William Thomson and the Creation of Thermodynamics: 1840–1855

CROSBIE W. SMITH

Communicated by M. J. KLEIN

Contents

1. Introduction

As the central issue in this paper, I am concerned with the emergence of the theoretical structure and basic concepts of classical thermodynamics in the period 1840–1855. An analysis of the work of WILLIAM THOMSON and his brother JAMES, RUDOLF CLAUSIUS, W. J. M. RANKINE and J. P. JOULE is clearly required for an understanding of this period in the development of thermodynamics, and my aim is to go behind accounts in the secondary literature which rely largely on the published papers of these personalities [1]. By focussing on both the published and unpublished writings of the THOMSON brothers, I hope to present an enrichment of our historical understanding of the period, and to show that mutual dependence among all the above-named thinkers is crucial to the formulation of the new scientific ideas. I shall attempt to analyse and correlate their thought through the examination of the THOMSONS' notebooks and correspondence with a view to uncovering both this interaction among the thinkers, and their debt to other, often earlier, scientists who have, through text-books, papers or treatises, discussed related topics [2].

---

[1] For example, S. P. THOMPSON, *The Life of William Thomson, Baron Kelvin of Largs* (London: Macmillan, 1910), **1**, 252–295; D. S. L. CARDWELL, *From Watt to Clausius. The Rise of Thermodynamics in the early Industrial Age* (London: Heinemann, 1971).

[2] The manuscripts, letters and notebooks of the THOMSONS are available in Cambridge University Library (KELVIN collection), Queen's University Library, Belfast (THOMSON papers) and Glasgow University Library.

From the published record we know that in the late 1840's WILLIAM and JAMES THOMSON discussed the implications of CARNOT's principle, that WILLIAM THOMSON and JOULE exchanged ideas from 1847 and that CLAUSIUS read THOMSON's paper of 1849 on CARNOT's theory and responded to it in 1850. We also know that THOMSON was aware of CLAUSIUS' paper of 1850 when he published his "Dynamical Theory of Heat" in 1851, and that RANKINE was in contact with THOMSON around 1850 [3]. What I am seeking to achieve in this paper, however, is an analysis of the underlying patterns of these interactions rather than a challenge to the basic picture provided by studies of the published record. Nevertheless, the parts played by the various characters in the emergence of thermodynamics demand careful reassessment after an examination of the details of their inter-relationships.

In particular, we have remarkably little understanding of WILLIAM THOMSON's older brother JAMES. Sir JOSEPH LARMOR referred to this brother of the revered Lord KELVIN as "the philosopher who plagued his pragmatical brother" [4], and to his distinctive style – his original way of looking at problems, his combination of the abstract physical with the practical engineering aspects, his great clarity in scientific writing, and his introduction of new and unambiguous terms into the scientific language. Yet LARMOR's remarks are some of the very few made about JAMES THOMSON, apart from the innumerable references to his paper on the depression of the freezing point of ice under pressure as a notable contribution to the science of thermodynamics.[5] In what follows, I shall therefore endeavour to show the extent to which JAMES THOMSON has been underestimated by most historians. Similarly, the work of W. J. M. RANKINE has not previously been seen in close relation to that of the THOMSONS.[6] From the available RANKINE correspondence with WILLIAM THOMSON, therefore, it becomes possible to assess the importance of this relation in the debates concerning the first and second laws of thermodynamics. At the same time, the interaction between these British thinkers and CLAUSIUS is one worthy of closer investigation. Overall, then, I shall endeavour to show that the formulation of classical thermodynamics around 1850 was a complex, and *ipso facto* fascinating, episode in the history of science which cannot be fully appreciated without a careful study of manuscript sources.

2. Prelude to "the Dynamical Theory of Heat": Joule and the Thomson brothers

Having attended many of the same courses (including natural philosophy), as WILLIAM at Glasgow College, JAMES THOMSON sought training in the engineering world of heavy industry,[7] instead of taking a mathematical degree at Cambridge as his brother did.[8] A long series of letters exchanged between the brothers

---

[3] See THOMPSON and CARDWELL, *op. cit.* (note 1) for standard histories of the period during which classical thermodynamics emerged.

[4] D'ARCY W. THOMPSON, *Year Book of the Royal Society of Edinburgh 1941-2*, 12.

[5] JAMES THOMSON, *Collected Papers in Physics and Engineering*, ed. Sir J. LARMOR & J. THOMSON (Cambridge: Cambridge University Press, 1912), pp. v–vii, pp. xiii–xci and pp. 196–203.

[6] See CARDWELL, *op. cit.* (note 1), p. 254.

[7] JAMES THOMSON, *op. cit.* (note 5), pp. xiii–xci.

[8] S. P. THOMPSON, *op. cit.* (note 1), **1**, 8–9; 23–112.

from 1841 shows on the part of JAMES a deep interest in engineering problems of all kinds – steam-engines, steam navigation, ship-building, water power and civil engineering – but often more from the point of view of interest in theoretical principles than in practical details, and thereby reflecting perhaps his early instruction in natural philosophy and his awareness of WILLIAM's interests, which were in that sphere.[9]

Looking for anticipations of later views is always a dangerous activity for the historian, but we shall have to attempt an understanding of the THOMSONS' specific interests during the 1840's in order to see why they were so fascinated, yet puzzled, by JOULE's papers on the mutual convertibility of heat and work.[10] Let us now turn, therefore, to a letter from JAMES to his brother dated 13th August 1863. The subject is mainly the TAIT-TYNDALL or JOULE-MAYER controversy, but, leaving the details of that debate aside,[11] the remarks made on the issues which concerned the THOMSONS in the 1840's are illuminating. JAMES THOMSON writes:

> Even you and I at Walsall (in 1842 I think) when watching the consumption of power in the flow of water into a canal lock were speculating on what became of the power as we could not suppose the water to be *worn* and therefore altered like as solids might be supposed to be when power is consumed in their friction.[12]

There is naturally some doubt as to the validity of reading importance into such remarks, made as they were twenty years in retrospect. Nonetheless, given that JAMES THOMSON was invariably honest and always sought the utmost accuracy, any historical fallacy here would seem not to be intentional. We have perhaps a tacit agreement on the accuracy of the above remarks by WILLIAM, who wrote a comment beside the next paragraph in the text which runs with rather less certainty:

> In those days you were (I think) taking into consideration Davy's melting of ice by the mere expenditure of power or work in it; and were I think cogitating between that and another idea to the effect that if heat were developed and thrown away in filing a metal (such as easily fusible metal) to powder we might perhaps find that a pound of the powder would have less latent heat in it than a pound of the unpowdered metal. My recollection or impression is to the effect that you gave pre-dominance to the conclusions from Sir HUMPHRY DAVY's experiment, and that you were at that time inclining to suppose that heat would be the true equivalent or substitute for the power.[13]

---

[9] Letters from JAMES to his brother held in the KELVIN Collection, University Library, Cambridge. My thanks are due to Dr. DAVID WILSON and the Librarian for providing access to the Collection.

[10] See OSBORNE REYNOLDS, "Memoir of James Prescott Joule", *Proceedings of the Manchester Literary and Philosophical Society* **6**, 1892 for an account of JOULE's life.

[11] See J.T. LLOYD, *Notes and Records of the Royal Society of London*, **25**, 211–225 (1970) and CARDWELL, *op. cit.* (note 1), pp. 282–286 for a discussion of the controversy.

[12] Letter from JAMES to his brother WILLIAM, 13th August 1863 held in the KELVIN Collection, University Library, Glasgow. My thanks are to the librarian for providing access to the Collection.

[13] *Ibid.* DAVY's experiment concerned the melting of ice by friction.

At the beginning of this paragraph, WILLIAM THOMSON has commented "No, not then" in refutation, it would seem, of the remarks on DAVY. This part of the letter, then, is a useful negation. In the early 1840's WILLIAM THOMSON was not concerned with the later, well-publicized DAVY experiment as evidence for a dynamical theory of heat. This conclusion would of course be compatible with the undisputed view that THOMSON had been more interested in CARNOT and FOURIER during the 1840's.[14] However, to the first paragraph quoted above, the "No, not then" comment does not necessarily apply. The concern there expressed about "loss of power" does not imply any views about the nature of heat. It was merely a query to which the answer was not known to the THOMSONS' satisfaction.

The question may indeed have been prompted by a reading of WILLIAM WHEWELL's new *Mechanics of Engineering* (1841).[15] Although there is no direct evidence to support this possibility, yet it would seem most unlikely if the THOMSONS, with JAMES' developing interest in the principles of engineering on the one hand, and WILLIAM's education at Cambridge from 1841 on the other, had not had access to the work. Thus they may have responded to WHEWELL's discussions of "Labouring Force" or "Work." Part of WHEWELL's book is aimed at an analysis of the laws of this concept:

> When by means of any machine work is done, labouring force is *expended* or *consumed*; and the measure of this force, and the laws which regulate its *expenditure* and *consumption*, belong also to the subject of the present volume.[16]

In general, labouring force is measured "by the product of the resistance overcome, and the space through which it is overcome,"[17] though more specifically it may be measured "by a given weight raised through a given vertical space," for any resistance may be expressed by weight. WHEWELL also gives a clear statement that the labouring force is proportional to the *vis viva* which the acting forces on the machine would have generated, acting through the same spaces, and he finds that "if we take the same measures on the two sides of the equation, *the labouring force is half the vis viva*."[18] He then proceeds to consider how the labouring force is consumed in doing work. Useful resistances are those exerted by the work, and must be overcome by the machine. Other resistances called impeding resistances, hinder the work, and they include air, friction, and "the forces producing waste and change of form in parts which we wish to have durable and invariable," and which exist continually in machines. Thus, he states, "the total labouring force is consumed by the useful and the impeding resistances taken together," since the total labouring force is measured by the resistances, multiplied into the spaces through which they are overcome, and including *all* the resistances which act upon the system.[19]

[14] S.P. THOMPSON, *op. cit.* (note 1), **1**, 41–42; 252–295; S.G. BRUSH, *Arch. Hist. Exact. Sci.*, **12**, 1–88 (1974).

[15] WILLIAM WHEWELL, *The Mechanics of Engineering* (Cambridge: J. & J.J. Deighton, 1841).

[16] *Ibid*, p. 1.

[17] *Ibid.*, p. 146.

[18] *Ibid.*, pp. 153–155.

[19] *Ibid.*, p. 156.

This labouring force, WHEWELL explains, may be considered as *consumed*, "for it is lost and cannot be recovered after being used." The force thus lost in all manner of processes cannot again be brought into operation, and disappears. Only in cases "where the force is employed in raising a weight, moving a mass, or bending a spring," can the force be stored up, and be again brought into play and used. "But if this future use of the force stored up is not contemplated, the force thus employed may also be considered as consumed," and so, not merely is force employed in overcoming friction, resistance of fluids, and the like, lost, but "the whole of the labouring force is consumed by the useful and the impeding resistances taken together."[20] Thus for WHEWELL, although there is a clear statement of a mathematical principle of *vis viva* and its proportionality to labouring force, there is no suggestion of the convertibility of work into heat; only the emphatic claim that work is consumed, that it is lost and disappears, when overcoming useful and impeding resistances in a machine.

The THOMSONS' query of 1842 had expressed their puzzle about the consumption of power in a particular case – the flow of water into a canal lock, which involved the friction of a fluid in motion. Solids, WHEWELL had stated, were supposed worn during friction, and so were altered by work consisting in "shaping or moving certain portions of matter. Thus we have to grind bodies, to polish them, to divide them into parts ...."[21] But the THOMSONS could not see how such an alteration could be involved in the case of the friction of fluids. We do not know the nature of their conclusions, if any, in 1842. Nevertheless after 1847 the thought of JOULE on the friction of fluids provided a key to the solution of this problem, and so we may begin to understand the background to WILLIAM THOMSON's special enthusiasm for this aspect of JOULE's researches.

The other strand which awakened WILLIAM THOMSON's interest in JOULE's papers also arose from JAMES' reflections in this period before 1847. WILLIAM THOMSON, according to S. P. THOMPSON, was inspired, during his stay in Paris through the early part of the year 1845, by the work proceeding in REGNAULT's laboratory to read CLAPEYRON's "Memoir on the Motive Power of Heat" (1834), and to seek unsuccessfully for the original CARNOT pamphlet.[22] However, in a letter to WILLIAM dated August 1844, that is, before the Paris visit, JAMES inquired of his brother who it was that proved that there is a definite quantity of mechanical effect given out during the passage of heat from one body to another.[23] He states his intention of writing an article for the *Artisan* about a proposal as to the theoretical possibility of working steam engines without fuel by using over again the heat which is thrown out in the hot water, from the condenser, and he indicates that "I shall have to enter on the subject of the paper you mentioned

[20] *Ibid.*, pp. 156–157.

[21] *Ibid.*, p. 145.

[22] S. P. THOMPSON, *op. cit.* (note 1), **1**, 132–133. For reprints of CARNOT's 1824 "Reflections" and CLAPEYRON's memoir of 1834, see *Reflections on the Motive Power of Fire by* SADI CARNOT *and other Papers on the Second Law of Thermodynamics by* E. CLAPEYRON *and* R. CLAUSIUS, ed. E. MENDOZA (New York: Dover Publications, 1960).

[23] Letter JAMES to WILLIAM THOMSON, 4th August, 1844 held in the KELVIN Collection, University Library, Cambridge. See S. P. THOMPSON, *op. cit.* (note 1), **1**, 275n for a brief reference to this letter as "containing a curious piece of primitive thermodynamics".

to me." Clearly, therefore, WILLIAM has already heard of a paper about the motive power of heat, which is undoubtedly CLAPEYRON's, and almost certainly the version translated in 1837 for TAYLOR' *Scientific Memoirs.*[24]

The discussions in JAMES' letter of 1844 are probably the earliest written record of references by the THOMSONS to CLAPEYRON and CARNOT's ideas, and show a considerable understanding of the basic principles involved in CARNOT's theory. JAMES continues by saying that, as he interprets the problem:

> ... *during the passage of heat from a given state of intensity to a given state of diffusion a certain quantity of mec[hanical] eff[ect] is given out whatever gaseous substances are acted on, and that no more can be given out when it acts on solids or liquids.*[25]

This, he claims, is all he can prove, because he does not know whether in solids or liquids a certain quantity of heat will produce a certain quantity of mechanical effect, and that the same mechanical effect "*will give back as much heat,*" this last point being stressed by THOMSON's emphasis. "That is, I don't know that the heat and mec[hanical] eff[ect] are interchangeable in solids and liquids, though we know they are so in gases." Thus, at least as far as gases are concerned, a certain quantity of heat produces a certain quantity of work and *vice versa*, but the two entities, heat and work are, by the use of the term produce, understood to be only effectively proportional to one another, but not interconvertible or transformable.

JAMES THOMSON is quite familiar with the CARNOT waterfall analogy, in which the quantity of water is conserved, as the quantity of heat remains constant, and a definite quantity of mechanical effect is obtained, in the one case by the substance falling between two heights, and in the other between two temperatures:

> The whole subject you will see, bears a remarkable resemblance to the action of a fall of water. Thus we get mec[hanical] eff[ect] when we can let water fall from one level to another or when we can let heat fall from one degree of intensity to another. In each case a definite quantity is given out but we may get more or less according to the nature of the machines we use to receive it. Thus a water mill wastes part by letting the water spill from the buckets before it has arrived at the lowest level and a steam engine wastes part by throwing out the water before it has come to be of the same temperature as the sea. Then again, in a water wheel, much depends on our not allowing the water to fall through the air before it commences acting on the wheel and in a steam engine the greatest loss of all is, that we do allow the heat to fall perhaps from 1000° to 220°, or so, before it commences doing any work. We have not materials by means of which we are able to catch the heat at a high level. At the same time, if we did generate the steam at 1000° a great part of the heat would pass unused up the chimney: and with the products of combustion, if we had a water wheel sufficiently high to receive the water of a stream almost at its source we would waste all the tributary streams which run in at a lower level.[26]

---

[24] E. CLAPEYRON, "Memoir on the Motive Power of Heat", in *Scientific Memoirs*, ed. RICHARD TAYLOR, **1**, 122–138 (1837).

[25] JAMES THOMSON, *op. cit.* (note 23). THOMSON's emphasis.

[26] *Ibid.*

In these reflections there is contained profound insight into problems of less-than-ideal engines and waste generally. Not only, therefore, does JAMES THOMSON grasp the fundamental principles of CARNOT and CLAPEYRON, but he recognises that in reality, by analogy with water wheels, heat is wasted – that is, produces no useful work – on passage from a state of intensity to one of diffusion, and that as a result real engines fall short of the ideal.[27] Out of the analogy too, we may suppose, comes the notion that the sea as a base level is important both for water moving from high levels to the lowest possible, and for heat diffusing itself to the temperature of the sea. The main issue, however, is one of harnessing heat in the course of its passage, and of thereby producing useful work.

In early 1845, WILLIAM THOMSON read CLAPEYRON in the French version,[28] and discussed its preliminary part verbally with his brother on his return from Paris in the summer of that year. In February 1846, JAMES acknowledged receipt of the full CLAPEYRON paper sent to him by WILLIAM, and remarked that the first sections were "a very beautiful piece of reasoning, and of course perfectly satisfactory."[29] Thus, much of the early interest in CARNOT and CLAPEYRON lay with JAMES, and it appears to have been his enthusiasm which inspired WILLIAM to go beyond a mere passive awareness of CLAPEYRON's memoir.

In April 1847 WILLIAM THOMSON read to the Glasgow Philosophical Society an account of STIRLING's hot air engine, with the theory of it deduced on CARNOT's principles.[30] The notice began by stating that at a previous meeting of the Society, Professor GORDON had given an explanation of CARNOT's theory, and that, in accordance with this theory, the mechanical effect to be obtained from an air engine from the transmission of a given quantity of heat depends "on the difference between the temperature of the air in the cold space above and the heated space below the plunger ...." Since this temperature difference is considerably greater than that in the best condensing steam engines, it is argued that, given the removal of the practical difficulties of constructing an efficient air engine, ... "a much greater amount of mechanical effect would be obtained by the consumption of a given quantity of fuel" in the case of the air engine.[31]

---

[27] See BRUSH, *op. cit.* (note 14), pp. 19–21.

[28] In early 1845, WILLIAM THOMSON read CLAPEYRON in the French version in *J. École Polyt.* **14**, 153 (1834). See S. P. THOMPSON, *op. cit.* (note 1), **1**, 132–133.

[29] Letter from JAMES to WILLIAM THOMSON, 22nd February, 1846 held in the KELVIN Collection, University Library, Cambridge.

[30] W. THOMSON, *Proc. Roy. Phil. Soc. Glasgow* **2**, 169 (1847). For a full account of the STIRLING hot air engine see E. E. DAUB, "The Regenerator Principle in the STIRLING and ERICSSON Hot Air Engines", *The British Journal for the History of Science* **7**, 259–277 (1974). The original hot air engine by ROBERT STIRLING dated from 1816, and was improved in 1827 by the development of the so-called regenerator or economizer principle. The regenerator was essentially a heat exchanger employed within the air engine cycle in the belief that the large loss of heat incurred in the condenser of the steam engine could be eliminated in the air engine. Suggested by JOHN ERICSSON in the 1830's, the term "regenerator" implied that the lost heat could be re-used to produce mechanical work. The real value of the regenerator was appreciated by W. J. M. RANKINE. See also JAMES STIRLING, "STIRLING's Air Engine", *Mechanics Magazine* **45**, 559–566 (1846).

