

THE WAVE THEORY OF HEAT:

A FORGOTTEN STAGE IN THE TRANSITION FROM THE CALORIC THEORY TO THERMODYNAMICS

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RESEARCH on thermal "black-body" radiation played an essential role in the origin of the quantum theory at the beginning of the twentieth century. This is a well-known fact, but historians of science up to now have not generally recognized that studies of radiant heat were also important in an earlier episode in the development of modern physics: the transition from caloric theory to thermodynamics. During the period 1830-50, many physicists were led by these studies to accept a "wave theory of heat", although this theory subsequently faded into obscurity.

According to the wave theory of heat,¹ heat is the vibrations of an ethereal fluid that fills all space, and which transmits vibrational motion from one atom to another. While this theory is in some respects similar to post-1850 conceptions—a similarity which, as we will see, was helpful in facilitating the transition between them—it differs in two significant respects. First, it denies that atomic vibrations alone could account for the phenomena of heat; the role of the ether is essential. Second, it is not assumed that atoms in a gas can move freely through space, as in the modern kinetic theory; they are still constrained to vibrate around fixed equilibrium positions. These two features helped to preserve the continuity with older ideas about heat and the structure of gases, but were gradually given less emphasis after 1845.

For a contemporary description of the wave theory of heat, it seems appropriate to turn to the article on Heat in the 8th edition of the *Encyclopaedia Britannica*, published in 1856, since this article is one of the few pieces of evidence that have been offered to support the common but erroneous view that scientists accepted the caloric theory until the middle of the nineteenth century. Now it must be noted first of all that the main part of this article was actually published in the 7th edition, so that it

¹ More often called the "undulatory" theory in the nineteenth century; but the modern terminology, already well established even in historical writings on the "wave" theory of light, seems preferable. It should be noted that by some modern criteria the wave theory of heat is not a distinct "theory" since it did not lead to quantitative predictions different from those of the caloric or mechanical theories, but instead could be viewed as a combination of the two (see discussion in text, below). However, physicists at the time did not seem to be worried by this circumstance, and usually presented it as a distinct alternative to the caloric theory. Another version of the wave theory of heat, which was perhaps more popular before the nineteenth century, asserted that heat itself is the vibrations of material particles, but that these can be transmitted from one particle to another by ether vibrations. This version would appear to be excluded by the postulate that heat and light are qualitatively identical and are vibrations of the same medium.

really indicates views that were prevalent before 1842.² Second, anyone who reads past the first two paragraphs will see that the author, T. S. Traill, does *not* still believe in the caloric theory of heat. It is true that he reports that the caloric theory has in the past been generally accepted, and criticizes the mechanical theory (based on *atomic* vibrations) as "vague and unsatisfactory". But, after noting that the mechanical theory cannot account for phenomena such as the radiation of heat through empty space, he says:

"It is possible, however, to modify this theory, by supposing that heat is produced not merely by the motions of the particles of the heated substance, but by the vibrations or undulations of a very subtile matter existing in all bodies. This will approximate the vibratory theory to that which has been generally considered as its antagonist, will accord well with some recently discovered facts, and will assimilate the vibratory hypothesis of heat to the undulations now so generally received as explanatory of the phenomena of light, to which heat has so intimately a relation . . . These views lead us to the conclusion that the phenomena of caloric are owing to the movements of a subtile fluid, the particles of which are strongly repellent of each other, and have an affinity for those of all other bodies, different in force according to each kind of matter."

The above quotation provides most of the clues needed to unravel the history of the wave theory of heat. Before proceeding with our detailed account, however, we shall first outline the main steps in the development of nineteenth-century ideas about heat, in order to show the wave theory in a wider context.

(1) Some of the pre-nineteenth century ideas are similar to the wave theory,³ but these lie outside the scope of the present account; we begin with the situation at the beginning of the nineteenth century, when both heat and light were supposed to be fluid substances, probably particulate.

(2) Widely publicized, and frequently referred to in the first half of the nineteenth century, were the experiments of Rumford, Davy and others, showing that heat lacks weight and can be generated in unlimited quantities by mechanical processes such as friction. But these experiments did not by themselves persuade most scientists to abandon the material ("caloric") theory of heat developed by Black, Lavoisier, Laplace and others.

(3) In the period 1800-35, experiments on radiant heat by William Herschel, John Leslie, Macedonio Melloni, and others showed that radiant

² T. S. T[raill], "Heat", *Encyclopaedia Britannica*, 7th ed. (Edinburgh, 1842), xi, 180-197. I am indebted to Mr. V. A. Stenberg, Director of Research at the *Britannica*, for providing me with a copy of this article. The 7th edition does not seem to be available in any major American library, which may partly account for the mistaken idea that this article first appeared in 1856. In the 8th edition the article is reprinted with an additional section at the end referring to the work of Joule, Rankine, and Thomson on the mechanical equivalent of heat; this work is said to support the "mechanical or dynamical theory of heat" of Rumford and Davy, thereby contradicting the remarks at the beginning of the article.

³ For our purposes the most important source is Isaac Newton, *Opticks*, 4th ed. (London, 1730), Qu. 18, which suggests that heat is transmitted through a vacuum by ether vibrations; but this seems to be contradicted by remarks in Qu. 28.

heat has most if not all of the properties of light. This led to a widespread belief that heat and light are essentially the same phenomenon, i.e. superficially different manifestations of the same physical agent. The adjective "radiant" was easily dropped, so that the problem of the nature of heat was reduced to the problem of the nature of light.

(4) Between 1815 and 1830 the Young-Fresnel wave theory of light replaced the Newtonian particle ("emission") theory, as attention was focused on properties such as interference and polarization.

(5) Hence, as the logical conjunction of (3) and (4) reinforced by (2), the wave theory of heat was adopted after 1830. This did not require a sharp break with the caloric theory. One could first identify caloric with ether, then assume that heat consists in the *vibrations* rather than the *amount* of this fluid, thereby preserving many of the explanations of the older theory, with only verbal modifications.

(6) Between 1842 and 1850, interest in steam engines and in the thermal effects of electromagnetic phenomena led to the enunciation of the principle of conservation of energy and the establishment of modern thermodynamics based on the idea of a "mechanical equivalent of heat". By this time the caloric theory was almost dead, and the wave theory of heat had already made it seem natural to treat heat as a form of mechanical energy. Some of the early statements of the mechanical theory of heat were clearly inspired by the wave theory.

(7) With the revival of the kinetic theory of gases (1848-70), the essential role of the ether in transmitting vibrations from one atom to another was eliminated in dealing with ordinary thermal properties of matter ("sensible heat"). The irrelevance of the wave theory to thermodynamics is already foreshadowed by 1850.

(8) In spite of (7), the wave theory of heat did not die out, since there was continuing interest in radiant heat throughout the rest of the nineteenth century. Since physicists still believed in the existence of an ether with mechanical properties, the question "Why doesn't the ether take its share of vibrational energy corresponding to thermal equilibrium with matter?" remained unsolved up to 1900. This question was seen retrospectively as a "crisis in classical physics" (the "ultraviolet catastrophe" posthumously baptized by Paul Ehrenfest in 1910) though as Martin Klein has shown,⁴ it was not viewed as such by Planck at the time he developed the quantum theory.

(9) Maxwell's electromagnetic theory (1866-73) indicated that heat radiation could be viewed as a special type of electromagnetic waves ("infrared radiation") which produces thermal effects when absorbed by

⁴ Martin J. Klein, "Max Planck and the Beginnings of the Quantum Theory", *Archive for History of Exact Sciences*, 1 (1962), 459-479. Unfortunately, scientists writing on the development of quantum theory continue to state that Planck was attempting to resolve a paradox discovered by Rayleigh, that the total energy of the ether would be infinite if each mechanical degree of freedom had an equal share of energy (see below and note 60).

matter. This led to a reinterpretation of step (3), making the qualifying adjective "radiant" essential: the nature of *heat* is not necessarily the same as the nature of *radiant heat*.

(10) Boltzmann and Wien, in the 1880's, showed that heat radiation could be treated with some success by combining thermodynamic concepts with Maxwell's energy-momentum relation for electromagnetic radiation and the Doppler principle. It was by following this phenomenological approach, rather than by worrying about the equipartition problems involved in the wave theory of heat, that Planck was led to his distribution law in 1900.

This article is primarily concerned with steps (5) and (6). Up to now, the significance of these steps has been hidden by the modern distinction between radiant heat and ordinary heat (cf. step (9)) and by two myths about the history of nineteenth-century physics: the first (now largely discredited), that the mechanical theory of heat was already established by Rumford and Davy at the beginning of the nineteenth century; the second (still prevalent), that most scientists accepted the caloric theory until it was replaced by thermodynamics. We shall see that both myths originated at least in part in the writings of the same person, William Thomson.

The establishment of the wave theory of light, on the one hand, and of the principle of conservation of energy and thermodynamics, on the other, are generally regarded as two separate events in the history of nineteenth-century physics. However, I think they should be seen as successive and closely related stages of the same transformation of physical theory, in which explanations of phenomena were increasingly based on *motion* rather than on *matter*.

Radiant heat and the decline of the caloric theory

The caloric theory of heat was at the height of its popularity around 1825, and its adherents included many of the leading scientists, especially in Paris. It was the "keystone of anti-phlogistic chemistry", as Lilley has pointed out,⁵ and was used in explaining phenomena such as thermal expansion, specific heats, changes of state, latent heat and the heat evolved in chemical reactions, even though one could not say that the majority of scientists considered the theory firmly established.⁶ The chief

⁵ S. Lilley, "Attitudes to the nature of heat about the beginning of the nineteenth century", *Actes du 5e congrès international d'histoire des sciences*, Lausanne, 1947, pp. 130-139; *Archives internationales d'histoire des sciences* (1940), 630-639. This is confirmed by the contemporary evidence I have seen; for example, E. S. Fischer, Professor of Mathematics and Natural Philosophy at Bonn, wrote that chemists were unanimous in adopting the caloric theory: see *Elements of Natural Philosophy* (Boston, 1827, translated from Biot's French translation), p. 57.

