or tube of communication must be carried from its lower orifice to some open place without the building, in order to establish a free circulation of fresh air, both upwards and downwards, in the *air-tube.* I say both *upwards* and *downwards,* for sometimes the current of air in the tube will be found to set upwards, and sometimes downwards. Its direction will depend on the winds that happen to prevail, or rather on the eddies they occasion in the air out of doors in the neighbourhood of the buildings; and it is no small advantage that will arise from leaving both ends of the air-tube open, that the tube will always be supplied with a sufficiency of air, whatever eddies the winds may occasion. It is easy to perceive how powerfully this must operate to prevent those puffs of smoke which, in high winds, are frequently thrown into some rooms by the eddies, and the partial rarefactions of the air that they occasion; but this is far from being the only or the most important of the advantages that will be derived from this air-tube. Those who consider what an immense quantity of air is required to supply the current that sets up the chimney of an open fireplace, where there is a fire burning, must perceive what an enormous loss of heat there must be, when all this expense of air is supplied by the warmed air of the room, and that all this warmed air is necessarily and constantly replaced by the cold air from without, which finds its way into the room by the crevices of the doors and windows. But all this waste of heat, or any part of it, at pleasure, may be prevented by the scheme proposed; for if the air necessary to the combustion of the fuel, and to the supplying of the current up the chimney, be furnished by the air-tube, the warmed air in the room will remain in its place; and as this will in a great measure prevent the cold currents from the crevices of the door and windows, the heat in the room will be the more equable, and consequently the more wholesome and agreeable on that account.

But there are, I am told, persons in this country, who are so fond of seeing what is called a great roaring fire, that even with its attendant inconveniences, of roasting and freezing opposite sides of the body at the same time, they prefer it to the genial and equable warmth which a smaller fire, properly managed, may be made to produce, even in an open chimney fireplace. To recommend the air-tubes to persons of that description, I would tell them, that, by closing up, by means of its register, the lower branch of communication (that which ends just under the grate) and setting that situated near the top of the room wide open, they may indulge themselves with having a very large fire in the room *with little heat*, and this with much less inconvenience from currents of cold air from the doors and windows than they now experience.

It is easy to perceive that by a proper use of the two registers, together with a judicious management of the fire, the air in the room may either be made hotter or colder, or may be kept at any given temperature, or the room may be most effectually ventilated; and that this change of air may be effected either gradually or more suddenly. And here it may perhaps be the proper place to observe, that in all our reasonings and speculations relative to the heating of rooms by means of open chimney fires, we must never forget that it is the *room that heats the air*, and not the air that heats the room.

The rays that are sent off from the burning fuel generate heat only *when* and *where* they are *stopped* or *absorbed;* consequently they generate no heat in the air in the room in passing through it, because they *pass through it*, and are not *stopped* by it, but, striking against the walls of the room, or against any solid body in the room, these rays are *there* stopped and absorbed, and it is *there* that the heat found in the room is *generated.* The air in the room is afterwards heated by coming into contact with these solid bodies. Many capital mistakes have arisen from inattention to this most important fact.

It is really astonishing how little attention is paid to events which happen frequently, however interesting they may be as objects of curious investigation, or however they may be connected with the comforts and enjoyments of life. Things near us, and which are familiar to us, are seldom objects of our meditations. How few persons are there who ever took the trouble to bestow a thought on the subject in question, though it is, in the highest degree, curious and interesting!

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END OF VOL. II.