4. COUNT RUMFORD'S EXPERIMENTS ON THE SOURCE OF THE HEAT THAT IS EXCITED BY FRICTION

The most celebrated experiments carried out by Count Rumford were those concerned with the heat produced during the boring of cannon. The caloric theory had been found especially helpful in explaining and predicting phenomena in which heat passes from one body to another by conduction — as when liquids are mixed or when a substance placed over a fire is warmed, melted, or vaporized. For such phenomena, it is worth noting, one can safely assume that there is no creation or destruction of heat during its passage from one object to another, or from a fire to an object.

But whence comes the heat when an object is warmed, not by fire, but by rubbing or hammering it? The supporters of the caloric theory believed that they could also answer this question satisfactorily, and still retain their principle that heat can be neither created nor destroyed.

Difficulties in Discerning the Nature of Heat

from the Memoires of Count Rumford
with commentary by Steven Brush
(an excerpt)

1797 – 1798

the more intense meditations of philosophers in the hours expressly set apart for study.

It was by accident that I was led to make the experiments of which I am about to give an account; and, though they are not perhaps of sufficient importance to merit so formal an introduction, I cannot help flattering myself that they will be thought curious in several respects, and worthy of the honor of being made known to the Royal Society.

Being engaged lately in superintending the boring of cannon in the workshops of the military arsenal at Munich, I was struck with the very considerable degree of heat [temperature] that a brass gun acquires in a short time in being bored, and with the still higher temperature (much higher than that of boiling water, as I found by experiment) of the metallic chips separated from it by the borer.

The more I meditated on these phenomena, the more they appeared to me to be curious and interesting. A thorough investigation of them seemed even to bid fair to give a farther insight into the hidden nature of heat; and to enable us to form some reasonable conjectures respecting the existence, or nonexistence, of an *igneous fluid* — a subject on which the opinions of philosophers have in all ages been much divided.

There was nothing extraordinary about his mere observation that heat is produced during the boring of a gun. Since the earliest times it had been known that heat could be developed by friction — as when primitive people made a fire by rapidly rotating a pointed stick inserted in a hole drilled in dry wood. Rumford's feats here were that he noticed the very high temperature quickly acquired by the gun during the boring and saw how further study of this phenomenon might throw new light on the "hidden nature of heat."

By "igneous fluid" Rumford means the "subtle elastic fluid" which, according to the caloric theory, constituted heat. In the literature of his period, the terms "igneous fluid," "heat fluid," "matter of heat," "caloric," "calor," and "heat" were used more or less interchangeably. The term "caloric" actually was not introduced until comparatively late in the history of the conception of heat as a substance, having been coined in 1787 by Lavoisier and other French scientists during a revision of chemical terminology which they carried out at that time. The term "calorimeter," which is still used today to refer to any apparatus for measuring quantities of heat, was coined by Lavoisier in 1789.

> In order that the Society may have clear and distinct ideas of the speculations and reasonings to which these appearances gave rise in my mind, and also of the specific objects of philosophical investigation they suggested to me, I must beg leave to state them at some length, and in such manner as I shall think best suited to answer this purpose.
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> From *whence comes* the heat actually produced in the mechanical operation above mentioned?
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> Is it furnished by the metallic chips which are separated by the borer from the solid mass of metal? If this were the case, then, according to the modern doctrines of latent heat and of caloric, the *specific heat* of the parts of the metal, so reduced to chips, ought not only to be changed, but the change undergone by them should be sufficiently large to account for *all* the heat produced.
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> But no such change had taken place; for I found, upon taking equal weights of these chips and of thin slips separated from the same block of metal by means of a fine saw, and putting them at the same temperature (that of boiling water) into equal weights of cold water initially at the temperature of 59½°F, the portion of water into which the chips were put was not, to all appearance, heated either less or more than the other portion in which the slips of metal were put. This experiment being repeated several times, the results were always so nearly the same that I could not determine whether any, or what, change had been produced in the metal, *in regard to its specific heat*, by being reduced to chips by the borer.