[31] *Ibid.*, 169. LEWIS GORDON was the first professor of engineering (1840–1855) at Glasgow College. The THOMSONS possibly heard of CLAPEYRON through GORDON in the early 1840's. THOMSON's account of the engine is rather confused, until one realises that the engine is driving rather than being driven.

The notice continues with "some illustrations, afforded by the Air Engine, of general physical principles ...." Thus, if the engine is turned "forwards" by the application of power, and if no heat is applied, the space below the plunger will become colder than the surrounding atmosphere, and the space above hotter. That is to say, given work done by the engine, heat will be transferred from the region of higher temperature to that of lower temperature. Once this new relation of temperatures is established, "contrary to that which is necessary to *cause* the engine to turn forwards," expenditure of work will be necessary to turn the engine, THOMSON argues. If, however, the temperature in one part is prevented from rising, and in the other part from falling, "the engine may be turned without the expenditure of any work, (except what is necessary in an actual machine for overcoming friction, & c.)" Apart from the obvious way of achieving this by immersing the machine in a stream of water, THOMSON advocates finding a solid body which melts at the temperature at which it is required to retain the engine. Thus, he suggests, let a stream of water at 32° be made to run across the upper part of the engine, while the lower part is held in a basin of water at the same temperature. When the engine is turned forwards, heat will be taken from the space below the plunger and deposited in the space above, this heat being supplied by water in the basin, all of which will gradually be converted into ice, at 32°, without the expenditure of work.[32] It is this argument to which JAMES THOMSON claims to have responded in his famous paper on the lowering of the freezing point of ice under pressure. However, as we shall now see, this response did not come directly.

A notebook of JAMES THOMSON, dated April 1848, on the "Motive Power of Heat", reveals a strong and continuing interest in the CARNOT principle, especially as applied to these air or gas engines. After some preliminary theoretical and practical investigations, he remarks that he had just conversed with the Reverend Dr. STIRLING of Galston about the air engine and had told him at the beginning "particularly not to tell me anything that he did not regard as entirely public, because I had some ideas on the subject myself."[33] STIRLING told THOMSON that he was presently making some improvements on his air engines, and THOMSON comments: "I found that, as I had previously thought, he does not understand his own engine; not knowing at all the way in which the heat is expended in generating work." THOMSON then records the course of their discussion, which is of much historical and conceptual interest.

STIRLING had said that the changes of temperature produced by changes of pressure of the air had long perplexed him, and even alarmed him in regard to the perfection of the engine, "... as it had appeared that the respirator would not even *theoretically* give back all the heat to the air; but that now he is inclined to think that 'a sort of average is struck' or a compensation is made by which all the heat is really given back if the air passages be small enough, the metal perfectly absorbent and non-conducting & c."[34]

---

[32] *Ibid.*, p. 170.

[33] JAMES THOMSON, notebook A 14(A) entitled "Motive Power of Heat: Air Engine" among the THOMSON papers, Queen's University Library, Belfast. The Rev. Dr. ROBERT STIRLING was minister of the Parish Church of Galston, a town in Ayrshire and around twenty miles south of Glasgow. His brother JAMES was a civil engineer.

[34] *Ibid.* The word "respirator" is used instead of the more usual term "regenerator". For the history of the term "respirator" see DAUB, *op. cit.* (note 30), p. 260.

JAMES THOMSON replies to these confusing and *ad hoc* remarks of STIRLING's by pointing out the essence of the CARNOT principle of which STIRLING seems ignorant. Thus THOMSON told him that ... "some transference of heat from the furnace to the water by means of the changes of temperature of the air is essential to the action of the engine; that otherwise it would be theoretically a perpetual motion," which is effectively the founding of the CARNOT principle on the same grounds as CARNOT himself and CLAPEYRON. However, the STIRLING-THOMSON discussion focusses attention on a dual meaning in the phrase "perpetual motion machine." STIRLING responds to THOMSON's claims for the CARNOT principle by saying ... "that there are plenty of theoretical perpetual motions if we have friction resistances &c out of consideration," to which THOMSON answered ... "there are these, but not perpetual sources of power." STIRLING, according to THOMSON, reflected awhile, and then replied ... "that perhaps what I [THOMSON] said was correct and that he had never thought particularly on the difference between a perpetual motion and a perpetual source of power."[35]

JAMES THOMSON, in 1848, founded CARNOT's principle on the impossibility of creating power, as did CARNOT and CLAPEYRON, and implicitly accepted, not the conversion of heat into mechanical effect, but only the production of the one from the other. Thus, for JAMES THOMSON in 1848, CARNOT's *principle* was taken to be the statement that transfer of heat from a high temperature to a low temperature produced motive power and *vice versa*, but we might say that his *theory* included certain assumptions necessary to supplement the principle itself, namely the assumption of the impossibility of creating power, and of the conservation of heat. It was for THOMSON a case of one law, the CARNOT principle, supplemented by the two above-mentioned assumptions, constituting the CARNOT theory of the motive power of heat.[36] I shall now explore these aspects for JAMES THOMSON's notes of 1848, where he concludes by summing up his disagreement with and criticism of STIRLING:

> In pointing out to me what he supposes to be the action of the air in the respirator and so endeavouring to prove that the respirator does really return all the heat to the air, and so that the machine is theoretically a perpetual source of power, he had no idea that the air ought to tend to be cooled by expansion at one part of the stroke so as to take in heat from the fire, and that at another it ought to tend to become heated by compression, so as to make it give out heat at the lower temperature; but he was strongly

---

[35] *Ibid.*

[36] Compare, for example, the useful analysis by PHILIP LERVIG, *Arch. Hist. Exact Sci.*, **9**, 222–239 (1972). LERVIG states CARNOT's laws as

I Perpetuum mobile impossible

II Heat is conserved

and the thermodynamic laws as

I Energy principle

II Perpetuum mobile of second kind impossible.

Hence LERVIG argues for the similarity in logical structure between the two theories, and their equivalence for reversible processes. However, for CARNOT's successors such as JAMES THOMSON, the impossibility of perpetual motion and the conservation of heat are seen as necessary supplements to the CARNOT principle itself, *viz.* the transfer of heat from a high temperature to a low temperature produces heat, and *vice versa.* For a criticism of part of LERVIG's thesis, see U. HOYER, *Arch. Hist. Exact Sci.*, **13**, 359–375 (1974).

> impressed with the supposition that the fire is merely useful to give a small supplement to the heat returned by the respirator so as to make up for incidental losses due to practical imperfections of the apparatus, such as conduction of heat, incomplete absorption &c. not that the removal of some heat from the fire is essentially connected with the development of work.[37]

THOMSON's view of the theory of the motive power of heat in the above passage is explicit. If a heat engine were perfect, that is, frictionless, &c., STIRLING's understanding of it would imply that heat, conserved in quantity, could be continually cycled and at the same time work produced, thereby obtaining a perpetual source of power, and not merely a perpetual motion. CARNOT's principle, however, as understood by THOMSON, entails the passage of heat in an engine from a *state of intensity*, that is, high temperature, to a *state of diffusion*, that is, low temperature, and it is this occurrence which *produces* the work or mechanical effect, even if *heat quantity* be conserved, and thus no heat actually converted into work. By reversing such an engine, that is, by putting work into it, the same quantity of heat would be raised back to the original high temperature. Overall, of course, no net work has been gained, with the result that the assumption of perpetual motion as impossible has not been violated. Here therefore we see the structure of CARNOT's theory as seen by JAMES THOMSON in 1848, and it is this structure which is of crucial importance for analysing the historical complexities of the period.

In May, 1848, JAMES THOMSON carried out his later-famous calculation of the lowering of the freezing point of water by the effect of pressure. These considerations appear to have been laid aside for some months, for in October of that year he notes: "WILLIAM and I have examined the investigation on the last page. The principles and the numerical result are extremely nearly true ...."[38] Publication of the well-known paper came in January, 1849, and in it he claims to have been influenced by his brother's suggestions at the end of the notice of 1847 on STIRLING's hot air engine.[39] In addition, we may reasonably now suppose that he was stimulated by the above discussions with STIRLING himself, and by his subsequent discussions with his brother. The contents of the paper of 1849 are familiar to all historians of thermodynamics. As S. P. THOMPSON explains, JAMES THOMSON reasoned through CARNOT's principle that, unless the absurdity of a perpetual motion, really a perpetual source of power, be admitted, it is necessary to conclude that the freezing point becomes lower as the pressure to which the water is subjected is increased. This claim was verified experimentally by WILLIAM a few months later.[40]

In October 1848, WILLIAM THOMSON published his paper "On an Absolute Thermometric Scale, founded on CARNOT's Theory of the Motive Power of Heat ..."[41] and, having at last received a copy in late 1848 of CARNOT's original

---

[37] JAMES THOMSON, *op. cit.* (note 33).

[38] *Ibid.*

[39] JAMES THOMSON, *op. cit.* (note 5), pp. 196–203.

[40] S. P. THOMPSON, *op. cit.* (note 1), **1**, 275–276. See also M. J. KLEIN *Physics Today* **27**, 23–28 (1974).

[41] WILLIAM THOMSON, *Mathematical and Physical Papers* (Cambridge: Cambridge University Press, 1882–1911), **1**, 100–106.

memoir from Professor LEWIS GORDON,[42] – who, it should be noted, appeared himself to be familiar with the theory –, published his lengthy "Account of CARNOT's Theory" in 1849.[43] Both these THOMSON papers are, generally speaking, concerned with expounding and developing the CARNOT-CLAPEYRON view, and as they have received repeated emphasis in the secondary historical literature,[44] and I shall have occasion to refer to them elsewhere, I need not deal further with them here.

Enough has been said to show that much of WILLIAM THOMSON's interest in CARNOT derived from discussions and exchanges of ideas with his brother, and I shall now examine the role of the same CARNOT theme in the famous JOULE-THOMSON debates from 1847. After WILLIAM THOMSON's encounter with JOULE at the Oxford meeting of the British Association in June 1847, the correspondence began between the two thinkers.[45] The first letter from JOULE to THOMSON referred to two of JOULE's papers, "On the Calorific Effects of Magneto-Electricity and on the Mechanical Value of Heat" (1843) and "On the Changes of Temperature produced by the Rarefaction and Condensation of Air" (1844) left by JOULE for THOMSON after the Oxford meeting.[46] On 12th July, 1847, WILLIAM sent the papers to his brother, remarking that they would astonish him and that "I think at present that some great flaws must be found. Look especially to the rarefaction and condensation of air, where something is decidedly neglected, in estimating the total change effected, in some of the cases ...."[47]

JAMES THOMSON replied to his brother on 24th July, 1847, commenting that:

> There is one blunder certainly. He [JOULE] encloses some compressed air in one vessel, connects that with another which is vacuous, and allows the air of the former to rush into the latter till the pressure is the same in both. Both vessels were immersed in water, and after the operation the temperature of the water remains the same as before. JOULE says that no mechanical effect has been developed outside of the vessels during the operation, and that therefore the heat remains unchanged. But in reality mechanical effect *was* developed outside, as the two vessels became of different temperature.[48]

The apparent confusion here is easily enough resolved when we bear in mind that at this stage JOULE and JAMES THOMSON were thinking within quite different frameworks regarding the motive power of heat – JOULE in terms of the mutual convertibility, and not merely the production, of heat and work, and THOMSON in terms of CLAPEYRON's theory as he saw it, of the production of work from a difference of temperature. And so, for JOULE, no work or mechanical effect meant no change of temperature outside and *vice versa*, while for THOMSON, the resulting

---

[42] S. P. THOMPSON, *op. cit.* (note 1), **1**, 132–133.
[43] W. THOMSON, *op. cit.* (note 41), 1, 113–164.
[44] CARDWELL, *op. cit.* (note 1), pp. 239–243; S. P. THOMPSON, *op. cit.* (note 1), **1**, 269–274.
[45] For an account of the famous Oxford meeting see S. P. THOMPSON, *op. cit.* (note 1), **1**, 263–265.
[46] Letter from J. P. JOULE to W. THOMSON 29th June, 1847, KELVIN papers, University Library, Cambridge. Reprints of JOULE's papers of 1843 and 1844 may be found in *The Scientific Papers of James Prescott Joule* (London: Dawsons, 1963), **1**, 123–159 and 172–189.
[47] S. P. THOMPSON, *op. cit.* (note 1), **1**, 266; JAMES THOMSON, *op. cit.* (note 5), p. xxviii.
[48] JAMES THOMSON, *op. cit.* (note 5), pp. xxx–xxxi.

difference of temperatures implied a development of mechanical effect outside the vessels.

THOMSON continues his appraisal of JOULE by admitting that, even given the above "blunder", "Some of his views have a slight tendency to unsettle one's mind as to the accuracy of CLAPEYRON's principles. If some of the heat can absolutely be turned into mechanical effect, CLAPEYRON may be wrong," but he sees the solution of the difficulty as requiring a more accurate definition of what is meant by a "*certain quantity of heat* as applied to two bodies at different temperatures." He remarks further that:

> Perhaps JOULE would say that if a hot pound of water lose a degree of heat to a cold one, the cold one may receive a greater absolute amount of heat than that lost by the hot one; the increase being due to the mechanical effect which might have been produced during the fall of heat from the high temperature to the low one.[49]

This passage is again in effect a recognition that JOULE holds to the conversion of work into heat, and is a bold attempt by JAMES THOMSON to reconcile the CLAPEYRON and JOULE positions. On the one hand he accepts the CLAPEYRON principle of production of mechanical effect from the fall of heat, its quantity being called here a "degree" (and not therefore to be confused with a degree of temperature), from a hot to a cold body in the ideal case. But in reality, on the other hand, mechanical effect is often not produced when heat passes from the one temperature to another. Thus THOMSON suggests on JOULE's view that in such cases this potential, but not realised, mechanical effect appears as heat, to *supplement* the constant quantity of heat passing from the hot body to the cold one. Hence the suggestion of the greater absolute amount of heat arriving at the latter body; a suggestion which, to anyone familiar with CLAUSIUS and classical thermodynamics, would seem somewhat absurd. Yet we must remember that it is here put forward only as a speculation in an attempt to resolve the complex conceptual difficulties, and had not so far been tested or debated.

The unsettlement which JAMES THOMSON felt was undoubtedly the result of his reading of JOULE's paper of 1844 which contained the well-known criticism of the CARNOT-CLAPEYRON view of the steam engine whereby its mechanical power arises simply from the passage of heat from a hot to a cold body, no heat being lost during the transfer. Analogous to the fall of water substance through a height, the caloric substances acquires *vis viva* through the temperature fall from boiler to condenser. JOULE, however, appears already committed to a principle of conservation of *vis viva* throughout nature stating in his paper of 1844:

> I conceive that this [CLAPEYRON's] theory, however ingenious, is opposed to the recognised principles of philosophy, because it leads to the conclusion that *vis viva* may be destroyed by an improper disposition of the apparatus.[50]

His own views, he claims avoid such a difficulty. The steam "expanding in the cylinder loses heat in quantity exactly proportional to the mechanical force

[49] *Ibid.*, p. xxxi.
[50] J. P. JOULE, *op. cit.* (note 46), **1**, 188.

which it communicates by means of the piston, and that on condensation of the steam the heat thus converted into power is *not* given back." He then supports *indestructibility* of *vis viva* by reference to FARADAY, ROGET and the Creator.[51]

WILLIAM THOMSON also wrote to his father immediately after the meeting of the British Association:

> I have just returned from Oxford .... The meeting of the Association was quite delightful, from the opportunity it afforded of seeing so many people engaged in various interesting researches. I need not give you any details, as you will of course see the Athenaeum containing the report (tell JAMES to look for the account of JOULE's paper on the dynamical equivalent of heat. I am going to write to JAMES about it and enclose him a set of papers I received from JOULE, whose acquaintance I made, as soon as I have time. JOULE is, I am sure, wrong in many of his ideas, but he seems to have discovered some facts of extreme importance, as for instance that heat is developed by the friction of fluids in motion.)[52]

From what I have already suggested regarding the period before 1847, the interest in fluid friction probably goes back to the discussion in 1842 between the brothers, and hence the emphasis here on that aspect. In addition the special interest of JAMES suggested by this letter undoubtedly reflects his fascination with the CARNOT-CLAPEYRON view of the motive power of heat, and we are forced to recognise the important part, to an extent greater than that hitherto claimed by historians, played by JAMES THOMSON. That is not to demote WILLIAM, but merely to stress that WILLIAM's interests were much wider in the field of natural philosophy and mathematics generally, as is also shown by the breadth of interest briefly expressed in the above letter. By contrast, JAMES as the "theoretical engineer" had rather more specialised and concentrated interests relating to the motive power of heat.[53]

We have already noted the debates on STIRLING's air engine up to April 1848. A further notebook of JAMES THOMSON in the same series, and dated May 1848, reveals that his attention was focussed on the accurate definitions of terms, especially with respect to heat theory. For his source he relied on LAMÉ, and he made quite extensive notes, largely, it would seem, to clarify his own thoughts.[54] The emphasis is on quantity of heat, capacities, and specific heats. Thus he writes: "I think we should define a *certain capacity* as being mass × spe-

---

[51] JOULE believed that only God could create or destroy *vis viva* or work. He believed that FARADAY and ROGET had made a similar claim about the indestructibility of these concepts. See REYNOLDS, *op. cit.* (note 10), p. 88 and pp. 187–189. REYNOLDS points out that strictly it was perpetual motion, and not annihilation of force or power to which FARADAY and ROGET objected. In addition, of course, FARADAY and ROGET did not emphasise that the concept in question was *vis viva* or work. FARADAY's fundamental concept was *force* in the Newtonian sense, and not an energy term. See L. P. WILLIAMS, *Michael Faraday* (London: Chapman and Hall, 1965), pp. 364–407.

[52] Letter from W. THOMSON to his father, 1st July 1847, KELVIN Papers, University Library, Cambridge.

[53] See my introductory remarks on JAMES THOMSON's outlook.

[54] GABRIEL LAMÉ, *Cours de physique de l'École Polytechnique* (Paris, 1836-7). See also ROBERT FOX, *The Caloric Theory of Gases from* LAVOISIER *to* REGNAULT (Oxford: Clarendon Press, 1971), pp. 263, 268–270, 316, for the position of LAMÉ in French physics.

cific heat or weight × specific heat," which provides him with a clearer conception of the distinction between capacity and specific heat.

