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Physics in Perspective

Rumford and the Reflection of Radiant Cold: Historical Reflections and Metaphysical Reflexes

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In this paper I examine the debate regarding the positive reality of *cold*: whether it is merely an absence of heat, or a quality or entity in its own right. Marc-Auguste Pictet stimulated this debate by showing that radiation from a cold object apparently could be focused by concave mirrors to cool another object some distance away from it. Pictet and other believers in material theories of heat, most notably Pierre Prevost, sought to understand this phenomenon as a result of the radiation of caloric in a peculiar arrangement. By contrast, Count Rumford saw in Pictet's experiment a genuine action of "frigorific rays," and performed striking new experiments to support his view. For Rumford heat and cold radiation consisted in sound-like undulations in the ether, a mechanism compatible with his own vibration theory of heat, and discordant with the caloric theory. Rumford's strong arguments were overruled only because of the general dominance of the caloric theory of heat. However, Rumford did push the caloric theory to develop in a direction that eventually led to its downfall. I revisit this debate without preconceived notions of the metaphysical nature of cold and heat.

Key words: Cold; frigorific; caloric; radiant heat; Count Rumford; Marc-Auguste Pictet; Pierre Prevost.

Why Celsius said Water Boiled at 0° and Rumford wore a White Hat in Winter

A curious fact in the history of meteorology provides a glimpse into the subject of this paper. The common attribution of the centigrade thermometer to the Swedish astronomer Anders Celsius (1701–1744) is correct enough, but his scale had the boiling point of water at zero degrees, and the freezing point at one hundred degrees. In fact, Celsius was not alone in adopting such an "upside-down" thermometric scale. The mercury thermometer designed by the French astronomer Joseph-Nicolas Delisle (1688–1768) in St. Petersburg also had a scale in that direction. In England the Royal Society thermometer had its zero point at "extream heat" (around 90° Fahrenheit), and the numbers increased going down the tube.[1] These "upside-down" scales were in serious scientific use up to the early eighteenth century, as the conversion device in figure 1 shows emblematically.**

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** Celsius's scale was adopted, for instance, in the meteorological reports from Uppsala, made at the observatory that Celsius himself had founded, for some time in the late 1740s. From 1750 we find the scale inverted into the modern centigrade scale. The Royal Society thermometer provided the chief

Fig. 1. "A General Thermometer" (*ca*. 1720), a convenient device for conversions among 16 temperature scales. In the Royal Society scale (the third outermost) and the Delisle scale (the seventh outermost), the numbers increase counterclockwise, while the others increase clockwise. Courtesy of the National Maritime Museum, Greenwich, London, where this instrument is held (reference number MT/Th.4).

Why these early pioneers of thermometry created and used such "upside-down" thermometers must remain to some extent a matter of speculation. There seems to be no surviving record of the principles behind the calibration of the Royal Society thermometer, and Delisle's account of his thermometers only concentrates on the concrete procedures of calibration. There is no clear record of Celsius's motivations either, and in Olof Beckman's view, "Celsius and many other scientists were used to both direct and reversed scales, and simply did not care too much" about the direction.[2] My own speculation is that those who designed upside-down thermometers may have been crafting an instrument for measuring the degrees of cold, rather than heat – a "frigometer," as it were. Today we consider that strange, but only because we have a metaphysical belief that cold is simply the absence of heat, not a real positive quality or entity in its own right. Although the existence of the upside-down temperature scales does not *prove* that their makers were trying to measure cold rather than heat, they at least reveal a lack of definite metaphysical commitment that cold is not a real positive entity or quality. Father Marin Mersenne (1588–1648), that diplomat among scholars and master of "mitigated skepticism," devised a thermometer, which he described in 1644 (figure 2), to accommodate all tastes, with one sequence of numbers going up and another sequence going down, whichever way one considered "up." The mercury thermometer devised by the French physicist Guillaume Amontons (1663–1705) had a similar double scale.[3]

In fact, there were a number of perfectly capable philosophers and scientists throughout the ages who had regarded cold as real as heat – starting with Aristotle, who took cold and hot as opposite qualities on an equal footing, as two of the four fundamental qualities in the terrestrial world. The mechanical philosophers of the 17th century were not united in their reactions to this aspect of Aristotelianism. Although many of them subscribed to kinetic theories that understood heat as motion and cold as the lack of it, the mechanical philosophy did not rule out giving equal ontological status to heat and cold. In the carefully considered view of Francis Bacon (1561–1626), heat was a particular type of expansive motion and cold was a similar type of contractive motion; therefore, the two had equal metaphysical status. Robert Boyle (1627–1691) wanted to rule out the positive reality of cold, but had to admit his inability to do so in any conclusive way after honest and exhaustive considerations. The French atomist Pierre Gassendi (1592–1655) had a more complex mechanical theory, in which "calorific atoms" caused heat by agitating the particles of ordinary matter; likewise, Gassendi postulated "frigorific atoms," whose angular shapes and sluggish motions made them suited for clogging the pores of bodies and damping the motions of atoms.[4]

Gassendi's sort of theory seems to have gained much popularity for some time, as reported in 1802 by Thomas Thomson (1773–1852),[5] Scotland's premier "chemist breeder" and early historian of chemistry:

British standard in the early eighteenth century, and it was sent out to agents in various countries who reported their meteorological observations to the Royal Society, which were summarized in reports in the *Philosophical Transactions of the Royal Society of London*. The use of the Royal Society scale is in evidence at least from 1733 to 1738. Delisle's scale was recognized widely and remained quite popular for some time, especially in Russia.

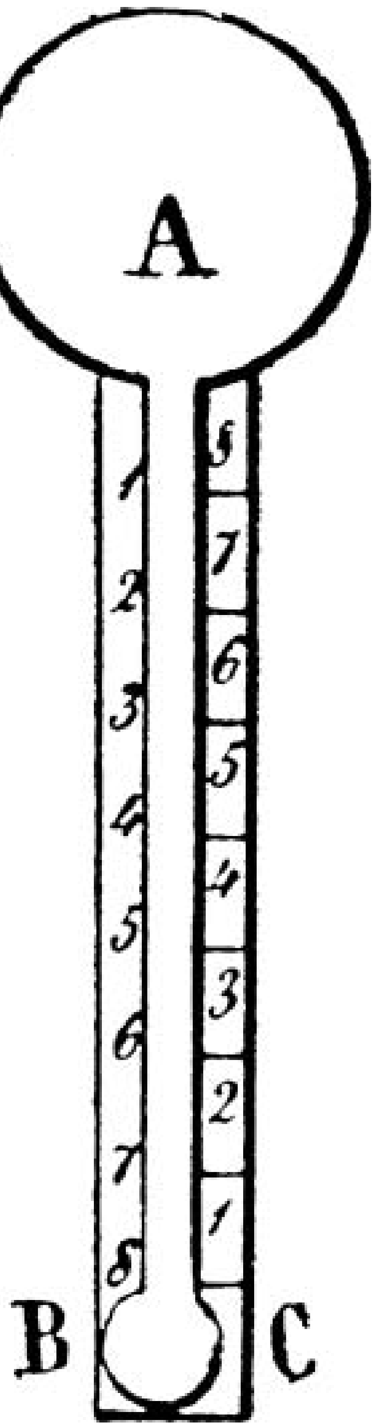

Fig. 2. Mersenne's thermometer, described in 1644. Courtesy of the Science Museum, London.

> There have been philosophers ... who maintained that cold is produced not by the abstraction of caloric merely, but by the addition of a positive something, of a peculiar body endowed with specific qualities. This was maintained by [Petrus van] Muschenbroek [1692–1761] and [Jean Jacques d'Ortous] De Mairan [1678–1771], and *seems to have been the general opinion of philosophers about the commencement of the* 18*th century*. According to them, cold is a substance of a saline nature, very much resembling nitre, constantly floating in the air, and wafted about by the wind in very minute corpuscles, to which they gave the name of frigorific particles.[6]

Even by the late 18th century the question had not been settled. The second edition of the *Encyclopaedia Britannica* in 1778 reported that there was no agreement on this question, but itself came down on the side of supposing the independence of cold from heat, giving some cogent reasons. This conception led to talk such as this: "if a body is heated, the cold ought to fly from it." This way of thinking persisted and even gathered strength by the third edition of *Britannica*, published a decade later. The author of the article on "heat" there admitted a good deal of uncertainty in current knowledge, and opined that the best way of proceeding was "to lay down certain principles established from the obvious phenomena of nature, and to reason from them fairly as far as we can." Ten such principles were offered, and the first one reads: "Heat and cold are found to expel one another. Hence we ought to conclude, that heat and cold are both *positives*."[7]

Into this confused field came a striking experiment that seemed to be a direct confirmation of the reality of cold and generated a controversy that became the

Fig. 3. Marc-Auguste Pictet (1752–1825). Courtesy of the Bibliothèque Publique et Universitaire, Geneva.

last and most crucial debate in the banishing of cold from the ontology of the universe. The experiment was the work of the Genevan physicist and statesman Marc-Auguste Pictet (1752–1825). Pictet's family was one of the most illustrious in the history of Geneva, and Marc-Auguste (figure 3) lived up to expectations as the highly respected Professor of Philosophy in the Academy (now University) of Geneva from 1786, a leading member of key scientific societies, and one of the central political figures mediating the French rule of Geneva in the period 1798–1814. An essential part of Pictet's contribution to science was promoting exchanges among scientists of various nations, impressively negotiating the treacherous boundaries of the Napoleonic era. With his younger brother Charles (1755–1824), Pictet founded the *Bibliothèque Britannique* in 1796, a periodical dedicated to the dissemination and discussion of British scientific work in Geneva and the rest of French-speaking Europe.[8]

Pictet reported numerous interesting experiments on heat in his *Essai sur le feu* (1790), including a series demonstrating that radiant heat, even when it was not accompanied by any light, could be reflected and focused like light.* Figure 4 shows a similar but more dramatic experiment, performed later at the Royal Institution of Great Britain with equipment originally commissioned by Rumford. Pictet set up two concave metallic mirrors facing each other (each a truncated spherical surface of radius 9 inches, made of polished tin, separated from each other by over 10 feet), and placed a sensitive thermometer at the focus of one of the mirrors; he then brought a hot object to the focus of the other mirror, and observed that the thermometer started rising immediately. A hot (but not glowing) iron bullet raised the thermometer reading at the other focus by 10.5 degrees Fahrenheit after six minutes. Even a flask of boiling water raised the temperature by over 3 degrees in just two minutes. Having tried the experiment with the mirrors separated by a distance of 69 feet without being able to detect any time delay between the insertion of the hot object at one focus and the movement of the thermometer at the other, Pictet concluded that the effect was due to heat being radiated at an extremely high speed like light, and certainly not due to its conduction through the air.[9] To appreciate how remarkable this was, we need to remember that Pictet's publication was still a full decade before the discovery of infrared radiation in sunlight by the German-British musician and astronomer William Herschel (1738–1822). The phenomenon of unmediated heat transfer through space was an unfamiliar one at that time, and not at all well understood.

Pictet then reported a curious variation on this already-stunning experiment:

> I conversed on this subject with Mr. [Louis] Bertrand [1731–1812], a celebrated professor of mathematics in our academy, and pupil of the immortal Euler. He

* There had been earlier experiments of this kind. Most widely recognized by Pictet himself and others were those of the German physicist Johann Heinrich Lambert (1728–1777), and of the Genevan meteorologist and geologist Horace Bénédict de Saussure (1740–1799), Pictet's mentor and predecessor in the Academy. In fact, Pictet had helped Saussure perform these experiments, which were directly inspired by Lambert's *Pyrometrie*: *oder vom Maaße des Feuers und der Wärme* (Berlin, 1779).

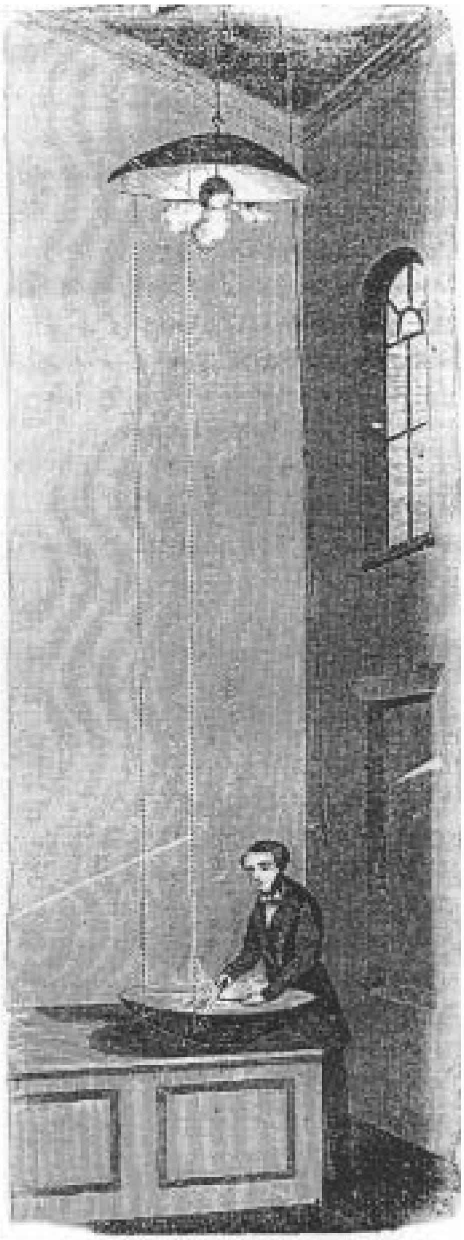

Fig. 4. Illustration showing a version of Pictet's "double-reflection" experiment. The spark ignited at the focus of the lower mirror causes the explosion of the hydrogen-chlorine balloon at the focus of the upper mirror. In an experiment more like Pictet's, a hot copper ball placed in the lower focus causes a blackened hydrogen-oxygen balloon at the upper focus to explode. John Tyndall found it wondrous to have the opportunity to do these experiments at the Royal Institution, since he recalled acquiring the yearning to become a natural philosopher in his youth from the excitement of reading an account of experiments performed by Humphry Davy with the very same mirrors. *Source:* John Tyndall, *Heat Considered as a Mode of Motion*, 6th ed. (London: Longmans, Green and Co., 1880), p. 290. Courtesy of the Science Museum, London.