⁶ In reading the literature of the early nineteenth century, I have been impressed by the great caution and open-mindedness with which many scientists presented their views on heat, particularly some of the writers who are usually labelled as supporters of the caloric theory. It was very common to say that most of the phenomena can be explained equally well by considering heat as a substance or as a quality (or "mode of motion"); even if the former view

opponents of the caloric theory at this time were Rumford and Davy, but the caloric theorists had so far been able to combat their arguments fairly successfully.⁷ Cajori has cited a number of American and German authors who favoured the caloric theory during the period 1800-30;⁸ his list could be extended without difficulty. Far from being a merely qualitative explanatory principle or crutch for the imagination, the theory could be presented, as Brown has shown, in a logically coherent, semi-quantitative manner to critical students of physics.⁹ Among the quantitative accomplishments of permanent value arising from the caloric theory may be mentioned the Laplace-Poisson calculation of the speed of sound (based on the ratio of specific heats of a gas)¹⁰ and Fourier's theory of heat conduction.¹¹ Looking at the situation in 1825, an observer might well jump to the conclusion that the caloric theory would be firmly entrenched for another generation at least. Yet in less than a decade its credibility had been seriously undermined, and the weight of scientific opinion had shifted in another direction.

One of the arguments for the materiality of heat at the beginning of the nineteenth century was the fact that heat can apparently travel through empty space without any accompanying movement of matter; hence it

was to be adopted for the sake of convenience, it was not to be regarded as firmly established beyond any doubt; furthermore, even if heat is really a fluid substance, it may have some association with molecular motion. This point has been emphasized by E. Mendoza in his introduction to *Reflections on the Motive Power of Fire by Sadi Carnot and other papers on the Second Law of Thermodynamics by É. Clapeyron and R. Clausius* (New York, 1960), p. xvi. As examples he cites Lavoisier and Laplace, "Mémoire sur la Chaleur" (Paris, 1780) and Lamé's *Cours de Physique* (Paris, 1836). I will discuss Lamé's opinions below—I do not think they can be put in the same category with those of Lavoisier and Laplace—but as additional evidence one can consult Denison Olmsted, "On the present state of chemical science", *American Journal of Science*, xi (1826), 349-358; xii (1827), 1-14, esp. p. 355; "Remarks on Dr. Hare's essay on the question, whether heat can be ascribed to motion?" *Ibid.*, xii (1827), 359-363, esp. p. 363; Joseph Black, *Lectures on the Elements of Chemistry* (Philadelphia, 1807), i, 29, 33. Thenard, in what Maurice Crosland has called "the standard textbook of chemistry in France for nearly a quarter of a century" [*The Society of Arcueil* (London, 1967), p. 330], accepted the caloric theory yet admitted that since caloric seems to be weightless its real existence is dubious: see Louis Jacques Thenard, *Traité de chimie élémentaire, théorique, et pratique*, 4 vols., 5th ed. (Paris, 1827), i, 35.

⁷ Sanborn Brown, "Count Rumford and the caloric theory of heat", *Proceedings of the American Philosophical Society*, xciii (1949), 316-325.

⁸ Florian Cajori, "On the history of caloric", *Isis*, iv (22), 483-492. It was probably Cajori who first used the *Britannica* article of Traill as evidence for the survival of caloric theory into the 1850's: see *A History of Physics* (New York, 1929, reprinted 1962), p. 122.

⁹ Sanborn Brown, "The caloric theory of heat", *American Journal of Physics*, xviii (1950), 367-373.

¹⁰ See Thomas S. Kuhn, "The caloric theory of adiabatic compression", *Isis*, xlix (1958), 132-140; Bernard S. Finn, "Laplace and the speed of sound", *Isis*, lv (1964), 7-19; Maurice Crosland, *The Society of Arcueil* (London, 1967), pp. 302-307.

¹¹ A convenient edition which includes most of Fourier's work is *The Analytical Theory of Heat* by Joseph Fourier, translated, with notes, by Alexander Freeman (New York, 1955). Among recent historical studies may be mentioned Florent Bureau, "La théorie analytique de la chaleur de J. B. J. Fourier", *Bulletin de la Classe des Sciences, Académie Royale Belge*, xxxix (1953), 1116-1127; Enrico Bellone, "Il significato metodologico dell'eliminazione dei modelli del calórico promossa da Joseph Fourier", *Physis*, ix (1967), 301-310. Fourier himself, as is well known, denied that he adopted any particular hypothesis about the nature of heat. However, he did accept the similarity of heat and light (see p. 32 in Freeman's translation).

cannot be simply molecular motion.¹² This conclusion was perfectly reasonable; but those who used the argument to bolster the caloric theory did not suspect that it would eventually prove treacherous. The increasing use of the phrase "radiant caloric" (*calorique rayonnante*) by writers on caloric theory reflects the growing interest among physicists of the period in the phenomena of radiant heat. This was becoming a very active area of research, beginning with Scheele, Pictet, and Prevost in the late eighteenth century,¹³ and continuing with major discoveries by William Herschel and others around 1800.¹⁴ But from our present viewpoint, the decisive contribution was that by the Italian physicist Macedonio Melloni (1798-1854).¹⁵ With the initial help of his compatriot Leopoldo Nobili (1784-1835) in designing a very sensitive thermopile or "*thermomultiplicateur*" for detecting heat from distant sources, Melloni was able to establish around 1830-32 a number of properties of radiant heat. I shall not attempt to sort out the discoveries of Melloni from those of his predecessors and followers; what is important is the fact that with the help of a favourable report on his work published by the French Academy of Sciences, and the award of the Rumford Medal of the Royal Society of London in 1835, these discoveries gained wide publicity in the scientific world.

For those who did not follow Melloni's work in detail, the most significant result was simply that radiant heat shares all the qualitative properties of light: reflection, refraction, diffraction, polarization, interference, etc.¹⁶ This meant that heat and light must be fundamentally the same, even though quantitative differences in such properties as wavelength might lead to different effects on the human sense organs. Melloni himself was slower to accept this conclusion than other scientists, but he eventually adopted it in 1842,¹⁷ and by 1847 he had become a strong advocate of the identity of heat and light.¹⁸

¹² William Henry, "A review of some experiments, which have been supposed to disprove the materiality of heat", *Memoirs of the Manchester Philosophical Society*, v (1802), 603-621; John Murray, *Elements of Chemistry* (Edinburgh, 1801), 163. The argument can be found in the literature as late as 1830, e.g. John Bostock's article "Heat" in Brewster's *Edinburgh Encyclopaedia* (Edinburgh, 1830), x, 690.

¹³ E. S. Cornell, "Early studies in radiant heat", *Annals of Science*, i (1936), 217-225; A. Wolf, *A History of Science, Technology & Philosophy in the 18th Century*, 2nd ed. (London, 1952), i, 206-212.

¹⁴ See E. Scott Barr, "The Infrared pioneers—I. Sir William Herschel", *Infrared Physics*, i (1961), 1-4; D. J. Lovell, "Herschel's dilemma in the interpretation of thermal radiation", *Isis*, lix (1968), 46-60; Baden Powell, "Report on the present state of our knowledge of the science of radiant heat", *British Association Report*, ii (1832), 259-300; S. P. Langley, "The history of a doctrine", *Popular Science Monthly*, xxxiv (1889), 212-221, 385-396.

¹⁵ See E. Scott Barr, "The Infrared pioneers—II. Macedonio Melloni", *Infrared Physics*, ii (1962), 67-73; G. Tedesco, "Opera e vita di Macedonio Melloni", *Nuovo Cimento Supplemento* [10] ii (1955), 501-509; G. Dascola, "Mostra di documenti e cimeli melloniani", *ibid.*, 518-522.

¹⁶ As soon as Faraday reported his discovery of the rotation of polarization of light by a magnet, French physicists looked for and found the same effect with radiant heat; see F. de la Provostaye and P. Desains, "Rotation du plan de polarisation de la chaleur produite par le magnétisme", *Annales de chimie et de physique* [3], xxvii (1849), 232-237.

¹⁷ In 1835, Melloni was reluctant to accept the complete identity of heat and light (as suggested by Ampère, see below), arguing that light and radiant heat "proceed from two distinct causes". But in a footnote he added that "These two causes themselves are, perhaps, but different effects of a single cause". He insisted that light and heat are "two essentially distinct modifica-

Before 1820, evidence for the identity of heat and light was at the same time evidence for the materiality of heat, since the particle theory of light was still generally accepted. This was a handicap for scientists such as Davy, who advocated both the particle theory of light and the mechanical theory of heat, and required some awkward contortions to make the two theories seem compatible.¹⁹ On the other hand, as soon as one rejected the particle theory of light, it was quite natural to reject the caloric theory of heat as well. Thus Thomas Young wrote in 1802:

“It was long an established opinion, that heat consists in vibrations of the particles of bodies, and is capable of being transmitted by undulations through an apparent vacuum (Newt. *Opt. Qu.* 18). This opinion has been of late very much abandoned, Count Rumford, Professor Pictet, and Mr. Davy, are almost the only authors who have appeared to favour it; but it seems to have been rejected without any good grounds, and will probably very soon recover its popularity.”²⁰

Young made the connection between heat waves and light waves a little more explicit in his *Lectures on Natural Philosophy* (1807):

“It was Newton’s opinion, that heat consists in a minute vibratory motion of the particles of bodies, and that this motion is communicated through an apparent vacuum, by the undulations of an elastic medium, which is also concerned in the phenomena of light . . . It is easy to imagine that such vibrations may be excited in the component parts of bodies, by percussion, by friction, or by the destruction of the equilibrium of cohesion and repulsion . . .”²¹

As in his attempts to establish the wave theory of light, Young operated on the principle that if the name of Newton can be firmly associated with a theory, physicists will eventually accept it.