In boring a cannon, or whenever one solid object is rubbed against another, the rubbing surfaces are abraded, that is, rubbed or cut into dust or chips. Might these chips possibly be the source of the large quantity of heat produced during the boring? If so, asserts Rumford, it follows from the "modern" caloric theory that the specific heat of the chips should be different from that of the metal in bulk. Suppose it were found that the specific heat of the chips was smaller than that of the metal in bulk. Then this could be interpreted to mean that the attractive force was smaller between the chips and caloric than between the bulk metal and caloric, and hence that caloric would be set free during the abrasion. Indeed, this was one way in which the calorists had tried to explain heat produced by friction; yet no one prior to Rumford seemingly had made experiments to determine whether there was any difference in the specific heats of the abraded and the bulk metal.

To find the specific heat of the cannon metal, Rumford here has made use of the technique in calorimetry known as the *method of mixtures* (Sec. 2). He took slices of the bulk metal, determined their weight w_b and initial temperature t_b, and then put them in a known weight w_w of water initially at a known temperature t_w. Let t be the observed, equilibrium temperature of the mixture of metal and water. Then the heat lost by the bulk metal is given by the expression $s_b w_b (t_b - t)$, where s_b is the unknown specific heat of the metal; and the heat gained by the water is given by $s_w w_w (t - t_w)$, where s_w is the specific heat of water. Assuming the experiment to have been so devised that the heat lost by the metal was equal to the heat gained by the water, we can write the equation

$$s_b w_b (t_b - t) = s_w w_w (t - t_w). \qquad (5)$$

Rumford next repeated this whole method-of-mixtures experiment, this time substituting *chips* bored from the same block of cannon metal for the bulkier slices, but keeping the weights and the initial temperatures the same as before; and he observed the temperature of the mixture of chips and water again to be t, the same as in the preceding experiment. If s_c denotes the specific heat of the metal chips, the equation for this second experiment is

$$s_c w_b (t_b - t) = s_w w_w (t - t_w). \qquad (6)$$

By comparing Eqs. (5) and (6) we can conclude, as did Rumford, that the specific heats s_b and s_c are equal.

Suppose it had been found that the specific heat of the chips was not equal to that of the metal in bulk. Then, as Rumford points out, the next question for investigation would have been whether this change in specific heat upon abrasion was "sufficiently large to account for *all* the heat produced." Often an explanation or a hypothesis will turn out to be admissible *qualitatively*, but not *quantitatively*. As a simple example, a very early hypothesis as to the source of solar energy was that the sun is a mass of fuel, such as coal, which is burning and thus emitting energy. This hypothesis passed the qualitative test in that it is true that a burning fuel emits energy. But when, later, our knowledge of fuels, of the size and distance of the sun, and of the energy given off by it was sufficiently complete to make even rough calculations possible, it became clear that the sun, if it were simply a ball of burning fuel, would have been completely consumed long ago. The hypothesis met the qualitative but not the quantitative test.

> As these experiments [on the specific heats of the abraded and bulk metal] are important, it may perhaps be agreeable to the Society to be made acquainted with them in their details. One of them is as follows:
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> To 4590 grains [0.66 lb] of water, at the temperature of 59½°F (in-

> cluded in this weight was an allowance, reckoned in terms of water, for the heat capacity of the containing tin vessel), were added 1016⅛ grains [0.16 lb] of cannon metal in thin slips, these being at 210°F, the temperature of boiling water at Munich. When they had remained together 1 minute, and had been well stirred about by means of a small rod of light wood, the temperature of the mixture was found to be 63°F. From this experiment the specific heat of the metal, calculated according to the rule given by Dr. Crawford, turns out to be 0.1100 [Btu/lb °F], that of water being 1.0000 [Btu/lb °F].
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> An experiment was afterwards made with the metallic chips as follows. To the same weight of water as was used in the afore-mentioned experiment, at the same temperature (59½°F), and in the same cylindrical tin vessel, were now put 1016⅛ grains of metallic chips bored out of the same gun from which the slips used in the foregoing experiment were taken, and at the same temperature (210°F). The temperature of the mixture at the end of 1 minute was 63°F, as before. Consequently, the specific heat of these metallic chips is 0.1100 [Btu/lb °F]. Each of the foregoing experiments was repeated three times, and always with nearly the same results.