> asked me if I believed cold susceptible of being reflected? I confidently replied no; that cold was only privation of heat, and that a negative could not be reflected. He requested me, however, to try the experiment, and he assisted me in it.

This new experiment was carried out with a flask filled with snow, and the result astonished Pictet. The thermometer immediately dropped "several degrees" when Pictet placed a flask filled with snow at the focus of the other mirror, as if the snow emitted rays of cold that were reflected and focused at the thermometer. When Pictet made the snow colder by pouring some nitrous acid on it, the cooling effect was enhanced.[10] But how could that be, any more than a dark object could radiate darkness that makes a light dimmer at the other focus? The question seemed frivolous at the outset, but now it had to be addressed seriously.

The situation here is reminiscent of recent philosophical debates surrounding Ian Hacking's argument that we are entitled to believe in the reality of unobservable objects postulated in our theories if we can manipulate them successfully in the laboratory, for instance when we can micro-inject a fluid into a cell while watching the process under a microscope. Hacking advanced a slogan that will be remembered for a long time: "if you can spray them, then they're real."[11] Hacking says that this insight came out of his own experience of overcoming his disbelief in the reality of the positron, the anti-particle of the electron. After learning how positrons can be "sprayed" onto a tiny niobium ball to change the electric charge of the ball (in a modern version of the Millikan oil-drop experiment), Hacking felt compelled to give up his previous notion that positrons were mere theoretical constructs. But what is Pictet's experiment, if not a successful *spraying* of cold onto the thermometer to lower its temperature, just as physicists spray positrons onto a niobium ball to change its electric charge?

Anyone wanting help in denying the reality of cold will have to look elsewhere, since the only answer based on Hacking's "experimental realism" has to be that cold is indeed real. The Edinburgh chemist John Murray (1778?–1820) summed up the quandary arising from Pictet's and some related experiments as follows:

> In these experiments, then, we have apparently the emanation from a cold body of a positively frigorific power, which moves in right lines, is capable of being intercepted, reflected and condensed, and of producing, in its condensed state, its accumulated cooling power; and they appear equally conclusive in establishing the existence of radiant cold, as the other experiments are in establishing the existence of radiant heat.[12]

Pictet was "amazed" by the outcome of his own experiment, which he found to be "notorious." As we shall see in detail shortly, Count Rumford (1753–1814) did take Pictet's experiment as evidence that the radiation of cold was as real as the radiation of heat. One of the implications of this view was that a highly reflective surface would serve to retard the cooling of an object in cold weather, since such a surface would reflect the frigorific radiation impinging on it from its colder surroundings. Never an armchair philosopher, Rumford late in life enhanced his reputation as an eccentric by defying Parisian fashion with his winter dress, which was "entirely white, even his hat."[13] One might wonder whether this actually kept Rumford warm. However, he had tested it by experiments on the cooling rates of hot metallic cylinders with different kinds of surfaces.

Pictet dispensed with the conundrum relatively quickly, by convincing himself that what he was observing was really only heat being radiated away from the thermometer and sinking into the ice; the thermometer loses heat in this way, so naturally its temperature goes down. But it is clear that someone with precisely the opposite picture of reality could give a perfectly good mirror-image explanation: "Heat doesn't really exist (being a mere absence of cold), yet the phenomena could fool us into thinking that it did. When we observe a warmer object apparently heating a colder one by radiation, all that is happening is that the colder object is radiating cold to the warmer one, itself getting less cold in the process." Such a thought did occur to the English polymath Thomas Young (1773–1829), best known today for his wave theory of light, who put the general point as follows in his Royal Institution lectures:

> Any considerable increase of heat gives us the idea of positive warmth or hotness, and its diminution excites the idea of positive cold. Both these ideas are simple, and each of them might be derived either from an increase or a from a diminution of a positive quality.[14]

More systematically, we can discern three different metaphysical possibilities concerning heat and cold, one symmetric and two asymmetric:

(1) Heat and cold (or hotness and coldness) are both real qualities or entities, opposite to each other.
(2a) Heat is a real positive quality or entity, and cold is the lack of heat.
(2b) Cold is a real positive quality or entity, and heat is the lack of cold.

To this list we also should add the reductionist view popular at least since René Descartes and John Locke:

(3) Heat and cold are secondary qualities, merely the way animals register certain configurations of other, primary qualities. (This opens up the possibility that heat and cold are not true opposites at all.)

How can we decide which of these pictures is correct? What we have here is a good instance of the underdetermination problem, much discussed in the philosophy of science: observed phenomena are not sufficient to determine completely the form of the theory that we might construct to explain the phenomena.

In the rest of this paper I will make a thorough examination of the arguments regarding what Pictet's experiment showed about the metaphysical nature of heat and cold. First I will discuss the initial caloric-based attempts to make sense of

Pictet's experiment. Then I will follow the potent challenge posed to the calorists by Count Rumford, who extended Pictet's experimental work to argue that the radiation of cold and heat both should be understood as vibrations in the ether, not as the transmission of material entities. This will be followed by a discussion of the calorist responses to Rumford. In the last two sections I will offer an analysis of the resolution of this controversy, examining the factors that led to Rumford's defeat and the contributions of Rumford's work in the end. In the appendix, I will attempt to indicate how the radiation of cold could be examined again with benefit, after an interruption of well over a century and a half.

My discussion will build on earlier work of physicists and historians, the most significant of which is that of Sanborn C. Brown, who published a biography of Rumford and a modern edition of Rumford's collected works, both of which I have used extensively. In addition, James Evans and Brian Popp have given an informative account of the debate arising from Pictet's experiment.[15] These works, however, do not provide a full and complete account of the controversy on cold radiation. I will examine the central arguments in greater detail, place them in a broader historical context, and give a more thorough analysis of the resolution of the controversy. Moreover, my analysis will proceed from a new perspective, one that is not based on the modern prejudice against the positive reality of cold. This will bring out the considerable persuasiveness of Rumford's position and offer a deeper explanation for its defeat.

The Calorist Interpretations of Pictet's Experiment

To have a true understanding of the reactions of many of Pictet's contemporaries to the apparent radiation of cold, we must recall that most investigators of thermal phenomena at the time worked on the basis of material theories of heat. For convenience, I will use the term "calorist" to refer to all those who considered heat as a material substance (including those who did not use the term "caloric" for it), making further qualifications necessary. There were many variants of the caloric theory, but they all shared the following key assumptions. Caloric is a material fluid, the cause of the sensation of heat and all other thermal phenomena. Caloric is self-repulsive but attracted to ordinary matter (with various degrees of affinity), which explains why heat tends to reach equilibrium, why gases are elastic, and why heated objects expand. Especially among chemists, working in traditions established by Joseph Black (1728–1799) in Scotland and Antoine-Laurent Lavoisier (1743–1794) in France, it was customary to distinguish two possible states of caloric: *latent* (or chemically *combined* with matter) and *sensible* (or *free* from matter). The idea of latent heat explained a large number of phenomena, such as phase transitions and the heat evolved or absorbed in various chemical reactions, though a significant number of calorists disagreed about the chemical interpretation of latent heat.[16]

We can appreciate the calorists' extreme annoyance over the apparent radiation of cold if we note that many of them heartily welcomed radiant-heat phenomena as a direct proof of the existence of caloric. In the early days of the caloric theories, it was usually conceded that caloric in its pure form had not been observed, or indeed that it could never be isolated because of its strong attraction to ordinary matter. However, the observations attesting to heat radiation, namely, the unmediated transmission of heat between distant objects, gave calorists courage. The French mineralogist and priest René-Just Haüy (1743–1822), in his official textbook of physics for the French *lycées*, identified radiant heat as caloric "in itself," or "in its natural form."[17] Even to those who were more cautious about seeing radiant heat as naked caloric, it at least seemed clear that the radiation of heat could be explained nicely as a consequence of a rapid projectile motion of caloric. In fact, there seemed to be hardly any other plausible way of conceptualizing it. The Manchester chemist and physicist William Henry (1774–1836) considered the radiation of heat through a vacuum as conclusive evidence against a kinetic theory of heat:

> Motion is an attribute of matter, independently of which it cannot possibly subsist. If, therefore, the phenomena of heat can be shewn to take place, where matter is not present, we shall derive, from the fact, a conclusive argument against that theory of heat, which assigns motion as its cause. Now, in the experiment of Count Rumford, heat passed through a Torricellian vacuum, in which, it need hardly be observed, nothing could be present to transport or propagate motion. This experiment, in my opinion, decidedly proves, that heat can subsist independently of other matter, and consequently of motion – in other words *that heat is a distinct and peculiar body*.[18]

Similarly, Thomas Thomson opined in 1802 that the long-running dispute between the kinetic and the material theories of heat had been finally resolved by Herschel's discovery of solar heat radiation, which demonstrated that "caloric is not a property, but a peculiar substance."[19]

Radiant *cold* would have been a very unwelcome addition to the calorist ontology. The calorists' exultation about heat radiation as a proof of material caloric would have turned sour at the prospect of having to allow cold radiation as a proof of material cold. It is not only that a theory with two different substances of heat and cold would have been less tidy than a theory with only one substance of heat. If there were a material entity that was the opposite of caloric, it was going to be extremely difficult to fit it into the neat calorist ontology of attractions and repulsions that explained so much. Therefore the calorist imperative was to show that the apparent radiation of cold could be understood somehow as a result of the radiation of heat.

As mentioned earlier, Pictet himself made the first attempt in this direction. In Pictet's theory, "fire" was seen as an elastic fluid,* and temperature was the degree of the "tension" of fire, in deliberate analogy to Alessandro Volta's conception of voltage as the tension of the electric fluid. The tension is proportional to the density

* Pictet's theory of heat was a subtle one combining aspects of materialistic and vibrationist theories, distinct from the French caloric theory that originated from Lavoisier. Still, Pictet's theory was similar enough to Lavoisier's that Pictet (*Essay*, ref. 9, pp. viii–x) worried about being seen as a plagiarist, though he regarded the similarity to Lavoisierian theory as reassuring.

of "liberated fire" accumulated in an object, and inversely proportional to the specific heat of the object. When a relatively hot object is in the vicinity of a relatively cold object, the higher tension of fire in the hotter object causes it to radiate to the colder body; the radiation ceases when the temperatures (tensions) become equal.[20] In the case of the apparent radiation of cold, the fire radiates from the thermometer to the ice; the insertion of the ice at one focus induces the radiation of heat from the other focus, by breaking the equilibrium of tension between the two foci. This was the explanation Pictet gave in 1790 in his *Essay on Fire*, to his own satisfaction.[21]

Although quite plausible at first glance, Pictet's explanation had two serious problems. One was noted by the Scottish natural philosopher James Hutton (1726–1797), best known for his *Theory of the Earth*, which made him the recognized leader of the Plutonist geologists.[22] If radiation starts when equilibrium is disturbed and stops when it is restored, then Pictet's experiment should result in the ice and the thermometer reaching the same temperature. However, as Hutton noted in his *Dissertation upon the Philosophy of Light, Heat, and Fire* of 1794, this is far from the case:

> if a free communication is established, by means of the mirrors, between the cold and hot bodies, as there would be by immediate contact, why is the temperature of the thermometer so extremely little affected? .... 30 or 40 degrees of cold in the matrass does not produce above one half or one fourth of a degree in the thermometer; and this is done almost immediately, after which the thermometer becomes stationary.

As Hutton explained, the reason why the thermometer goes down a little bit and stabilizes there is that the heat being lost by the thermometer to the cold body at the other focus is continually compensated by an influx of heat from the air surrounding the thermometer. Pictet probably would have agreed that there had to be a loss of heat to the air by slow conduction (facilitated by convection), but in Hutton's view this kind of dynamic equilibrium was not compatible with Pictet's static view that fire radiated if and only if the equilibrium of tension was broken.[23]

Hutton used this occasion to develop some tentative ideas he had had previously about the relation between light and heat,[24] to arrive at the following hypothesis: bodies, at however low temperatures, "are always emitting invisible light proportionably to their sensible heat." This was "a species of light, which is reflexible by metallic surfaces, and which has great power in exciting heat." Through the mutual exchange of invisible light between bodies, "their temperatures with regard to heat must be always changing, and always tending to be equalified."[25] Given this picture, it is easy to see how a cold emanation is not required to explain Pictet's experiment. Initially there is a balanced exchange of "invisible light" between the thermometer and the surrounding bodies. Inserting the cold matrass cuts down on the input of invisible light to the thermometer, while the output from the thermometer does not change immediately. Consequently, the temperature reading begins to drop. Hutton thought that Pictet's original experiment was "far from being decisive of the important question, however well it is adapted for raising doubt with regard to the theory of heat and cold." He was confident that appropriate further experiments would confirm his own theory beyond doubt.[26]

Meanwhile, unbeknownst to Hutton, similar developments were occurring in Geneva, where another major difficulty of Pictet's theory was promptly pointed out by one of his friends, the lawyer Louis de Végobre (1752–1840). Végobre noted that Pictet's picture fell apart as soon as each step of the radiation process in the experiment was considered. The heat in the thermometer first would have to be radiated to the mirror on that side, before getting reflected to the other mirror and the ice. But the thermometer and its mirror are initially in equilibrium (at the same temperature), so no radiation could start. If we ignore that difficulty and assume that the radiation somehow begins, the problem only gets worse. The mirror only reflects the radiation that is directed to it, so its temperature would not change; the temperature of the thermometer, however, would be dropping. So, if anything, there should be radiation of fire from the mirror to the thermometer, certainly not in the opposite direction.[27]

The person who rose to the challenge of fixing this defect in Pictet's theory was Pierre Prevost (1751–1839). Like Pictet, Prevost (figure 5) was from a prominent patrician family and played an active role in Genevan politics. Initially trained in theology and law, Prevost made an early reputation as a classicist before his work in physics was widely recognized. After a decade of travel and work in the Netherlands, France, and Prussia, he settled back in Geneva and took up the chair of "rational philosophy and general physics" in the Academy in 1793, becoming Pictet's immediate colleague. Prevost remained in the Genevan Academy until his retirement, but maintained an active connection with numerous scholars in Britain, France, and Prussia. He published actively in various fields including political economy, psychology, and education, in addition to the physical sciences; among other things, he translated into French Adam Smith's *Wealth of Nations* and Thomas Malthus's *Essay on Population*. Prevost was an impressive figure to the end of his long and productive life, when he contributed to the study of ageing by calmly recording observations on the deterioration of his own physical and mental faculties.[28]

Végobre shared his criticism of Pictet's theory with Prevost, thereby stimulating the latter into developing his innovative and influential theory of radiant heat. As in the case of Bertrand urging Pictet to experiment on the radiation of cold, Végobre's role in motivating Provost's work illustrates the importance of informal personal interactions in the close-knit Genevan scientific community at the time.[29] Prevost's theory had important similarities to Hutton's, but Prevost's version gained much wider recognition, perhaps because it basically conformed to the calorist ontology. On the contrary, Hutton's idea of "invisible light" did not fit into any existing conceptual schemes; the notion of infrared light did not become firmly established until the 1840s, despite the retrospective identification of its discovery by Herschel in 1800. In any case, Hutton died three years after the publication of his dissertation on light and heat, and did not have a chance to develop his ideas further.