On the next day, May 20, 1848, he notes in parenthesis[55] that "the following occurs to me at present," and he outlines the trend of his speculations on the JOULE-CLAPEYRON problems in a tabular and schematic form. He sets up one column for concepts in the theory of heat – heat or caloric, capacity, temperature, and motive power – and a second column for what he supposes might be corresponding terms in dynamics – momentum, mass, velocity and work or *vis viva.* Thus, he states, count the quantity of heat or caloric in a mass or set of masses as being equal to the heat or caloric given out in descending to 0° [Absolute], while the quantity of motion (momentum) in a mass or set of masses is the motion or momentum given out in stopping so as to be at rest with reference to a thing regarded as stationary, and then, he says, quantity of caloric is measured by capacity × temperature, quantity of motion by mass × velocity, and temperature corresponds to velocity. Motive power or work from heat is as capacity × $(\text{temperature})^2$, while motive power, work, or *vis viva* in dynamics is as mass × $(\text{velocity})^2$, he claims. Furthermore, "no quantity of Heat is lost during a fall of heat just as no momentum or quantity of motion is lost in impact. But Motive Power (or whatever it is to be called) is lost ... just as vis viva is lost."

THOMSON proposes calling the motive power of heat *vis calida* to correspond with *vis viva*, the two then being equated, and not merely correlated by analogy as it were. He refers to this point on an additional slip inserted on the same page, and dated also on the same day, justifying this equality by the common mechanical term of "work." Thus he argues that: "During the passage of a given quantity of motion from a velocity V to a velocity v, work is given out," while "during the passage of a given quantity of heat from a temperature T to a temperature t, work is given out." "Work, equivalent to force × space, equivalent to $\frac{1}{2}$ *vis viva*, equivalent to $\frac{1}{2}$ mass of body × $(\text{velocity})^2$" and also "Work, equivalent to force × space, equivalent to $\frac{1}{2}$ *vis calida*, equivalent to $\frac{1}{2}$ capacity of body × $(\text{temperature})^2$" implies that *vis viva* and *vis calida* are equivalent, and, indeed, mutually convertible, he claims here. He stresses that by impact or mutual frictions, *vis viva* is lost, but quantity of motion is not lost, and that "$\frac{1}{2}$ *vis viva* is work locked up in velocity" while "$\frac{1}{2}$ *vis calida* is work locked up in temperature."

The structure of this system is a clear attempt to rethink prevailing views, and to overcome JOULE's objections to CLAPEYRON. JAMES THOMSON here retains the CLAPEYRON principle of temperature difference, not this time explicated by means of the analogy to the waterfall of two heights but by the case of two velocities in a dynamical problem. Conservation of heat is made analogous to conservation of momentum, little or no reference being made to heat as a substance. Thus both for mechanics and for the theory of heat *vis viva* or *vis calida* need not be, and is not, conserved in all situations and under all circumstances. Consequently, the immediate objection of JOULE that by improper disposition of the apparatus *vis viva* would be lost on CLAPEYRON's interpretation, loses its force for THOMSON. JOULE asserts that *vis viva* cannot be lost, while

[55] See Appendix I.

THOMSON observes that *vis viva* can be lost, but that it is rather momentum which is conserved. The intention here on the part of THOMSON is reconciliation rather than debate, and with this aim in mind he tries to bring in an additional term, *vis calida*, which in retrospect seems superfluous, but appeared to him at the time of this speculation to be an aid to clarifying the conceptual problems, and possibly to providing an eventual reconciliation. A view of loss of *vis viva* would of course be quite within the traditions of mechanics, and it can be seen that JOULE's objections to CLAPEYRON result from his framework of mutual convertibility and his emphasis on *vis viva* as the new primary concept. JAMES THOMSON sees the conflict plainly enough, and he continues: "According to what is given on the last page the question at issue between JOULE and us may be stated thus. Is a certain *quantity of heat* (capacity × temperature) equivalent to a certain *vis viva* the two being mutually convertible?" For JOULE the answer is certainly and emphatically "yes," that heat is a form of *vis viva*. For THOMSON, "on the last page it is shown that *vis calida* and *vis viva* are mutually convertible," a position which saves CLAPEYRON, but which has not yet been shown to be compatible with JOULE. Therefore, THOMSON proceeds, "if JOULE is right we should have a certain *quantity of heat* equivalent to (and convertible) a certain *vis calida*," but quantity of heat is equal to $\frac{\textit{vis calida}}{\text{temp[erature]}}$. Therefore "we should have a certain $\frac{\textit{vis calida}}{\text{temp[erature]}}$ equivalent & convertible to *vis calida* wh[ich] appears to be nonsense (Is my reasoning good?)." Thus it seems that THOMSON's efforts at reconciliation regarding CARNOT-CLAPEYRON and JOULE are unsuccesful, foundering as they do on the question of whether or not heat is equivalent and convertible to *vis viva*. If JAMES THOMSON's speculations are correct, then JOULE is wrong, and *vice versa*. The removal of the *specific* objection to CLAPEYRON by THOMSON still leaves a fundamental conflict of frameworks, which is conceptually not at all a simple matter to resolve, however obvious it may appear in retrospect. The tortuous moves between 1847 and 1851 bear testimony to that complexity, as we shall see throughout this section. Nor is experiment to be considered the final arbiter here, for, in one of the first references to DAVY's experiment, JAMES THOMSON remarks that "DAVY's experiment of melting 2 pieces of ice by their mutual friction w[oul]d be incompatible with my view of the subject as it w[oul]d show that *vis viva* or work actually produced *quantity of heat*. (Is the experiment to be trusted?)" By produced, he means here converted into, for work produced heat in the CARNOT-CLAPEYRON view.

In October 1848, WILLIAM THOMSON published a brief footnote commenting on JOULE's

> ... very remarkable discoveries which he has made with reference to the *generation* of heat by the friction of fluids in motion, and some known experiments with magneto-electric machines, seeming to indicate an actual conversion of mechanical effect into caloric. No experiment however is adduced in which the converse operation is exhibited; but it must be confessed that as yet much is involved in mystery with reference to these fundamental questions of Natural Philosophy.[56]

[56] W. THOMSON, *op. cit.* (note 41), **1**, 102n. See also S. P. THOMPSON, *op. cit.* (note 1), **1**, 268–269.

JOULE responded to this paper in a letter to THOMSON dated 6th October 1848, taking "the present opportunity of communicating some of my notions on heat &c and asking your opinion thereon ...." He notes that THOMSON still adheres to CARNOT's theory, but, as we might suppose, THOMSON's refusal to admit the conversion of heat into mechanical effect concerns JOULE most. JOULE confesses that one or two points in the experiment of the electro-magnetic engine are not demonstrated with regard to the proof of the conversion of heat into mechanical effect and he remarks on the experiments concerning changes of temperature in which "I thought I had proved the convertibility of heat into power; for I found that on letting the compressed air escape into the atmosphere, a degree of cold was produced *equivalent* to the mechanical effect estimated by the column of atmosphere displaced." He continues:

> The cold could not be explained by an increase of the capacity of the rarefied air because no cold was produced on the *whole*, when air was let escape into a vacuum. Cold was observed in the vessel whence the air escaped and Heat in the vacuum vessel and adjoining stop-cocks, but the *whole* result was 0. The heat evolved at the stop-cocks evidently arose from friction, which friction would have been prevented had the motion of the air been retarded by making it pass through an air engine, in such a case there would have been cold observed in the vessels while an equivalent of power would have been produced. Being exceedingly anxious to see this subject altogether freed from difficulty and objections I am about to get either an air engine or a steam engine constructed which will I hope serve to clear up the subject.[57]

I have indicated that THOMSON in particular was unimpressed with this part of JOULE's work, and thus it is not surprising that WILLIAM THOMSON did not in his paper of 1848 admit of the conversion of heat into power. Nor, from the defensive tone of these letters, does JOULE himself appear entirely satisfied with his early interpretation of either the magneto-electric experiments (1843) or experiments on rarefaction (1844). The debate between the THOMSONS and JOULE is thus far from settled, with conceptual and empirical arguments constantly being exchanged.

Thirdly, in the same letter, JOULE reflects on the steam engine:

> It appears to me that a theory of the steam engine which does not admit of the conversion of heat into power leads to an absurd conclusion. For instance, suppose that a quantity of fuel *A* will raise 1000 lbs. of water 1°.– Then according to a theory which does not admit the convertibility of heat into power the same quantity *A* of fuel working a steam engine will produce a certain mechanical effect, and besides that will be found to have raised 1000 lbs. of water 1°. But the mechanical effect of the engine might have been employed in agitating water and thereby raising 100 lbs. of water 1°, which added to the other makes 1100 lbs. of water heated 1° in the case of the engine. But in the other case, namely without the engine the same amount of fuel only heats 1000 lbs. of water. The conclusion from this would be that a steam

[57] Letter from J. P. JOULE to W. THOMSON, 6th October, 1848, KELVIN Papers, University Library, Cambridge.

> engine is a *manufacturer* of heat, which seems to me contrary to all analogy and reason.

Thus JOULE has pointed out the absurdity of THOMSON's partial acquiescence in JOULE views, namely in the conversion of power into heat as in the friction of fluids, while remaining sceptical of the converse process. THOMSON's scepticism derives of course from the THOMSON interpretation of the CLAPEYRON theory, and so, given JOULE's forceful argument here, WILLIAM THOMSON has virtually to reject JOULE or CLAPEYRON outright, but because of the strength of both, he can do neither.[58] His reply to JOULE in a letter dated 27th October 1848 demonstrates the nature of the tensions involved, and reveals still deeper problems which begin to go beyond the above initial debates on the interpretation of JOULE's experiments.[59]

THOMSON begins by saying that he despairs of stating everything in one letter, ... "especially as I must think and work upon the subject a good deal longer before I can collect my ideas ... and I now merely write a few remarks which will I hope lead towards an ultimate reconciliation of our views." The two main themes of this lengthy letter are again the friction of fluids and the conceptual problems of the steam engine and motive power. THOMSON defers at present saying anything about the magneto-electric experiments for ... "indeed I have not yet sufficiently considered the subject to see it in its bearings to our views on the Heat question," and neither does he discuss as such the condensation and rarefaction experiments. On the friction of fluids he describes apparatus which he has constructed for investigating the heat developed by a rotating disc of tin-plate with radial vanes on each side, and he remarks that by this means ... "we may be able to boil water by friction alone." Various points are made on the experimental details and the calculations of the ratio of work to heat (W/Q). JOULE replies to these particular suggestions on November 6, stating that he had read this section on the friction of fluids to the Manchester Literary and Philosophical Society, and claims that he had found ... "considerable difficulty in persuading our scientific folks here that the heat derived in my experiments was not derived from the friction of the bearings under water, but your experiments were not so easily cavilled at."[60] Thus JOULE seized the opportunity of using the more dramatic experimental suggestions of THOMSON to convince the numerous sceptics. He continued:

> I have also always found a difficulty in making people believe that fractions of a degree could be measured with any great certainty, but your experiments showing a rise of temperature of 30° or 40° would prove the truth of the fact by the warmth as felt by the hand .... At present I am working with a view to get the equivalent more exactly .... Could you not by ascertaining the force requisite to maintain your apparatus at a certain velocity obtain

---

[58] See S. P. THOMPSON, *op. cit.* (note 1), **1**, 276–277; CARDWELL, *op. cit.* (note 1), pp. 241–244. Relying as they do largely on the published papers of THOMSON, these historical accounts tend to miss the underlying debates from which the reshaping of the scientific ideas emerge.

[59] W. THOMSON to J. P. JOULE, 27th October, 1848, KELVIN Papers, University Library, Cambridge (copy).

[60] J. P. JOULE to W. THOMSON, 6th November, 1848, KELVIN Papers, University Library, Cambridge.

an estimate of the force absorbed in your experiments. A confirmation of the equivalent obtained that way would I think be important as the force you employ is so immensely greater than mine.[61]

Thus both THOMSON and JOULE continue to be in accord over the friction of fluids, the special appeal of JOULE's view being that it provided a satisfactory answer to the old query on the loss of power.

However, in the October letter, THOMSON turns to the second main theme, dealing with CARNOT's theory of the motive power of heat. He continues by stating that he was quite aware of the importance of the objection JOULE adduces to CARNOT's theory, an objection "which I admit in its full force, agreeing as I do with you when you say that you coincide with FARADAY and ROGET."[62] That is, THOMSON supports JOULE in his claim that the power to destroy or annihilate *force* or *vis viva* is the privilege of the Creator alone. It is not at all obvious why THOMSON should have apparently quite suddenly adopted this position on the indestructibility of *vis viva* or living force, until we realise the important shift of emphasis with regard to primary concept that has been taking place for THOMSON *within* a framework of dynamical scence.

In his "Introductory lecture to the course of Natural Philosophy," in 1846, the primary concept for THOMSON was force. Thus he claimed that "the fundamental subject of Natural Philosophy is Dynamics, or the *science of force* .... Every phenomenon in nature is a manifestation of force." NEWTON's laws of motion constituted the science of abstract dynamics, within which the principle of conservation of *vis viva* would be seen as a theorem derivative from the primary laws of motion.[63] From abstract dynamics, THOMSON could proceed to a study of the phenomena of nature or of the branches of natural philosophy in two ways. Either he could seek to employ the laws of motion, and in particular the concept of force, as *the* basis for explanation of all natural phenomena, that is, he could attempt to *reduce* all the branches of physics to the simple laws and concepts of dynamics.[64] Or he could adopt a non-reductionist approach by aiming to establish separate laws involving only terms resulting from observation within the various branches of physics and employ the laws of abstract dynamics *in combination with* these laws. Thus physical astronomy provided the model. The inverse square law of gravitation was a separate law which involved only observable terms. It could be used in conjunction with the laws of abstract dynamics, the concept of force providing the common link. Unlike the reductionist approach, no attempt would be made to reduce the inverse square law itself to the laws of dynamics. THOMSON in 1846 appears to have been working within just such a non-reductionist framework. For him, abstract dynamics constituted the core

---

[61] *Ibid.*

[62] See above note 51.

[63] See S. P. THOMPSON, *op. cit.* (note 1), **1**, 241. THOMSON's lecture is printed (pp. 239–251) in full. For texts on mechanics in which the *vis viva* principle is held to be a derivative theorem, see WILLIAM WHEWELL, *An Elementary Treatise on Mechanics* (5th edition, Cambridge: J. & J. J. Deighton, 1836), p. 161 and J. H. PRATT, *The Mathematical Principles of Mechanical Philosophy* (Cambridge: J. & J. J. Deighton, 1836), pp. 201–202.

[64] For an example of such (unsuccessful) attempts at reduction with respect to the second law of thermodynamics see M. J. KLEIN, *The American Scientist* **58**, 84–97 (1970).

of a dynamical science which in principle would have its various branches — astronomy, electricity, magnetism, and heat, for example — *linked* to this core by the key concept of force.[65]

WILLIAM THOMSON's shift of emphasis from force to energy as the primary concept in dynamical science came after 1847. JAMES THOMSON and JOULE, as I have shown, both began to realise the importance of *vis viva* and power as concepts in their own right, and this point is especially true of JOULE. For JAMES THOMSON these concepts were crucial in the theory of the motive power of heat, linking it to abstract dynamics and thus displacing force from its privileged position as far as heat as a branch of natural philosophy was concerned. From CARNOT and CLAPEYRON, too, comes the axiom that a perpetual source of power is impossible. That power cannot be created had been accepted by JAMES THOMSON from that source.[66] For JOULE, power and *vis viva* were now convertible and indestructible concepts in all the phenomena of nature. So he had claimed not only the impossibility of creating power, but also the impossibility of destroying it, in contrast to the CARNOT-CLAPEYRON view that power could be destroyed. It was this aspect — the full and complete indestructibility of power and living force — which WILLIAM THOMSON seems to have specifically accepted in the above letter as a new feature in his thought.

THOMSON's letter, therefore, pursues the problem involved in the motive power of heat where, according to JOULE's interpretation of CLAPEYRON, living force may be destroyed by an improper disposition of the engine. WILLIAM THOMSON confesses that he has never seen any way of explaining the difficulty, "although I have tried to do so since I read CLAPEYRON's paper; but I do not see any modification of the general hypothesis which CARNOT adopted in common with many others, which will clear up the difficulty. That there really is a difficulty in nature to be explained with reference to this point (just as there is with reference to the loss of mechanical effect in fluid friction) the consideration of the following case will I think convince you."[67] He then proceeds to outline to JOULE a thought-experiment which illustrated JOULE's objections to CLAPEYRON by dealing with the two extremes — perfectly reversible production of work by allowing heat to fall through a difference of temperature, and perfect conduction where apparently neither *vis viva* nor work result from the passage of heat. Work produced in the former case is not produced in the latter and is thus in a sense "lost." THOMSON commences by supposing a mass of air filling a cylinder of which the mouth is closed by a piston, "the temperature of the air being lower than that of the sea." As JAMES THOMSON did earlier, he here employs the concept of the sea as an

---

[65] For a discussion of the sources of this view, see section 2 of my paper "Mechanical Philosophy and the Emergence of Physics in Britain: 1800–1850", *Annals of Science* **33**, 2–16 (1976). For the origins of THOMSON's view, see my forthcoming paper "Natural Philosophy and Thermodynamics" in *The British Journal for the History of Science*. It must be stressed that the two approaches, reductionist and non-reductionist, employing dynamics, are not in conflict. THOMSON at this stage in his career seems to have preferred not to commit himself to hypotheses about unobservable processes. As we shall see, such a non-reductionist approach led him to the creation of a classical thermodynamics, in which little attempt was made to go beneath the laws of the phenomena.

[66] See above for the discussions of STIRLING's air engine and perpetual motion.

[67] W. THOMSON, *op. cit.* (note 59).

infinite reservoir of heat. Then "let the piston be pushed down till the temperature of the air becomes that of the sea," and let the whole set-up be plunged below the sea. He continued:

> ... let the bottom & sides of the cylinder become perfectly permeable to heat. The piston may now be allowed to rise gradually, doing work, the temperature of the air expanding in the cylinder remaining the same as that of the sea. When the piston has arrived at its original position we shall have the mass of air at its original volume but raised in temperature to that of the sea. Now the work spent in compressing the air when its temperature was being raised is clearly less than the work obtained by allowing it to expand retaining the higher temperature. Hence there is an amount of work gained.[68]

So far there is nothing about which to be concerned, the arguments being dealt with by simple application of CARNOT's theory to the expansion of a gas. Thus, where there exists a temperature difference, there motive power must be produced. However, ... "the original mass of air, with the piston held fixed, might at once have been plunged into the sea & been allowed to have its temperature inc[reased] by conduction gradually" and so ... "the effect might have been produced without getting any work. What has become of the work that might have been gained in arriving at the same result after having gone through the process first described?" THOMSON adds that he as yet sees no way of explaining this difficulty, but that "there *must* be an answer." "I do not see how it can be explained by saying that a greater quantity of heat is taken from the sea in the first process than in the second; and although perhaps an experimental test of the truth is necessary, I believe it would be universally admitted that the quantity of heat absorbed in the two cases would [be] precisely the same."