Strictly speaking, Rumford's demonstration that the chips and bulk metal have the same specific heat at the same temperature did not constitute a complete refutation of the calorists' explanation of how heat was developed by friction. To show that the two specific heats are equal may have been *necessary*, but it was not *sufficient*. For a more conclusive test, it was still necessary to show that equal weights of the chips and bulk metal always contained equal *quantities of heat* when at the same temperature. The calorists could have said that, even though the chips

and bulk metal had the same specific heats at the same temperature, yet the bulk metal contained more latent heat than the chips, the difference having been evolved during abrasion. A possible test of this contention would have been to measure the quantities of heat needed to *melt* equal weights of chips and of bulk metal. If these quantities of heat were found equal, and if it were granted that the resulting liquids were in all respects exactly the same, then one could conclude that equal weights of the chips and bulk metal contained equal quantities of heat at the same temperature. However, we shall see that Rumford did not bother to carry out such an experiment; a possible reason for this is suggested in our note on page 80.

One supporter of the caloric theory asserted, in 1830, that none of the afore-mentioned experiments on specific heats had any significance in determining whether or not the heat evolved during boring came from the metal chips. He contended that this heat could have come, not from the chips, but from the layer of bulk metal in contact with the borer. The large force to which this layer was subjected would tend to compress it and increase its density; and it had long been known that when any piece of metal is compressed, as by hammering it, heat is evolved. This heat, the calorists said, was squeezed out of the metal as a result of the compression. Thus, in the boring of the cannon, successive fresh layers of cannon metal were exposed to compression as the result of the abrasion, and hence each layer in succession would release a certain quantity of heat. If any changes in density or specific heat occurred, it would therefore be confined to the surface layer of the bulk metal, and this Rumford did not test.

The passages that follow will be easier to interpret if it is remembered that Rumford and some other scientists of his period used the expression "latent heat" to refer, not solely to heats of fusion and vaporization, as Black had done (Sec. 2), but also to heat which the calorists assumed was stored in an inactive form in any substance and released upon rubbing or hammering it.

> It is evident that the heat produced could not possibly have been furnished at the expense of the latent heat of the metallic chips. But, not being willing to rest satisfied with these trials, however conclusive they appeared to me to be, I had recourse to the following still more decisive experiments.
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> Taking a cannon (a brass six-pounder), cast solid, and rough as it came from the foundry (see Fig. 1 in Plate II), and fixing it horizontally in the machine used for boring, and at the same time finishing the outside of the cannon by turning (see Fig. 2), I caused its extremity to be cut off and the metal in that part to be turned down to form a solid cylinder, 7¾ inches in diameter and $9\frac{8}{10}$ inches long. This cylinder, when fin-