In a recent article on Dulong and Petit, Robert Fox has discussed the role of electrochemical experiments and theories in undermining the caloric theory and pointing instead toward a “vibrational” theory of heat

tions which the aethereal fluid suffers in its mode of existence”. See M. Melloni, “Observations et expériences relatives à la théorie de l’identité des agents qui produisent la lumière et la chaleur rayonnante”, *Comptes Rendus . . . Académie des Sciences, Paris*, i (1835), 503-508; *Annales de chimie et de physique* [2], lx (1836), 418-426; *Taylor’s Scientific Memoirs*, i (1837), 388-392; *Annalen der Physik* [2], xxxvi (1836), 486-494.

Melloni declared himself a definite supporter of the wave theory and affirmed the identity of heat and light in 1842: see “Sur l’identité des diverses radiations lumineuses, calorifiques et chimiques”, *Comptes Rendus . . . Académie des Sciences, Paris*, xv (1842), 454-460.

¹⁸ M. Melloni, “Recherches sur les radiations des corps incandescents et sur les couleurs élémentaires du spectre”, *Archiv der Sciences Physiques*, Geneva, v (1847), 238-258; *Phil. Mag.* [3], xxxii (1848), 262-276 (quoted from pp. 274-275). For further discussion see his book *La Thermo-chrôse* (Naples, 1850), long footnote on pp. 272-276.

¹⁹ H. Davy, *Elements of Chemical Philosophy* (Philadelphia, 1812), pp. 46-53, 120. [This and other books cited below were originally published in Britain, but I have cited the American edition when it was the only one easily available.]

²⁰ T. Young, “On the theory of light and colours”, *Phil. Trans.*, xcii (1802), 12-48 (quoted from p. 32).

²¹ T. Young, *A course of lectures on Natural Philosophy and the Mechanical Arts* (London, 1807), i, 654.

in the period 1815-20.²² He quotes a letter from Dulong to Berzelius (1820) on this subject, which contains the following sentence:

“New experiments have brought me to regard as an incontestable truth that all phenomena that are not related to radiant caloric are only the result of vibratory motions of the material molecules themselves. Radiant caloric propagates itself, according to this viewpoint, by the vibrations of the same fluid which, with a greater speed, produces in us the sensation of light.”²³

Of course the fact that something propagates itself by vibrations does not necessarily mean that it consists of nothing more than vibrations; but this vagueness is characteristic of the initial period of development of many scientific theories.

Although most writers on heat in the 1820's still accepted the caloric theory, some of them recognized the importance of the radiant heat studies and began to give equal prominence to the wave theory, concluding that the issue could not be decided yet.²⁴ Moreover, as Gay-Lussac pointed out, the development of a theory of heat in space (“*calorique du vide*”) would require a significant conceptual change in the description of heat: whereas in describing heat in a material substance, “whether one considers the caloric as a body, or as a motion, one can measure its quantity; but in an empty space . . . one can only conceive of caloric in motion”.²⁵

But a few years later Sadi Carnot, one of the founders of thermodynamics, wrote the following in his manuscript notes:

“We may be allowed to express here a hypothesis concerning the nature of heat.

At present, light is generally regarded as the result of a vibratory movement of the etherial fluid. Light produces heat, or at least accompanies the radiant heat and moves with the same velocity as heat. Radiant heat is therefore a vibratory movement. It would be ridiculous to suppose that it is an emission of matter while the light which accompanies it could only be a movement.

Could a motion (that of radiant heat) produce matter (caloric)?

²² Robert Fox, “The background to the discovery of Dulong and Petit’s law”, *British Journal for the History of Science*, iv (1968-69), 1-22.

²³ *Ibid.*, quoted from *Jac. Berzelius Bref*, ed. H. G. Söderbaum (Uppsala, 1912-1932), ii, Part 1, 13-14 (translation by S.G.B.).

²⁴ J. B. Biot, *Précis élémentaire de physique expérimentale*, 2nd ed. (Paris, 1821), ii, 627, reprinted with little or no change in the 3rd ed. (Paris, 1824), ii, 625. Biot discusses the similarity of light and heat, and concludes that whether caloric has a material existence “like light”, or consists in vibrations propagated through a medium, the results would be the same. But still he leans toward the former view.

Johann Carl Fischer concluded a long article on heat, with much emphasis on radiant heat experiments, with the statement that whether heat is a substance or motion still remains to be decided: see *Physikalisches Wörterbuch* (Göttingen, 1827), x, 633-699. Thomas Thomson, in *An Outline of the sciences of heat and electricity* (London, 1830), claimed that the wave theory of heat was held by “the greater number of the French and German chemists of the last century” but concluded (p. 335) that the problem of heat is still insoluble, though he leaned slightly toward the vibrational theories.

²⁵ J. L. Gay-Lussac, “Sur la calorique du vide”, *Annales de chimie et de physique* [2], xiii (1820), 304-308. I am indebted to Mr. Stuart Pierson of the Smithsonian Institution for bringing this reference to my attention. A detailed discussion of the historical background of this paper of Gay-Lussac may be found in the recent article by Pierre Costabel, “Le ‘Calorique du vide’ de Clément et Desormes (1812-1819)”, *Archives internationales d’histoire des sciences*, xxi (1968), 3-14.

Undoubtedly no; it can only produce a motion. Heat is then the result of a motion.

Then it is plain that it could be produced by the consumption of motive power and that it could produce this power."²⁶

Though he had accepted the caloric theory (with some reservations) in his *Reflexions* of 1824, it seems clear from the above passage that Carnot has accepted the wave theory of light shortly afterwards, and by thinking about radiant heat has been led to the mechanical theory of heat. Here was a possible route to thermodynamics which other scientists *might* follow; a few of them did.

Ampère's theory

By 1830 the wave theory of heat was being seriously considered as an alternative to, or modification of, the caloric theory. But the first extended discussion of it seems to have been Ampère's paper published in 1832.²⁷ A subsequent article, going over much of the same ground, appeared in 1835 and received wide publicity,²⁷ so much so that later writers sometimes simply mentioned "Ampère's theory" without giving a specific reference. The combination of Ampère's prestige, and the developments in radiant heat and the wave theory of light, was sufficient to elevate the wave theory of heat to a prominent place in the scientific world for the next decade.

Ampère recognized at the outset a major difficulty in using the same theory to explain the transmission of radiant heat through space and the conduction of heat through material bodies:

"instead of a vibratory motion propagated in undulations or waves in such a manner that every wave leaves at rest the fluid which it sets in motion at the instant of its passage, we have a motion propagated gradually in such a manner that the part which originally was the hottest, and consequently the most agitated (explaining the phenomena of heat by the theory of vibratory motions), although losing heat by degrees, preserves, however, more than the parts to which it is communicating heat."

In modern terms, the problem was to reconcile the propagation of heat by waves (second-order differential equation in time) in free space, with its propagation as described by Fourier's heat conduction equation (first-order time derivative) in matter. But Ampère thought he could answer this and other possible objections to the theory.

²⁶ Translated in Mendoza's edition, *op. cit.* (6), p. 63. According to Mendoza, most of these notes were written around the time of the original composition of the *Reflexions*: see "Contributions to the study of Sadi Carnot and his work", *Archives internationales d'histoire des sciences*, xii (1959), 377-396.

²⁷ "Idées de M^r Ampère sur la chaleur et sur la lumière", *Bibliothèque Universelle*, Geneva, xlix (1832), 225-235. A. M. Ampère, "Note sur la chaleur et sur la lumière considérées comme résultant de mouvement vibratoires", *Annales de chimie et de physique*, lviii (1835), 432-444; *Bibliothèque Universelle*, Geneva, lix (1835), 26-37; *Phil. Mag.* [3], vii (1835), 342-349. The quotations from Ampère's paper are taken from the English translation in *Phil. Mag.*

Ampère postulated that the total *vis viva* of the system is conserved, *vis viva* being defined²⁸ as $\Sigma mv^2 + 2 \int \Sigma \mathbf{F} \cdot d\mathbf{x}$. In equilibrium arrangements the integral term is zero, while it is positive for all positions near equilibrium, this being a condition for the stability of the equilibrium positions. If the atoms vibrate while immersed in a fluid, they will gradually lose *vis viva* to it; if initially one atom is vibrating and the others are at rest, then the fluid will transfer some *vis viva* to these others. However, the total *vis viva* of all the atoms will decrease as waves are propagated through the fluid out of the system, unless we suppose it to be enclosed in a container of vibrators which are maintained in a state of vibration at a constant *vis viva*. Then eventually all the vibrators will approach the same *vis viva* (though they never reach exact equality). If we assume that the rate of flow of *vis viva* between groups of atomic vibrators is proportional to the difference of the *vires vivae* of the groups, then we obtain Fourier's heat conduction equation. This of course will be true only if this difference of *vires viva* is proportional to the difference of temperatures. (The modern reader can hardly restrain himself from putting into Ampère's mouth such phrases as "the *vis viva* itself is assumed to be proportional to the absolute temperature" when he reads this paper!)