The above query therefore is the origin of the well-known footnote in THOMSON's "Account of CARNOT's Theory" of 1849 which poses the issue in very similar terms:

> When "thermal agency" is thus spent in conducting heat through a solid, what becomes of the mechanical effect which it might produce? Nothing can be lost in the operations of nature – no energy can be destroyed. What effect then is produced in place of the mechanical effect which is lost?[69]

Attention was now being focussed on conduction which of course was the very concept with which THOMSON had been dealing during the 1840's in his work on FOURIER.[70] Both the letter of 1848 and the footnote of 1849 highlight the problem of the relation between conduction of heat and dynamics, and I would suggest that it is here that THOMSON begins to see more clearly a conflict,

---

[68] *Ibid.*

[69] W. THOMSON, *op. cit.* (note 41), **1**, 118n. Here THOMSON uses the term "energy" for the first time. In a letter to his father on 13th May, 1846, preserved in Cambridge University Library, he remarked that "I have had Young's Lectures for some time (commenced reading them a few days ago) but I have not had time to see much of them yet". It was in THOMAS YOUNG's *A Course of Lectures on Natural Philosophy and the Mechanical Arts*, ed. P. KELLAND (London: Taylor and Walton, 1845) that YOUNG used the term "energy" to express *vis viva*.

[70] See S. P. THOMPSON, *op. cit.* (note 1), **1**, 40–42; 111–112; 184–188 for an account of THOMSON's work on FOURIER during the early 1840's.

not just between CARNOT and JOULE, but more specifically, between the concept of conduction and the dynamical framework of natural philosophy which THOMSON had adopted. The letter of 1848, however, brings out more strongly than the footnote the crucial part played by interaction between the two thinkers, THOMSON and JOULE, throughout the period.

In his immediate reply to THOMSON's letter, JOULE admitted that he had not yet studied its contents or CARNOT's theory sufficiently "to enter into a discussion of this theory of the motive power of heat ...." However, he continued with a comment on one of THOMSON's points:

> In reference to the hypothetical experiment you mention, that of filling a cylinder with air at a lower temperature than the sea and then having pushed down the piston so as to raise the temperature equal to that of the sea, to immerse it in the sea, and allow the air to keep the temperature of the sea on expanding, I certainly should say that more heat will in that case be abstracted from the sea than if the temperature of the air had been raised by it without any motion of the piston developing force.[71]

He remarks further that he would like to see the experiment tried in practice even though it presented difficulties. If its result could in any way be decisive proof against CARNOT's theory, then the experiment would be especially worthwhile. JOULE suggested a bath of mercury to represent the sea, and requested THOMSON rather than himself to carry out the experiment because of his own "bias." JOULE's claim in the above passage is in harmony with his view of the convertibility of heat and work, while THOMSON's contrary claim arises from CARNOT's view that heat is merely transferred between the two temperatures.

In JOULE's next letter of December 9, 1848,[72] he acknowledged receipt of tables from THOMSON probably with reference to the experiments on fluid friction, and included calculated values of the work – ratio of heat (W/Q) corresponding to different temperatures. He again admits not having studied them fully "so as to be able to comprehend the whole scope of your views," but does mention that his calculation of W/Q may suffer for want of accurate experimental data. He also calculates μ, the CARNOT coefficient, and he suggests that the values of μ vary inversely as the absolute temperature from zero, a claim which THOMSON later attributed to JOULE.[73] JOULE concludes the letter with what he sees as a disagreeable task of having to write to the *Comptes Rendus* in order to defend his claims for priority, and he states clearly his reluctance to enter into controversy. The problem is the discovery of the work of MAYER and this letter to THOMSON contains the first discussion in a long battle extending to the public controversies from 1862 – mainly between JOHN TYNDALL and P.G. TAIT. Thus JOULE writes:

> I perceive that a German of the name of MAYER has set up a claim for the discovery of the equivalent upon the ground that he asserted in 1842 that

[71] J. P. JOULE, *op. cit.* (note 60).

[72] J. P. JOULE to W. THOMSON, 9th December 1848, KELVIN Papers, University Library, Cambridge.

[73] This point concerning the interpretation of the CARNOT coefficient or function is discussed by THOMPSON, *op. cit.* (note 1), **1**, 274; 292.

> the heat produced by compressing air was the equivalent to the force employed although he had made no experiments to prove it.[74]

This is the theme which finds a constant echo throughout the controversy, that MAYER's claim was a mere hypothesis compared to the experimental "proof" involved in JOULE's papers. On March 10, 1849, JOULE emphasized this point, while stating that he does not wish to detract from MAYER's real merits. The dispute rested until March 1851, when JOULE again refers to MAYER in a letter to THOMSON, stressing that MAYER's claims were not warranted by the facts at the time.[75]

Several points emerge here relevant to the historical perspective of JOULE and MAYER. It can be seen, first of all, that many of JOULE's objections and criticisms regarding MAYER long pre-date the public controversies of the 1860's, when these criticisms received new weight. More importantly, MAYER's discussions of the 1840's gave fresh impetus to debates on JOULE's paper of 1840, which, as we saw earlier, did not at first impress the THOMSON brothers. The questions arising from the condensation and rarefaction of air had been discussed by JOULE in 1844, and his arguments defended in his correspondence, but emphasis on them had largely been neglected compared to the dominating themes of fluid friction and CARNOT's theory. Recognition of MAYER's "hypothesis" by JOULE in 1848, I would claim, brought the old issues of rarefaction and condensation of air to the fore again, and led to Part IV of WILLIAM THOMSON's "Dynamical Theory of Heat" entitled "On a Method of discovering experimentally the Relations between the Mechanical Work spent and the Heat produced by the compression of a Gaseous Fluid."[76] There THOMSON states that the researches of JOULE (in the paper of 1844) ... "have introduced an entirely new method of treating questions regarding the physical properties of fluids." THOMSON's aim in the paper is to show how ... "by the use of this new method, in connexion with the principles explained in my preceding paper, a complete theoretical view may be obtained of the phenomena experimented on by JOULE; and to point out some of the objects to be attained by a continuation and extension of his experimental researches." In addition, much of the JOULE-THOMSON correspondence subsequent to 1851 deals with details of joint experiments concerned with verifying MAYER's hypothesis, and, for example, JOULE remarks in 1852 that the experimental result on the specific heat of air ... "goes to confirm MAYER's hypothesis."[77] The discussion continues with plans for greater experimental accuracy in the measurement of results.

Overall, therefore, the point which should be stressed is the way in which, as early as 1848, the interaction of the British thinkers with the apparently-neglected MAYER comes into play, and his work provides an important supplementary strand which, given JOULE alone, might well have been itself neglected.

---

[74] JOULE, *op. cit.* (note 72). See also LLOYD, *op. cit.* (note 11) for the controversy in the 1860's.

[75] J. P. JOULE to W. THOMSON, 10th March, 1849, and 17th March, 1851, KELVIN Papers, University Library, Cambridge.

[76] W. THOMSON, *op. cit.* (note 41), **1**, 210–222.

[77] J. P. JOULE to W. THOMSON, 21st February, 1852, KELVIN Papers, University Library, Cambridge.

Thus, although in his "Dynamical Theory," THOMSON gives pride of place to JOULE's researches, MAYER occupies a not unfavourable position as the originator of the so-called "MAYER's Hypothesis."

3. Rankine's Contribution

It was in the years of conflict and debate (1847–1851) that another thinker joined the network of interactions centred on WILLIAM THOMSON. On 4th February, 1850, W.J.M. RANKINE read to the Royal Society of Edinburgh his paper "On the Mechanical Action of Heat, especially in Gases and Vapours."[78] The Introduction contained a summary ... "of the principles of the hypothesis of molecular vortices, and its application to the theory of temperature, elasticity, and real specific heat," and there he claimed to have commenced his researches in 1842, but to have laid them aside because of the lack of accurate data until 1849 when he was able to resume them in consequence of REGNAULT's experiments on gases and vapours. The molecular hypothesis of heat had many conceptual affinities with prevailing views, and some important differences.[79] However, what concerns us here is the relevance to THOMSON, and *vice versa*, for it was THOMSON who gave a report on the paper, a report which probably led to modification of certain aspects by RANKINE before it was published in the *Transactions of the Royal Society of Edinburgh*. Thus, in the first letter of the series exchanged between RANKINE and THOMSON, RANKINE, writing from Edinburgh in April 1850, states:

> I shall take the opportunity of being in Glasgow tomorrow to wait upon you, at the suggestion of Professor FORBES with the enclosed papers. They consist of 1st My paper on the Theory of Heat, as modified by me in the course of the last few days. 2nd Your report on that paper. 3rd A letter from myself to Professor FORBES which he thinks should be shewn to you as it may be useful in pointing out to you the modified passages in the paper .... I beg leave to return you my best thanks for the permission which you gave the Council to allow me to peruse your report ....[80]

A fragment of this report, written in the hand of THOMSON, survives and is entitled "RSE Memorandum for report on RANKINE's original paper on molecular vortices."[81] Some of the rather scattered remarks are worth noting, while

---

[78] W.J.M. RANKINE, *Miscellaneous Scientific Papers*, ed. W.J. MILLAR (London: Charles Griffin, 1881), p. 234f.

[79] *Ibid.*, pp. 234–236. RANKINE mentions FRANKLIN, AEPINUS, MOSSOTTI, DAVY and JOULE as putting forward suppositions similar to his own hypothesis on the nature of matter. Each atom of matter consists of a nucleus surrounded by an elastic atmosphere. For RANKINE, as for DAVY and JOULE, quantity of heat is the *vis viva* of the revolutions or oscillations among particles of the atomic atmospheres. His claim to originality, apart from developing the mathematical consequences of the vortex hypothesis, is that the medium which transmits light and radiant heat consists of the nuclei of the atoms, vibrating independently, or nearly so, of their atmospheres. See also B.G. DORAN, *Historical Studies in the Physical Sciences* **6**, 185–189 (1975) for an analysis of RANKINE's hypothesis in the context of nineteenth century aether debates.

[80] Letter from RANKINE to WILLIAM THOMSON, 19th April, 1850, KELVIN Papers, University Library, Cambridge.

[81] Manuscript PA 119, KELVIN papers, University Library, Cambridge.

others are of purely technical interest or concern points of presentation. THOMSON writes that RANKINE ... "claims to be the first who applies Math[ematical] Anal[ysis] to the theory of molecular vortices wh[ich] was first intelligibly stated by S.H.D. [Sir HUMPHRY DAVY]" and he comments that RANKINE has "Good remarks on JOULE." This comment could either refer to RANKINE's view that JOULE's valuable experiments aimed to establish the convertibility of heat and mechanical power, or to his remarks that JOULE supported the supposition of molecular vortices "that the elasticity due to heat arises from the centrifugal force of revolutions or oscillations," or both.[82] In this connection it is essential to note the dichotomy present in RANKINE's paper of 1850 between a hypothetical approach involving molecular vortices, and an approach through laws or axioms, in this case of mutual convertibility. The emphasis in the paper of 1850 is obviously on the first approach, the law being derivative. THOMSON emphasised the second approach in these early years, being much less concerned with hypothetical entities, and certainly wary of hypotheses in the sense of speculations, than with having a system of laws as the corpus of a natural philosophy and dynamical science. Thus THOMSON's comment above is more likely to refer to JOULE's law of convertibility. Otherwise, THOMSON's criticisms are directed more to specific inconsistencies and points of detail in RANKINE's paper which do not concern us here. Rather, I want to suggest that the importance of this report lies in the quite definite establishment of an early interaction between the two thinkers and we shall now see the course of development of this interaction.

In August 1850, RANKINE wrote to THOMSON to thank him for "calling my attention to the paper by CLAUSIUS, in POGGENDORFF's *Annalen*, on the Mechanical Theory of Heat. I approve of your suggestion to send a copy of my paper either to CLAUSIUS or to POGGENDORFF."[83] Thus THOMSON was by this stage aware of the first paper by CLAUSIUS, published by April 1850, "On the Motive Power of Heat, and on the laws which can be deduced from it for the Theory of Heat."[84] This fact, however, does not at all imply that THOMSON had assimilated or approved its contents, and therefore the period between August 1850 and March 1851 requires careful interpretation.

RANKINE, in his paper of 1850, makes reference to CARNOT's theory, but his remarks do not occupy a particularly prominent position.[85] From his hypotheses he derives a function U "depending on molecular forces," and the nature of which is as yet unknown. The only case in which it can be calculated directly is that of a perfect gas, though in all other cases the value of U can be determined "by introducing into the investigation the principle of the *conservation of vis viva*." By a method *analogous* to that employed by CARNOT, using cyclic processes, RANKINE ascertains U, founding his investigations not on heat as a substance, but on the convertibility of heat and power.[86] Thus he writes:

---

[82] RANKINE, *op. cit.* (note 78), p. 235.

[83] Letter from RANKINE to WILLIAM THOMSON, 19th August, 1850.

[84] See S. P. THOMPSON, *op. cit.* (note 1), **1**, 223 for the entry in WILLIAM THOMSON's diary dated 15th August, 1850: "I have just written to RANKINE telling him of CLAUSIUS' paper in POGGENDORFF ...." For an account of CLAUSIUS' 1850 paper see CARDWELL, *op. cit.* (note 1), pp. 244–249.

[85] RANKINE, *op. cit.* (note 78), pp. 252–253.

[86] *Ibid.*, pp. 244–253.

> According to the theory of this essay ... and to every conceivable theory which regards heat as a modification of motion, no mechanical power can be given out in the shape of expansion, unless the quantity of heat emitted by the body in returning to its primitive temperature and volume is *less* than the quantity of heat originally received.[87]

In a very real sense, therefore, RANKINE's theory, developed according to his hypothesis of molecular vortices, supersedes CARNOT's theory entirely, unlike CLAUSIUS who reconciles CARNOT's principle with the axiom of convertibility. At this stage the only resemblance seen by RANKINE between his own theory and CARNOT's is that they both treat of the motive power of heat in ideal, reversible cycles. RANKINE certainly does not place any emphasis on CARNOT's principle as a fundamental axiom or principle of nature. The vortex hypothesis and the principle of the conservation of *vis viva* obviate the necessity for that.

Both CLAUSIUS and RANKINE had indeed initially responded to CLAPEYRON and CARNOT; CLAUSIUS by reading THOMSON's "Account of CARNOT's Theory" in 1849, and RANKINE by reading the translation of CLAPEYRON in TAYLOR's *Scientific Memoirs* (1837). Thus in the same letter to THOMSON, RANKINE could write that his first attempt to apply mathematical reasoning to the subject arose from his seeing the translation of CLAPEYRON's paper "on the opposite theory," opposite that is to a mechanical theory of heat. He continued:

> The mechanical convertibility of heat has always (since I was first able to reason on the subject) appeared to me as approaching the nature of a necessary truth. I do not of course believe that it is really so; but I speak merely of the feeling, from whatsoever cause arising, which it has produced in my own mind. I have consequently always felt a confident anticipation of its being proved by experiment.[88]

The reference to a "necessary truth" occurs again in the letter of November 1853 to the *Philosophical Magazine* concerning his "Prefatory Remarks" to the Mechanical Action of Heat, the paper of 1850 being republished in that journal. There he claims that "The law of the mutual convertibility [of physical powers] has long been a subject of abstract speculation, and may appear to some minds in the light of a necessary truth. As we cannot, however, expect it to be generally received as such, its practical demonstrations must be considered as having been effected by the experiments of Mr. JOULE."[89] In the phrase "to some minds" he is clearly thinking of his own remarks in the letter above, and it is interesting to note the way he viewed the work of JOULE. In his paper of 1850 also he was concerned not only with the mutual conversion of heat and expansive power, but with the wider framework of physical powers generally. There he spoke of JOULE's experiments to ascertain the quantity of heat developed in various substances by mechanical power, and he listed some of JOULE's methods employed

---

[87] *Ibid.*, p. 253. The heat "disappearing", RANKINE also states, "appears" as expansive power, the sum of the *vis viva* of heat and expansive power continuing unchanged.

[88] RANKINE, *op. cit.* (note 83).

[89] W.J. RANKINE, *Phil. Mag.* [4], 7, 1-3 (1854).

to obtain the mechanical equivalent of heat.[90] RANKINE saw such evidence of the convertibility of heat and power as unexceptional, even if "the smallness of the difference of temperature measured in those experiments renders the numerical results somewhat uncertain," and he believed the true mechanical equivalent of heat to be considerably less than any of JOULE's values. This is because in all these experiments and apparatus ... "there are causes of loss of power the effect of which it is impossible to calculate." Thus he continues:

> In all machinery, a portion of the power which disappears is carried off by waves of condensation and expansion, along the supports of the machine and through the surrounding air: this portion cannot be estimated, and is, of course, not operative in producing heat within the machine. It is also impossible to calculate, where friction is employed to produce heat, what amount of it has been lost in the production of electricity, a power which is, no doubt, convertible into heat, but which, in such experiments, probably escapes without undergoing that conversion.[91]

RANKINE, then, has a general conception of the framework of conversion, and his detailed references to JOULE suggest that he had studied JOULE's papers in some depth. For instance, RANKINE argued that in order to:

> Make the determination of the mechanical equivalent of heat by electro-magnetic experiments correct, it is necessary that the whole of the mechanical power should be converted into magnetic power, the whole of the magnetic power into what are called electric currents, and the whole of the power of electric currents into heat, not one of which conditions is likely to be exactly fulfilled.[92]

Mutual conversion, that is, these conversion processes and their reverse effect, appears to be implicitly assumed by RANKINE in this clear reference to JOULE's electromagnetic researches of 1843. RANKINE also referred to JOULE's paper of 1844, claiming that "Even in producing heat by the compression of air, it must not be assumed that the whole of the mechanical power is expended in raising the temperature," and he stated that "the best means of determining the mechanical equivalent of heat are furnished by those experiments in which no machinery is employed" as, for example, those on the velocity of sound and other gases according to the theory of LAPLACE. RANKINE estimates a "better" value at 695 feet, which was the height to which one pound of water needed to be raised in order to have done work equivalent to one British thermal unit, that is, to the quantity of heat required to increase the temperature of one pound of water by one degree Fahrenheit.[93]

JOULE wrote to THOMSON on 30th August 1850 to remark that "RANKINE was so good as to give me a copy of his very interesting & valuable paper ..." but also to comment that ... "I do not think that the errors in my experiments are such as to cause the great difference between mine, and the equivalent deduced

[90] RANKINE, *op. cit.* (note 78), pp, 244–245.
[91] *Ibid.*, p. 245.
[92] *Ibid.*, p. 245.
[93] *Ibid.*, p. 245.

by RANKINE."[94] RANKINE later, in November 1850, recanted, writing to THOMSON that he had ... "attentively considered Mr. JOULE's experiments on the production of heat by friction in water, mercury & cast iron; and I think there can be little doubt that his equivalent of 772 feet per degree of Fahrenheit in liquid water is very nearly correct: probably to about 1/300 part." He concludes that the results of the experiments of DE LA ROCHE and BERARD on the specific heat of air under constant pressure, from which he deduced the equivalent of 695 feet, must be wrong by about 1/10.[95] This reference to JOULE undoubtedly concerns his meticulous paper of 1850 in the *Philosophical Transactions*, "On the Mechanical Equivalent of Heat," which seems to have been inspired by a combination of WILLIAM THOMSON's suggestions for boiling water by friction alone, and the need to convince the sceptics by more decisive experiments.[96] RANKINE noted that the discrepancy did not affect any of his formulae and tables relative to the steam engine "to an extent appreciable in practice," and he sent a statement of corrections and modifications of results to the Royal Society of Edinburgh, and a summary to THOMSON and JOULE.[97] These amendments were read before the society on 2nd December 1850, and published as a supplement to the paper on "The Mechanical Action of Heat," giving this time full credit to the accuracy of JOULE's results. RANKINE made it clear that his recent reading of JOULE's paper of 1850 had convinced him ... "that the agreement amongst the results from substances so different, shows that the error by unknown losses of power is insensible, or nearly so ..." and he refers to the dynamical equivalent being close to JOULE's figure.[98] Thus, by the end of 1850, RANKINE has fully adopted JOULE's framework of mutual convertibility, and his figure for the dynamical equivalent of heat.