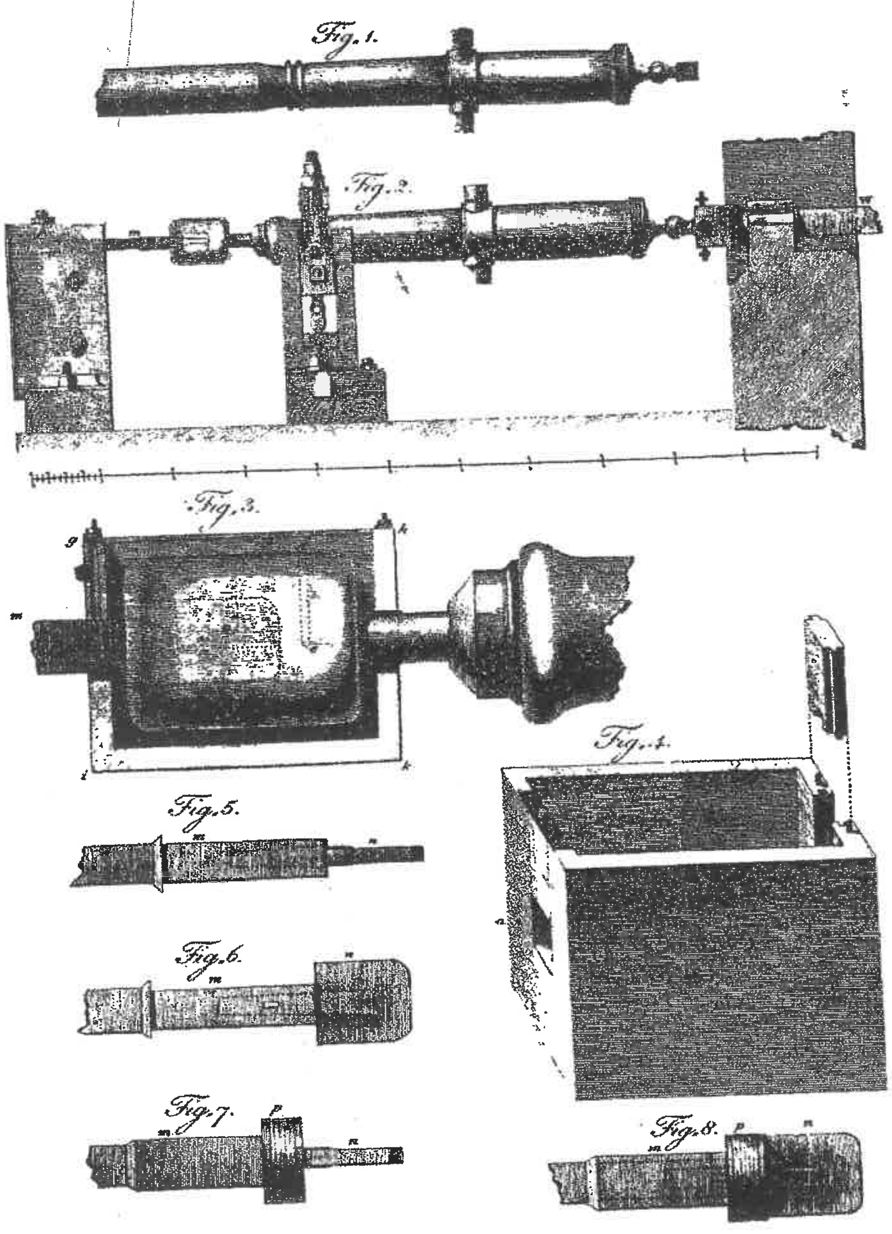

Plate II: Rumford's diagrams of his apparatus.

ished, remained joined to the rest of the metal (that which, properly speaking, constituted the cannon) by a small cylindrical neck only 2⅕ inches in diameter and 3⁸⁄₁₀ inches long.

This short cylinder, which was supported in its horizontal position and could be turned round its axis by means of the neck that united it to the cannon, was now bored with the horizontal borer used in boring cannon; but its bore, which was 3.7 inches in diameter, instead of being continued through its whole length (9.8 inches) was only 7.2 inches in length; so that a solid bottom was left to this hollow cylinder, which bottom was 2.6 inches in thickness. This cavity is represented by dotted lines in Fig. 2 and also in Fig. 3, where the cylinder is represented on an enlarged scale.

This cylinder was designed for the express purpose of generating heat *by friction*, by forcing a blunt borer against the solid bottom of the cylinder at the same time that the latter was turned round its axis by the force of horses. In order that the temperature of the cylinder might from time to time be measured, a small round hole (see *de*, Fig. 3), only 0.37 inch in diameter and 4.2 inches in depth, was made in it for the purpose of introducing a small cylindrical mercurial thermometer. This hole was on one side, in a direction perpendicular to the axis of the cylinder, and ended in the middle of the solid bottom.

The volume of the hollow cylinder, exclusive of the cylindrical neck by which it remained united to the cannon, was 385¾ cubic inches, English measure, and it weighed 113.13 lb avoirdupois; this I found on weighing it at the end of the course of experiments, and after it had been separated from the cannon.

Experiment No. 1

This experiment was made in order to ascertain how much heat was actually generated by friction when the blunt steel borer was so forcibly shoved (by means of a strong screw) against the bottom of the bore of the cylinder that the pressing force on it was equivalent to the weight of about 10,000 lb avoirdupois and the cylinder was being turned round on its axis (by the force of horses) at the rate of about 32 times in a minute. This machinery, as it was put together for the experiment, is represented by Fig. 2. Here *w* is a strong horizontal iron bar, connected with proper machinery carried round by horses, by means of which the cannon was made to turn round its axis.