Does Ampère reject the caloric theory? Well, not quite, for he says:

"We find manifestly the same result by considering the subject as we have just enunciated it, according to the system of emission [i.e. the material theory] or according to that of vibrations, substituting for the quantity of caloric in the first system, the *vis viva* of the vibratory motions of the molecules in the second. It was in order to render the analogy between the propagation of heat in bodies and that of sonorous vibrations from solid to solid, through the medium of air, more easy of comprehension that I supposed in this explanation that the molecules of bodies do not transmit their vibratory motions one to another [immediately—word added by translator in *Philosophical Magazine*]; that, in the change of form of a molecule, whatever may remain, at the distance at which it is situate[d] from the neighbouring molecule, of the attractive and repulsive forces of the atoms of which the two molecules are composed, is susceptible of experiencing any changes which tend to make the atoms of the second molecule vibrate. But this manner of considering the subject requiring calculations which I have not made, I have not thought proper to insist on the development of the consequences of this idea. My object in these considerations is only to demonstrate how the vibrations by which heat is propagated in bodies may follow a law entirely different from that of the vibrations of sound, of light and of radiant heat . . ."²⁷

In this remarkable paragraph we find juxtaposed three ideas about the nature of heat: first, that it really makes no difference whether heat is

²⁸ ". . . the summation of the products of the masses of all its molecules by the squares of their velocities at a given moment, adding double the integral of the sum of the products of the forces multiplied by the differentials of the spaces described, in the direction of those forces, by each molecule." This is clearly just twice the sum of the kinetic and potential energies of the system; Ampère refers to the two terms as explicit and implicit *vis viva*, respectively.

matter or motion, since in principle the same phenomena can be explained either way—this idea is clearly on the way out, though Ampère (writing in Paris, the stronghold of the caloric theory) makes a polite bow to it; second, heat involves vibrations of atoms transmitted always by vibrations of the ether—this idea seems to be most convenient for mathematical or analogical reasoning at the moment; and third, one might be able to dispense with the intervening ether entirely in treating ordinary heat within a material body, using instead only atomic vibrations—this idea is to be held in reserve pending further calculations. If I may suggest a historical analogy, Ampère's paper is strongly reminiscent of Einstein's "Heuristic point of view about the creation and conversion of light" (1905) in which, ostensibly without attempting to overthrow the wave theory of light, he proposed that certain phenomena could be more satisfactorily explained by a particle theory.

Almost as an afterthought, Ampère rejected quite firmly a doctrine that had dominated atomic speculation during the preceding half-century:

"Now, it is clear that if we admit the phenomena of heat to be produced by vibrations, it is a contradiction to attribute to heat the repulsive force of the atoms requisite to enable them to vibrate."

Reception of the wave theory, 1831-45

Among the early supporters of the wave theory of heat we find C. Matteucci, an Italian physicist (later a prominent politician);²⁹ August de la Rive, Swiss physicist who had provided facilities for some of Melloni's experiments;³⁰ Dionysius Lardner, British encyclopaedist;³¹ Mrs. Mary Somerville, British science writer;³² possibly David Brewster, British physicist;³³ James Forbes, British physicist who made important experimental contributions to the study of radiant heat;³⁴ Gabriel Lamé, Professor of Physics at the Ecole Polytechnique in Paris, best known for

²⁹ "Lettre de M. C. Matteucci à M. Arago sur quelques phénomènes relatifs au calorique (*Annali delle Scienze*, Marzo e Aprile 1832)", *Bibliothèque Universelle*, Geneva, 1 (1832), 1-6.

³⁰ [A. de la Rive], editorial remarks on Pierre Prevost's *Exposition élémentaire des principes qui servent de base à la théorie de la chaleur rayonnante* (Geneva & Paris), in *Bibliothèque Universelle*, Geneva, li (1832), 243-258; and on S. D. Poisson's *Théorie Mathématique de la Chaleur* (Paris, 1835), in *ibid.*, lix (1835), 144-166 (esp. p. 154); lx (1835), 279-309.

³¹ Dionysius Lardner, *Treatise on Heat* [The Cabinet Cyclopaedia] (London, 1833), pp. 394-398.

³² Mrs. Somerville, *On the connection of the physical sciences* (Philadelphia, 1834), p. 195.

³³ See editorial note in *Phil. Mag.* [3], vii (1835), 157 (the editors at that time were Brewster, Richard Taylor and Richard Phillips).

³⁴ James D. Forbes, "On the refraction and polarization of heat", *Transactions of the Royal Society of Edinburgh*, xiii (1835), 131-168 (esp. p. 147); "Note respecting the undulatory theory of heat, and on the circular polarization of heat by total reflection", *Phil. Mag.* [3], vii (1836), 246-249.

his work on elasticity theory;³⁵ William Whewell, British scientist-historian-philosopher,³⁶ and several others.³⁷ Equally significant, perhaps, in indicating the state of scientific opinion are statements by the few remaining caloric theorists such as Poisson, who seem to realize that the wave theory is now the most popular even though they still refuse to accept it themselves.³⁸ Thus, as early as 1834, William West in England wrote:

“I am aware that the once prevalent doctrine of the materiality of caloric and electricity has given way before the conclusions deduced from certain optical phenomena; but . . .”³⁹

Even the terms in which the issue is stated are favourable to the wave theory; no longer is it a question (as it had been 20 or 30 years earlier) of whether heat is substance or quality; it is now a question of whether one is to accept the “emission” theory or the “undulation” theory. Both sides agree that heat and light must be considered together, and those who still maintain the emission theory of heat often seem to be compelled to maintain also the emission theory of light.⁴⁰

It might appear that the considerable interest shown in molecular

³⁵ G. Lamé, “Mémoire sur les lois de l'équilibre de l'éther dans les corps diaphanes”, *Annales de chimie et de physique*, lv (1833), 322-335; “Mémoire sur les vibrations lumineuses des milieux diaphanes”, *ibid.*, lvii (1834), 211-219. In the textbook cited by Mendoza, *op. cit.* (6), Lamé clearly prefers the wave theory to the emission theory, though he states (perhaps to avoid offending Poisson) that it is not necessary to decide between them. [*Cours de Physique de l'École Polytechnique* (Paris, 1836), pp. 297-298]. We may infer something about how much weight Lamé's opinion might carry from the assessment in a recent article by J. W. Herivel, “Aspects of French theoretical physics in the nineteenth century”, *British Journal for the History of Science*, iii (1966-67), 109-132. In addition to providing much useful information about the situation in Paris which is relevant to the background of the wave theory of heat, Herivel points out that in the period 1850-1870, “and for that matter in the decade immediately preceding 1850, [there was] no creative French theoretical physicist remotely of the calibre of Thomson or Clausius, let alone Maxwell”. In a footnote he specifies that “creative” is to be taken “as opposed to a competent, and even original, theoretical physicist such as G. Lamé (1795-1870)” (*ibid.*, p. 115). Stretching this just a bit, we could say that the best physicist in France was a supporter of the wave theory of heat, even if some of the others opposed it.

³⁶ William Whewell, *History of the Inductive Sciences* (London, 1837), ii, 180-184. Aside from its value as contemporary evidence for the acceptance of the wave theory of heat, Whewell's work is almost the only publication on the *history* of science which discusses this theory. Rosenberger mentions Ampère's theory but states incorrectly that Ampère was the only scientist who attributed both heat and light to vibrations of the same ether; see Ferd. Rosenberger, *Die Geschichte der Physik* (Braunschweig, 1887-1890), iii, 230-223; see also note 76, below.

³⁷ [J. C. Poggendorff] editorial note added to the German translation of a paper by Melloni, in *Annalen der Physik und Chemie* [2], xxxviii (1836), 2; anonymous article on “Recent researches on heat” in *Magazine of Popular Science*, i (1836), 145-160; l'Abbé Pinault, *Traité élémentaire de physique*, 2nd ed. (Paris, 1836), p. 292; Andreas Baumgartner, *Anfangsgründe der Naturlehre* (Wien, 1837), pp. 131-132 [this book contains one of the first uses of the word *Thermodynamik*, defined here as the study of the motion of heat]; Thomas Webster, *The elements of Physics* (London, 1837), p. 280; C. C. Person, *Éléments de physique* (Paris, 1837), ii, 224-225.

³⁸ S. D. Poisson, *Théorie Mathématique de la Chaleur* (Paris, 1835); *Mémoire et notes formant un supplément . . .* (Paris, 1837), p. 27 f.

³⁹ William West, “On a remarkable analogy between ponderable bodies, and caloric and electricity”, *Phil. Mag.* [3], v (1834), 110-112.

⁴⁰ Poisson, *op. cit.* (38); John Barton, “On the physical causes of the principal phenomena of heat”, *Phil. Mag.* [3], x (1837), 342; Robert Hare, *A compendium of the course of chemical instruction in the Medical Department of the University of Pennsylvania*, new section added to the fourth edition (Philadelphia, 1840), 75-76; John Johnston, *A Manual of Chemistry* (Philadelphia, 1842), 59; H. W. Brandes, *Vorlesungen über die Naturlehre* (Leipzig, 1844), pp. 471, 558; Leopold Gmelin, *Handbook of Chemistry*, trans. from German ed. of 1843 (London, 1848), pp. 163, 167, 212.

theories such as that of Mossotti⁴¹ contradicts my assertion that most scientists accepted the wave theory of heat after 1830. Mossotti's molecular model—a nucleus surrounded by an atmosphere of fluid particles which attract the nucleus but repel each other—is quite similar to that used by caloric theorists, who identified the fluid particles with caloric. But careful examination of later papers on this subject shows that this identification was gradually forgotten.⁴² An especially interesting case is the theory of Philip Kelland (Professor of Mathematics at Edinburgh University) who actually did state that the atmosphere of an atom is composed of “caloric” particles; yet then went on to suggest that heat is transmitted by the *vibrations* of the particles of caloric.⁴³ This example shows how a scientist may adhere to the terminology of the caloric theory yet abandon its substance; perhaps one should translate “caloric fluid” as “ether” in all works written after 1830.