An important proposition in RANKINE's paper of 1850 took on special relevance for THOMSON and JOULE with regard to fluid friction in this critical period of late 1850. THOMSON had apparently brought it to JOULE's notice, for in the August letter, JOULE wrote:

> The point you mention about dry steam from a high pressure boiler is very interesting, and I hope you will not delay to publish your remarks upon the subject in the Phil[osophical] Magazine .... The friction against the orifice will undoubtedly liberate heat. This circumstance also will account for the good duty performed by some steam engines, although the narrowness of the passages to the cylinder appears such as must seriously obstruct the flow of steam from the boiler.[99]

This non-scalding property of steam from a high pressure boiler was fully discussed in a letter to JOULE from THOMSON dated October 1850, which JOULE

[94] Letter from J. P. JOULE to WILLIAM THOMSON, 30th August, 1850, KELVIN Papers, University Library, Cambridge.

[95] Letter from RANKINE to WILLIAM THOMSON, 28th November, 1850, KELVIN Papers, University Library, Cambridge.

[96] JOULE, *op. cit.* (note 46), **1**, 268f. See section 2 above for THOMSON's suggestions in his letter of October 1848.

[97] RANKINE, *op. cit.* (note 95).

[98] RANKINE, *op. cit.* (note 78), pp. 285–287.

[99] JOULE, *op. cit.* (note 94).

published in the *Philosophical Magazine*.[100] RANKINE's proposition was: "If vapour at saturation is allowed to expand, and at the same time is maintained at the temperature of saturation, the heat which disappears in producing the expansion is greater than that set free by the fall of temperature, and the deficiency of heat must be supplied from without, *otherwise a portion of the vapour will be liquified in order to supply the heat necessary for the expansion of the rest*."[101] That such liquefaction does not take place is shown by the dry nature of the steam and therefore, THOMSON claims, RANKINE's conclusions can be reconciled with such facts only by JOULE's discovery that heat is evolved by the friction of fluids in motion, that is, heat is acquired by the steam as it issues through the orifice.[102]

THOMSON in this published letter also refers to JOULE's fundamental principle regarding the convertibility of heat and mechanical effect, "adopted also by Mr. RANKINE," and to the point that "the demonstration which Mr. RANKINE gives of his proposition is partially founded on certain hypotheses regarding the specific heats of gases and vapours." However, THOMSON himself clearly favours the JOULE principle as he states in conclusion that "we may demonstrate Mr. RANKINE's remarkable theorem without any other hypothesis than the convertibility of heat and mechanical effect."[103] RANKINE had already written to THOMSON in August in general terms about the employment of "hypothesis" noting:

> As to the hypothetical part of my investigations, although it undoubtedly rests on a much less firm basis than that which is founded on the general law of the mechanical convertibility of heat, and although I believe you did my paper essential service by inducing me to make it less prominent & less detailed than it was originally, still I conceive it may lead to some useful results.[104]

Here the influence of THOMSON is clearly admitted, and in his later philosophy, RANKINE adopts the view that laws or axioms are to be sought, though hypotheses and hypothetical entities may be useful in the early stages of research.[105]

We must now examine the state of THOMSON and RANKINE's views in relation to those of CLAUSIUS towards the end of 1850. RANKINE in a postscript to a letter of September 1850 wrote to THOMSON:

---

[100] Letter from WILLIAM THOMSON to J. P. JOULE, *Phil. Mag.* [3] **37**, 386 (1850).

[101] RANKINE, *op. cit.* (note 78), pp. 260–261.

[102] W. THOMSON, *op. cit.* (note 100), p. 387–388.

[103] *Ibid.*, pp. 387–388. RANKINE himself approved of THOMSON's interpretation as he wrote to THOMSON in the letter of November 1850: "I am glad that you have published the suggestion you mentioned to me last summer that the dryness of high-pressure steam rushing from an orifice to a boiler is owing to the reproduction by friction of part of the heat consumed by expansion."

[104] RANKINE, *op. cit.* (note 83).

[105] For an account of the philosophical sources for RANKINE's later views of axioms and hypotheses, see RICHARD OLSON, *Scottish Philosophy and British Physics 1750–1880* (Princeton: Princeton University Press, 1975), pp. 271–286. From the nature of RANKINE's exchanges with THOMSON it seems to me likely that RANKINE consequently reinterpreted the role of hypotheses in physics. Hypotheses for him became valuable for their suggestiveness rather than for their truth. Such a view would then, as OLSON argues, be consistent with Scottish Common Sense philosophy in which the emphasis was on the establishment of laws containing only observable concepts.

> I have looked over the Second Part of the paper of CLAUSIUS "Über die bewegende Kraft der Wärme ...." The First Part, consisting entirely of deductions from the law of the convertibility of Heat and Power, agrees, as far as it goes, with my own investigations.[106]

Thus RANKINE is in full agreement with the first part of CLAUSIUS' paper of 1850 where CLAUSIUS follows JOULE in adopting a principle of the equivalence of heat and work. As far as this principle is concerned, both RANKINE and CLAUSIUS recognise that it does not depend on the specific kind of motion which can be conceived of as taking place within bodies, or, as RANKINE expresses it in his paper:

> Those phenomena, according to the hypothesis [of molecular vortices] now under consideration as well as every hypothesis which ascribes heat to motion, are simply the transformation of mechanical power from one shape to another.[107]

Thus the fundamental principle adopted by CLAUSIUS in 1850 that,

> In all cases in which work is produced by the agency of heat a quantity of heat is consumed which is proportional to the work done; and, conversely, by the expenditure of an equal quantity of work an equal quantity of heat is produced,[108]

is accepted by RANKINE as equivalent to his own (and JOULE's) view of the equivalence of heat and work, though CLAUSIUS' statement is not wholly expressed in the language of mutual convertibility.

The situation is more complex in the case of the second part of CLAUSIUS' paper. RANKINE continues the above letter thus:

> The Second Part consists of deductions from the same law [of heat-work equivalence], taken in conjunction with *a portion* of the principle of CARNOT, viz., that the power produced by transmitting a given quantity of heat *through* any substance, is equal to the quantity of heat transmitted multiplied by a function of the temperature only: in other words, that the ratio of the q. [quantity] of heat converted into expansive power to the q. [of heat] not so converted, is a function of the temp[erature] only. CLAUSIUS gives a sort of *a priori* proof of this second law, which so far as I have yet been able to consider the subject, seems to me very unsatisfactory.[109]

RANKINE here *describes* what he sees as CLAUSIUS' adoption of part of CARNOT's principle to constitute what RANKINE terms a second law. He does not explicitly state his approval or disapproval of CLAUSIUS' move, but only expresses his scepticism over CLAUSIUS' "proof" of the principle. The "proof" to which RAN-

---

[106] Letter from RANKINE to WILLIAM THOMSON, 9th September, 1850, KELVIN Papers, University Library, Cambridge.

[107] RANKINE, *op. cit.* (note 78), p. 246.

[108] *Reflections on the Motive Power of Fire by Sadi Carnot* ..., ed. E. MENDOZA (New York: Dover Pubs., 1960), p. 112.

[109] RANKINE, *op. cit.* (note 106).

KINE refers was the argument by CLAUSIUS that if the modified CARNOT principle were false, then "it would be possible, without any expenditure of force or any other change, to transfer as much heat as we please from a cold to a hot body, and this is not in accord with the other relations of heat, since it always shows a tendency to equalize temperature differences and therefore to pass from hotter to colder bodies."[110] CLAUSIUS therefore attempted to found the modified CARNOT principle on what he sees as a more widely-based principle, namely, that the transfer of heat from a cold to a hot body is impossible without compensation. Later, in a letter of March 1851, RANKINE remarks on CLAUSIUS again, making his own position, retrospectively at least, a little clearer: "I always thought the principle of CLAUSIUS to which you refer had an appearance of probability; but I was not satisfied with his mode of proving it."[111]

WILLIAM THOMSON, for his part, knew of CLAUSIUS from mid-1850, but he does not seem to have assimilated the content of that paper at the time. Thus he wrote at the end of the October letter to JOULE concerning RANKINE's "proposition":

> I have not yet been able to make myself fully acquainted with this [CLAUSIUS'] paper; but, from the principles and methods of reasoning explained at the commencement, which differ from those of CARNOT only in the adoption of your axiom instead of CARNOT's, I have no doubt but that the demonstration of the proposition in question is the same in substance as Mr. RANKINE's modified in the manner I have suggested.[112]

From these remarks themselves it is not completely certain how THOMSON viewed CLAUSIUS's formulation, but taken together with the above letter about RANKINE describing CLAUSIUS' "second law," we may reasonably suppose that THOMSON was aware of the fundamental principles – the two "laws of thermodynamics" mentioned above – though he was probably not aware at this stage of CLAUSIUS' "proof" of the second law. We shall see the validity of this interpretation subsequently when dealing with the historical analysis of the "Dynamical Theory of Heat."

In the same published letter, THOMSON formulates a supposition which puts some of the issues in non-commital language as far as he is personally concerned. A quantity of saturated vapour, he states, is allowed to expand through a small orifice wasting all its "work" in friction, and *if* JOULE's principle of convertibility, also adopted by RANKINE, is true, the quantity of vapour ... "will, in its expanded state, possess the "total heat" which has been given to it; but, on the contrary, if it be allowed to expand, pushing out a piston against a resisting force, it will in the expanded state possess less than that total heat by the amount corresponding to the mechanical effect developed."[113] THOMSON's acceptance of JOULE's

[110] CLAUSIUS, *op. cit.* (note 108), pp. 132–134. See also CARDWELL, *op. cit.* (note 1), pp. 247–249; 253–254.

[111] Letter from RANKINE to WILLIAM THOMSON, 17th March, 1851, KELVIN Papers, University Library, Cambridge.

[112] W. THOMSON, *op. cit.* (note 100), p. 389.

[113] *Ibid.*, p. 388. See J. T. MERZ, *A History of European Thought in the Nineteenth Century* (New York: Dover Pubs., 1965), **2**, 128n. MERZ interprets THOMSON's statement here as implying his final acceptance of the doctrine of the convertibility of heat and work, an acceptance which, it is

principle is here stated in very cautions terms. Nevertheless he does seem on the point of substituting JOULE's axiom of convertibility for the axiom of the conservation of heat, while still retaining the main CARNOT principle. RANKINE's letter of September describing CLAUSIUS' theory may indeed have suggested to THOMSON the appeal of CLAUSIUS' arguments on the motive power of heat which he then formulated for himself. To THOMSON's own formulation, therefore, I shall now turn.

4. William Thomson's draft of "the Dynamical Theory of Heat"

WILLIAM THOMSON's famous series of papers "On the Dynamical Theory of Heat" was published between 1851 and 1855, and was essentially a development of the two laws of thermodynamics in mathematical-physical language.[114] It is my aim in the present section to analyse the manuscript of the early draft (February–March 1851) for the dynamical theory, particularly in the light of the material discussed in the preceding sections.[115] I shall thus attempt to show the synthetic nature of the papers on the "Dynamical Theory", and to set them in their historical context. THOMSON's beginning of the "Dynamical Theory" is modest enough. In the draft he makes it clear that his aim is to communicate the new theory as he had done in 1849 with CARNOT's theory, rather than to lay down a work of original discovery. Thus he states that he lays no claim to discovery, his main object being to show what general and numerical conclusions in his paper of 1849 still hold when the dynamical theory is adopted.[116]

He commences the draft with a restatement of the central axiom of CARNOT's Theory of the Motive Power of Heat, that thermal agency consists in the transference of heat between a body at a high temperature to one at a low temperature. However, he continues, the theory was founded on an axiom which was regarded by even CARNOT himself as doubtful, that is, that quantity of heat is conserved during this transfer, an axiom which may be incompatible with experimental facts.[117] There is evident in THOMSON's draft a certain fluidity of thought concerning the status of this axiom, and therefore his published remarks need to be treated with due care. Thus he replaces his original view *within* the draft that the axiom of conservation of heat was adopted "with a warning that it might have to be modified or abandoned" or "as a temporary exposition" with the more familiar published view that it was adopted "only because, its truth never

---

further claimed, took place as a result of RANKINE's paper of 1850. The real story is rather more complex, not being confined to a straight reading of RANKINE's paper by THOMSON, and resulting instead from much debate.

[114] W. THOMSON, *op. cit.* (note 41), **1**, 174–332.

[115] See Appendix II for a transcript of the draft. This transcript is referred to below as the draft.

[116] Draft, pages one and three.

[117] *Ibid.*, pages one and two. See also CARNOT, *op. cit.* (note 108), p. 46. CARNOT remarks there that "The fundamental law [CARNOT's principle] that we proposed to confirm seems to us to require, however, in order to be placed beyond doubt, new verifications. It is based upon the theory of heat as it is understood today, and it should be said that this foundation does not appear to be of unquestionable solidity. New experiments alone can decide the question". THOMSON himself had echoed CARNOT's words in a footnote to his "Account" of 1849: "It is in reality to experiment that we must look – either for a verification of CARNOT's axiom ... or for an entirely new basis of the Theory of Heat." See W. THOMSON, *op. cit.* (note 41), **1**, 119 n.

having been previously doubted, it had been almost universally considered as a fundamental principle in the theory of H[eat]."[118] THOMSON, therefore, by placing emphasis on the latter view, as BRUSH has rightly argued, misled historians into thinking that the axiom on the conservation of heat was accepted by almost everyone except JOULE.[119] The draft, however, shows that THOMSON was aware of the prevailing doubts, and that even he himself was perhaps holding to conservation of heat as a mere expedient. The real emphasis was on the CARNOT principle proper, as I discussed in relation to JAMES THOMSON, and it was this powerful principle which WILLIAM was so reluctant to abandon before the criticism of JOULE. The axiom of the conservation of heat, though of great significance in theories of heat, was not so indispensable as to provide a primary stumbling block towards a synthesis of JOULE's and CARNOT's views. Thus it is true of the later stages of the debates, 1850–51, when JOULE and the other strands were being closely assessed, that THOMSON readily accepted the conversion of work into heat in the friction of fluids.[120] In a more general sense, this point makes it clear that THOMSON was not just concerned to defend a particular theory of heat, but that his perplexities must have a wider and deeper origin. The suggestion from the draft here is that the conservation of heat became increasingly a pragmatic statement in THOMSON's thought. And so, in the draft, he remarks that in 1849 "I felt very great doubt as to the fundamental axiom on which CARNOT's theory depends," a doubt reinforced by objections raised by JOULE to some of CARNOT's conclusions.[121] Thus, while THOMSON was open-minded as to the possibility that more complete experimental knowledge might lead to the removal of JOULE's objections – presumably through the completeness, not as yet achieved, of REGNAULT's observations and data – and while he was not "fully prepared to admit his [JOULE's] able arguments in favour of a contrary axiom," THOMSON's aim in 1849 had been to communicate an account of CARNOT's theory ... "and some calculations I had made in connection with it." He therefore ... "carefully avoided committing myself by any decisive expression of my own opinion on the subject."[122] The apparent ambiguity of his views in 1849 on the nature of heat must be understood in the light of this interpretation, which, although THOMSON's own, and so liable to personal distortion, accords well with the complexity of interactions and conceptual moves discussed in the previous section.

THOMSON, then, could accept many of JOULE's arguments for the dynamical theory of heat, but for the crucial objection of JOULE to CARNOT concerning the difficult issue of heat conduction. This, THOMSON claims explicitly, was the principal issue weighting against his acceptance of JOULE. Mechanical effect in CARNOT's theory is held to be absolutely lost by conduction. Mechanical effect in the dynamical theory is *asserted* in such cases not to be lost, and is not otherwise accounted for.[123] Why a visible effect *qua* available mechanical effect here

[118] See W. THOMSON, *op. cit.* (note 41), **1**, 115–116.
[119] S.G. BRUSH, *The British Journal for the History of Science* **5**, 165–167 (1970).
[120] Compare CARDWELL, *op. cit.* (note 1), p. 244.
[121] Draft, page two.
[122] *Ibid.*, pages two and three.
[123] *Ibid.*, page five.

should not be produced was unexplained and unclear to THOMSON. In his "Account of CARNOT's Theory," in 1849 THOMSON had significantly employed the term "thermo-dynamic engine" (hence "thermodynamics") to indicate the link between CARNOT's theory and dynamical concepts. This move, it should be noted, was prior to his adoption of a theory of the dynamical *nature* of heat. When, therefore, no motive power or work was produced by conduction of heat, the clearly-defined link between CARNOT's theory and dynamics was broken. FOURIER's laws could describe the movement of heat by conduction from a state of concentration to one of diffusion, whether one regarded heat as a substance or as a motion. But there was no firm connection forged between FOURIER and dynamics. The only connection apparent to THOMSON was the rather unsupported *assertion* that in conduction mechanical effect is not lost because heat is *vis viva* in the form of particulate motion. Mention of particulate motion suggests the need to investigate the nature of heat at the molecular level, and we must see the extent to which THOMSON explored this aspect in the draft.

With his knowledge of at least some parts of CLAUSIUS' paper of 1850, THOMSON in the draft follows CLAUSIUS very closely on the question of the nature of heat, and thereby avoids the specificity of RANKINE's model. THOMSON states that the most probable hypothesis is of heat as a state of motion. If a number of bodies of different temperatures are mixed together, he says, "the *vis viva* of the motions of heat in the whole must remain constant." He therefore supposes the principle of conservation of *vis viva* as a mechanical principle rather in the way RANKINE does. But, he continues, the quantity of heat also stays constant under the same circumstances, and he concludes that therefore ... "quantity of heat corresponds to *vis viva*," a view which rejects JAMES THOMSON's earlier reflections, and which agrees with JOULE and CLAUSIUS. If, however, any of the bodies expand or contract so as to become of different "resilience" or "innere Arbeit" (CLAUSIUS' term), he notes, the *vis viva* in the system will be changed and therefore the quantity of heat will be changed.[124] This "hypothesis"—and he is fairly clear on the point that it is a hypothesis—can then be developed in one of two directions. Either it can be reformulated with axiomatic status as a principle of the convertibility of heat and work, or it may be seen as derivative from a specific molecular hypothesis. THOMSON, though not averse to considerations of the latter kind, has at this stage a clear preference for seeking after the fundamental laws of nature "as made manifest to man," and seeing them as part of a dynamical framework. Only later, when the result of this enterprise is not wholly satisfactory, does he begin to resort to the unobservable realm of molecular entities in order to pursue the role of dynamics there.[125] In the draft, however, his starting point is the "general" hypothesis of CLAUSIUS, which is, at it were, intermediate between the specific RANKINE approach and the axiomatic approach.