To prevent, as far as possible, the loss of any part of the heat that was generated in the experiment, the cylinder was well covered up with a fit coating of thick and warm flannel, which was carefully wrapped round it, and defended it on every side from the cold air of the atmosphere. This covering is not shown in Fig. 2.

I ought to mention that the borer was a flat piece of hardened steel, 0.63 of an inch thick, 4 inches long, and nearly as wide as the cavity of the bore of the cylinder, namely, 3½ inches. Its corners were rounded off at its end, so as to make it fit the hollow bottom of the bore; and it was firmly fastened to the iron bar *m* which kept it in its place. The area of the surface by which its end was in contact with the bottom of the bore of the cylinder was nearly 2⅓ square inches. This borer, which is distinguished by the letter *n*, is represented in most of the figures.

At the beginning of the experiment, the temperature of the air in the shade, as also that of the cylinder, was 60°F. At the end of 30 minutes, when the cylinder had made 960 revolutions about its axis, the horses being stopped, a cylindrical mercurial thermometer, whose bulb was 32/100 of an inch in diameter and 3¼ inches in length, was introduced into the hole *de* in the side of the cylinder; the mercury rose almost instantly to 130°F.

The heat could not be supposed to be quite equally distributed in every part of the cylinder, yet, as the length of the bulb of the thermometer was such that it extended from the axis of the cylinder to near its surface, the temperature indicated by it could not be very different from the *mean temperature* of the cylinder; and it was on this account that a thermometer of that particular form was chosen for this experiment.

To see how fast the heat escaped from the cylinder (in order to be able to make a probable conjecture respecting the quantity given off by it during the time the heat generated by the friction was accumulating), the machinery standing still, I suffered the thermometer to remain in its place nearly three quarters of an hour, observing and noting down, at small intervals of time, the temperature indicated by it. [See Table 1.]

As Rumford states, the boring in experiment No. 1 was carried on for 30 min, during which time the temperature of the cylinder rose from 60° to 130°F. The cylinder was wrapped in flannel, but as the cooling data in Table 1 show, some of the heat evolved during the boring escaped to the surrounding objects and atmosphere. From these cooling data, Rumford could have estimated how much above 130°F the final temperature of the cylinder would have been if it had been perfectly insulated from the surroundings. However, we see from Table 1 that, at the highest temperatures, the time-rate of cooling never exceeded one degree per minute, and at temperatures near that of the air (60°F) the rate was much smaller. So he does not bother to complete the calculations or to comment further on these cooling data.

[Table 1. Cooling data.]

Total time, in minutes, since machinery was stopped	Temperature, in degrees Fahrenheit, as shown by the thermometer
[0]	[130°]
4	126°
5	125°
7	123°
12	120°
14	119°
16	118°
20	116°
24	115°
28	114°
31	113°
34	112°
37½	111°
41	110°

Having taken away the borer, I now removed the metallic dust, or, rather, scaly matter, which had been detached from the bottom of the cylinder by the blunt steel borer. I carefully weighed this dust and found its weight to be 837 grains.

Is it possible that the very considerable quantity of heat produced in this experiment (a quantity which actually raised the temperature of above 113 lb of gun metal at least 70 Fahrenheit degrees, and which, of course, would have been capable of melting 6½ lb of ice) could have been furnished by so inconsiderable a quantity of metallic dust? And this merely in consequence of *a change* of its specific heat?

As the weight of this dust (837 grains) amounted to no more than 1/948th part of that of the cylinder, the dust would have had to give up a quantity of heat equal to that which it would lose in cooling through 948 Fahrenheit degrees to have been able to raise the temperature of the

> cylinder 1°; and consequently it would have had to give up a quantity corresponding to a cooling through 66,360 Fahrenheit degrees to have produced the effects that were actually observed in the experiment!

Rumford had previously found the specific heat of the cannon metal to be 0.11 Btu/lb °F. The cylinder weighed 113 lb, and its temperature during boring increased *at least* (130–60) °F, or 70 Fahrenheit degrees. So the quantity of heat entering the cylinder during boring was *at least* (0.11 Btu/lb °F) × 113 lb × 70°F, or 870 Btu.