Although Pictet and Prevost were both believers in material theories of heat, their notions of heat were quite different from each other. Prevost was strongly influenced by his mentor and friend George-Louis Le Sage the Younger (1724–1803), which meant that Prevost considered caloric or fire to be a *discrete* fluid, made up

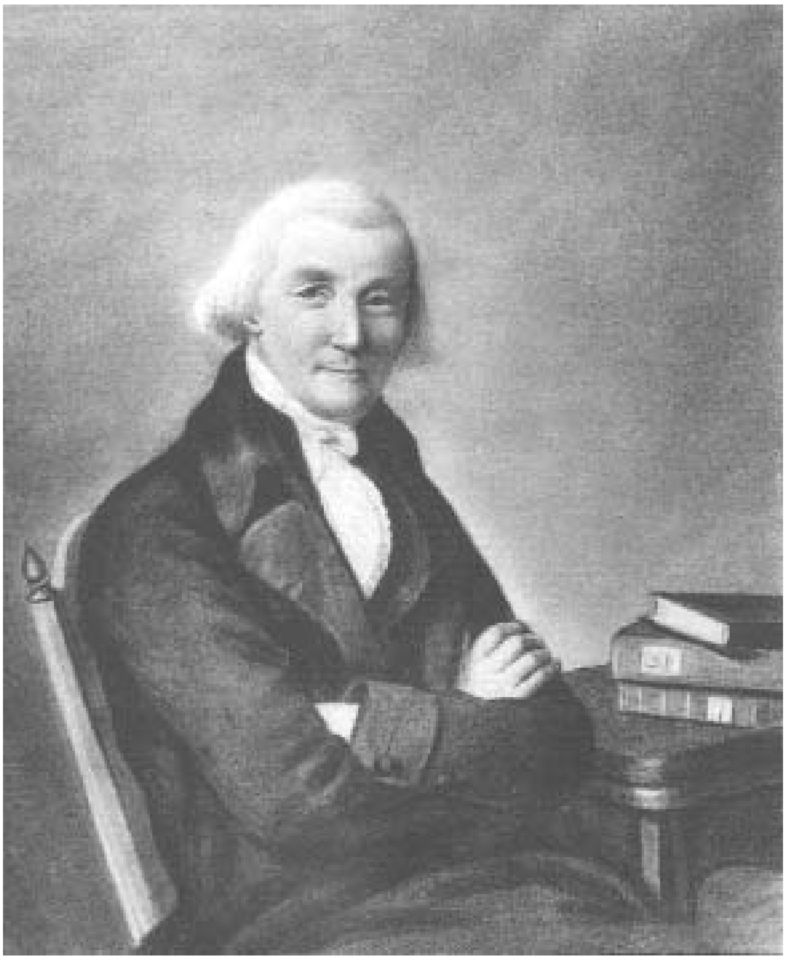

Fig. 5. Pierre Prevost (1751–1839). Courtesy of the Bibliothèque Publique et Universitaire, Geneva.

of point-like particles. Le Sage proposed a theory of gravitation, published in full posthumously (in fact in an edition by Prevost[30]), in which he sought to give a mechanical explanation of the inverse-square gravitational attraction by postulating that space was filled with tiny "gravific particles" everywhere, moving rapidly in straight lines in all directions. Two objects lying next to each other would tend to shield each other from this continual bombardment, with the result that they would be pushed toward each other. Prevost, like many others in Geneva, was quite impressed with Le Sage's theory, and also associated it with Daniel Bernoulli's treatment of air as a discrete fluid.[31]

Now, to avoid the above-mentioned difficulty in Pictet's theory that was pointed out by Végobre, it was necessary to allow the passage of some radiant heat even from a cold body to a warmer one. In other words, Pictet's picture of a static equilibrium of heat needed to be replaced by a dynamic one. This is where Prevost found Le Sage's ideas helpful. In his paper published in the French *Journal de Physique* shortly after the publication of Pictet's book, Prevost postulated that space was pervaded with radiant heat, because every object, at whatever temperature, continually radiated and absorbed heat (which he increasingly identified with the French *calorique*). The rate of emission was proportional to the temperature of a body, and the rate of absorption was proportional to the amount of radiant caloric impinging upon the body from the outside at a given moment. Even when all bodies in a system were at the same temperature, they were still continually exchanging heat with each other by radiation; it was simply that the give and take cancelled each other out, so there were no changes in temperature. This is Prevost's notion of *l'équilibre mobile*, which I will render in English as "dynamic equilibrium." The equilibrium could be broken by introducing a warmer or colder body into the system, or by somehow changing the temperature of a body already in the system; from such disturbances the equilibrium would be restored in due course through unbalanced radiation between the bodies.[32] In his own later appraisal, Prevost's crucial contribution was to view heat equilibrium as a matter of "reciprocal radiation," not as an "equality of tension" as Pictet had done.[33]

Rumford's Challenge

It seems that Prevost's view was immediately accepted quite happily by many others, including Pictet.[34] However, trouble was brewing for Prevost in the form of the eccentric and resourceful Benjamin Thompson (1753–1814), better known (then and now) as Count Rumford. In the entire history of science there have been few careers as colorful and strange as Rumford's. Born in Woburn, near Boston, Massachusetts, young Thompson established himself by marrying Sarah Rolfe (née Walker), a wealthy widow of Concord, New Hampshire. During the American Revolutionary War Thompson ended up serving on the British side, soldiering and spying, eventually being knighted by King George III for his services. In 1784 he entered the service of Elector Carl Theodor of Bavaria, where he reformed the army, rounded up the beggars of Munich into public workhouses, and created the English Garden against much initial opposition. By 1792 the Elector was sufficiently pleased with Thompson to take a rare opportunity to elevate him to the rank of Count of the Holy Roman Empire; Thompson chose to call himself Rumford, after the old name of Concord. Count Rumford (figure 6) spent the decade between 1795 and 1805 moving between Munich, London and Paris. During this period he acquired an international reputation through his numerous technological innovations involving the practical uses of heat (particularly fireplaces and cookery equipment) and his ingenious and careful experiments on heat and light. In 1801 he established the Royal Institution of Great Britain, and then moved permanently to Paris and married Marie Paulze Lavoisier (1758–1836), the widow of the chemist.[35]

The course of Rumford's debate with Pictet and Prevost on the radiation of cold makes a complex and fascinating story. Pictet admired Rumford's work greatly, and Rumford's papers were helpful in generating interest in his new *Bibliothèque Britannique*. Pictet sent Rumford his book probably in 1796, and Rumford was captivated by the experiment on the reflection of cold. He wrote to Pictet:

> You know, I suppose that Doctor Hutton has written a treatise for the express purpose of explaining one of your experiments, – that in which there was an apparent reflection of cold. I was much struck with that experiment, as its result was not only unexpected, but most extraordinary. Your explanation of the phenomena is clear and ingenious yet I cannot help wishing that a matter of so much consequence and which leads to conclusions of such importance in the doctrine of heat might be thoroughly investigated.[36]

Rumford's initial attempt to replicate this experiment met with some difficulties, but he succeeded in 1800 while visiting Hutton's Edinburgh. He reported excitedly to Pictet:

> We repeated your interesting experiment on the reflection of cold, two days ago, at Dr [Thomas] Hope's house, and with complete success … . The slower vibrations of ice in the bottle caused the thermometer to sing a lower note.[37]

Fig. 6. Count Rumford (1753–1814). Portrait displayed as the frontispiece to Pictet's *Voyage de trois mois en Angleterre, en Ecosse, et en Irlande pendent l'Eté de l'an IX* (1801 *v. st.*) (Geneva: l'Imprimérie de la Bibliothèque Britannique, 1802). Courtesy of the British Library, London.

This statement indicates how Rumford was beginning to understand the phenomenon of heat radiation, quite in keeping with his developing notion of heat as vibration. Radiant heat consisted in undulations in an all-pervading ether, set off by the vibrating molecules of ordinary matter.[38]

Rumford's thinking on radiation must have been encouraged by a close affinity with the early ether-wave theory of light being developed by Thomas Young (1773–1829), whom he hired in 1801 as Professor of Natural Philosophy at the Royal Institution, on the recommendation of Sir Joseph Banks (1743–1820), the longtime President of the Royal Society.[39] Young had just published his first paper on the wave theory of light in 1800, in which he employed an analogy between sound and light, and also made a brief approving reference to Rumford's experiments arguing against the caloric theory. Shortly after his appointment at the Royal Institution, Young delivered a Bakerian Lecture before the Royal Society in which he referred to Rumford again and argued that radiant heat consisted in ether-vibrations of lower frequency than light.[40] This was a speculation that Rumford, too, soon would advance in print.[41] It is difficult to assess the direction of influence, if any, between Young and Rumford, but it is clear that by 1804 they shared a view of radiant heat as sound-like vibrations in the ether closely related to light.

According to Rumford's theory of heat, the temperature of a body is a function of the speed of the vibration of its molecules. Each body sends out an undulation in the ether with a characteristic frequency depending on its temperature.* When this undulation reaches another body, it has a tendency to bring that body's vibrations closer to its own frequency. As in the case of light and sound, it is possible to think of these undulations as rays propagating in straight lines unless reflected, refracted, or diffracted. Such a ray will heat up an object that is cooler than itself, and cool down an object that is warmer than itself. Therefore the effect of a given ray is relative: it will be *calorific* or *frigorific*, depending on whether it lands on an object that is cooler or warmer than its source:

> 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.[42]

This gives a perfectly natural picture of how the cold flask in Pictet's experiment causes the thermometer to cool down; in fact, Pictet himself had considered an undulatory theory of radiation as a possibility, and saw it as entirely compatible with his own view.[43] In Rumford's theory, heat radiation and cold radiation are both real phenomena, but neither consists in the transportation of a material substance, and heat and cold are only relative designations. Radiation was an essential part of Rumford's kinetic theory of heat, since it was the mechanism of all communication of heat and cold except for what was effected by the transportation of material molecules themselves in a process later called "convection." According to Rumford, conduction was merely intermolecular radiation in solids, and non-existent in fluids.[44]

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* Evans and Popp note some difficulties with Rumford's use of both velocities and frequencies in this connection; see "Pictet's Experiment" (ref. 15), p. 749.

Note that Rumford seized upon the radiation of cold as another weak spot in the caloric theory that he could exploit in his ongoing crusade against it. By this time Rumford already had argued against the caloric theory by showing that an apparently unlimited amount of heat could be generated by friction (his famous "cannon-boring" experiment, performed while he was in charge of the Bavarian army), and that heat had no appreciable weight.[45] However, the calorists had found easy ways of explaining away both of these facts without modifying their theory. Rumford had not demonstrated that the production of heat by friction was not due to a mechanical release of caloric that had combined chemically with matter. And the failure to detect any weight of caloric only showed that its density was very low, and in any case it did not trouble the calorists that caloric, a "subtle fluid," might well be entirely weightless.[46]

In view of these frustrating experiences, Rumford became hopeful that the radiation and reflection of cold would provide another and more decisive way of throwing the calorist camp into disarray. If heat and cold were metaphysical equals, and if the calorists refused to think of cold as a material substance, then they would be forced to conclude that heat was not a material substance either. What he was learning about cold radiation fitted conveniently with his growing conviction 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."[47] Rumford focused almost exclusively on radiation phenomena with frigorific rays at center stage when he was given a high-profile opportunity to strike against the calorists in a public session of the French National Institute in 1804.[48]

Rumford had a clear view of his target. In his most definitive paper on heat radiation, presented earlier in the same year to the Royal Society of London, he had stated that Prevost's theory was "one of the most plausible" of the calorist explanations of Pictet's experiment.[49] Rumford thus set out to devise some stunning further experiments that seemed to provide conclusive evidence against Prevost. The combination of ingenious instrumentation and forceful, uncomplicated reasoning shown in these experiments was typical of Rumford's work at its best.

One series of experiments utilized Rumford's knowledge of the effect of surface quality on radiation. In 1802 he had chanced to notice that different qualities of surfaces had marked effects on the cooling rates of bodies.[50] This led him to much further investigation, spurred on by the prospect of applying the results to the design of clothing.[51] Rumford concluded from these studies that surfaces reflected and absorbed frigorific and calorific rays in much the same way as they reflected and absorbed light. There were three notable aspects: (1) darker and duller surfaces tend to reflect radiation less efficiently; (2) what is not reflected is absorbed, and causes a temperature change in the absorbing body; and (3) surfaces that are better absorbers (poorer reflectors) are also better emitters of radiation.[52] These observations led Rumford to a crucial experimental procedure: blackening a surface enhances its radiative effects. If the blackening of a surface results in an increased heating effect, the causal power of the surface (relative to the receiving end) is shown to be calorific; if, however, blackening results in an increased cooling effect, the causal power involved must be frigorific.

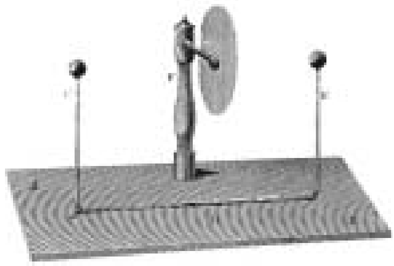

Fig. 7. Rumford's thermoscope. The horizontal connecting tube was 15 to 16 inches in length; at both ends the tube was curved upwards, with the bulbs 6 to 7 inches higher than the horizontal portion. The index was a drop of spirit (alcohol). *Source: Philosophical Transactions of the Royal Society of London* **94** (1804), plate 4 (adjacent to p. 182), figure 2. Courtesy of the Royal Society.