Another apparent exception to the general adoption of the wave theory was the persistent idea that heat (caloric) is a “repulsive principle” or “repulsive force”. (Despite Ampère's remark quoted at the end of the last section, even some of the wave theorists retained this idea.) It has

⁴¹ O. F. Mossotti, *Sur les forces qui régissent la constitution intérieure des corps, aperçu pour servir à la détermination de la cause et des lois de l'action moléculaire* (Turin, 1836); *Taylor's Scientific Memoirs*, i (1837), 448-469; other works on this subject reprinted in his *Scrutti* (Pisa, 1951). The editors of *Philosophical Magazine* said that his “mutual identification of the attractive forces of electricity, aggregation, and gravitation” constituted “one of the most remarkable discoveries of the present area in science” (see vol. x (1837), 320-321).

⁴² James Challis, “On capillary attraction and the molecular forces of fluids”, *Phil. Mag.* [3], vii (1836), 89-96; Philip Kelland, “On the motion of a system of particles, considered with reference to the phenomena of sound and heat”, *Trans. Cambridge Phil. Soc.* vi (1837), 235-288; “On molecular equilibrium, Part I”, *ibid.*, vii (1839), 25-59; “Reply to some objections against the theory of molecular action according to Newton's law”, *Phil. Mag.* [3], xxi (1842), 124-130, 202-208, 263-270; “On Mossotti's theory of molecular action”, *ibid.*, xx (1842), 8-10; S. Earnshaw, “On the nature of the molecular forces which regulate the constitution of the luminiferous ether”, *Trans. Cambridge Phil. Soc.*, vii (1839), 97-112. This last paper by Earnshaw contains the famous “Earnshaw theorem” in electrostatics which was used as an argument against all static atomic models based on the equilibrium of some arrangement of charged particles, around 1900; see *American Journal of Physics*, xxvii (1959), 418.

See also Charles Babbage, *The Ninth Bridgewater Treatise, a fragment* (London, 1837), pp. 180-185; Thomas Exley, “Remarks on M. Mossotti's theory of physics, suggested by Mr. Babbage's notice of the same”, *Phil. Mag.* [3], xi (1837), 496-504; Paul Cooper, “Notice of a theory of molecular action”, *Phil. Mag.* [3], x (1837), 355; R.L.E., “Remarks on M. Mossotti's theory of molecular action”, *ibid.*, xix (1841), 384-387. Various theories of this kind are summarized by Isaac Todhunter, *A History of the theory of elasticity and of the strength of materials from Galilei to the present time* (Cambridge, 1886).

⁴³ Philip Kelland, *Theory of Heat* (Cambridge, 1837), pp. iii, 104, 145, 181-182. Kelland published a further explanation of his views in a note added to a new edition of Thomas Young's *Course of Lectures on Natural Philosophy* (London, 1845), p. 506. He states that although recent experiments on polarization and conduction do show the wave nature of heat, most other phenomena such as latent heat cannot be explained by a purely wave theory as yet. The facts “appear to demand a corpuscular theory, wholly or partly accompanied by transverse vibrations. The hypothesis which I have advanced [in *Theory of Heat*] is, that heat is due to the existence of repulsive atoms which penetrate all material substances; so that expansion arises from the accumulation of such atoms; but that the transmission of heat is partly effected by transverse pulses . . . Solar heat is transmitted altogether by such transverse pulses, so that its intensity is measured by the intensity of the pulses, whilst the heat of a fire is perhaps due in part to normal ones, or, which is the same thing, to a flow of atoms impelling by their repulsion those which are in advance of them.” (I am indebted to Dr. Charles Weiner for this reference.)

been suggested that the identification of heat as a force rather than as matter could have played a role in the decline of the caloric theory, and there is indeed some evidence of this as an influence of *Naturphilosophie*.⁴⁴ But the idea was common in the eighteenth century *before* the rise of the caloric theory, and remained popular long after that theory was dead.⁴⁵ It also inspired occasional attempts to detect a force between macroscopic heated bodies.⁴⁶ Though the "repulsive power of heat" affected the language of various theories, it did not help very much to discriminate between them.

Laplace died in 1827, Fourier in 1830, and Poisson in 1840. Who was left to defend the caloric theory? Only a few minor scientists such as Pouillet, Pinaud, and Soubeiran in France,⁴⁷ Comstock and Grund in America,⁴⁸ and a handful of others.⁴⁹ The Academy of Sciences in Paris did continue to resist the newer ideas, despite the influence of Ampère and Lamé; the *Comptes Rendus* records several papers on the nature of heat, of which only the title or a brief summary was allowed to be published; some of them probably contained views favourable to the mechanical

⁴⁴ L. Pearce Williams, "The physical sciences in the first half of the nineteenth century; Problems and sources", *History of Science*, i (1962), 1-15; Robert Kargon, "The decline of the caloric theory of heat: a case study", *Centaurus*, x (1964), 253-257; H. C. Oersted, *Recherches sur l'identité des forces chimiques et électriques* (Paris, 1813), pp. 159-200.

⁴⁵ See Grove's lecture of 1843, cited below in note 52, p. 53; William and Robert Chambers, *Elements of Natural Philosophy* (New York, 1849), p. 31; William Petrie, "On the relation between the changes of temperature and volume of gases", *Edinburgh New Philosophical Journal*, li (1851), 120-125; S. E. Coues, *Outlines of a System of Mechanical Philosophy* (Boston, 1851), p. 26; Zerah Colburn, *An inquiry into the nature of heat* (London, 1863); A. Cazin, "Recent progress in relation to the theory of heat", *Smithsonian Institution Report*, 1868, pp. 231-244.

⁴⁶ A. Fresnel, "Note sur la répulsion que des corps échauffés exercent les uns sur les autres à des distances sensibles", *Annales de chimie et de physique* [2], xxix (1825), 57-62; "Observation à ajouter à la note sur les répulsions des corps échauffés", *ibid.*, 107-108; Baden Powell, "On the repulsive power of heat", *Phil. Trans.*, cxxiv (1834), 485-589; "Notes on repulsion by heat, etc.", *Phil. Mag.* [3], xii (1838), 317-321; R. Addams, "Notice of some experiments which show a repulsive action between heated surfaces and certain pulverulent bodies", *Phil. Mag.* [3], vi (1835), 415-417; William Crookes, "On repulsion resulting from radiation", *Phil. Trans.*, clxiv (1874), 501-527. The Crookes paper shows the connection between this earlier research on the repulsion of heat, possibly associated with an emission theory, and the radiometer fad of the 1870's. Maxwell showed in 1873 that the wave theory does predict radiation pressure (for both light and radiant heat), but the magnitude of this is so small in ordinary circumstances that it is completely masked by gas-surface interactions even at fairly low pressures. See A. E. Woodruff, "William Crookes and the Radiometer", *Isis*, lviii (1966), 188-198; S. G. Brush and C. W. F. Everitt, "Maxwell, Osborne Reynolds, and the Radiometer", *Historical Studies in the Physical Sciences*, i (1969), 105-125.

⁴⁷ [Claude] Pouillet, *Éléments de physique expérimentale et de météorologie*, 2nd ed. (Paris, 1832), pp. 237-238; "Mémoire sur la théorie des fluides élastiques et sur la chaleur latente des vapeurs", *Comptes Rendus* . . . Académie des Sciences, Paris, xxiv (1847), 915-943. Pouillet was Professor of Physics at the École Polytechnique. He did not change his statements about heat in the 1847 and 1856 editions of his textbook, but later German editions (1847, 1852, etc.) edited by Joh. Müller dropped the support for caloric theory.

E. Soubeiran, *Précis élémentaire de physique*, 2nd ed. (Paris, 1846); Aug. Pinaud, *Programme d'un cours élémentaire de physique*, 5th ed. (Paris, 1848), p. 113.

⁴⁸ J. L. Comstock, *Elements of Chemistry*, 10th ed. (New York, 1834), p. 11 and 29th ed. (New York, 1839), p. 11; also in the 1852 edition; Francis J. Grund, *Elements of Natural Philosophy*, 2nd ed. (Boston, 1835), p. 186.

⁴⁹ Neil Arnott, *Elements of Physics* (Philadelphia, 1841), p. 281 [Arnott was at the Royal College of Physicians in London]; F. A. Clemens, *Grundriss der Naturlehre* (Königsberg, 1839), ii, 75-76; Josiah P. Cooke, *Elements of Chemical Physics* (Boston, 1860), p. 430.

theory.⁵⁰ At least we know that Mayer's paper was refused publication until after a translation of Joule's work had appeared in 1848.⁵¹

By 1842 William Robert Grove, lecturing at the London Institution, could assume that his audience already knew that the wave theory of heat was considered the most satisfactory, although he personally thought it was superfluous to assume a peculiar ethereal fluid: ordinary matter diffused through space would be sufficient to transmit the vibrations.⁵² Similarly, Mohr in Germany⁵³ and Joseph Henry in America⁵⁴ were acknowledging that they had been led to a wave or vibratory conception of heat by the facts of *radiant* heat, even though they preferred to minimize the importance of the ether. Melloni, as I have already noted, declared his allegiance to the wave theory in 1842 and more definitely in 1847.^{54a} Berzelius, sceptical in 1839, accepted it by 1845.⁵⁵ In 1845, the physiologist, Ernst Brücke, published a critical review of the evidence against the identity of heat and light, in connection with his studies on the physical properties of the eye; he apparently wanted to believe in this identity and to accept the wave theory of heat, though there were still some

⁵⁰ Babinet, "Sur la chaleur dans l'hypothèse des vibrations", *Comptes Rendus . . . Académie des Sciences, Paris*, vii (1838), 781 [cf. *ibid.*, lxiii (1866), 581-588, 662-666]; Couche, "... présente, à l'occasion d'une note récente de M. Melloni, des considérations théoriques sur les phénomènes de chaleur et de lumière", *ibid.*, xviii (1844), 312; Paget, "... une Note sur une nouvelle théorie de la chaleur", *ibid.*, xix (1844), 1406; Briot, "Essai sur la théorie mécanique de la chaleur", *ibid.*, xxiv (1847), 877.