Initially, then, THOMSON adopts *vis viva* as expressing the nature of heat, and in this follows CLAUSIUS and JOULE. But already we have seen the way in which THOMSON had come to agree with JOULE that *vis viva*, and the principle of its conservation and convertibility into work or mechanical effect, were not

---

[124] *Ibid.*, page four. See CLAUSIUS, *op. cit.* (note 108), pp. 112–114.

[125] See, for example, W. THOMSON, *Proc. Roy. Soc. Edinburgh* **8**, 325–331 (1874).

secondary but primary in the created universe. THOMSON therefore goes on to employ the name "energy" in the draft to avoid terminalogical confusion and to signify the special status assigned now to *vis viva* and its related concepts. This latter move is very significant in expressing the conceptual revolution which has taken place to provide a key part of the new theory. No destruction of energy, says THOMSON, can take place in the material world "without an act of power possessed only by the supreme ruler."[126] Energy has here clear theological associations, being an indestructible entity, created, sustained, and destructible only by God's power. Creation and annihilation of energy, with respect to human beings, is impossible. And furthermore, energy is a definite quantity within a framework of dynamics, and is a concept, no longer secondary as *vis viva*, entering into a wide range of subjects involved in the study of nature and the natural world. This consolidation results in a law of energy conservation and convertibility which includes the notion of heat as *vis viva*, but which thereby raises its status from a general hypothesis to a more certain principle. In 1874, THOMSON could refer to this principle as the "all-pervading law of the conservation of energy," proved by JOULE, the essence of whose discovery "is the subjection of physical phenomena to dynamical law."[127]

For THOMSON in 1851, however, the main unresolved tension is the connection of FOURIER's laws of conduction with dynamics. Where conduction occurs, THOMSON believes that the work which might have been done as a result of a temperature difference, is ... "lost to man irrecoverably" and is not available to man even if it is not lost in the material world. Such transformations therefore remove from man's control sources of power ... "which if the opportunity of turning them to his own account had been made use of might have been rendered available."[128] Here the use of work or mechanical effect depends on man's creativity – on his efficient deployment of machines to transform concentrations of energy into mechanical effect, and is thus a problem of *arrangement* and not of creation *ex nihilo*. Unlike the issue of the indestructibility of energy, which depends on God, the problem with machines is in a sense subjective as far as man is concerned. Such was the interpretation given by LARMOR,[129] and holds fairly well if we understand the subjectivity to mean man's creativity as distinct from God's. However, although this deployment of machines to harness power is one important aspect of the problem, THOMSON realises that there is an "objective" side as it were. That is to say, he recognises a feature belonging to the created universe, that ... "everything in the material world is progressive," and that "The material world could not come back to any previous state without a violation of the laws which have been manifested to man; that is without a creative act or an act possessing similar power."[130] This property of the world is thus additional to the indestructibility of energy, and soon became familiar in the terms "irreversibility" and "dissipation," the diffusion of energy from sources of concentration, and its successful or unsuccessful use by man during

[126] Draft, page five.
[127] W. THOMSON, *op. cit.* (note 125), 325.
[128] Draft, page six.
[129] Sir JOSEPH LARMOR, *Proc. Roy. Soc., Series A*, **81**, xxix (1908).
[130] Draft, page six.

the course of that process. Thus in the draft, THOMSON states his belief in the tendency for motion to become diffused, and ... "that as a whole the reverse of concentration is gradually going on." He regards physical action to restore the heat emitted from the sun as impossible, and sees this source of energy as "not inexhaustible." In addition, he believes that the motions of the planets, including the earth, are losing *vis viva* which is converted into heat, and that although some *vis viva* may be restored by heat received from the sun, for example, "the loss cannot be *precisely* compensated & I think it probable that it is under-compensated." BRUSH, quoting briefly from the draft, refers these statements, especially that on the "progressive" feature, to a general geological context and to the probable influence on THOMSON of HOPKINS.[131] More specifically, however, THOMSON argues here against certain views of PRATT, whose *Treatise* of 1836 was very much in a mechanical and dynamical framework.[132] PRATT expresses the view that sources of mechanical effect such as volcanoes are found to compensate for these losses. THOMSON claims that ... "it ought first to be shown that the losses if uncompensated at all, could have produced any appreciable effect on the rotation or motion in general of the earth within the short period during which man has lived on it."[133] PRATT, although rejected here by THOMSON, has clearly been closely studied, for the language employed by both thinkers is very similar. After explaining the principle of *vis viva* as a general mechanical principle or theorem, and providing a proof by the principle of virtual velocities, PRATT discusses its relevance to cosmological phenomena, to the real world. Loss of *vis viva* in the earth's mass, he argues, may be caused by such processes as ... "The degradation of rocks and the consequent action of collision which is incessantly taking place in large portions of matter on the surface of the Earth, the unceasing action of waves on the sea shore and the collision of the waters of the ocean upon the solid nucleus of the Earth." If this loss were allowed to act, he writes, without compensating phenomena, it would ... "in the course of time produce a sensible effect in the length of the day." In addition, a sensible increase in the length of day would result from causes tending to remove large portions of matter nearer to the earth's centre, as for example, ... "the downward motion of rivers, the descent of vapour and cloud in the form of rain, the descent of boulders and avalanches."[134]

However, according to PRATT, compensations are effected by the explosions of volcanoes, the ascent of vapour by evaporation and the effect of earthquakes all removing matter to a greater distance from the earth's centre, and thus, he concludes: "On the whole all these causes balance each other, since observations have shewn that the length of the day has been invariable for many ages ...."[135] This conclusion THOMSON dismisses as nonsense, and he is in addition striking

[131] BRUSH, *op. cit.* (note 14), pp. 15–19. WILLIAM HOPKINS was THOMSON's mathematical coach at Cambridge.

[132] PRATT, *op. cit.* (note 63). PRATT was also one of the works consulted by THOMSON for his Glasgow University essay "On the Figure of the Earth" (1839–1840). See THOMPSON, *op. cit.* (note 1), **1**, 10n.

[133] Draft, pages eight and nine.

[134] PRATT, *op. cit.* (note 63), pp. 492–493. See also THOMSON, *op. cit.* (note 125), pp. 325–326 for very similar language employed in 1874.

[135] PRATT, *op. cit.* (note 63), p. 493.

a blow at followers of a HUTTONIAN cosmology in which the universe is seen as a self-regulating system through the balance of compensating causes. Much later in the *Treatise*, 1867, he noted in the margin that the dissipation of energy was disregarded by many followers of HUTTON.[136] But at this early stage, PRATT provides the explicit target for THOMSON's view that irreversibility is a feature of the created universe. Nevertheless PRATT's specific discussion of cosmology in relation to the *vis viva* principle provides a key link between the real world and the energy principle for THOMSON, and is more definite than a general geological context. Thus THOMSON does not reject PRATT's mechanical or dynamical framework as such, but rather the consequence of a self-regulating universe which PRATT took from it. THOMSON has now made the energy concept and its two fundamental laws of conservation and dissipation primary for the real, physical universe. In 1874, while reflecting on this work of 1851, THOMSON distinguished between abstract dynamics – applicable to reversible phenomena and of the kind which he employed in 1846 – and physical dynamics – based on the energy concept and applicable also to irreversible phenomena. Physical dynamics by 1851 became for THOMSON the fundamental subject of his natural philosophy, and the energy concept unified all the various branches without necessarily involving a *reduction* of these branches to mechanics.[137]

In the draft, before he began sketching the basic ideas of the Introduction as published in his paper of 1851, he had adopted CLAUSIUS's view of the most probable hypothesis that heat was merely a state of motion and that quantity of heat corresponds to *vis viva*. He expands this explanatory hypothesis by attributing the quantity of heat to the *vis viva* of molecular motions which exist within it, and by arguing that ... "the evolution of mechanical effect from thermal agency consists in the diminution of such motions by resistance." Furthermore, the conduction or propagation of heat "consists in the communication of *vis viva* from molecules in motion to contiguous molecules; and unless any portion of the *vis viva* be lost in producing changes in the dimensions or arrangement of bodies against resistance, or some be gained, as much mechanical effect as can be obtained by any means from the same quantity of heat with the same extreme temperatures" will be produced. He employs a proof analogous to the reasoning of CARNOT and JOULE, which shows that if a greater mechanical effect be obtainable from the same quantity of heat by any other means, at the end of a complete cycle of operations the axiom of conversion would have been contradicted, and work gained from nothing.[138] In this "proof" he employs the axiom of convertibility successfully without reference to the molecular hypothesis, which is, however, explanatory of conduction phenomena in qualitative terms. As can be seen from this portion of the draft, THOMSON rather confusedly moves from

---

[136] W. THOMSON & P. G. TAIT, *Treatise on Natural Philosophy* (Oxford: Clarendon Press, 1867), p. 711. (Margin note to reprint of THOMSON's paper 1862 "On the Secular Cooling of the Earth".) For the debates between THOMSON and the geologists from the 1860's, see J. D. BURCHFIELD, *Lord Kelvin and the Age of the Earth* (London: Macmillan, 1975).

[137] These ideas of THOMSON on the concepts of energy dissipation were published in his famous paper "On a Universal Tendency in Nature to the Dissipation of Mechanical Energy", *Proc. Roy. Soc. Edinburgh* **3**, 139 (1857) [read 1852]; *Phil. Mag.* [4] **4**, 304 (1852); *op. cit.* (note 41), **1**, 554. See BRUSH, *op. cit.* (note 14), p. 25 and p. 48.

[138] Draft, pages twelve and thirteen.

one approach to the other in his emphasis, even though he recognises that the axiom does follow from adoption of the hypothesis. The problem is that of emphasis – whether to stress the explanatory hypothesis concerning unobservable entities, or the axiom of convertibility.

In the published version of "the Dynamical Theory" THOMSON indicates that DAVY concluded that "heat consists of a motion excited amoung the corpuscles of bodies," which is the basis of the dynamical theory of heat. THOMSON then states that no one supported the view of heat as motion until the advent of MAYER and JOULE, when two themes are seen as crucial by THOMSON – the generation of heat through the friction of fluids in motion, and the magneto-electric excitation of galvanic currents, both of which were features of the early correspondence between JOULE and THOMSON. Either of these discoveries, THOMSON argues in his paper of 1851, would be sufficient to demonstrate the immateriality of heat. The work of JOULE and MAYER is seen as expressing the mutual convertibility of heat and mechanical effect ... "which follow[s] from the fact, that heat is not a substance but a state of motion."[139] In this statement by THOMSON we see a recurrence of the conceptual dichotomy between a hypothesis concerning the nature of heat, and an axiom simply expressing the convertibility of heat and work. The latter of course follows from the adoption of the former, but the former, relating as it does to unobservables, lacks the certainty of the latter. We saw the existence of this very tension in the thought of RANKINE, and we shall now see its continuation in THOMSON's thoughts of 1851, when he is first of all concerned to negate the view that heat is substantial.

The published version may have been concerned to put across the experimental arguments through the work – as THOMSON saw it – of DAVY, JOULE and MAYER. However, it appears from the continuation of the draft that in his own mind, THOMSON is still not entirely satisfied, perhaps for the reason that experiments, even if they showed the equivalence of heat and work, did not necessarily disprove that heat was conceptually a substance. Thus in the draft he resorts to a syllogistic form of argument: "Man cannot create matter, or matter cannot be created by operations under human control. Heat may be created by man, or heat may be created by operations under human control. Therefore heat is not matter."[140] Matter, being a fundamental entity of the universe, can be altered only by God: creativity by man can only be in the form of rearrangement and not creation *ex nihilo*. The issue is no doubt put in the syllogistic form to clarify the reasoning of THOMSON's own mind, and never as such received publication. However, it serves to demonstrate that for THOMSON heat cannot be itself an ultimate and basic constituent of the created world. The axiom of conservation of heat becomes all the more a matter of pragmatic consideration, a convenient rule for certain specific problems.

In the draft, THOMSON claims that although the convertibility of heat with mechanical effect is a necessary consequence of the dynamical theory (that heat is not a substance but a form of motion), this concept or feature of convertibility was not noticed by DAVY, and was contradicted by CARNOT in the statement

---

[139] W. THOMSON, *Trans. Roy. Soc. Edinburgh* **20**, 261 (1851); *op. cit.* (note 41), **1**, 175.
[140] Draft, note facing page nineteen.

of the axiom of the conservation of heat. Therefore, says THOMSON, "Mr. JOULE was the first to assert the mutual convertibility of heat and mechanical effect and so to complete the fundamental principles ... of the dynamical theory," an assertion which for THOMSON is backed up by the general confirmation of the theory afforded by JOULE's experiments.[141] THOMSON inserts here a footnote referring to JOULE's work on fluid friction, when heat is always generated:

> Mr. JOULE applied the following argument, which to me was perfectly convincing, to show that steam expanding & doing work must issue with less than the total heat it carries away from the boiler.[142]

Then, the argument goes, let the work be spent on fluid friction. If the steam does not issue with less the amount of heat exactly equivalent to the work done, "there would be ultimately no thermal agency & more or less than the total heat." This point is a fairly clear reference to JOULE's letter of October 1848, with the section on the steam engine, which argued that a rejection of JOULE's view would imply that the engine would be a manufacturer of heat.[143] In the draft of 1851, THOMSON admits that the above "demonstration" was communicated or at least suggested to him by JOULE ..." but has nowhere so far as I am aware, been published," and that he subsequently wrote the letter of October 1850, ... "pointing out the effect of friction in the orifices."

However, while THOMSON sees JOULE as asserting and supporting a framework of mutual convertibility he still does not himself believe that a satisfactory demonstration of the conversion of heat into work by experiment has been given. Nonetheless, THOMSON now ... "considers it certain that the fact has only to be tried to be established experimentally, having been convinced of the mutual convertibility of the agencies by Mr. JOULE's able arguments."[144] So THOMSON has in effect come to accept JOULE's conceptual framework before he has been convinced by actual experiments of the validity of the conversion of heat into work. While little of this discussion appears in the Introduction as published in 1851, THOMSON there sums up his position, having rejected heat as having a substantial nature, and holding heat to be instead "a dynamical form of mechanical effect" wherein ... "there must be an equivalence between mechanical work and heat, as between cause and effect."[145] Such a statement tends to gloss over the distinction between a hypothesis on the dynamical nature of heat at a molecular level, and the axiom of mutual convertibility; a distinction which is more strongly expressed in the draft version, a version which demands attention in order to gain a fuller appreciation of the conceptual problems and subtleties in THOMSON's thought.

5. The Establishment of Classical Thermodynamics

When WILLIAM THOMSON comes to formulate his aims for the paper of 1851, which are published as three in the Introduction, he is still rather uncertain of

---

[141] *Ibid.*, pages twenty and twenty one.
[142] *Ibid.*, footnote facing page twenty one.
[143] See above, section 2.
[144] Draft, page twenty one.
[145] W. THOMSON, *op. cit.* (note 41), **1**, 175.

whether to place his emphasis on the hypothesis or axiom. Thus his first aim, as published, is:

> To show what modifications of the conclusions arrived at by CARNOT ... regarding the motive power of heat, must be made when the *hypothesis* of the dynamical theory, contrary as it is to CARNOT's fundamental *hypothesis*, is adopted.[146]

The use of the term "hypothesis" is a clear reference to the nature of heat as either *vis viva* or a substance respectively, and to the not-entirely-certain character of these theories. What is interesting, however, is that in the draft of these aims, he replaces the word "axiom" by "hypothesis,"[147] a step which could either imply that he still regards the view of the nature of heat as speculative and therefore hypothetical, or that he wishes to indicate the nature of heat in the hypothesis of heat as motion, beyond the straight and certain axiom of the mutual convertibility of heat and mechanical effect. There is probably an element of truth in both these interpretations, and any lack of clarity would seem to lie in the conceptual difficulties which arise during the formulation of new fundamental principles and the construction of a new theory. At any rate, as we shall now see, THOMSON founded his view of the motive power of heat on the *proposition* or *axiom* of JOULE, which THOMSON called the First Proposition, clearly recognisable to us as the First law of Thermodynamics, no explicit mention being made of the actual nature of heat:

> When equal quantities of mechanical effect are produced by any means whatever from purely thermal sources, or lost in purely thermal effects, equal quantities of heat are put out of existence or are generated.[148]

The demonstration THOMSON provides of this proposition is similar to the discussions of CLAUSIUS, and to some of THOMSON's earlier remarks in the draft. Thus, he states, the dynamical theory of heat implies that the temperature of a substance can only be raised by working upon it in some way as to produce increased thermal motions within it and to modify the mutual distances or arrangements of its particles. The work required to produce this total mechanical effect is proportional to the quantity of substance, and therefore to the quantity of heat which the body emits or absorbs. The total mechanical effect, however, is equal to the sum of the work done by external forces, the change in half *vis viva* of the thermal motions and the work done by internal molecular forces, but if, as in a closed cycle,[149] the latter two terms reduce to zero, and the mechanical effect is equal to the mechanical equivalent of the heat emitted, then the heat emitted or absorbed is the thermal equivalent of the work done upon it by external forces – the proposition to be proved.

THOMSON also recognises here that this first proposition has a much wider dimension: that it is included in the general principle of mechanical effect,[150]

---

[146] *Ibid.*, **1**, 176. My italics.

[147] Draft, page fifteen.

[148] W. THOMSON, *op. cit.* (note 41), **1**, 178.

[149] THOMSON has already defined a closed cycle. *Ibid.*, p. 177.

[150] *Ibid.*, p. 178. See above, sections 2 and 4, for discussions of the wider dimensions of the energy principle.

by which he means the relation between *vis viva* and work, and which relates to conservation of *vis viva*, all these theorems having in the past belonged largely to abstract dynamics as secondary principles. But we have seen THOMSON's growing recognition of the primacy of these entities, exemplified by his choice of the term "energy" to cover them, especially as laid out in the early part of the draft of 1851. Thus, while in the first part of the "Dynamical Theory" his stated aims are modest, being confined mainly as we saw to a reformulation of CARNOT's theory and his own paper of 1849, this does not imply that he was unaware of the deeper aspects of the first proposition – indeed, that he *was* so aware, is clearly shown in the draft. The First Law of Thermodynamics is expressed in much the same terms as the axioms of CLAUSIUS and RANKINE, being limited to the relation of heat and work, but behind this law lies the whole range of deeper considerations which we have noted previously in connection with THOMSON's view of energy. When we turn to the second proposition, the interaction of THOMSON's thought with that of RANKINE and CLAUSIUS becomes still more central. THOMSON, in the published Introduction, remarks that:

> Important contributions to the dynamical theory of heat have recently been made by RANKINE and CLAUSIUS; who, by mathematical reasoning analogous to CARNOT's on the motive power of heat, but founded on an axiom contrary to his fundamental axiom, have arrived at some remarkable conclusions.[151]

It was in early 1850, we saw, that THOMSON had read and criticised RANKINE's paper "On the Mechanical Action of Heat," and he had been in almost constant correspondence with him ever since then. The two thinkers had also been referring to CLAUSIUS' paper of 1850 since the middle of that year. RANKINE, for his part, had criticised the second part of the CLAUSIUS memoir, particularly the axiom on which the modified CARNOT principle was founded, while THOMSON mentioned CLAUSIUS in the letter of October 1850 to JOULE, confessing that he was not *fully* acquainted with CLAUSIUS' paper.