In these experiments Rumford was helped enormously by the "thermoscope," an instrument of his own invention, which we would now describe as a differential thermometer.* As shown in figure 7, it consisted of two small glass bulbs connected by a long and narrow glass tube, bent upward at both ends; in the middle of the connecting tube was an indicator or an "index," a small drop of colored liquid. When the air in one bulb became warmer than the air in the other one, it gained higher pressure and pushed the index away from it. This was an extremely sensitive instrument; Rumford reported that holding up a hand at about 3 feet from one of the bulbs resulted in a sensible movement of the indicator.[53] Rumford employed the thermoscope in two different ways. First, he would set two sources of radiation at the two bulbs of the thermoscope, to see which had greater effect. For instance, he filled two identical brass cylinders with hot water (both at 180°F), and set each next to a thermoscope bulb. When the distance between the cylinder and the thermoscope bulb was the same on both sides, the thermoscope index did not move at all, indicating an exact balance of radiative effects; when one cylinder was placed nearer to the bulb, the index moved away from that side. When he took this setup at equilibrium and blackened one of the cylinders, the equilibrium was promptly broken, indicating a superior calorific effect from the blackened surface.[54]

Now Rumford came to a crucial point: What happens to the radiative equilibrium set up with two cylinders filled with water much colder than the ambient temperature, when one of the cold cylinders is blackened? To his delight Rumford found that the index of the thermoscope moved *toward* the blackened cold cylinder, indicating enhanced cooling. How else could this be explained, except by saying

* The Scottish physicist John Leslie (1766–1832) had invented a very similar instrument, which he called a differential thermometer and described in detail in his influential treatise, *An Experimental Inquiry into the Nature and Propagation of Heat* (London: Mawman; Edinburgh: Bell and Bradfute, 1804), pp. 409ff. This and other similarities in Rumford's and Leslie's works raised questions of priority and perhaps even plagiarism, voiced particularly in a hostile review of Rumford's work in the *Edinburgh Review* **4** (1804), 399–415. Rumford did his best to dispel any suspicions, by giving a detailed account of the order, timing, and motivations of his own experiments, at the end of which he concluded that he and Leslie made independent simultaneous discoveries, and had "not borrowed one from the other in the slightest degree." See Rumford, "Historical review" (ref. 37), pp. 492–493.

that "the frigorific rays from the blackened surface were more powerful in generating cold than those which proceeded from the naked metal"?[55]

In the second method of employing the thermoscope, as shown in figure 8, Rumford took one bulb of the thermoscope and exposed it simultaneously to the influence of two metallic cylinders (meanwhile shielding the other thermoscope bulb). The two cylinders were placed on opposite sides with the thermoscope bulb exactly in the middle. One of the cylinders was hotter than the thermoscope bulb (initially set at the ambient temperature), the other one colder; the temperature differential on either side was the same (ambient temperature at 72°F, hot cylinder at 112°F, cold cylinder at 32°F). The result of this experiment was that the thermoscope did not show any movement. This, for Rumford, exhibited the balancing of the simultaneous effects of the calorific and frigorific rays: "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 temperature of neighbouring bodies." And as expected, when the hot cylinder was blackened the equilibrium was broken as the heating effect on that side was increased.[56]

Rumford admitted that Prevost's theory could explain these results plausibly enough. In the equilibrium case, we can suppose that the thermoscope bulb suffers a net loss of caloric in its exchange with the cold cylinder, which exactly cancels the gain from its exchange with the hot cylinder. If the hot cylinder is blackened, that would increase the caloric input from there to the thermoscope bulb. However, Rumford presented a further experiment that he declared inexplicable in Prevost's terms.

This crucial experiment,* which I will refer to as the "blackening experiment" in subsequent discussions, consisted in blackening both the hot and cold cylinders in the above setup. When this was done, the equilibrium was not disturbed. Rumford now announced triumphantly:

> 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 [hot] disk B was *increased* by the blackening of the surface of that disk, we must conclude that the intensity of the radiation of the [cold] disk A was likewise *increased* by the use of the same means; but if those radiations be *caloric*, emitted by those bodies ... , 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 [cold] disk A, was actually *more cooled*?

Rumford anticipated that the advocates of Prevost's theory might try to get around this difficulty by arguing "that the blackening of the surface of the [cold] disk A caused a greater quantity of caloric to be sent off to it by the ball of the thermoscope." However, he dismissed this possibility quite quickly, giving two

* Rumford's crucial experiments were, of course, only as conclusive as any other crucial experiments –that is to say, less than watertight.

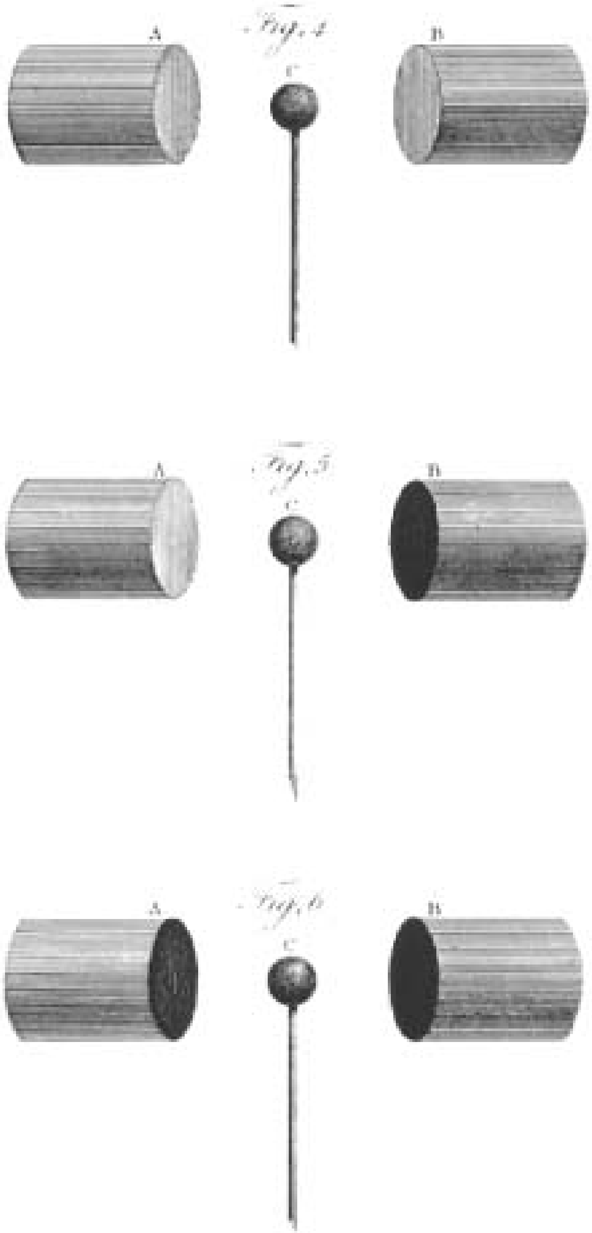

Fig. 8. Hot and cold cylinders working simultaneously on a thermoscope bulb situated between them. *Source: Philosophical Transactions of the Royal Society of London* **94** (1804), plate 5 (adjacent to p. 182), figures 4–6. Courtesy of the Royal Society.

reasons. First, it is difficult to conceive of any mechanism for such an effect. (Here Pictet's theory might have had an advantage over Prevost's.) Second, if the blackening of a neighboring surface should induce a greater rate of caloric emission from the thermoscope, then the same effect must be induced by the blackening of the hot cylinder as well as that of the cold cylinder; therefore it is not clear what help this presumed effect would give to Prevost. On Rumford's own theory there were no such difficulties; the blackening of any surface increases the strength of radiation, at whatever frequency; the effect of the radiation, whether it be calorific or frigorific, will be increased.[57]

Perhaps too vigilant by this time to rest with one argument against the calorists, Rumford added one more crucial experiment to his arsenal. This experiment, which I will refer to as the "speaking-tube experiment," was clearly motivated by the analogy between sound and radiant heat, which Rumford and Young shared, as mentioned above. Rumford summarized the design and result of the experiment, illustrated in figure 9, as follows:

> 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

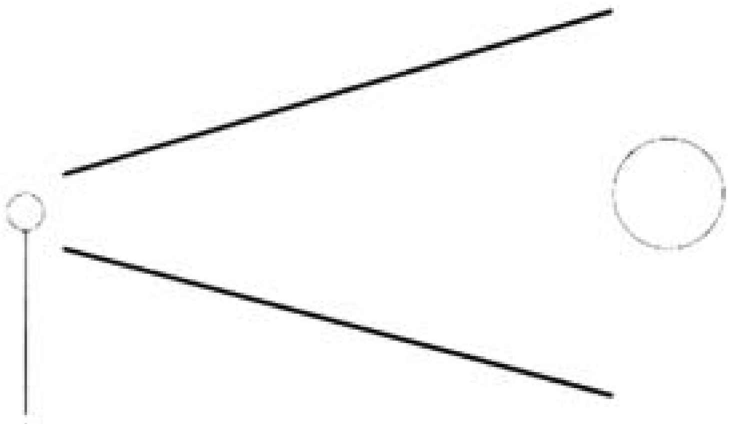

Fig. 9. The speaking-tube experiment, a cross-sectional view. The circle on the right represents the copper ball filled with ice, and the smaller circle on the left the thermoscope bulb. The two slanted lines between the circles represent the speaking-tube, which is in the shape of a truncated cone.

spoke at the larger opening of the speaking-tube while the bulb of the thermoscope *listened* at the smaller opening.[58]

It is easy to understand the result of this experiment if we think in terms of real frigorific undulations being concentrated by the speaking-tube. On Prevost's theory, however, one would be forced to say that the interposition of the cone either caused the thermoscope bulb to radiate out more caloric, or somehow decreased the amount of radiant caloric impinging on the thermoscope bulb. Either alternative would seem quite implausible at first glance.

Thus having satisfied himself about the existence and nature of calorific and frigorific radiations, Rumford proceeded to consider some practical implications. These included some straightforward advice on the design of cookware and heating systems, but perhaps most striking were his reflections on how human bodies stay warm or cool. The new theory of radiation allowed him to understand some basic facts of human existence. Why do people living in warmer climates have darker skin? Rumford noted that in very hot climates, there is urgent need for human bodies to cool down, to lose the heat continually produced in the lungs. The surroundings typically still will be somewhat cooler than the human body temperature, especially at night and indoors, so there will be frigorific rays continually impinging on the body. The design of the body's surface should be conducive to the absorption of these frigorific rays; dark skin is the logical solution here. But what about the daytime, especially outdoors with the sun shining? Rumford cheerfully noted: "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."

In cold regions, the overwhelming need would be to keep warm by reflecting away as many frigorific rays as possible. Lighter skin would be a good start, and Rumford noted that the reported custom of certain "savage tribes" (identified as Laplanders in one place) who "besmear their skins with oil" should be understood as an admirable device designed to further increase the reflectivity of their skin. Clearly feeling apologetic about appearing to attribute too much rationality to these natives, Rumford quickly turned his admiration to God, who gave them the wise custom whose true rationale they could not possibly understand. His fellow Europeans, however, should put this knowledge to conscious use. Coming back to considerations of clothing, he insisted 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." That reflection resulted in the following counter-intuitive recommendation: "as a white surface reflects more light than an equal surface, equally polished, of any other colour, [and therefore it also would be the best reflector of frigorific radiation], there is much reason to think that white garments are warmer than any other in cold weather."[59] I have already mentioned how Rumford put this recommendation into practice in his own life.*

* Interestingly, Rumford's recommendation has been put to use in the late 20th century. The "Space Blanket," developed in 1964 for use by NASA for the thermal insulation of spacecraft and now marketed commercially by MPI Outdoors, is a shiny membrane made by depositing aluminum vapor onto a very thin film substrate. As the advertisement on the packaging states, "in earth's atmosphere the Space Blanket has provided many millions of users worldwide the comfort, warmth, security and safety

In all of the above observations, Rumford would seem to be blind to the most natural explanation of cooling rates. Should he not have seen that a white coat would keep him warm by making him radiate less heat, rather than by reflecting away frigorific rays? After all, Rumford himself had shown experimentally that poor absorbers of heat were also poor emitters. But he insisted on saying that "the cooling of a hot body is effected solely by the rays which proceed from colder bodies," and sought to show by experiments that cooling was not the result of an emission.[60]

All of this seems odd only because we mistakenly tend to read a modern energy-conservation principle into Rumford's kinetic theory of heat. Although he did make a vague statement that "the sum of the active forces in the universe must always remain constant,"[61] he did not see a connection between the emission of calorific rays and any changes in the temperature of the emitting body. After all, such a connection could not have been supposed coherently by Rumford, since whether the rays were calorific or frigorific depended on the temperature at the receiving end. For Rumford the mechanisms of temperature change were agitation or calming by an external influence, whether it be a mechanical impact or an ethereal undulation. A body creating undulations in the ether did not thereby suffer a change in its own state of vibration.* In this respect, the modern kinetic theory of heat with its principle of energy conservation has closer kinship with the material theory of heat with its principle of caloric conservation, than with Rumford's kinetic theory.

The Defence of Calorist Interpretations

Rumford's new experiments clearly presented a problem to the calorists. Given Rumford's high reputation as an experimenter, it would not have been the most plausible strategy to question the validity of his results. Moreover, Rumford personally demonstrated the blackening experiment to Pictet and his colleagues in Geneva in 1803. The speaking-tube experiment was demonstrated in 1804 at the National Institute of France in the presence of Pierre-Simon Laplace (1749–1827),

of personal *thermal reflectance against the cold* or in the aftermath of a natural disaster to prevent post-trauma shock." The manufacturer, of course, hastens to add that the blanket works because it "captures and helps to *retain* and focus over 80% of *a person's radiated body heat*" (emphasis added).