⁵¹ See "Considerations sur la production de la lumière et de la chaleur du soleil (Commissaires, MM. Pouillet, Babinet)", *ibid.*, xxiii (1846), 220; "Mémoire sur la production de la lumière et de la chaleur de soleil (Commissaires, MM. Arago, Cauchy)", *ibid.*, xxiii (1846), 544; "Sur la transformation de la force vive en chaleur, et réciproquement", *ibid.*, xxvii (1848), 385-387.

⁵² William Robert Grove, *A lecture on the progress of physical science since the opening of the London Institution* (London, 1842), p. 27; see also "On the correlation of physical forces" (1843) in E. L. Youmans (ed.), *The correlation and conservation of forces* (New York, 1865), p. 55.

⁵³ F. Mohr, "Views of the nature of heat", *Phil. Mag.* [5], ii (1876), 110-113, trans. from Liebig's *Annalen der Chemie*, xxiv (1837), 141-147. In his note at the end of this translation, P. G. Tait asserts that this paper "contains, in a considerably superior form, almost all that is correct in Mayer's paper". See also [Karl] Friedrich Mohr, *Allgemeine Theorie der Bewegung und Kraft, als Grundlage der Physik und Chemie* (Braunschweig, 1869), which includes a reprint of the original 1837 paper in German; Ralph E. Oesper, "Karl Friedrich Mohr", *Journal of Chemical Education*, iv (1927), 1357-1363. In his textbook on heat (London, 1884, reprinted with corrections 1904, p. 247) Tait discussed the experiments showing the identity of light and radiant heat, and remarked: "It is curious to notice that the original speculations of Mohr, of date 1837, as to the true nature of heat were mainly based on these discoveries."

⁵⁴ Joseph Henry, "Remarks on the corpuscular hypothesis of the constitution of matter", *Proceedings of the American Philosophical Society*, iv (1846), 287-290; "On the theory of the so-called imponderables", *ibid.*, vi (1851), 84-91; see also his report on the interference of heat-rays, *ibid.*, iv (1846), 285, and "On heat, and on a thermal telescope", *American Journal of Science*, v (1848), 113-114. In the last paper cited he says, "The facts with regard to heat as well as light therefore show that the theory of undulation is not an imagination, but the expression of a law". Henry met Melloni in Paris in 1837 and this encounter may have stimulated his interest in radiant heat: see *Edinburgh New Philosophical Journal*, xxvi (1839), 300, and Thomas Coulson, *Joseph Henry* (Princeton, 1950), p. 122.

^{54a} See note 18.

⁵⁵ J. J. Berzelius, *Traité de Chimie*, nouvelle édition entièrement refondue d'après la 4^{me} édition allemande, publiée en 1838 (Bruxelles, 1839), i, 35; *Traité de chimie minérale, végétale et animale*, seconde édition française (Paris, 1845), i, 35.

obstacles.⁵⁶ A few writers, while acknowledging that the material theory of heat was probably wrong, continued to use it for the sake of "simplicity" in explaining the phenomena.⁵⁷ This attitude was criticized by a French textbook-writer, Bailly, who insisted that since the wave theory had been established by the latest results of scientific research it must also be used in teaching about heat.⁵⁸ Bailly also recognized the possibility of a third theory, in which heat would be attributed to the vibrations of bodies rather than to the ether, but he asserted that such a theory is refuted by direct observations and has been generally abandoned.

Although there is thus abundant evidence for the popularity of the wave theory during the 1840's,⁵⁹ no one seems to have gone beyond Ampère's work and tried to deduce quantitative consequences different from those of the caloric theory (Ampère himself died in 1836). The most famous prediction of the wave theory of heat is the so-called Rayleigh-Jeans law, which was not published until 1900, and was not believed to be valid even by Rayleigh.⁶⁰ Aside from this, the primary significance of the theory for the subsequent development of physics was the part it played in the discovery of energy conservation and thermodynamics, which we must now examine.

⁵⁶ Ernst Brücke, "Ueber das Verhalten der optischen Medien des Auges gegen Licht—und Wärmestrahlen", *Annalen der Physik* [2], lxxv (1845), 593-607. The identity of heat and light was rejected by L. Moser, "Ueber die Verschiedenheit der Licht—und Wärmestrahlen", *ibid.*, lviii (1843), 105-111.

⁵⁷ E. Pecelet, *Traité élémentaire de physique*, 4th ed. (Bruxelles, 1838), pp. 234-235; J.-M.-M. Peyré, *Cours de Physique*, 2nd ed. (Paris, 1840), p. 256; J. Persoz, *Introduction à l'étude de la chimie moléculaire* (Paris and Strasbourg, 1839), p. 218; C. Despretz, *Traité élémentaire de physique*, 3rd ed. (Paris, 1832), p. 77; John Johnston, *A Manual of Chemistry* (Middletown, 1840), pp. 13, 57; J. Frederic Daniell, *An introduction to the study of chemical phenomena* (London, 1843), pp. 208-209.

⁵⁸ C. Bailly, *Nouveau manuel complet de physique* (Paris, 1841), pp. 204-207.

⁵⁹ [Antoine] Becquerel, *Traité de Physique* (Paris, 1842), i, 163; Mrs. Somerville, *On the connection of the physical sciences*, 7th ed. (London, 1846), p. 258; John W. Draper, *Textbook on Natural Philosophy* (New York, 1847), p. 253; E. Pecelet, *Traité élémentaire de physique* (Bruxelles, 1847), p. 361; Johann Müller, *Principles of Physics and Meteorology*, trans. from German (London, 1847), p. 497 [Müller argues that because radiant heat consists in ether vibrations, therefore sensible heat must consist in vibrations of the material parts of bodies; many others followed this line of argument at least implicitly]; Golding Bird, *Elements of Natural Philosophy* (London, 1848), p. 487; C. H. D. Buys-Ballot, *Scheets eener physiologie van het onbewerktaigte ryk de natuur* (Utrecht, 1849), as summarized by Rosenberger, *op. cit.* (36), pp. 538-540; Thomas Graham, *Elements of Chemistry*, 2nd American ed. based on the 2nd English ed. of 1850 (Philadelphia, 1852), p. 96.

The authors of many of the textbooks cited in note 74 below probably held similar views in the 1840's. I have not attempted to track down all the first editions, since the evidence already obtained seems to be sufficient to establish the point.

An elaborate critical review of opinions about the nature of heat, with references to a number of works published in the early nineteenth century, may be found in Muncke's article "Wärme" in *Gehler's Physikalisches Wörterbuch*, Bd. 10, 1st Abt. (Leipzig, 1841).

⁶⁰ Rayleigh, "Remarks upon the law of complete radiation", *Phil. Mag.* [5], xlix (1900), 539-540; reprinted with a note, dated 1902, on Planck's formula, in Rayleigh's *Scientific Papers* (Cambridge), iv (1903), 483-485. Rayleigh's intention in this paper was not to deduce a distribution function for black-body radiation as a rigorous consequence of classical physics, but to improve Wien's distribution by using the assumption that equipartition applies *only to low frequencies*. In this way he obtained the formula $\theta k^2 dk$ (θ = absolute temperature, k = wave number), which if integrated over all k would of course diverge; but Rayleigh explicitly stated that for large k one must introduce an exponential factor $e^{-ck/\theta}$. Thus he recognized that equipartition could not apply to high frequencies, but did not by any means imply that this was to be regarded as a failure of classical physics.

Transition from wave theory of heat to thermodynamics

In his classic article on the history of energy conservation, Thomas S. Kuhn lists 12 co-discoverers of the principle.⁶¹ Four of them (Mayer, Joule, Colding, and Helmholtz) are considered the primary discoverers because they not only announced the general principle but also provided concrete quantitative applications. Four others (Carnot, Marc Seguin, Karl Holtzmann, and G. A. Hirn) computed a mechanical equivalent of heat but did not bother to make a general statement about the convertibility of *all* forms of energy. A third quartet (C. F. Mohr, W. R. Grove, Faraday, and Liebig) did make such a general statement but failed to develop the numerical aspects of energy conversions. Kuhn argues convincingly that the "simultaneous" nature of this discovery—all but two published their work between 1837 and 1847, probably independently—implies the existence of some common factors in the environment of early nineteenth-century science, factors not present earlier. He identifies three such factors: the development of a quantitative book-keeping approach in steam-engine technology; discoveries of many conversion processes linking electricity, magnetism, and heat; and speculations of *Naturphilosophie* suggesting the basic unity of all forces in nature. Having examined some of the writings of these 12 men, I propose to add a fourth factor: investigations of radiant heat, and in particular the wave theory of heat.

The views of Carnot on radiant heat have already been quoted. The case of C. F. Mohr is also fairly clear-cut since he states:

"The phenomena of heat have been till now almost exclusively explained in textbooks by the assumption of a heat-substance. The discoveries of Melloni have made this view inapplicable to the phenomena of Radiant Heat; they require the assumption of vibrations similar to those of the Undulatory Theory of Light. The Propagation, Transmission, and Polarization of Radiant Heat have been completely explained by these assumptions; and, with such facts to guide us, it is certainly no mere idle speculation to attempt to extend this view to the phenomena of common or stationary heat . . . Heat is thus no longer a particular kind of matter, but an oscillatory motion of the smallest parts of bodies."^{61a}

Grove, in his 1842 lecture cited above, and in his general statement of energy conservation a year later, indicated his qualified acceptance of the wave theory of heat.^{61b} Helmholtz, in his 1847 memoir on the conservation of force, concluded that heat must be explained in terms of motion, preferably by a wave theory such as Ampère's.⁶² The importance of the wave theory of heat in his thinking can be better judged from an article on

⁶¹ Thomas S. Kuhn, "Energy conservation as an example of simultaneous discovery", in Marshall Claggett (ed.), *Critical Problems in the History of Science* (Madison, 1959), pp. 321-356; reprinted in Bernard Barber & Walter Hirsch (eds.), *The Sociology of Science* (New York, 1962), pp. 486-515.