In the draft, THOMSON states that the following proposition is due to CLAUSIUS:

> I make no claims to it as he published it first, but I discovered it independently. An engine which satisfies CARNOT's condition ... "that if as much mechanical effect as it derives from a given thermal agency be spent in working it backwards an equal reverse thermal agancy will be obtained."[152]

THOMSON's second proposition in Part I therefore emerges from this as:

> If an engine be such that, when it is worked backwards, the physical and mechanical agencies in every part of its motions are all reversed, it produces as much mechanical effect as can be produced by any thermo-dynamic engine, with the same temperatures of source and refrigerator, from a given quantity of heat.[153]

---

[151] *Ibid.*, p. 176.

[152] Draft, page fourteen.

[153] W. THOMSON, *op. cit.* (note 41), **1**, 176. The proposition was formulated by THOMSON in 1849 within the framework of CARNOT's theory. *Ibid.*, p. 119.

This proposition, he explains later, was first enunciated by CARNOT in the expression of his criterion for a perfect thermo-dynamic engine, but involving the assumption of the conservation of heat, which CARNOT himself doubted. CARNOT's demonstration is false, says THOMSON, but the proposition not necessarily so. Thus, he claims:

> The truth of the conclusion appeared to me, indeed, so probable, that I took it in connexion with JOULE's principle, on account of which CARNOT's demonstration of it fails, as the foundation of the motive power of heat in air-engines or steam-engines through finite ranges of temperature, and obtained about a year ago results, of which the substance is given in the second part of the present paper ....[154]

In other words, THOMSON is here claiming that Part II of the "Dynamical Theory" entitled "On the Motive Power of Heat through Finite Ranges of Temperature" was in fact thought out in essence *before* Part I, though it was not of course published. THOMSON therefore claims to have successfully reconciled CARNOT, applied to perfect reversible cycles, and JOULE independently of CLAUSIUS and RANKINE, but failed to achieve priority, mainly, I have argued elsewhere, because of the problem of dissipation as it appeared to him.[155] For CLAUSIUS, RANKINE, and indeed WATERSTON before them,[156] as well as for THOMSON himself, the reconciliation between a mechanical view of heat and CARNOT's theory *vis a vis* reversible cycles was achieved quite simply by abandoning the axiom of heat conservation attached to the original CARNOT principle, and replacing it by the axiom of the convertibility of heat and work. Thus in falling between two temperature levels in an ideal engine, part of the heat was converted into work, and the rest descended to the lower temperature and *vice versa*. The convertibility of heat and work became the first law of thermodynamics, and CARNOT's principle, when applied to reversible cycles, the second.

Reconciliation of CARNOT and JOULE in this manner was one aspect, but demonstration of the second principle quite another. THOMSON claims in Part I that:

> It was not until the commencement of the present year [1851] that I found the demonstration given above ....[157]

He states that he gives the demonstration exactly as it occurred to him before he knew that CLAUSIUS had either enunciated or demonstrated the second proposition. THOMSON's demonstration was founded on an axiom which appears to have emerged first in the early part of the draft of 1851:

> Is it possible to continually get work by abstracting heat from a body till all its heat is removed? Is it possible to get work by cooling a body below

---

[154] *Ibid.*, pp. 180–181.

[155] See my forthcoming paper in *The British Journal for the History of Science* (note 65).

[156] See J.J. WATERSTON, *Collected Scientific Papers*, ed. J.S. HALDANE (Edinburgh: Oliver and Boyd, 1928), pp. xl–xlii; 275–278.

[157] W. THOMSON, *op. cit.* (note 41), **1**, 181.

> the temperature of the medium in which it exists? I believe we may consider a negative answer as axiomatic. Then we deduce the propostion that μ [CARNOT's Coefficient] *is the same for all substances at a given temperature.*[158]

This remark becomes in the published Part I the famous statement that:

> It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects.[159]

Closely linked to this statement is the footnote to the effect that denial of such an axiom would entail that:

> ... a self-acting machine might be set to work and produce mechanical effect by cooling the sea or earth, with no limit but the total loss of heat from the earth and sea, or, in reality, from the whole material world.[160]

THOMSON also stresses that he has no wish to claim priority, for ... "the merit of first establishing the proposition upon correct principles is entirely due to CLAUSIUS."[161] Yet THOMSON adds that in early 1851 the demonstration occurred to him, and that he stated it as such before he knew of CLAUSIUS' demonstration. This claim is at first sight a curious one, because we know that he was aware of CLAUSIUS' paper in mid-1850. In order to make sense of THOMSON's claim as a true one, we must suppose that he had indeed not fully read CLAUSIUS, as far as the second part of CLAUSIUS' paper of 1850 was concerned. Even so, it is still puzzling that RANKINE's criticism of CLAUSIUS' "sort of a priori proof" of the second proposition did not bring a response from THOMSON. There may be, however, a considerable degree of truth in TAIT's later argument that in CLAUSIUS' paper of 1850, the axiom that heat cannot pass from a cold to a hot body (the phrase "of itself" being omitted) was not stated explicitly, and that it was THOMSON who formulated this fundamental axiom of CLAUSIUS in clearly-defined terms as:

> It is impossible for a self-acting machine, unaided by any external agency, to convey heat from one body to another at a higher temperature.[162]

In this version the phrase "of itself" is clearly implied, and was the formulation which THOMSON claimed was different in form but equivalent to his own fundamental axiom, in the sense that either is the consequence of the other. It is possible, in other words, that THOMSON did not see CLAUSIUS' original version as an axiom at all. Indeed, on a close reading of CLAUSIUS' paper, although it is easy in retrospect to view the statement as containing all the elements of later formulations, it might well have appeared to THOMSON as no more than an empirical generalisation which has little more than expedience or pragmatic value, and no fundamental certainty, even though it had pretensions of being "a sort of a

[158] Draft, page ten.
[159] W. THOMSON, *op. cit.* (note 41), **1**, 179.
[160] *Ibid.*, p. 179n.
[161] *Ibid.*, p. 181.
[162] *Ibid.*, p. 181. See P. G. TAIT, *Phil. Mag.* [4], **43**, 516 (1872).

priori proof." For CLAUSIUS in 1850 simply argues that the transference of as much heat as we please from a cold to a hot body ... "is not in accord with the other relations of heat, since it always shows a tendency to equalise temperature differences and therefore to pass from *hotter* to *colder* bodies."[163] The equivalence between this version and THOMSON's own only comes about after THOMSON has reformulated the former to his satisfaction.

THOMSON, having thus "established" two propositions, each of which is set in a wider context of his own thought, relating to more general laws, theology and the created universe, and having given "demonstrations" of these propositions, is now in a commanding position to draw together some of the detailed themes discussed during the years 1847–1851 in particular, to incorporate them into his new scheme of things, and to formulate the theory of the motive power of heat in mathematical language. Thus he states the aim of thermodynamics in Part I:

> A complete theory of the motive power of heat would consist of the application of the two propositions demonstrated above, to every possible method of producing mechanical effect from thermal agency.[164]

He adds a reference in a footnote to his "Account" of 1849 indicating that there are at present only two distinct ways in which this can be done – by alterations of volume which bodies experience through the action of heat or through the medium of electric agency.

Dealing with the cases of electric agency first, THOMSON observes that here as yet the second proposition, with its criterion of a perfect engine, has not been applied. However the application of the first proposition has been thoroughly investigated through the work of JOULE, especially in his paper of 1843. JOULE's achievement there, as THOMSON views it, is to express the heat generated as proportional to the whole work spent, in a process by which mechanical work through a magneto-electric machine produces galvanism, and ultimately heat, and to conclude that heat may be created by working such a machine. Provided all the current is used to produce heat, the total quantity of heat produced is exactly proportional to the quantity of work spent. THOMSON also integrates JOULE's views on the PELTIER effect, and quotes from JOULE's letter of July 1847, as well as from the paper of 1843.[165] These discussions of electrical phenomena formed the starting point for THOMSON's extensive work on thermo-electricity, and he read in May 1854 a paper, subsequently Part VI of the "Dynamical Theory," which dealt with that subject in relation to both fundamental propositions.[166]

Turning to the other method of producing mechanical effect from thermal agency – *via* expansive engines – THOMSON states that here both the fundamental propositions may be applied in a perfectly rigorous manner ...," an application

---

[163] CLAUSIUS, *op. cit.* (note 108), p. 134.

[164] W. THOMSON, *op. cit.* (note 41), **1**, 181.

[165] *Ibid.*, pp. 182–183. JOULE made reference to PELTIER in both these writings. See JOULE, *op. cit.* (note 46), **1**, 124 (1843) and letter from J. P. JOULE to W. THOMSON, 8th July, 1847, KELVIN Papers, University Library, Cambridge.

[166] *Ibid.*, pp. 232–255.

he admits already achieved with respect to the first proposition by RANKINE and CLAUSIUS, and by CLAUSIUS with respect to the second proposition by employing "CARNOT's unmodified investigation of the relation between the mechanical effect produced and the thermal circumstances from which it originates, in the case of an expansive engine working within an infinitely small range of temperatures."[167]

THOMSON then proceeds, quite directly, to derive an analytical expression for each proposition, which I state:

$$\frac{dp}{dt} = J\left(\frac{dM}{dt} - \frac{dN}{dv}\right) \tag{I}$$

and

$$\frac{dp}{dt} = \mu M \tag{II}$$

where μ denotes CARNOT's function interpreted by THOMSON as the ratio of the maximum amount of work obtainable from an engine to the quantity of heat transferred from a higher to a lower level of temperature. It is a quantity quite independent of the nature of the substance employed.

J is the mechanical equivalent of heat defined as the number of units of work which much be done to raise the temperature of a unit mass of water by one degree of temperature.

M, N are two independent variables [M is what used to be called the "latent heat of expansion;" N the specific heat at constant volume].

p, t, v are pressure, temperature and volume respectively.[168]

The first of these equations ... "expresses, in a perfectly comprehensive manner, the application of the first fundamental proposition to the thermal and mechanical circumstances of any substance whatever, under uniform pressure

---

[167] *Ibid.*, p. 183, 183n.

[168] *Ibid.*, pp. 185–188. To establish equation (I) THOMSON supposes that a mass of any substance, occupying a volume v, under a pressure p uniform in all directions, and at a temperature t, expands in volume to $v+dv$ and rises in temperature to $t+dt$. Then the quantity of work it produces is $p\,dv$, and the mechanical equivalent of the heat which must be added to make its temperature rise to $t+dt$ is given by

$$J(M\,dv + N\,dt).$$

The total external effect produced $=(p-JM)\,dv - JN\,dt$.

After a finite amount of expansion, the total external effect is the integral of this expression, which equals 0 for a closed cycle according to the First Proposition of the dynamical theory. Thus, THOMSON argues, the expression

$$(p-JM)\,dv - JN\,dt$$

must be the differential of a function of two independent variables, or, in THOMSON's notation,

$$\frac{d(p-JM)}{dt} = \frac{d(-JN)}{dv}$$

that is,

$$\frac{dp}{dt} = J\left(\frac{dM}{dt} - \frac{dN}{dv}\right). \tag{I}$$

Equation (II) is derived by considering a reversible cycle of operations for the substance. First it is allowed to expand from v to $v+dv$ at constant temperature t. Second, it expands further when in all directions, when subjected to any possible variations of temperature, volume and pressure."[169] The second equation, expressed as $\frac{dp}{dt}\Big/M=\mu$ gives values of μ the same for all substances at the same temperature, as claimed by CARNOT and CLAPEYRON, though not in mathematical terms by the former. Hence, says THOMSON:

> ... all CARNOT's conclusions and all conclusions derived by others from his theory, which depend merely ... [on this equation] ... require no modification when the dynamical theory is adopted.[170]

Furthermore, it follows that CARNOT's expression:

> ... for the mechanical effect derivable from a given quantity of heat by means of a perfect engine in which the range of temperatures is infinitely small, expresses truly the greatest effect which can possibly be obtained in the circumstances; although it is in reality only an infinitely small fraction of the whole mechanical equivalent of the heat supplied; the remainder being irrevocably lost to man and therefore "wasted," although not *annihilated*.[171]

The use of these statements is indicative of the way in which the diffuse strands are gradually being integrated into the new theoretical structure. If on the other hand, THOMSON argues, the quantities of mechanical effect obtained are finite, a finite quantity of heat must be converted, in falling through a finite range of temperature, to give results which ... "will differ most materially from those of CARNOT." The investigation of this aspect of the theory is contained in Part II of the "Dynamical Theory," which, as we have seen, THOMSON asserted was worked out prior to his commencement of the writing of the first part.[172]

We noted in section three the interaction between the thought of WILLIAM THOMSON and RANKINE in the period of publication of the early part of RANKINE's molecular vortex theories and prior to the Part I of THOMSON's "Dynamical

---

temperature falls to $t-\tau$, where τ is an infinitely small range of temperature. Third, it is compressed at temperature $t-\tau$, such that, fourth, it is returned to volume v and temperature t on further compression. By the Second Proposition of the dynamical theory, it must produce the same amount of work for the same quantity of heat absorbed in the first operation, as any other substance similarly operated upon through the same range of temperatures, the quantity of heat being $M\,dv$. But the whole work done in the complete cycle is given by

$$\frac{dp}{dt}\tau\cdot dv.$$

The ratio of this expression to $M\,dv$ is then the same for all substances with the same values of t and τ. Since τ is only a factor, THOMSON states

$$\frac{\frac{dp}{dt}}{M}=\mu \quad \text{or} \quad \frac{dp}{dt}=\mu M \tag{II}$$

where μ depends only on t.

[169] *Ibid.*, p. 187.

[170] *Ibid.*, p. 188.

[171] *Ibid.*, pp. 188–189.

[172] *Ibid.*, p. 189.

Theory," with discussions of CLAUSIUS also involved in the correspondence. We saw how, in the letter of March 1851 to THOMSON, RANKINE claimed to have regarded the principle of CLAUSIUS as having had "an appearance of probability," though he was not satisfied with the proof. He further remarks that, being in London, he regrets not being present at the reading of THOMSON's paper on the "Dynamical Theory" which refers to CLAUSIUS, but he thinks that without doubt the paper "will render the question more clear than it first appeared to me."[173] This letter, then, makes the implicit point that RANKINE does not immediately see his own work as wholly coincident with that of CLAUSIUS or indeed as laying down *two* fundamental principles.

Later in March 1851, RANKINE wrote again to THOMSON. He is once more concerned to understand and interpret the thought of both THOMSON and CLAUSIUS with precision:

> Although I have not had the advantage of hearing your late paper on the Theory of Heat read, yet I understand from the information you have been so good as to send me in your letters, that by adopting a principle analogous to that of CARNOT & to CLAUSIUS's second fundamental principle, you establish a definite relation between the total quantity of heat *expended* in a machine and the maximum quantity *convertible* into power; which two quantities bear to each other a ratio, being a function of the temperatures of expansion and condensation, & independent of the nature of the expanding and condensing substance.[174]

That these are correct interpretations made by RANKINE will be evident from my previous outline of CARNOT and the papers of WILLIAM THOMSON. RANKINE continues, that as a result of these discussions of CARNOT, THOMSON and CLAUSIUS, he had been induced to investigate the subject himself:

> ... and I have arrived at the conclusion, that a definite relation between those quantities may be deduced from the principles laid down in my paper of last year, without the introduction of any additional principle.[175]

This conclusion was repeated in a footnote in the *Philosophical Magazine* (July, 1851) of a letter originally sent to POGGENDORFF's *Annalen der Physik* concerning CLAUSIUS' paper of 1850.[176] RANKINE read in April 1851 the fifth section of his work "On the Mechanical Action of Heat" entitled "On the Economy of Heat in Expansive Machines," which sets out his development of thermodynamics in more detail, and which acknowledges the priority of CLAUSIUS and THOMSON in combining the CARNOT law with the law of convertibility.[177] It is at this stage

---

[173] Letter from RANKINE to W. THOMSON, 17th March, 1851, KELVIN Papers, University Library, Cambridge.

[174] Letter from RANKINE to W. THOMSON, 27th March, 1851, KELVIN Papers, University Library, Cambridge.

[175] *Ibid.*

[176] Letter from Mr. MACQUORN RANKINE to Dr. J.C. POGGENDORFF, 7th June, 1851, *Phil. Mag.* [4], **2**, 65n (1851).

[177] RANKINE, *op. cit.* (note 78), pp. 300–306.

that we see more clearly the contrasting approaches of these thinkers. For RANKINE, the first axiom, of mutual convertibility, was seen as a nearly self-evident and necessary principle which was also derivable from the principle of the conservation of *vis viva* as involved in the mechanical-molecular hypothesis, while CLAUSIUS' axiom was accorded no independent status at all by RANKINE being deducible from the foregoing principles of the hypothesis and *vis viva*.

On RANKINE, CLAUSIUS has some remarks in his historical review of thermodynamics "On an Axiom in the Mechanical Theory of Heat" of 1863. Regarding the papers of 1850, CLAUSIUS stakes what he sees to be his own claims to periority. RANKINE there ... "deduced conclusions concerning the deportment of bodies, particularly of gases and vapours, which agree in some measure with those at which I arrived in the *first part* of my memoir by means of the law of the equivalence of heat and work. The subject of the *second part* of my memoir, CARNOT's theorem, as modified by me, and its consequences, is, however, not contained in RANKINE's memoir." Only in 1851 was the second fundamental theorem dealt with by RANKINE in CLAUSIUS' opinion, and he adds:

> He [RANKINE] arrived thereby at the conclusion that this theorem ought not to be treated as an independent principle in the theory of heat; but that it might be deduced as a consequence of the equations established by him in the first section of his memoir.[178]

CLAUSIUS rejects what he sees as RANKINE's approach *via* molecular vortices as unsatisfactory, largely in view of the complications arising from it. Thus, for CLAUSIUS:

> ... in my memoirs I have taken especial care to base the development of the equations which enter into the mechanical theory of heat upon certain general axioms, and not upon particular views regarding the molecular constitution of bodies.[179]

However, although CLAUSIUS' interpretation of the historical position of RANKINE is, I believe, substantially accurate, RANKINE himself cannot be easily categorised as a scientist who employs purely hypothetical assumptions. Thus while his position in the history of thermodynamics is not that of a co-equal founder in a straightforward sense that he happened to formulate the two laws of thermodynamics at nearly the same time as, and independently of, THOMSON and CLAUSIUS, he is a complex and important figure in the network of interactions taking place in the early 1850's. His subsequent paper of 1855, "Outlines of the Science of Energetics," was clearly intended to restore the balance, so to speak, regarding hypotheses and axioms, with its explicit system of an "axiomatic philosophy."[180]

---

[178] R. CLAUSIUS, *The mechanical theory of heat ...*, trans. T. A. HIRST (London: John van Voorst, 1867), p, 273.

[179] *Ibid.*, pp. 273-274.