* It is interesting to note that Rumford's rival John Leslie thought that it would. However, Leslie would only embarrass the modern kinetic theorists, since he used this reasoning as one of his major *objections* to the kinetic theory: "Admitting that hypothesis [that heat consists in the vibration of material particles] to be real, all heat must gradually relax and die away: for this is the fate of every species of motion experienced upon earth; and with regard to the intestine motions in particular, they suffer such manifold obstructions, and are attended with such waste of power, that they speedily terminate." Thus Leslie perceived the prospect of the "heat death" of the universe, and turned away in horror from the gates of future thermodynamics. Generally, he despised the vibrationist theory as something from "the infancy of science," a "shapeless hypothesis" that "explains nothing," a seductive idea that "throws out a delusive gleam, and then leaves us in tenfold darkness." See Leslie, *An Experimental Inquiry into the Nature and Propagation of Heat* (London: Mawman; Edinburgh: Bell and Bradfute, 1804), pp. 139–141.

Claude-Louis Berthollet (1748–1822) and Jacques A. C. Charles (1746–1823), and also replicated in Geneva by Pictet on Prevost's request.[62] So Rumford's facts were undisputed, but his conclusions were resisted strenuously by the calorists. Prevost was greatly disturbed by Rumford's challenge; his journal exhibits a tormented period of ten and a half months during which he made all sorts of ingenious attempts to come to terms with Rumford's experiments.[63] In this section I will present and evaluate the defence of the caloric theory against Rumford's new experimental arguments, mostly in terms of Prevost's theory.*

Both of Rumford's crucial experiments were designed to support the following scheme of argument. In each case Rumford devised an enhancement technique: a contrivance (a speaking-tube, or blackening) to increase the effect of radiation. The concave mirrors in Pictet's original experiment also constituted such an enhancement technique, but it created an ambiguity in interpretation because the setup was symmetric between the cold body and the thermoscope. By contrast, Rumford's techniques were designed to avoid enhancing the effects of any emanations from the thermoscope, allowing one to obtain a clearer view of the nature of the radiation coming out of the cold source. According to Prevost, what emanates even from a very cold body is caloric, so the result of the enhancement will be to increase the heat sent to the receiving end. If, however, what the cold body emits is a frigorific influence, then the enhancement will result in more cooling at the receiving end. The demonstrated increase of cooling in Rumford's experiments showed that the emanations from the cold body are frigorific, not calorific.

In itself, the above argument is unanswerable. Therefore the calorist strategy was to argue that there were factors left out in Rumford's reasoning. More specifically, the calorists argued that Rumford's enhancement techniques had unintended side effects that cut down on the caloric input to the thermoscope. In the case of the speaking-tube, the side effect was the shielding of caloric radiation coming from the rest of the environment. In the case of blackening, the side effect was the decrease in reflectivity, cutting down on the amount of caloric coming from elsewhere that the cold surface reflects to the thermoscope. Therefore, according to the calorists, in each case Rumford's enhancement technique resulted in an unintended *decrease* of caloric input to the thermoscope. Let us now examine these responses more carefully.

In countering Rumford's blackening experiment, Pictet took a first and gentle step in his summary of Rumford's 1804 Royal Society paper, "Inquiry Concerning the Nature of Heat, and the Mode of its Communication," in the *Bibliothèque Britannique*. Pictet argued that Prevost's theory was compatible with Rumford's results, if one considered that the cold cylinder also reflected caloric rays back to

* Although there were some who followed Pictet's original theory, Prevost's was the one that won general approval, and the most careful arguments against Rumford were couched in Prevost's terms. As for Hutton's theory, it seems to have had few followers, and only in Scotland. Thomas Thomson, in his *System* (ref. 6), vol. 1, p. 340, initially thought that Hutton's theory was needed to explain Pictet's experiment on cold; however, later Thomson, in his *Outline* (ref. 46), p. 161, cited Prevost's theory as the correct explanation. John Leslie was generally a follower of his friend Hutton on heat; however, Leslie insisted that radiation of heat (and cold) was effected by vibrations of the air.

the thermoscope, and that this reflection must be diminished by the blackening of the surface. He closed his discussion with an air of curiosity and openness, admitting that both Prevost's and Rumford's explanations worked perfectly well, hence only concluding that this new experiment did not decide conclusively between the competing theories.[64]

Prevost himself promptly entered the fray with a series of three papers published in the *Bibliothèque Britannique*. He started with an explanation of the blackening case that was similar to Pictet's.[65] An additional factor that came to Prevost's aid here was, ironically, Rumford's own experimental discovery of the positive correlation between emissive power and absorptive power. Blackening the cold cylinder does increase the caloric emission from it, but it also increases the amount of caloric absorbed by it; when it is all added up, the overall effect of blackening could be an actual decrease in the net amount of caloric radiation sent to the thermoscope from the cold cylinder. Prevost put this point succinctly in 1815: when we have equally cold objects with different surfaces, "it is known that the blackened body will soonest acquire the temperature of the place [environment], and therefore will sink the [nearby] thermometer most powerfully during the time of its heating."[66] These qualitative arguments created a breathing space for Prevost's theory, though there was no guarantee that they would work out quantitatively.*

* In their modern commentary, James Evans and Brian Popp state that there is "no difficulty in explaining the thermoscope experiment in terms of a material theory of heat" if we take reflected radiation into consideration. Then they proceed to offer a qualitative explanation that is much like Pictet's and Prevost's. See Evans and Popp, "Pictet's Experiment" (ref. 15), p. 748, and note 47 on p. 753. Drawing on Evans and Popp's scheme, and also incorporating some aspects of a rough quantitative account given by Prevost in 1809 in *Du Calorique rayonnant* (ref. 27, pp. 127–130), I have constructed the following simplistic quantitative explanation. The reasoning below is also based on Prevost's conception of the nature of reflective surfaces, to be explained shortly in the text.

Let us start by assuming that the inner portion of the hot cylinder sends to its surface an amount of caloric denoted by H_h in unit time; likewise, the cold cylinder sends to its surface a certain amount of caloric denoted by H_c. At the same time, the thermoscope bulb sends out to the cylinders a certain amount of heat; call this amount 2H, with H going towards each cylinder. Suppose for the moment that the reflectivity of the cylinder surfaces is zero, so that the whole amounts H_c and H_h go through the surface and are emitted. Also assume that these amounts are all absorbed by the thermoscope. Then at equilibrium (when the thermoscope stays at the same temperature), we must have $2H = H_h + H_c$, because the net rate of absorption of caloric by the thermoscope bulb should be zero. This equality should hold for all cylinder surfaces, since the nature of the surface will not affect H_h and H_c. Now consider the case in which the surfaces have some degree of reflectivity denoted by r. From the hot cylinder the caloric sent out to the thermoscope would be $(1 - r)H_h + rH$, the first term being the proportion of H_h that gets emitted through the surface, and the second term being the amount of caloric originally emitted by the thermoscope but reflected back to it by the cylinder. Similarly, the caloric input from the cold cylinder would be $(1 - r)H_c + rH$. Adding the two, we get a total of $(1 - r)(H_h + H_c) + 2rH$ as the caloric input to the thermoscope. But as we established earlier, $H_h + H_c = 2H$, so the total reduces to 2H, independently of r. Therefore, if equilibrium holds at one degree of reflectivity, it should hold at all degrees of reflectivity. That is as shown by Rumford's blackening experiment.

However, this account has serious shortcomings (in addition to the obvious idealizations). First, it is a highly questionable assumption that all of the caloric reflected from each cylinder goes back to the thermoscope bulb. Second, it was assumed that the thermoscope bulb absorbs all the caloric directed to it, which is not likely. Moreover, to be consistent with Prevost's treatment of the speaking-tube case, one must take into consideration radiation from all sources that can reflect off the blackened cylinder

However, even qualitatively, Prevost could not answer Rumford completely without departing significantly from Lavoisierian caloric theory. Prevost was mindful of a general complaint that Rumford had voiced about the basic idea of dynamic equilibrium of caloric: "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." This Rumford regarded as "an operation not only incomprehensible, but apparently impossible, and to which there is nothing to be found analogous, to render it probable."[67] This conceptual problem became even more acute in explaining the effect of blackening a surface. In the framework of Rumford's theory it is not so difficult to imagine that blackening enhances the coupling between the material objects and the ether, so that the calorific and frigorific vibrations are both more readily emitted and also more readily absorbed. But such a mechanism would not have been available for calorists. The typical calorist explanation of an increase in the absorptive power would have postulated an enhancement of the attractive force of the surface for caloric, but such a stronger caloric-matter bond should have resulted in a *decrease* of emissive power.

Prevost's solution to this thorny problem was to abandon force-based explanations of the absorption and emission of caloric from bodies. Prevost realized that surfaces of objects could be treated as simplistic barriers that acted in the same way in both directions; this seems to have been the most crucial insight in Prevost's struggle to satisfy himself that Rumford's experiments did not refute his theory. Schematically, any surface could be modelled as a grille, made up of bars that are perfect reflectors allowing no passage of caloric in either direction; the spaces between the bars would allow completely free passage of caloric; the reflectivity of a surface then would be a straightforward function of the proportion of the surface area covered by the bars. Later on Prevost proposed to model surfaces as dotted by microscopic holes that allow free passage of caloric and also shape the flow of caloric into ray-like bundles.[68] In these models the correlation between absorption and emission became axiomatic, since both were defined as non-reflection, one internal and the other external.* This was a dramatic metaphysical shift, forgoing any attempt to explain why caloric was retained or repulsed by bodies in the first place. Prevost's new model of surfaces provided a credible counter to Rumford's criticism, but only at the expense of a very significant departure from the earlier traditions of the caloric theory, as I will discuss further in the final section.

Prevost now turned to Rumford's speaking-tube experiment. Initially this case plunged Prevost into confusion. Attempting to reach a general understanding of the functioning of reflectors, he predicted that even in a uniform-temperature environ-

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on to the thermoscope bulb. These include not only the radiation from the thermoscope bulb itself, but the radiation from the environment, as well as the radiation from the cylinder on the other side. Another non-negligible factor is the radiation emitted and absorbed by the air standing between all other sources and receivers of radiation.

* Interestingly, Rumford had come up with a similar way of understanding internal reflection as a hindrance to emission, in his own attempt to understand the connection between absorptive and emissive power. See Rumford, "Research on Heat: Second Memoir" (ref. 60), p. 439.

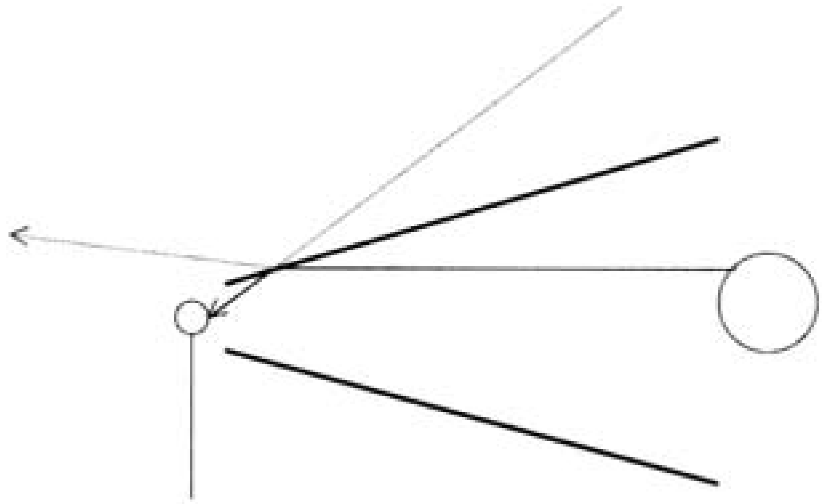

Fig. 10. The speaking-tube's shielding action. The thin solid line from the cold object to the thermoscope represents a caloric ray reflected by the speaking-tube toward the thermoscope. The broken line represents a corresponding ray of caloric, which is "warmer"; the latter would have reached the thermoscope, but is reflected away from it by the speaking-tube.

ment the insertion of a speaking-tube would lead to a rise in temperature in a thermometer that is placed next to its narrow end.[69] Alas, no such effect was detected when Pictet performed this experiment for Prevost. This experimental refutation disturbed him greatly but, with the help of a certain eminent physicist whom he declined to name, Prevost managed to modify his theoretical treatment of the case appropriately in the second installment of his three-part paper.[70] In his new treatment Prevost pronounced the following general principle: "According to the theory of dynamic equilibrium, in an environment of uniform temperature a reflector does not change at all the temperature of bodies exposed to its influence."[71] The action of the reflector directing a ray of heat towards an object that would otherwise not have received it is exactly matched by the action of the other side of the same reflector blocking a ray of heat that would have reached the object otherwise. In a uniform environment, the added ray and the blocked ray would be exactly equal in their caloric contents, so the overall effect of the reflector is nil.*

Now Prevost argued: in Rumford's speaking-tube experiment with a cold body at the wide end, the overall amount of caloric impinging on the thermoscope bulb indeed will be decreased. As illustrated in figure 10, the speaking-tube reflects away some of the caloric from the surroundings that would have reached the thermo-

* The exactness of the compensation required a proof. In Rumford's theory there was no such need, since it was axiomatic 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"; see Rumford, "Reflections" (ref. 42), p. 313. In any case, Rumford had already obtained an experimental confirmation of this proposition in his "Inquiry" (ref. 41), Experiment 16, p. 362, but Prevost apparently had not been aware of Rumford's results.

scope otherwise. This loss of heat to the thermoscope more than compensates for the small gain occasioned by the concentration of the feeble caloric radiation from the cold body.[72] The geometry of the reflections is the same as in the case of uniform environment, but this time what is added to the total caloric input to the thermoscope will be rays originating from the cold body, which contain less caloric than the subtracted rays, which originate from the warmer surroundings. This account of the speaking-tube case was only qualitative, but at least Prevost succeeded in creating a logical living space in which his theory could survive while awaiting a more quantitative treatment. That was the same situation as with the blackening experiment. The most important point is that Prevost had avoided a clear *qualitative* refutation of his theory, which was what Rumford had intended.