^{61a} Mohr, *op. cit.* (53).

^{61b} See note 52.

⁶² Hermann von Helmholtz, *Ueber die Erhaltung der Kraft* (Berlin, 1847); the passage referred to may be found on pages 108-109 of my anthology, *Kinetic Theory*, vol. 1 (Oxford, 1965).

physiological heat which he wrote for a medical encyclopaedia two years earlier; here, Helmholtz wrote:

“Recently, especially through the complete equality of the laws of heat radiation with those of light, not only the similarity but indeed the identity of both agents has been made probable, and we are thereby led to a wave theory of heat, as to a wave theory of light. Moreover, it is found that heat can actually be generated by various other natural forces, without the occurrence of such changes in the molecular properties of the body to which one might attribute the liberation of latent heat. In particular, first, heat is liberated by the annihilation of mechanical force in the friction of solids against solids, or solids against fluids; second, by the equalization of electric tension, which can again be produced by rubbing or by the motion of magnets . . . Thus the possibility of a material theory of heat disappears, since the conservation of quantity would be the most necessary consequence of such a theory, and we are forced to consider heat as well as light to be motion. The relation between free and latent heat discussed above in the language of the material theory would still remain unchanged, if in place of quantity of substance we put quantity of motion, according to the basic laws of mechanics; there is only a difference when we are concerned with the creation of heat motion by other forces of motion and we have to determine the equivalent amount of heat produced by a definite quantity of mechanical or electrical force.”⁶³

For the other discoverers of energy conservation, the influence of the wave theory was much weaker. Joule, in 1845, made a brief reference to it, suggesting that Davy’s idea of rotating molecules might be revived; in order to apply that theory to radiation,

“we have only to admit that the revolving atmospheres of electricity possess, in greater or less degree, according to circumstances, the power of exciting isochronal undulations in the aether which is supposed to pervade space.”⁶⁴

But Joule was soon to discard Davy’s theory of molecular motion in favour of Herapath’s (i.e. the kinetic theory of gases), and in his later writings on the nature of heat he implies and sometimes even explicitly states that radiant heat is irrelevant to thermodynamics.⁶⁵

⁶³ Hermann von Helmholtz, “Wärme, physiologisch”, *Encyclopädisch Handwörterbuch der medicinischen Wissenschaften* (1845), reprinted in his *Wissenschaftliche Abhandlungen* (Leipzig, 1882-1895), ii, 680-725; quotation from pp. 699-700.

⁶⁴ J. P. Joule, “On the changes of temperature produced by the rarefaction and condensation of air”, *Phil. Mag.* [3], xxvi (1845), 369-383.

⁶⁵ J. P. Joule, “On the mechanical equivalent of heat and on the constitution of elastic fluids”, *British Association Report*, xviii (1848), 21-22 [transition to Herapath theory, no mention of radiant heat]; “On the mechanical equivalent of heat”, *Phil. Trans.*, cxi (1850), 61-82 [radiant heat and similar subjects “do not exactly come within the scope of the present memoir”]. Further indication of Joule’s ambivalent attitude toward the wave theory of heat is found in an undated draft manuscript at Manchester University: “Fresh arguments were, however, constantly adduced in favour of the vibratory hypothesis and the labours of Forbes and others added new proofs of the real nature [the word “character” is deleted] of heat [phrase “when in the year 1843” deleted]. To these I need not advert at any length [phrase “but will proceed to the researches made by” deleted] as the subject of radiation of heat is [phrase “not necessarily connected with our subject” deleted] an exceedingly complicated one and would occupy too much time nor is the proof derived from the phenomena of radiation a decisive one . . .” (from papers held at the Department of History of Science and Technology, The University of Manchester Institute of Science and Technology; a microfilm copy was kindly provided by Dr. Arnold J. Pacey).

Faraday, in lectures on heat at the Royal Institution in 1845, reviewed Melloni's experiments on radiant heat and endorsed the "analogy that Melloni has drawn between the various rays of light and those of heat" but did not commit himself to any specific theory of the nature of heat.⁶⁶ In his speculations on ray-vibrations, disclosed (somewhat unwillingly) the following year, he preferred to discard the conventional ideas about the ether in favour of his lines of force. Thus "radiation" (both luminous and calorific) might consist in vibrations of lines of force; but this did not seem to entail any particular consequences for the nature of heat.⁶⁷

Mayer, as has often been noted, was somewhat contemptuous of all attempts to reduce heat to motion, preferring to think of it as a "force" of equivalent status to other forces; his attitude is best illustrated for our purposes by the following remark which he published in 1851:

"We are taught by history that . . . the most sagacious hypotheses concerning the state and nature of a peculiar 'matter' of heat, concerning a 'thermal aether', whether at rest or in a state of vibration, concerning 'thermal atoms', supposed to exercise their functions in the interstices between the material atoms, or other hypotheses of like nature, have not availed to solve the problem."⁶⁸

These examples (taken with my failure to find enthusiasm for the wave theory of heat among those co-discoverers concerned primarily with the engineering aspects of heat and work) suggest that the speculations about radiant heat did contribute something to the climate of scientific opinion that favoured the emergence of energy conservation in the 1840's; but, like the other factors mentioned by Kuhn, they were neither necessary nor sufficient in leading to that discovery. That the wave theory of heat was a partial but not a *sufficient* basis for thermodynamics is shown by the case of W. J. M. Rankine, the Scottish engineer-physicist who was one of the three founders of thermodynamics (with Clausius and Thomson). Rankine tells us that the object of his researches on the hypothesis of molecular vortices was

"to deduce the laws of elasticity and of heat as connected with elasticity, by means of the principles of mechanics, from a physical supposition consistent with and connected with the theory which deduces the laws of radiant light and heat from the hypothesis of undulations. Those researches were commenced in 1842 . . ."

but put aside for several years for lack of experimental data, then resumed when Regnault's experiments were published.⁶⁹ Rankine continued to develop his own version of the wave theory of heat, though it was not

⁶⁶ Michael Faraday, "On Heat", *Magazine of Science*, vi (1845), 126, 131-132, 139-140, 151-152, 215-216.

⁶⁷ Michael Faraday, "Thoughts on ray-vibrations", *Phil. Mag.* [3], xxviii (1845), 447-452.

⁶⁸ J. R. Mayer, *Bemerkungen über das mechanische Aequivalent der Wärme* (Heilbronn and Leipzig, 1851); *Phil. Mag.* [4], xxv (1863), 493-521 (quoted from p. 498).

⁶⁹ W. J. M. Rankine, "On the mechanical action of heat, especially in gases and vapours", *Transactions of the Royal Society of Edinburgh*, xx (1850), 147-190.

recognized as such by his contemporaries, and was generally ignored after the revival of the kinetic theory of gases.

Disappearance of the wave theory of heat after 1850

The wave theory of heat might have been the starting-point for the new kinetic theory of gases, but the circumstances were unfavourable. In 1845, a Scottish scientist, J. J. Waterston, submitted a paper to the Royal Society of London, containing a comprehensive development of the kinetic theory. Waterston's paper began with the remark:

"Of the physical theories of heat that have claimed attention since the time of Bacon, that which ascribes its cause to the intense vibrations of the elementary parts of bodies has received a considerable accession of probability from the recent experiments of Forbes and Melloni. It is admitted that these have been the means of demonstrating that the mode of its radiation is identical with that of light in the quantities of refraction and polarization. The evidence that has been accumulated in favour of the undulatory theory of light has thus been made to support with a great portion of its weight a like theory of the phenomena of heat . . ."70

But the Royal Society referees (Baden Powell and John William Lubbock) did not think Waterston's paper deserved publication, and it remained unknown in the Royal Society archives until 1891 when Lord Rayleigh disinterred it.⁷¹ When Joule, Clausius, and Maxwell revived the kinetic theory they based their assumptions on the mechanical theory of heat but tended to treat molecular thermal motion completely apart from radiant heat. Moreover, as Waterston himself had pointed out, in order to accept the kinetic theory of gases, it was necessary to assume that molecules can move freely through empty space (except when they collide with each other or with solid objects) so that any kind of energy exchange with an ether has to be ignored.⁷² Thus the role of ether vibrations had to be eliminated from the theory of gas properties, even though it was still important in spectroscopy.

As Louis Soret in Geneva pointed out in 1854, the wave theory of heat is completely consistent with thermodynamics, and at that time there seemed to be no reason why the two theories could not peacefully coexist.⁷³

⁷⁰ J. J. Waterston, "On the physics of media that are composed of free and perfectly elastic molecules in a state of motion", *Phil. Trans.*, clxxxiii (1893), 5-79 [received 11 December 1845, read 5 March 1846]; reprinted in *The Collected Scientific Papers of John James Waterston*, ed. J. S. Haldane (Edinburgh, 1928).

⁷¹ See R. J. Strutt, *Life of John William Strutt, Third Baron Rayleigh* (London, 1924; augmented edn., Madison, 1968, pp. 169-171, 417); S. G. Brush, *Kinetic Theory*, vol. i (Oxford, 1965), pp. 17-18; Waterston's *Papers*, *op. cit.* (70).

⁷² Waterston, *Papers*, pp. 278-279.

⁷³ Louis Soret, "Sur l'équivalence du travail mécanique et de la chaleur. Revue des recherches expérimentales", *Archiv des Sciences Physiques*, xxvi (1854), 33-54. Soret quotes Joule's remark [*op. cit.* (64)] about heat waves excited by rotating molecules, not realizing that Joule has since dropped his interest in radiant heat (see text above). See also G. von Quintus Icilius, *Experimental-Physik* (Hannover, 1855) who accepts the wave theory of heat and implies that it is compatible with the mechanical theory; J. Jamin, *Cours de Physique de l'École Polytechnique* (Paris, 1859), ii, 248, 436.