That this really is a considerable quantity of heat is strikingly illustrated by Rumford's remark that it is sufficient to melt 6.5 lb of ice. Using the modern value of the heat of fusion of ice, namely, 144 Btu/lb, we see that the weight of ice that would be melted by 870 Btu of heat is 870 Btu/(144 Btu/lb), or about 6.0 lb. Rumford, in arriving at the larger figure of 6.5 lb for the weight of ice that would be melted, probably took into account the fact that, if a correction for cooling had been made, the value for the final temperature of the cylinder would have exceeded 130°F. Moreover, in his day the heat of fusion of ice was often taken to be Black's value of 141 Btu/lb, instead of the more correct modern value of 144 Btu/lb.

In saying that the metallic dust would have had to cool through 66,360 Fahrenheit degrees to have produced the effects observed, Rumford is not implying that the dust is to be thought of as actually having undergone this drop in temperature. The British thermal unit had not yet come into use and so Rumford, like Black (Sec. 2), had to express quantity of heat in some other way — for instance, in terms of the temperature change that it would produce in some specified substance. If the British thermal unit had been available to him, he very likely would have stated his argument somewhat as follows. The quantity of heat evolved in 30 min was at least 870 Btu. To gain an idea of the magnitude of this quantity, notice that it would be sufficient to melt more than 6 lb of ice or, as another illustration, sufficient to produce a temperature change of 66,360 Fahrenheit degrees in 837 grains of metallic dust.

This temperature change can be computed as follows. The quantity of heat H_m absorbed by the bulk metal was equal to $sw \times 70°\mathrm{F}$, where s and w are the specific heat and weight of this metal, respectively. The quantity of heat H_d needed to produce a temperature change Δt in the dust is given by $s(w/948)\Delta t$, on the assumption that the specific heat of the dust is practically equal to that of the bulk metal. If $H_m = H_d$, *then*

$$sw \times 70°\mathrm{F} = s\,(w/948)\Delta t,$$

or

$$\Delta t = 948 \times 70°\mathrm{F} = 66{,}360°\mathrm{F}.$$

Incidentally, there is no justification for retaining five significant figures in this result. Rumford was not always consistent in his use of significant figures.

If Rumford could have used modern units, he might have continued his argument in this fashion. If the release of 870 Btu of heat actually had been due to a change in the specific heat of the metal upon abrasion, this would mean that "so inconsiderable a quantity" of metal as 837 grains (0.1197 lb) must have released 870 Btu upon being reduced to dust. In other words, the release of heat would have been 870 Btu/0.1197 lb, or about 7300 Btu per pound of metal, "and this merely in consequence of a change in its specific heat." This could have well seemed "improbable" to Rumford, in view of his knowledge of the quantities of heat that are released under other somewhat comparable circumstances. For instance, compare this figure of 7300 Btu per pound with the 144 Btu of heat released by a pound of water when it freezes, or even with the 970 Btu released by a pound of steam when it liquefies.

> But without insisting on the improbability of this supposition, we have only to recollect that, from the results of actual and decisive experiments made for the express purpose of ascertaining that fact, the specific heat of the metal of which great guns are cast *is not sensibly changed* by being reduced to the form of metallic chips in the operation of boring cannon; and there does not seem to be any reason to think that it can be much changed, if it be changed at all, in being reduced to much smaller pieces by means of a borer that is less sharp.
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> If the heat, or any considerable part of it, were produced in consequence of a change in the specific heat of a part of the metal of the cylinder, as such change could only be *superficial*, the cylinder would by degrees be *exhausted*; or the quantities of heat produced in any given short interval of time would be found to diminish gradually in successive experiments. To find out if this really happened or not, I repeated the last-mentioned experiment several times with the utmost care; but I did not discover the smallest sign of exhaustion in the metal, notwithstanding the large quantities of heat actually given off.
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> Finding so much reason to conclude that the heat generated — *or excited*, as I would rather choose to express it — in these experiments was not furnished *at the expense of the latent heat or combined caloric* of the metal, I pushed my inquiries a step farther and endeavored to find out whether or not the air contributed anything in the generation of it.