It may seem like an *ad hoc* move on Prevost's part suddenly to invoke the caloric radiation from the environment. It terms of motivation it certainly was, since he had not considered this factor in his initial explanation of Pictet's experiment, nor in his explanation of Rumford's blackening experiment. As we have seen, Prevost called in the environmental radiation only after a failed prediction forced him to revise his initial attempt to explain away the result of the speaking-tube experiment. However, in another sense this was not an *ad hoc* move at all. Recall the original inspiration for Prevost's theory, namely, Le Sage's theory of gravitation. In that theory it is absolutely essential that the entire universe is pervaded by the randomly moving gravific particles. Similarly, if we follow the logic of Prevost's theory, a true equilibrium of caloric for a body means that its caloric output is matched by the caloric input from *all possible sources*, including objects in all directions and even the surrounding air and vacuum. In other words, Prevost's early application of his own theory had been careless, but there was no fault to be found in his new analysis.

The Paris physicist Claude Pouillet (1790–1868) expressed the same idea in the following remarkable statement:

> A thermometer always exists in an enclosure … ; when it is outdoors, it is the surface of the earth, the clouds and the sky that constitute a more vast and more irregular enclosure, every point of which still radiates heat onto the thermometer and similarly receives heat from it.[73]

This idea was not so fanciful as it might seem. When the American-born British physician William Wells (1757–1817) used Prevost's theory to understand the formation of dew, the key factor he considered was how much caloric terrestrial objects were radiating away into the open skies. For this brilliant application of Prevost's theory, Wells received the Rumford Medal of the Royal Society. There may be a good deal of irony in that, but in fact Rumford himself had considered the radiative effects of the cosmic environment when he attempted to explain the coldness at high altitudes and the abundance of frost on clear nights by reference to the ease with which frigorific rays arrive "from every part of the heavens." Later the influential work of Joseph Fourier (1768–1830) on heat transmission made it axiomatic that radiant heat phenomena should only be considered in an enclosure.[74]

Why Did Rumford Lose?

Prevost's 1804 paper was the decisive contribution on the subject of cold radiation.[75] The ideas in that paper, variously extended and elaborated by Prevost himself and others, eventually won nearly universal agreement among physicists. A case that is significant in itself and also illustrative of general trends is Haüy's textbook of physics, mentioned briefly earlier. This was a school text commissioned personally by Napoleon in 1803, soon adopted as a recommended text in the *Ecole Polytechnique* as well, and translated into English within a few years. Having given Pictet's explanation of the reflection of cold in the first edition, Haüy replaced it by a succinct exposition of Prevost's work in the second edition.[76] Between the two editions he had studied Prevost's work with the assistance of Laplace and Jean-Baptiste Biot (1774–1862), and also Berthollet and Joseph-Louis Lagrange (1736–1813). At the same time Haüy also studied Rumford's works, and concluded that Prevost's theory was superior.[77]

Crucial to Prevost's success was the endorsement of the powerful Laplacian school in Paris. When Prevost published an extended version of his theory in 1809, as *Du calorique rayonnant*, Biot promptly gave it a favorable and extended review, signalling continuing support from the Laplacians.[78] Laplace himself built on Prevost's conceptions, as I will discuss further in the next section. In addition to the Laplacians, Fourier also was very favorably impressed by Prevost's theory. Fourier explicitly endorsed Prevost's explanation of cold radiation, incorporated Prevost's key ideas into his mathematical theory of the movement of heat, and made improvements in Prevost's analysis of surfaces.[79] While Fourier and the Laplacians sustained a bitter dispute with each other that arose from their very different theoretical outlooks, they all adopted and applied Prevost's dynamic equilibrium of heat. Impressed by these authorities, numerous other texts endorsed Prevost's theory without adding anything, often simplifying the discussion with significant loss or distortion of content.[80]

Of all of the post-1804 textbooks and papers I examined, not a single one endorsed Rumford's idea of frigorific radiation completely.* A small number of authors did manage to publish objections or doubts about Prevost's explanation of cold radiation,[81] but even these authors were skeptical of Rumford's explanation. The chemist John Murray was unyielding in his complaint that Pictet and Prevost ascribed too much effect to reflection and too little to emission itself, but he was a committed calorist and did not endorse Rumford's view of heat.[82] The Scottish mining engineer Henry Meikle, who wrote prolifically about heat, regarded the apparent reflection of cold as a prominent case showing that the nature of heat was "still in darkness." He discussed Rumford's speaking-tube experiment in particular, and offered a geometric solution that did not appeal to the shielding of environmental radiation. So Meikle rejected Rumford's explanation as well as Prevost's. In

* One person who might have been expected to follow Rumford is Thomas Young, whose theory of heat was very close to Rumford's, as discussed above. However, on the radiation of cold his textbook reported Prevost's argument uncritically, without a mention of Rumford. See Young, *Course of Lectures* (ref. 14), p. 638.

fact, he ended his paper by expressing his hope that by means of his own explanation "the paradox may be solved without the sorry aid of frigorific rays, which certainly deserve no encouragement in our northern climate. How soon they might be followed by a kindred system of *tenebrific* [darkness-causing] rays is not easy to say."[83]

Perhaps the person who followed Rumford most closely in using radiation to criticize the caloric theory was the English physiologist and physician Marshall Hall (1790–1857). Hall published a paper in 1811 that advocated a vibration theory of heat, in which he gave a thorough and careful comparative assessment of the material and kinetic theories of heat.[84] Hall took due notice of Rumford's experiment on the generation of heat by friction. Like Rumford, he believed that heat consisted in vibration, and he was quite willing to postulate an all-pervading ether to transmit radiation, remarking that it was at least no worse than postulating caloric. Hall also regarded the positive correlation between a body's propensities to emit and absorb radiation as a major difficulty for the caloric theory. And Rumford himself could have written the following:

> It is however in the radiation of *cold*, I conceive, that we have the most forcible and direct objection to the hypothesis of material caloric … . It is scarcely necessary to say, that no unexceptionable explanation of this phenomenon has been proposed. According to Prevost's supposition, the effect of radiation from a cold surface ought in reality to be that of heating, and not of cooling the opposed thermometer.

But even Hall offered some mild criticism of Rumford's explanation, saying that it did not take into account the influence of the ambient air, and that his ideas did not really explain the observed differences between solar heat and terrestrial radiant heat.[85] At that time Hall was a young medical student in Edinburgh absorbed in chemistry, and there is no indication that his precocious views on the nature of heat had much effect on any others, or influenced his own later distinguished career in medicine.[86]

All in all, although Prevost failed to secure a universal consensus for his theory, it must be admitted that Rumford's theory of heat and cold radiation suffered as complete a defeat as ever befell a scientific idea that was once entertained seriously by respectable scientists. Perhaps satisfied by the general acceptance of his own theory, Prevost made hardly any mention of Rumford by the time he published his last major elaboration of the theory of radiant heat in 1832. All the same, the discussion of the controversy presented in the last two sections above does not indicate any convincing advantage on Prevost's side against Rumford's. That clearly raises a historical puzzle: why did Prevost defeat Rumford so unequivocally? I will examine several possible answers.

(1) The first possibility, which I will reject unequivocally, is that Rumford's view on cold radiation was simply ignored or dismissed by his opponents without due consideration. The previous section should constitute a sufficient argument against this line of thought. There is no mystery here, as Rumford was a difficult man to ignore in the years around 1800. His philanthropic, technological, and scientific works were extremely well known throughout Europe. The caustic critique of his

work in the *Edinburgh Review* in 1804 complained: "The merits of Count Rumford, too, have been so much a theme of conversation, and have had such an active influence in the fashionable world, that it is proper his pretensions should at length be sifted."[87] The leading calorists were for the most part respectful, patient, and often quite cordial towards Rumford. When they disagreed with his theoretical ideas, it was almost always with a sincere acknowledgment of his experimental achievements.*

Specifically, there was great admiration for Rumford's work in the strongholds of Prevost's theory, namely, Paris and Geneva. Rumford's first visit to Paris in 1801 was a flattering experience. He received special attention from Napoleon, hobnobbed with Laplace, Berthollet, and other leaders of the French scientific community, got elected to the National Institute of France, and spent much pleasant time courting Madame Lavoisier.[88] Within a few years he found himself settling permanently in Paris. In Geneva even the poor would have been familiar with Rumford, thanks to one of his inventions, the soup kitchen; tickets were reportedly stamped with Rumford's name and portrait.[89] Pictet was a close personal friend as well as a great admirer of Rumford, and happily propagated Rumford's ideas. The admiring friendship was mutual, as amply demonstrated in Rumford's letters to Pictet. Prevost did not have a personal relationship with Rumford, but he was one with Pictet in acknowledging the general merits of Rumford's scientific work.[90] In short, Prevost and his advocates regarded Rumford as a worthy opponent at the height of the debate about radiant cold, and considered his ideas seriously.

(2) The second possibility is that Rumford himself was persuaded by Prevost and withdrew his arguments. There is some initial plausibility to this idea, since Rumford seems to have published no rebuttal of Prevost's arguments in the ten-year period prior to his death in 1814, though he remained active in science for many of those years. This could be a sign of quiet capitulation. However, I find it highly unlikely that Rumford would have agreed with Prevost. For one thing, Rumford still published a discussion of frigorific rays in 1807, three years after his battle with Prevost. More generally there is no evidence that Rumford abandoned his kinetic theory of heat at any stage in his life, and the only known method of explaining radiation on that theory was his idea of ether vibrations.[91] So he would have held on to his own account of radiation, while perhaps recognizing that Prevost turned out to be difficult to defeat.

(3) The remaining possibility is that most scientists involved in the debate considered both accounts and decided that Prevost's was superior, although Rumford himself and a small number of others continued to disagree. Then we must ask *why* the majority judged Prevost's account to be superior to Rumford's. Again, there are several possibilities.

* Rumford himself noted that his "controversy on the reality of caloric" with the leading French calorists (including Laplace, Berthollet, and Biot) was "in all respects very friendly," and his papers on heat were readily accepted for presentation and publication by the French Institute and the Royal Society, by his own reckoning "two of the most illustrious bodies of learned men that ever existed." See Rumford, "Historical Review" (ref. 37), pp. 493–494; and Rumford, "Experiments on Cooling Bodies" (ref. 60), p. 7.

(3a) A straightforward interest-based explanation will not work well in this case. As far as the major European scientific establishments of the day were concerned, this was a battle between two outsiders that did not affect their own welfare directly. There was little to be gained by the Paris academicians, or the Scottish professors, or the English gentlemen scientists, for example, in following the doctrines of a man whose political influence was considerable in Geneva but nowhere else. And I have not found any evidence that this debate on cold was linked to any significant social, economic, or cultural divides in any of the major scientific centers.

It is true that Prevost and the Genevan scientific community in general had strong links to Paris, though they were not such links as to make it particularly beneficial for the Parisian scientists to adopt Prevost's views. What is also true is that Rumford's standing in Paris suffered seriously soon after the argument about cold radiation started, so that Parisian scientists may have been prompted to side with Prevost merely to be against Rumford. In 1806 Rumford entered into an acrimonious controversy with Laplace by criticizing the latter's cherished theory of capillary action. Also around the same time his relationship with his new wife, who called herself Madame Lavoisier de Rumford, deteriorated irrevocably, leading to separation and divorce. She maintained a pivotal place in the Parisian scientific community, and there are reports that she, as well as Laplace, proceeded to isolate Rumford.[92] However, the mistreatment of Rumford in Paris does not quite explain the consensus against his ideas in other places, such as Britain in its state of war against Napoleonic France.

(3b) The next possibility is that Prevost persuaded the majority of scientists because they thought that he had exposed a fatal weakness in Rumford's theory of radiation. But, as mentioned earlier, there is no evidence that anyone took such a view. Pictet, as we have seen, freely admitted at least in 1804 that Rumford's theory worked perfectly well. Prevost was keen to argue that *only* his theory could explain the experiments, but nowhere did he produce credible arguments that Rumford's theory did not work. In a retrospective work published shortly after his retirement, he admitted that the wave theory of heat radiation remained a possibility, while he maintained that simplicity and "reasons taken from general physics" favored his own theory.[93]

(3c) It is possible that Prevost's account of radiation was considered to be superior to Rumford's although Rumford's was not considered particularly defective in itself. Again, however, there is little evidence that this was the case for a significant number of commentators. What would have been the criteria for such a judgment of superiority? Prevost's account was hardly more quantitative than Rumford's.[94] Prevost did argue that his own theory was simpler than Rumford's theory,[95] but I have not seen any evidence that this argument was widely taken up by others. Naturalness, or freedom from *ad hoc* maneuvers would be another common criterion. On this count, however, Prevost only managed to defend himself against Rumford's accusation that he had cooked up his theory of dynamic equilibrium of caloric to explain away the radiation of cold.[96] On Rumford's side, no one disputed that the idea of heat and cold radiation as ethereal vibrations was a very natural consequence of his general vibrational theory of heat, which had been crafted independently of knowing anything about Pictet's experiments. In summary, it does not seem that there was any clear epistemic sense in which Prevost's theory of radiation was in itself superior to Rumford's.

(3d) By a process of elimination, then, I now come to an answer that I hinted at in various places in the foregoing discussion: Rumford's theory of radiation was rejected not because of any shortcomings of its own, but only because Rumford's general view of heat was rejected. Recall that most of the defences of Prevost's theory were just that: defences. The common argument against Rumford was that the caloric theory (as elaborated by Prevost for this purpose) *also* provided a perfectly satisfactory explanation of radiation phenomena. This was rhetorically sufficient only because the calorists had the advantage of defending a theoretical framework that was already widely accepted. Prevost's theory of radiation was widely preferred to Rumford's because it fitted better with a prevailing general theory of heat that had other merits and powerful supporters. Given a general adherence to the caloric theory, Prevost's was the only plausible account of the phenomena involving the radiation of heat and cold. Whatever merits Rumford's theory of heat radiation had, they were clearly not sufficient to overturn the perceived general advantages of the caloric theory.

Was Rumford's Work Futile?

If my analysis of the controversy on the radiation of cold is correct, it calls for a revision in our understanding of Rumford's place in the history of the caloric theory. The revision I propose stems from a synthesis of the following two theses that are general conclusions from the foregoing discussion.

(1) Rumford was dealt with reasonably. Starting with James Prescott Joule, the later advocates of kinetic theories of heat tended to regard Rumford's work (especially his cannon-boring experiment) as a decisive argument against the caloric theory, which failed to convert the calorists only because they were dogmatic.[97] On the contrary, we have seen ample evidence that the rejection of Rumford's theory resulted from a careful and reasoned discourse.