Indeed, many books and papers by minor scientists continued to use or refer to the wave theory as if it were still acceptable for several decades after 1850.⁷⁴ However, the leading physicists of this period, Joule, Thomson, Clausius, Helmholtz, Maxwell, Boltzmann, etc., seemed to ignore it.

While there may have been good reasons for dropping the wave theory of heat at this particular stage of physics, it is still rather puzzling that it has been so completely forgotten in works on the history of physics. With the exception of a few nineteenth-century historians,⁷⁵ almost all accounts state or imply that the caloric theory was accepted until it was replaced by thermodynamics around 1845-50.⁷⁶ Sometimes this myth is combined with the other one, and it is stated that although Rumford and Davy "really" established the mechanical theory, the caloric theorists obstinately persisted in their error for another 40 or 50 years, until Mayer and Joule finally persuaded other scientists to accept a truth that should have been obvious in 1800. Without becoming involved in an extensive digression on the historiography of nineteenth-century physics, I would like to suggest one possible origin for both myths.

In 1849, William Thomson wrote a remarkable paper on heat, in which he referred to "the ordinarily-received, and almost universally-acknowledged, principles" with reference to "quantities of caloric" and

⁷⁴ Zachariah Allen, *Philosophy of the mechanics of nature* (New York, 1852), pp. 41, 344; *Solar Light and Heat* (New York, 1879), pp. 28, 68; C. B. Greisz, *Lehrbuch der Physik* (Wiesbaden, 1853), p. 390; Johann Müller, *Grundriss der Physik und Meteorologie*, 4th ed. (Braunschweig, 1853), p. 460; L. Soret, *op. cit.* (73); Andrew Brown, *The Philosophy of Physics* (Redfield, N.Y., 1859), pp. 215-225, 273-277; Quintus Icilus, *op. cit.* (73); P. A. Daguin, *Traité élémentaire de Physique* (Toulouse and Paris, 1855, 1861), i, 626, ii, 9-10, and similar remarks in the 4th ed. (Paris, 1878); A. Ganot, *Traité élémentaire de Physique*, 6th ed. (Paris, 1856), p. 210; English translation of Ganot's book, *Elementary Treatise on Physics*, 12th ed. (New York, 1886), p. 260; F. Redtenbacher, *Das Dynamiden-System* (Mannheim, 1857); Laurens P. Hickok, *Rational Cosmology* (New York, 1858), pp. 175 f.; Julius Wenck, *Die Physik* (Leipzig, 1858), p. 342; Benjamin Silliman, *First Principles of Physics* (Philadelphia, 1859), p. 303; J. Jamin, *op. cit.* (73); Alb. Mousson, *Die Physik* (Zürich, 1860), ii, 4; Jabez Hogg, *Elements of experimental and natural philosophy* (London, 1861), p. 236; Marié-Davy, art. "Chaleur" in the Privat-Deschanel and Focillon *Dictionnaire General des Sciences* (Paris, 1864), pp. 430-436; W. A. Norton, "On molecular physics", *American Journal of Science*, xxxviii (1864), 61-78, 207-233, and several subsequent papers; John Tyndall, "The constitution of the universe", *Fortnightly Review*, iii (1865), 129-144; Carl Puschl, *Das Strahlungsvermögen der Atome* (Wien, 1869); James Challis, *Notes on the Principles of Pure and Applied Calculation* (Cambridge, 1869); Denison Olmsted, *An Introduction to Natural Philosophy*, 4th ed. (New York, 1870), p. 310; Henry Hudson, "Electrical repulsion", *English Mechanic*, xix (1874), 121; Favé, "Consequences vraisemblables de la théorie mécanique de la chaleur", *Les Mondes*, xli (1876), 336; Achille Cazin, "Effets mécaniques de la chaleur", *Revue Scientifique*, ii (1865), 431; Amédée Guillemin, *Le Monde Physique* (Paris, 1884), iv, 6; Albert R. V. Miller-Hauenfels, *Richtigstellung der in bisheriger Fassung unrichtigen mechanischen Wärmetheorie und Grundzüge einer allgemeinen Theorie der Aetherbewegungen* (Wien, 1889); Joannis, "Chaleur. I. Généralités", *La Grande Encyclopédie* (Paris, 1886-1902), x, 239-243; Rudolf Mewes, "Zusammenhang zwischen der kinetischen und der Vibrations Theorie der Gase", *Dinglers Polytechnisches Journal* (Stuttgart), cccxvii (1902), 758-760, 800-804, cccxviii (1903), 42-45, 75-78.

The above is not to be regarded as merely a list of cranks or third-rate scientists; many of these men may have had considerable influence through their teaching positions and the use of their textbooks.

⁷⁵ Whewell, *op. cit.* (36); Rosenberger, *op. cit.* (36).

⁷⁶ To my knowledge the only modern writer who gives a reasonably accurate (though greatly abbreviated) statement on this subject is T. W. Chalmers, in his book *Historic Researches* (New York, 1952), pp. 28-29.

“latent heat”.⁷⁷ Thomson claims that the principle of conservation of *heat* has been accepted “by almost everyone who has been engaged on the subject” except Joule; and so generally is this principle admitted “that its application in this case has never, so far as I am aware, been questioned by practical engineers”. The paper is of course remarkable mainly because it reveals Thomson on the brink of abandoning the caloric theory himself, but I think it also displays an amazing ignorance of the current state of opinion among physicists on the nature of heat.

How could William Thomson have been unaware of the fact that most physicists had accepted the wave theory of heat by 1849, if they had not already adopted the mechanical theory? We know, thanks to the work of Elinor Barber and Robert Merton, that Thomson made at least 32 discoveries “which he eventually found . . . had also been made by others”⁷⁸—a record that could hardly be compiled by a scientist who bothered to read the literature before plunging ahead with his own research. If we assume that Thomson was not familiar with any works other than those he explicitly mentions, then we would conclude that his knowledge of theories of heat was gained primarily from Fourier, Philip Kelland (whose ambiguous views have been mentioned above), Carnot, Clapeyron, and some anonymous engineers. This is probably overstating the case, but I do think it is quite fair to say that Thomson’s statement about the status of caloric theory in physics in 1849 was simply wrong. (It may have been accurate for engineering.)

Having published a paper that later convinced one group of readers that the caloric theory was generally accepted up to 1849, Thomson wrote two years later a paper which seems to have convinced another group of readers that it had been demolished 40 years earlier. During this interval someone has told him about the wave theory of heat, and of course the famous incident with Joule at the British Association has finally had its effect. After quoting Davy, Thomson says:

“The Dynamical Theory of Heat, thus established by Sir Humphry Davy, is extended to radiant heat by the discovery of phenomena, especially those of the polarization of radiant heat, which render it excessively probable that heat propagated through vacant space, or through diathermane substances, consists of waves of transverse vibrations in an all-pervading medium.”⁷⁹

He then refers to Mayer’s and Joule’s discoveries which “would so afford,

⁷⁷ William Thomson, “An account of Carnot’s theory of the motive power of heat; with numerical results deduced from Regnault’s experiments on steam”, *Transactions of the Royal Society of Edinburgh*, xvi (1849), 541-574. J. P. Joule, in a paper read to the Royal Society of London on 21 June 1849, stated that “the scientific world [was] preoccupied with the hypothesis that heat is a substance” but it was not clear that he thought this was still true in 1849; see Joule’s *Scientific Papers* (London, 1884), i, 302.

⁷⁸ Robert K. Merton, “Singletons and multiples in scientific discovery: A chapter in the sociology of science”, *Proceedings of the American Philosophical Society*, cv (1961), 470-486.

⁷⁹ William Thomson, “On the dynamical theory of heat, with numerical results deduced from Mr. Joule’s equivalent of a thermal unit, and M. Regnault’s observations on steam”, *Transactions of the Royal Society of Edinburgh*, xx (1851), 261-288.

if required [!], a perfect confirmation of Sir Humphry Davy's views".⁸⁰ Presumably contemporary physicists only bothered to read the later "correct" paper, and thus learned that the caloric theory had been demolished in 1800; whereas historians went back to the earlier paper for evidence as to views about the nature of heat just before the adoption of thermodynamics. Both were misled.

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After this article was accepted for publication, I learned that Dr. Charles Weiner had reached some of the same conclusions in his unpublished Ph.D. Dissertation at Case Institute of Technology, *Joseph Henry's Lectures on Natural Philosophy; Teaching and Research in Physics, 1832-1847* (Cleveland, 1965). In particular, he shows that Henry used the wave theory of heat to correlate a number of phenomena in his lectures, and that the theory was widely discussed in the physics literature of the 1830's and 1840's. Some of this material will be presented in Weiner's introduction to his new edition of Henry's works, to be published by the Smithsonian Institution Press.

⁸⁰ Kuhn, *op. cit.* (61), note 98, has called attention to this curious statement, and asks: "But if Davy established the dynamical theory in 1799 and if the rest of conservation follows from it, as Kelvin implies, what had Kelvin himself been doing before 1852?" In Thomson's article on "Heat" for the 9th edition of the *Encyclopaedia Britannica* (Edinburgh and New York, 1880), xi, 495-526 (replacing Traill's article quoted at the beginning of this paper), he gave a classic statement of the "combined myth" mentioned in the text above: "It is remarkable that, while Davy's experiment alone sufficed to overthrow the hypothesis that heat is matter, and Rumford's, with the addition of just a little consideration of its relations to possibilities or probabilities of inevitable alternatives, did the same, fifty years passed before the scientific world became converted to their conclusion—a remarkable instance of the tremendous efficiency of bad logic in confounding public opinion and obstructing true philosophic thought." The article does not mention the wave theory of heat.