(2) Rumford's argument on cold was powerful. In my view, the controversy on cold radiation was Rumford's finest hour in his fight against the caloric theory, though later whiggish champions of Rumford have tended to ignore, misunderstand, or repudiate this aspect of his work, puzzled and perhaps embarrassed by Rumford's advocacy of what looks like pre-modern nonsense.[98] Moreover, he offered a sensible alternative theory of radiation, which was in harmony with his own conception of heat in matter as the vibration of molecules, and made a powerful block of theories when allied with the wave theories of light and sound.*

* It is interesting to note that Rumford's view of radiant heat and light as interrelated vibrations in the ether was gradually revived in the later decades of the 19th century, largely owing to changes occurring in optics. Young's early advocacy of wave optics had not found much support, and Rumford in his last years tried to make an argument against the material theory of light, to no avail; see Brown, *Benjamin Thompson* (ref. 13), pp. 300–301. However, within several years of Rumford's death the

Putting (1) and (2) together: if Rumford's powerful argument was defeated in a process of reasonable debate, then we can conjecture that the winning calorist side also must have made some concessions. In the remainder of this section, I will point out some tentative pieces of evidence, some already discussed, that this is indeed what happened.

The key change was the general acceptance of Prevost's view of caloric as a "discrete fluid" whose emission and absorption were to be considered only in terms of whether its particles pass through surfaces or bounce back from them. This marked a great shift from the earlier chemical view of caloric as a continuous fluid whose essential attributes included various degrees of chemical affinity to ordinary matter. The observed radiation of cold prompted the creation of Prevost's theory. With Rumford's new experiments on cold radiation, Prevost's emerged as the only theory that presented any hope for the calorists. And Rumford's challenge helped Prevost refine his theory into its final form. In sum, Rumford was instrumental in stimulating a fundamental change in the caloric theory.

That was not the end of Rumford's contribution. Although the shift to Prevost's theory seemed to be a positive development for the caloric theory at the time, in the end it spelled the death of caloric. This is because chemistry was the strongest domain of the caloric theory, and Prevost's reformulation drew the caloric theory away from chemistry. No kinetic theory of heat at that time could come even close to matching the chemical successes of the caloric theory, which were squarely based on the concept of latent caloric (that is, the state of caloric in chemical combination with matter). Significantly, Rumford spoke of "the caloric of modern *chemists*," while he identified his own enterprise as "physics."[99]

When attacked by Rumford in the realm of physics, the calorists could have responded by conceding that territory and securing their position in chemistry. Instead their ambitions led them to attempt to rule physics as well, by creating a physicist's version of the caloric theory. This was the work both of Prevost and of the Laplacian school. In Laplace's mature caloric theory, caloric was conceived in the Prevostian way as a discrete fluid; the bulk of it was trapped inside material molecules, but there was a portion existing in the intermolecular spaces, being continually exchanged between the molecules. Its actions were explained and calculated through a putative short-range force function.[100] Prevost noted with pleasure this "wise and judicious" application of his theory.[101] With this new

ascendancy of the wave theory of light began, especially with the work of Augustin Fresnel (1788–1827). With light seen as an ether-wave with increasing certainty, pressure increased to view radiant heat in the same way, for all those who believed in some close kinship between light and radiant heat. Seeing radiant heat as an ether-wave, in turn, made it more plausible to see heat in matter as vibrations of particles; this was another, often neglected factor that led to the eventual demise of the caloric theory. In his paper, "The Wave Theory of Heat: A Forgotten Stage in the Transition from the Caloric Theory to Thermodynamics," *British Journal for the History of Science* **5** (1970–71), pp. 145–167, Stephen G. Brush has made a persuasive case that an undulatory theory of heat was widely held in the period 1830–1850: "heat is the vibrations of an ethereal fluid that fills all space, and which transmits vibrational motion from one atom to another." But this was essentially the ontology originally advocated by Rumford (and Young), though it is difficult to discern to what extent its popularity actually derived from Rumford's work, since its advocates rarely made serious references to Rumford. In Brush's view, the wave theory of heat played an essential role in the transition from the caloric theory to thermodynamics.

theoretical framework it seemed that the caloric theory was establishing itself successfully in physics, as the Laplacian school made significant advances in the understanding of the physics of gases, all-important in the age of the steam engine.[102]

The extension of the caloric theory into physics, however, came at the expense of chemistry. The Prevost-Laplace version of the caloric theory did not advance any chemical explanations. First of all, Laplace did not care to discuss the effects of the attraction between particles of matter and caloric, focusing instead on deriving the known gas laws from the mutual repulsion of caloric particles from each other. Laplace's calculations explicitly excluded any effects of latent caloric, that key explanatory device in chemistry, since he postulated that latent caloric did not exert any repulsive force.[103] The caloric physicists do not seem to have been concerned about the adverse effects of their innovations on chemistry. Meanwhile, chemical researches became less and less concerned with caloric, as the debates on combining ratios, atomic theory, and the study of new substances increasingly took center stage in the first two decades of the 19th century.* When the caloric theory was attacked by mid-century energy physics, it crumbled like an ageing empire that had committed too many of its resources to the defence of a particular periphery, leaving the center unattended. In the first decade of the 19th century, the calorists won the debate by matching Rumford in physics and stressing their superiority in chemistry. With the revival of kinetic theories in the 1840s and 1850s, the faltering caloric physics could not match the attractions of the new thermodynamics,[104] and caloric chemistry by then had receded too far into the background to be relevant.

My proposed revision of the importance of Rumford in the history of heat theory thus can be summarized as follows. Historians have tended to agree that Rumford's attack on the caloric theory was in the end unsuccessful. My view, on the contrary, is that he did contribute to the downfall of the caloric theory, albeit in roundabout and unintended ways. Rumford's work on heat and cold radiation helped draw the calorists away from the arena of chemistry and pushed them into the dead-end alley of discrete-fluid caloric theory.

Appendix: The Way Ahead

I have argued that the chief reason for the rejection of Rumford's theory of radiation was the better coherence of Prevost's theory with the general calorist theory of heat. If that is the case, the verdict needs to be reconsidered by all those who have rejected the caloric theory. To the best of my knowledge, Rumford's experiments have not been given full treatments in terms of classical thermodynamics or later theories of heat. This raises the question: how can Rumford's results be explained, to *our* satisfaction?

* This was the case even as chemistry textbooks continued to stress the importance of caloric in the theoretical system of chemistry. I thank Professor Frederic L. Holmes for directing my attention to this factor.

The first steps in a further investigation should be experimental: to confirm Rumford's results with more precision, and to perform some new experiments designed to clarify the phenomena further. Some preliminary work already has been undertaken by Brown, who reconstructed some of Rumford's instruments, and by Evans and Popp, who replicated Pictet's experiment and devised a convenient version of it for lecture demonstrations.[105] The following are among the effects that we would do well to measure with precision: the radiative power of a cold object; the degree to which that power is enhanced or lessened by a speaking-tube, and by alterations of the radiating surface; and the exact point of equilibrium in Rumford's setup with hot and cold cylinders working simultaneously on the thermoscope. The experiments could also be carried out in conceptually simpler settings: use of a good vacuum would eliminate questions about the role of air; better control of environmental radiation could be achieved by putting the apparatus in a large uniform sphere with good temperature control.*

A slight variation in Rumford's speaking-tube experiment would produce a more powerful crucial experiment. If we place a thermometer with a very small bulb exactly at the vertex of the truncated cone, as shown in figure 11, no rays can reach the thermometer bulb by simple reflection.[106] If Prevost is correct about the nature of radiation, the speaking-tube in this setup should have no effect at all. However, if Rumford's wave theory of radiation is correct, it is possible that the calorific or frigorific rays would still be concentrated by the speaking-tube, since a wave front hitting the inner surface of the tube will be propagated in all open directions. (An exact prediction is difficult to make, since Rumford's theory is not precisely formulated.)

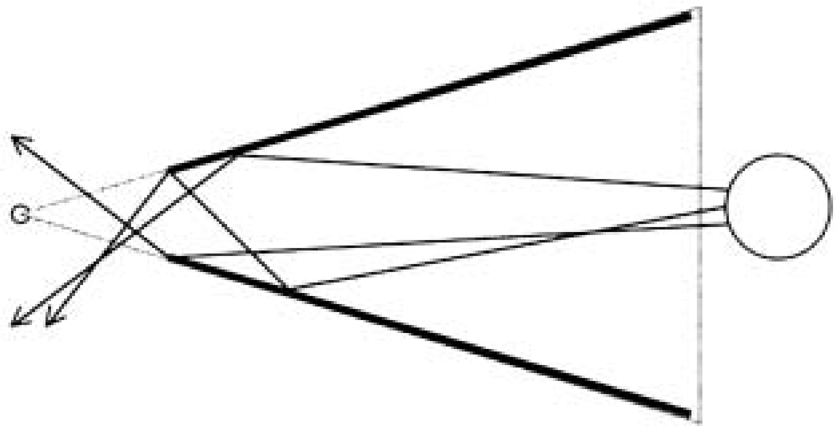

Fig. 11. The proposed speaking-tube experiment with the thermometer bulb at the vertex of the cone. No corpuscular rays can reach the vertex by reflection on the inner walls of the speaking-tube.

* Rumford actually had made a start in this direction. See Rumford, "Inquiry" (ref. 41), pp. 412–414; and Rumford, "Short Account of a New Experiment" (Section 1 of "Experimental Investigations Concerning Heat") [1804], Vol. 1, *Collected Works* (ref. 18), pp. 285–291.

It would be interesting to construct modern theoretical treatments of the results of these experiments. It also would be an interesting exercise to see whether Rumford's theory could be developed further so as to provide a quantitative explanation of these phenomena, and others.

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2 Private communication, February 28, 2001; I thank Professor Beckman for his advice.

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5 For more information about Thomson, see J. B. Morrell, "The Chemist Breeders: The Research Schools of Liebig and Thomas Thomson," *Ambix* **19** (1972), 1–46, and Herbert S. Klickstein, "Thomas Thomson: Pioneer Historian of Chemistry," *Chymia* **1** (1948), 37–53.

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7 *Encyclopaedia Britannica*, 2nd ed., Vol. 2 (1778), pp. 2065–2067; 3rd ed., Vol. 8 (1797), p. 351.

8 There are many biographical accounts of Pictet, but the most convenient summary is Robert Fox, "Pictet, Marc-Auguste," in *The Dictionary of Scientific Biography*. For further details of Pictet, his family, and especially the *Bibliothèque Britannique*, see David M. Bickerton, *Marc-Auguste and Charles Pictet*, *the Bibliothèque Britannique (1796–1815) and the Dissemination of British Literature and Science on the Continent* (Geneva: Slatkine Reprints, 1986).

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13 See the anonymous obiturary, "Memoirs of Sir Benjamin Thompson, Count of Rumford," *Gentleman's Magazine*, *and Historical Chronicle* **84:2** (1814), 394–398, on p. 397. See also Sanborn C. Brown, *Benjamin Thompson*, *Count Rumford* (Cambridge, Mass. and London, England: The MIT Press, 1979), p. 260.

14 Thomas Young, *A Course of Lectures on Natural Philosophy and the Mechanical Arts*, 2 Vols. (London: Joseph Johnson, 1807), Vol. 1, p. 631. He continued: "but there are many reasons for supposing heat to be the positive quality, and cold the diminution or absence of that quality"; however, he did not care to state any of those "many reasons."

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19 Thomson, *System of Chemistry* (ref. 6), Vol. 1, p. 259.

20 Pictet, *Essay on Fire* (ref. 9), pp. 10–13.

21 *Ibid*., pp. 121–123.

22 On Hutton's geological contributions, see Frank Dawson Adams, *The Birth and Development of the Geological Sciences* (New York: Dover, 1938), pp. 238–245.

23 James Hutton, *A Dissertation upon the Philosophy of Light*, *Heat*, *and Fire* (Edinburgh and London: Cadell and Davies, 1794), pp. 85–92.

24 For an exposition of these earlier ideas, see James Hutton, *Dissertations on different subjects in natural philosophy* (Edinburgh: A. Strahan; London: T. Cadell, 1792), pp. 473, 487–496. Hutton had been discussing these ideas since the 1770s, according to Richard G. Olson, "Count Rumford, Sir John Leslie, and the Study of the Nature and Propagation of Heat at the Beginning of the Nineteenth Century," *Annals of Science* **26** (1970), 273–304, on p. 214.

25 Hutton, *Philosophy of Light* (ref. 23), pp. 86–88, and also pp. 32–56.

26 *Ibid*., pp. 95–98.

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28 For further details on Prevost's life and work see A. Cherbuliez, *Discours sur la vie et les travaux de feu Pierre Prevost, ancien Professeur de Philosophie à l'Académie de Genève* (Geneva: Ramboz, 1839), and also Auguste de Candolle, "Notice sur M. Pierre Prevost," *Archives des sciences physiques et naturelles* **19** (1839), 1–10. For a convenient summary in English, see John G. Burke, "Prevost, Pierre," in Charles Coulston Gillispie, ed., *Dictionary of Scientific Biography*, Vol. 11 (New York: Charles Scribner's Sons, 1975), pp. 134–135.

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30 This is the first part of Pierre Prevost, *Deux traités de physique mécanique* (Geneva and Paris: J. J. Paschoud, 1818).

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32 Prevost, "Sur l'équilibre du feu" (ref. 27), pp. 315–318, gives the essential ideas of his theory of dynamic equilibrium; the application of this theory to the reflection of cold is given on pp. 318–320. In the more extended treatment in Prevost, *calorique rayonnant* (ref. 27), the basic exposition of the theory can be found in Section 1, and the treatment of the reflection of cold in Section 4.

33 Pierre Prevost, *Exposition élémentaire des principes qui servent de base a la théorie de la chaleur rayonnante*, *faisant suite a l'ouvrage intitulé du calorique rayonnant* (Geneva and Paris: Abraham Cherbuliez, 1832), p. 71. Prevost's theory was often referred to as the "theory of exchanges."

34 On Pictet's attitude, see Prevost, "Sur l'équilibre du feu" (ref. 27), p. 315.

35 There are many accounts of Rumford's life and work, but the definitive modern account is Sanborn C. Brown, *Benjamin Thompson* (ref. 13). The definitive 19th-century work is George E. Ellis, *Memoir of Sir Benjamin Thompson*, *Count Rumford* (Boston: American Academy of Arts and Sciences, 1871). A brief and useful introduction can be found in G. I. Brown, *Scientist*, *Soldier*, *Statesman*, *Spy – Count Rumford: The Extraordinary Life of a Scientific Genius* (Phoenix Mill: Sutton Publishing, 1999).

36 Rumford to Pictet, January 5, 1797. I quote from a copy of Rumford's letters to Pictet, preserved in the American Academy of Arts and Sciences, Cambridge, Massachusetts.

37 Rumford to Pictet, October 18, 1800. See Rumford, "Historical Review of the Various Experiments of the Author on the Subject of Heat" [1804], Vol. 1 *Collected Works* (ref. 18), pp. 443–496, on pp. 477–478, for a published account of this event. According to the latter account, the experiment was also witnessed by John Playfair, Dugald Stewart, and other luminaries of the Scottish Enlightenment; Thomas Hope (1766–1844), who hosted this occasion, was Joseph Black's successor in the Edinburgh chair of chemistry. By this time both Black and Hutton had passed away.

38 Rumford had come up with an analogy between sound and heat at least a few years before this. He wrote to Pictet (November 8, 1797): "A bell when struck with a hammer gives off sound, – but I do no[t] think it would be speaking philosophically to call sound a material substance."

39 For the circumstances of Young's appointment, see Henry Bence Jones, *The Royal Institution: Its Founder and Its First Professors* (London: Longmans, Green, and Co., 1871), p. 238, and pp. 223ff for Young's life and work more generally.

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41 See, *e.g*., Rumford, "An Inquiry Concerning the Nature of Heat, and the Mode of its Communication" [1804], Vol. 1 *Collected Works* (ref. 18), pp. 323–433, on p. 416.

42 *Ibid*., p. 408, emphases original; see pp. 376 and 405ff, for further explanations. A succinct summary can be found in Rumford, "Reflections on Heat" [1804], Vol. 1 *Collected Works* (ref. 18), pp. 301–322, on p. 313.

43 Pictet, *Essay* (ref. 9), p. 123.

44 Rumford, "Inquiry" (ref. 41), pp. 400–401; Rumford, "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" (Section 3 of "Experimental Investigations Concerning Heat") [1804], Vol. 2 (1969) *Collected Works* (ref. 18), pp. 9–23.

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47 Rumford, "Inquiry" (ref. 41), p. 323.

48 The published version of this paper is Rumford, "Reflections" (ref. 42).

49 Rumford, "Inquiry" (ref. 41), p. 421.

50 Rumford, "Historical Review" (ref. 37), p. 484.

51 On how Rumford commenced and applied these studies, see Brown, *Benjamin Thompson* (ref. 13), pp. 259–260; and Rumford, "Inquiry" (ref. 41), pp. 324ff.

52 Rumford, "Inquiry" (ref. 41), pp. 337–338, 377–378; Rumford, "Reflections" (ref. 42), p. 313.

53 The design of the thermoscope is described in Rumford, "Inquiry" (ref. 41), pp. 348ff; Rumford, "Reflections" (ref. 42), pp. 309–312; and Rumford, "Description of a New Instrument of Physics" ("First Memoir on Heat") [1804], Vol. 2 (1969) *Collected Works* (ref. 18), pp. 25–30.

54 Rumford, "Inquiry" (ref. 41), Experiments 12 and 13, pp. 357–360.

55 *Ibid*., Experiment 20, pp. 365–366.

56 *Ibid*., Experiment 23, pp. 370–372, and also pp. 422–425; see also the brief summary in Rumford, "Reflections" (ref. 42), p. 314.

57 Rumford, "Inquiry" (ref. 41), Experiment 24, pp. 375–376, and 425–427; see also the brief summary in Rumford, "Reflections" (ref. 42), p. 314.

58 Rumford, "Reflections" (ref. 42), pp. 314–315; see also Rumford, "Description" (ref. 53), pp. 29–30.

59 See Rumford, "Inquiry" (ref. 41), pp. 379–383, 429–433; Rumford, "Reflections" (ref. 42), pp. 319–321.

60 For the quoted claim, see Rumford "Inquiry" (ref. 41), p. 414. The experiments in support of this claim are reported in Rumford, "Research on Heat: Second Memoir" [1804], Vol. 1 *Collected Works* (ref. 18), pp. 434–441; and "Experiments on Cooling Bodies" (Section 2 of "Experimental Investigations Concerning Heat") [1804], Vol. 2 (1969) *Collected Works* (ref. 18), pp. 1–8. See also the summary in Rumford, "Reflections" (ref. 42), pp. 315–317.

61 Rumford, "Reflections" (ref. 42), p. 307.

62 For records of these demonstrations, see Rumford, "Inquiry" (ref. 41), p. 372; Rumford, "Historical Review" (ref. 37), pp. 490, 494; Prevost, "Considérations" (ref. 31), p. 215; and Prevost, *calorique rayonnant* (ref. 27), pp. 102–103.

63 Cherbuliez, *Discours* (ref. 28), p. 31.

64 Marc-Auguste Pictet, "An enquiry concerning the nature of heat, etc. … par Benjamin Comte de Rumford," *Bibliothèque Britannique* **25** (1804), 185–221, 273–311; see especially pp. 214, 303–304, and 311.

65 Prevost, "Considérations" (ref. 31), pp. 21–22.

66 Pierre Prevost, "Some Remarks on the Theory of the Equilibrium of Radiant Heat, and on some Difficulties started [*sic*] against that Theory," trans. by Thomas Thomson, *Annals of Philosophy* **6** (1815), 379–385., on 380.

67 Rumford, "Inquiry" (ref. 41), p. 422.

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69 Prevost, "Considérations" (ref. 31), pp. 25–26.

70 *Ibid*., pp. 26–27 (including an editor's note there by Pictet reporting on the experiment), and pp. 207–208.

71 *Ibid*., p. 209.

72 Prevost, "Considérations" (ref. 31), p. 213. A similar reasoning, regarding Pictet's original setup with two concave mirrors, was elaborated by J. L. Tremery, "Observations sur les expériences à l'aide desquelles les physiciens démontrent la réflexion du calorique," *Nouveau Bulletin des Sciences*, *par la Société Philomathique de Paris* **3** (1813), 323–328; and Richard Davenport, "Observations on the Objections that have been made to Mr. Prevost's Explanation of the Effect produced by a Mass of Ice on a Thermometer in the Focus of a Reflecting Mirror," *Annals of Philosophy* **5** (1815), 338–345, esp. on p. 342.

73 C. S. M. M. R. Pouillet, *Élémens de physique expérimentale et de météorologie*, 2 Vols. (Paris and Brussels: Béchet Jeune, 1827), Vol. 1, p. 381.

74 William Charles Wells, *An Essay on Dew*, *and Several Appearances Connected with it* (London: Taylor and Hessey, 1814), esp. pp. 66–68 for reference to Prevost; Rumford, "Inquiry" (ref. 41), p. 433; Joseph Fourier, *The Analytic Theory of Heat*, trans. with notes by Alexander Freeman (New York: Dover, 1955; originally published as *Théorie analytique de la chaleur* in 1822), pp. 32–33 (§§38–40).

75 On the general reception of Prevost's ideas, see Cherbuliez, *Discours* (ref. 28), p. 32.

76 Haüy, *Elementary Treatise* (ref. 17), pp. 111–112; René-Just Haüy, *Traité élémentaire de physique*, 2nd ed., 2 Vols. (Paris: Courcier, 1806), Vol. 1, pp. 88–90, 99–100.

77 See Haüy's letters to Prevost, reprinted in Alfred Lacroix, "La vie et l'oeuvre de l'Abbé René-Just Haüy," *Bulletin de la Société Française de Minéralogie* **67** (1944), 15–226, on pp. 154–156, 160–162; and also in Lucien de la Rive, *Pierre Prevost: Notice relative à ses recherches sur la chaleur rayonnante* (Geneva: Aubert-Schuchardt, 1890), pp. 19–20.

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80 Some authors did seek to improve upon Prevost's treatment. For instance, see the following simplified quantitative analyses: Thomas Crompton Holland, "On the Radiation of Caloric," *Transactions of the Royal Society of Edinburgh* **9** (1823), 179–185; Alfred Ainger, "On the Theory of the Radiation of Heat," *Quarterly Journal of Science*, *Literature and Art* **29** (1830), 378–383.

81 Prevost, "Some Remarks" (ref. 66), gives a partial list of his critics and their arguments.

82 John Murray, "Defence of the Objections to Prevost's Theory of Radiant Heat," *Annals of Philosophy* **7** (1816), 223–225, on p. 224; Murray, *System* (ref. 12), Vol. 1, p. 361.

83 Henry Meikle, "On Calorific Radiation," *The Philosophical Magazine and Journal*, **53** (1819), 260–262. Ainger, "Radiation of Heat" (ref. 80), p. 383, expressed a more serious fear that accepting the "unphilosophical idea of a separate principle of cold" would lead to the idea of "a principle of darkness independent of light," given the close link between light and heat.

84 Marshall Hall, "On the Nature of Heat," (Nicholson's) *Journal of Natural Philosophy*, *Chemistry*, *and the Arts*, new series **29** (1811), 215–222, 257–268.

85 *Ibid*., pp. 217–218.

86 On Hall's life and work, see "Hall, Marshall," in *Encyclopaedia Britannica*, 11th ed., Vol. 12 (New York: Encyclopaedia Britannica, Inc., 1910), pp. 848–849, and also *Memoirs of Marshall Hall*, *by His Widow* [Charlotte Hall] (London: Richard Bentley, 1861).

87 "An Inquiry Concerning the Nature of Heat, and the mode of its Communication, by Benjamin Count of Rumford," *The Edinburgh Review, or Critical Journal* **4** (1804), 399–415, on 400.
88 For details of Rumford's 1801 visit to Paris, see Brown, *Benjamin Thomson* (ref. 13), pp. 244–248.
89 Jones, *Royal Institution* (ref. 39).
90 See, *e.g.*, Prevost, "Considérations" (ref. 31), p. 27; Prevost, *calorique rayonnant* (ref. 27), pp. 111, 121; Pictet, "Enquiry" (ref. 64), pp. 185–188; and most effusive of all, Marc-Auguste Pictet, *Voyage de trois mois en Angleterre, en Ecosse, et en Irlande pendent l'Eté de l'an IX (1801 v. st.)* (Geneva: l'Imprimérie de la Bibliothèque Britannique, 1802).
91 Rumford, "Experiments and Observations on the Cooling of Liquids in Vessels of Porcelain, Gilded and Not Gilded," Vol. 2 (1969) *Collected Works* (ref. 18), pp. 135–144, on p. 138. A close acquaintance in Rumford's last years testified in his obituary: "He has repeatedly declared to me, it was his decided opinion that heat and light were the result of vibrations of bodies, and were not bodies themselves." See "Memoirs of Sir Benjamin Thompson" (ref. 13), p. 397.
92 About Rumford's isolation in his last years in Paris, see Brown, *Benjamin Thompson* (ref. 13), pp. 275–306; "Memoirs of Sir Benjamin Thompson" (ref. 13), pp. 397–398.
93 Pierre Prevost, "De quelques phénomènes dépendans de la radiation du calorique," *Mémoires de la Société de Physique et d'Histoire Naturelle de Genève* **2:2** (1824), 161–199, on 171.
94 Prevost's work does include various calculations, but none bearing directly on his dispute with Rumford on the radiation of cold, except a sketchy and suspect demonstration in Prevost, *calorique rayonnant* (ref. 27), pp. 127–130.
95 See Prevost, "Considérations" (ref. 31), p. 14.
96 *Ibid.*, p. 15; and Prevost, *calorique rayonnant* (ref. 27), pp. 16–17. The accusation was made in Rumford, "Inquiry" (ref. 41), p. 425.
97 A useful critical discussion, with references, of retrospective appraisals of Rumford's work can be found in Stephen J. Goldfarb, "Rumford's Theory of Heat: A Reassessment," *British Journal for the History of Science* **10** (1977), 25–36, especially p. 26.
98 Even the most sympathetic and thorough authors have been liable to misunderstanding on this point. See, for instance: Olson, "Count Rumford" (ref. 24), pp. 290–291; W. J. Sparrow, *Knight of the White Eagle: A Biography of Sir Benjamin Thompson, Count Rumford 1753–1814* (London: Hutchinson, 1964), pp. 230–231; and Robert James McRae, *The Origin of the Conception of the Continuous Spectrum of Heat and Light* (University of Wisconsin, Ph. D. thesis, 1969), pp. 143–151. Brown, *Benjamin Thompson* (ref. 13), pp. 259–262, avoids any explicit errors in presenting Rumford's views, but repudiates frigorific rays and downplays Rumford's notion that cooling is effected by the absorption of frigorific rays.
99 Rumford, "Inquiry" (ref. 41), p. 374 (emphasis added); Rumford, "Reflections" (ref. 42), p. 306. Similarly, Thomas Young vilified the followers of the caloric theory as those "who look up with unqualified reverence to the dogmas of the modern schools of chemistry"; see Young, *Course of Lectures* (ref. 14), p. 656.
100 See Pierre-Simon Laplace, Vol. 5, *Traité de mécanique céleste* (Paris: Bachelier, 1825), book 12.
101 Prevost, "De quelques phénomènes" (ref. 93), pp. 171–172, footnote 1, p. 171ff.
102 For a detailed description of the development of Laplacian caloric theory, see Robert Fox, *Caloric Theory*, (ref. 16), ch. 5.
103 Laplace, *Mécanique céleste* (ref. 100), pp. 93, 113.
104 About the difficulties encountered by caloric physics, see Fox, *Caloric Theory* (ref. 16), ch. 7.
105 Evans and Popp, "Pictet's Experiment" (ref. 15), pp. 750–751.
106 The English physician and natural philosopher William Hyde Wollaston (1766–1828) had pointed this out in a private communication to Prevost. This communication, in a letter of June 12, 1804, is cited in Weiss, "Das Schalltrichter-Experiment" (ref. 15), p. 120.

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