[180] RANKINE, *op. cit.* (note 78), p. 209; *Proc. Roy. Phil. Soc. Glasgow* **3** [read 2nd May, 1855]. See OLSON, *op. cit.* (note 105), pp. 271-286.

As I indicated in the Introduction, the central issue in this paper is the emergence of the theoretical structure and basic concepts of classical thermodynamics. I have therefore attempted to analyse the complexity of themes in terms of individual thinkers whose views are seen as being shaped by mutual interactions, and by interaction with certain key conceptual traditions inherited by the period from 1840. Thus in particular I have been concerned to discuss the way in which WILLIAM and JAMES THOMSON came to see the problems of heat and mechanical effect, problems which provided the essential background to the subsequent debates with JOULE and RANKINE and, less directly but no less importantly, with CLAUSIUS. What was emerging by the early 1850's, then, was classical thermodynamics as a science based on two axioms or laws, and independent of hypothetical or unobservable entities. The development of this science conceptually, mathematically and experimentally constituted one of the most important and far-reaching phases of nineteenth century physics.[181]

Appendix I

*Extract from Notebook of James Thomson (Brother of William): 1848**

Parenthesis

The following occurs to me at present.	May 20, 1848.
Count quantity of [heat *del.*] caloric, in a mass or set of masses, as being = [heat *del.*] caloric given out in descending to 0°.	Quantity of motion (momentum) in a mass or set of masses as being = motion (or momentum) given out in stopping so as to be at rest with reference to a thing regarded as stationary.
then temperature	corresponds to velocity.
Quantity of caloric is = or α [mass *del.*] capacity × temp[erature]	Quantity of motion = mass × vel[ocity]
Motive power (work) = or α capacity × temp^2.	Motive power (work) (vis viva) α mass × vel^2.
In motive power of heat we must take instead *mass* of different substances the *number of atoms* or *capacity for heat.*	

[181] For a discussion of CLAUSIUS' refinements from the 1850's, and his introduction of the term "entropy", see M.J. KLEIN, *Hist. Stud. Phys. Sci.* **1**, 127 (1969). For subsequent developments of the irreversibility concept, see BRUSH, *op. cit.* (note 14), 1–88.

* Notebook A 14(B) of JAMES THOMSON, THOMSON Papers, Queen's University Library, Belfast. Deletions in the original text are represented by the abbreviation *del.* following the deleted words.

a certain *capacity for heat* corresponds to a certain *mass*.

no. of atoms *one atom* corresponds to	*mass of atoms.* *mass of one atom.*
No quantity of Heat is lost during a fall of heat	just as no momentum or *quantity of motion* is lost in impact.
But Motive Power (or whatever it is to be called) is lost	just as *vis viva* is lost.
let it be called *vis calida* = see slip	*vis viva*

[SLIP] J.T. May 20, 1848.
During the passage of a given *quant*[ity] *of motion* from a vel[ocity] V to a vel[ocity] v, work is given out.
During the passage of a given *quant*[ity] *of heat* from a temp T to a temp t, work is given out.

Work, equiv[alen]t to force × space, equivt to $\frac{1}{2}$ *vis viva*, equivt to $\frac{1}{2}$ mass of body × vel^2.
Work, equiv[alent] to force × space, equivt to $\frac{1}{2}$ *vis calida*, equivt to $\frac{1}{2}$ capacity of body × temp^2.
capacity defined as α number of atoms.

By impact or mutual frictions: *vis viva* is lost, but quantity of motion is not lost.
By conduction or radiation: *vis calida* is lost, but quantity of heat is not lost.
$\frac{1}{2}$ *vis viva* is work locked up in *velocity*.
$\frac{1}{2}$ *vis calida* is work locked up in *temperature*.
[END OF SLIP.]

According to what is given on the last page the question at issue between JOULE and us may be stated thus. Is a certain *quantity of heat* (capacity × temp[erature]) equivalent to a certain *vis viva* the two being mutually convertible? On the last page it is shown that *vis calida* and *vis viva* are mutually convertible.
Therefore if JOULE r[igh]t we sh[oul]d have
a certain *quantity of heat* equiv[alent] to (and convertible) a certain *vis calida*.

but quantity of heat $= \dfrac{\textit{vis calida}}{\text{temp[erature]}}$

∴ we sh[oul]d have

a certain $\dfrac{\textit{vis calida}}{\text{temp[erature]}}$ equiv[alent] & convertible to *vis calida*.

wh[ich] appears to be nonsense (Is my reasoning good?).
DAVY's exp[eriment] of melting 2 pieces of ice by their mutual friction w[oul]d be incompatible with my view of the subject as it w[oul]d show that *vis viva* or work actually produced *quantity of heat*. (Is the exp[eriment] to be trusted?)

Appendix II

Text of William Thomson's Preliminary Draft for the "Dynamical Theory of Heat." *

[page one]

state axiom.

In CARNOT's Theory of the Motive Power of Heat Thermal Agency by which mechanical effect may be obtained is concluded to be the transference of heat from one body to another at a lower temperature. But this Theory is founded on an axiom which [is *del.*] was regarded by the author as doubtful; perhaps inconsistent with some experimental facts; and which was adopted, with a warning that it might have to be modified or abandoned [as a temporary exposition *del.*] only because, its truth never having previously been doubted, it had been almost universally considered as a fundamental principle in theory of H[eat].

The principal object of my present commun[icatio]n is to show what conclusions general &c. & num[erica]l given in my acc[oun]t of CARNOT's Th[eory] hold when the dynamical Th[eory] is adopted. But I may be allowed to preface it with a few remarks regarding my own opinions & what has influenced them since the date of the former commun[icatio]n.

[page two]

Feb. 27, 1851.

About two years ago I had the honour of communicating to the R[oyal] S[ociety] "an account of CARNOT's Theory of the Motive Power of Heat". At that time I felt very great doubt as to the fundamental axiom on which CARNOT's Theory depends; a doubt [which even if it had not been *del.*] suggested very forcibly very strikingly by CARNOT himself, and rendered to my mind still more pressing by the [powerful arguments adduced by Mr. JOULE unanswerable *del.*] objections raised by Mr. JOULE to some of CARNOT's conclusions. I did not then consider it impossible that a fuller experimental knowledge might lead to the removal of Mr. JOULE's objections nor was I fully prepared to admit his able arguments in favour of a contrary axiom; but, as the task before me was to communicate

[page three]

an account of CARNOT's Theory and some calculations I had made in connection with it, I carefully avoided committing myself by any decisive expression of my own opinion on the subject. If I may here be permitted to mention that the only part of JOULE's principles

I may be allowed to state here that I lay no claims to discovery in this theory; that my present object regarding the dynamical theory is the same as my former object regarding CARNOT's Theory but that I enter upon it more cordially from feeling convinced that the theory of which I now propose to communicate an acc[oun]t to the R[oyal] S[ociety] is true while I was in a state of very perplexing doubt when I comm[unicate]d CARNOT's.

---

* Manuscript PA 128, KELVIN Papers, University Library, Cambridge. Insertions in square brackets are my own except when followed by *del.* This abbreviation indicates non-trivial deletions in the original text.

[page four]
The hypothesis then which appears to me most probable regarding the nature of heat is that heat is merely a state of motion will be adopted [explicitly *del.*] throughout in this paper.
A number of bodies of different temperatures are mixed together [put into the same space *del.*] the vis viva of the motions of heat in the whole must remain constant. But the measurement of "quantity" in heat implies that in those circumstances the quantity of heat is unchanged.
∴ qu[antit]y of heat corresponds to vis viva.
Here it ought to be observed that if any of the bodies expand or contract so as to become of different (resilience) innere arbeit the vis viva in the system will be changed & ∴ the qu[antit]y of heat will be [different *del.*] changed.

[page five]
Mar. 1, 1851.
The difficulty which weighed principally with me in not accepting the theory so ably supported by Mr. JOULE was that the mechanical effect stated in CARNOT's Theory to be *absolutely lost* by conduction, is not accounted for in the dynamical theory otherwise than by asserting that *it is not lost*; and it is not known that it is available to mankind. The fact is, it may I believe be demonstrated the work is *lost to man* irrecoverably; but not lost in the material world. Although no [agency *del.*] destruction of energy can take place in the material world without an act of power possessed only by the supreme ruler yet [actions take place *del.*] transformations take place

[page six]
which remove irrecoverably from the control of man sources of power which if the opportunity of turning them to his own account had been made use of might have been rendered available.
Everything in the [physical *del.*] material world is progressive. The material world could not come back to any previous state without a violation of the laws which have been manifested to man, that is without a creative act or an act possessing similar power.
The problem of Natural Philosophy wh[ich] includes all physical science is this. Given at any instant the position & motion of each atom of matter. Required the position & motion of each at any time past or future. This is

[page seven]
a problem the conception of which is possible to man's intelligence; although of course the solution can never be effected. I believe those data are sufficient to imply the solution; & that this is the great distinction that between the ways of God in the physical & the moral world; that with distinct and exceptional cases wh[ich] we are justified in calling miracles* man can foresee the future with certainty in the material world; that he cannot & that in this world he never will be able to foresee even the simplest fact with certainty in the operations of mind.

* [foot-note facing pages seven and eight]
If a stone should stand in the air and we should be able to assure ourselves that there is no thread supporting it, no *sufficient* magnetic electrical or other action

bearing it, we should assert that a miracle was wrought before our eyes. But if a man should resist the strongest apparent motives to commit some action; if we could be certain that he was convinced that this action would if committed, give him immediate gratification, we should not say that a *miracle* was wrought upon him but we might recognise the influence of the spirit working the will of God in a way which man cannot investigate & reduce to "laws" like those of matter. I do not think *any* operations of a single mind could properly be called miraculous since there are no absolute laws manifested to man – by which each operations are determinate from any possible data. But there are absolute laws regarding the mutual action of different minds, or of one mind to future events or unknown past and present events, certainly regarding mutual consciousness, and I think a distinct definition of a miraculous deviation from such laws might be laid down. The vision of Peter & some of the circumstances connected with it are I think satisfactory illustrations of such miracles. Any vision, however intense, of a single mind, cannot be called miraculous. If the vision related to persons & those dead, we cannot know in this world whether those other beings were conscious. I think such visions are generally confined to the consciousness of the person seeing them. If the person by means of one gets knowledge which he could not have acquired by recognisable means, I think we should be right in calling the vision miraculous. Although all dreams are sent by God, yet they are not sent to give us any knowledge except of our own minds (and much knowledge of this kind they sometimes give). Any single case wh[ich] sh[oul]d give us other knowledge than this would be a miracle. The speculations or pretended prophesies or ravings of enthusiasts or mad men are not miraculous but the utterances of a true prophet are.
Mar. 1, 1851.
[Note with regard to the first line above:]
Better a dynamical than a statical instance such as I first thought on, of a stone moving by itself.

[page eight]
I believe the tendency in the material world is for motion to become [equalised *del.*] diffused, and that as a whole the reverse of concentration is gradually going on –
I believe that no physical action can ever restore the heat emitted from the sun, and that this source is not inexhaustible; also that the motions of the earth & other planets are losing vis viva wh[ich] is converted into heat; and that although some vis viva may be restored for instance to the earth, by heat received from the sun, or by other means, that the loss cannot be *precisely* compensated & I think it probable that it is under-compensated. What many writers, for instance Pratt, say that volcanoes & other sources of mechanical effect are found to compensate

[page nine]
the losses is (I believe) nonsense; since it ought first to be shown that the losses if uncompensated at all, could have produced any appreciable effect on the rotation or [other *del.*] motion in general of the earth within the short period during wh[ich] man has lived on it – "The earth shall wax old &c." The perma-

nence of the present forms & circumstances of the physical world is limited. Mechanical effect escapes not only from agencies immediately controlled by man, but from all parts of the material world, in the shape of heat, & escapes *irrecoverably*, though without *loss of vis viva.*
Mar, 4. 10.40 p.m.
CARNOT's Theory had a great charm in the *relation* which it established among certain physical characteristics of various substances. The dynamical theory has a far greater in *establishing* two relations, instead of assuming one

$$\frac{d\left[\left(\frac{dQ}{dv}\right)\right]}{dt}-\frac{d\left[\left(\frac{dQ}{dt}\right)\right]}{dv}=0.$$

& deducing another as CARNOT does.

[page ten]
CLAUSIUS is most unfortunate in p. 392 wh[ich] spoils all that follows by an assumption wh[ich] if he had tried to verify it by reference to REGNAULT's experiments (my Table of the values of μ deduced from them) he would have found to be very far from approximately true.
Is it possible to continually get work by abstracting heat from a body till all its heat is removed? Is it possible to get work by cooling a body below the temperature of the medium in which it exists? I believe we may consider a negative answer as axiomatic. Then we deduce the prop[ositio]n that *μ is the same for all substances at a given temperature.*

[page eleven]
Mar. 10, 1851.
The problem which I propose to solve is the following.
Given a perfect thermodynamic engine with the hot part at a constant temperature S & the cold part at a constant temperature T, it is required to find how much work can be obtained by means of it from a given quantity of heat introduced into it.
Let $M dv$ denote the accession of heat received by a mass of any kind, of indestructible texture, when its volume is increased by dv, its temperature remaining unaltered, and let $N dt$ be the quantity of heat which must be added to it to raise its temperature by dt, when its volume is kept constant.
[THOMSON here sets out the equations (see Section 5 of my paper):

$$\frac{\frac{dp}{dt}}{\frac{dM}{dt}-\frac{dN}{dv}}=J;\qquad \frac{\frac{dp}{dt}}{M}=\mu.]$$

[page twelve]
Mar. 11, 1851.
Having heard today that there is no meeting of the R[oyal] S[ociety of] E[dinburgh] on March 28, my paper must be commun[icate]d next Monday, so I must try & commence in earnest. The Theory which forms the subject of

the present communication is contradictory in its fundamental axiom to that of CARNOT.

According to the Dynamical Theory of Heat, the quantity of heat in a body is measured by the vis viva of the molecular motions which exist within it and the evolution of mechanical effect from thermal agency consists in the diminution of such motions by resistance. [The conduction of heat through matter from one part of a body to another *del.*] The propagation of heat consists in the communication of vis viva from molecules in motion to contiguous molecules; and unless any portion of the vis viva be lost in producing changes in the dimensions or arrangements of bodies against resistance, or some be gained

[page thirteen]

as much mechanical effect as can be obtained by any means from the same quantity of heat with the same extreme temperatures. For if possible let a greater mechanical effect be obtained by other means from the same quantity of heat supplied by the source. Then all the heat which is thus deposited in the cold body may be taken back to the hot body by a less expenditure of work than that which has been obtained; & thus at the end of the complete cycle of operations of the complex engine, the cold body will neither have gained or lost heat; while the hot body will have lost heat which is converted into work. But this is contrary to the axiom $\therefore$ &c. Let the temperature of the source be $t+\tau$, & that of the refrigerator t; the difference, τ, being infinitely small. Then, if $M\,dv+N\,dt$ be the quantity of heat which must be added to the medium so that its temperature

[page fourteen]

by the reverse the quantity of heat remains constant. Thus the ordinary method of estimating quantities of heat is consistent with the dynamical theory. A greater quantity of heat cannot require a greater quantity of ether or of any substance; for heat may be generated by the friction of fluids in motion. Hence the vis viva *must* be the measure of quantity. Thus the questions (1) and (2) proposed in [paragraph] 2 of my account of CARNOT's Theory are very simply answered in the Dynamical Theory. [Keep CARNOT's definition of a perfect thermodynamic engine ([paragraph] 13). *del.*] explain fully: A Thermal source? agency? is a hot body supplied with a given quantity of heat.

[The following important proposition which corresponds *del.*] The following prop[ositio]n is due to CLAUSIUS. I make no claims to it as he published it first, but I discovered it independently.

An engine which satisfies CARNOT's condition [paragraph] 13 "that if as much m.e. [mechanical effect] as it derives from a given therm[al] ag[ency] be spent in working it backwards an equal reverse thermal agency will be obtained."

[facing page fifteen]

The medium of an engine is the expanding & contracting substance. The source is the locality from which heat enters the engine. The refrigerator is the locality into which the waste heat is discharged & from it is carried off and diffused.

[propositions of CARNOT's Theory and other conclusions *del.*]

[Aims of the paper]

(1) To show what modifications of the conclusions arrived at by CARNOT and by others who have followed his peculiar method of reasoning regarding the motive power of heat must be made when the [axiom *del.*] hypothesis of the Dynamical Theory, contrary as it is to CARNOT's fundamental axiom, is adopted. (2) To point out the significance in the Dynamical Theory, of the numerical results calculated from REGNAULT's observations on steam which were communicated to the Society with an Account of CARNOT's Theory by the author of the present paper about two years ago ... [A repetition of these aims follows].

[Notes facing page sixteen]
Sir H[UMPHRY] D[AVY] Born 17 Dec. 1778.
"I began the pursuit of chemistry by speculations and theories ... more mature reflections convinced me of my errors."
Essay on Heat, Light, and the Combinations of Light with a Theory of Respiration early in 1799 in "Contributions to Physical & Medical Knowledge chiefly from the West of England collected by TH[OMAS]. BEDDOES MD. Collected Works 1839 Vol. II. *The* discovery undervalued by nearly every one.

[page seventeen]
Introduction.
Sir HUMPHRY DAVY, in his first published work, laid down the following proposition.
"The phenomena of repulsion are not dependent on a peculiar elastic fluid for their existence, or caloric does not exist."
[Whatever may be thought of the demonstration which he gave. Even if the form of demonstration by which he supported this proposition be not considered satisfactory, it must, the author of the present paper conceives, be admitted that the experiment *del.*] and in support of it he describes an experiment

[page eighteen]
in which two pieces of ice in circumstances so arranged as to prevent the possibility of any heat being received by either from external sources, were melted by being rubbed together. Whatever may be thought of the reasoning founded on this and some other experiments which he gave as a demonstration [practically it has not been convincing *del.*] (it has certainly not been generally *convincing* perhaps because not generally known) this one experiment must be admitted as sufficient in the present state of science to establish the proposition. In the train of reasoning which follows Sir H[UMPHRY] D[AVY] concludes that heat consists of a motion excited among

[page nineteen]
the corpuscles of bodies; and he says "To distinguish this motion from others, and to signify the cause of our sensations of heat" and of the expansion, or expansive pressure produced in matter by heat "the name *repulsive motion* has been adopted." The Dyn[amica]l T[heory] of H[eat] thus laid down by Sir H[UMPHRY] D[AVY] has not met with any efficient support [that has contributed to its advancement *del.*] until Mr. JOULE of Manchester commenced the valuable series of researches in which he has confirmed [and illustrated it by a great variety of most original experiments; most strikingly by his discovery of the

heat developed by the friction of fluids in motion *del.*] & determined the amount of mechanical effect in foot-lbs necessary to produce as much heat as will raise the temperature of a pound of water by one degree.

[Note facing page nineteen]
Syllogism.
Man cannot create matter.
Heat may be created by man
∴ Heat is not matter.
or Matter cannot be created by operations under human control.
Heat may be created by operations under human control.
∴ Heat is not matter.

[pages twenty and twenty one]
The [mutual *del.*] convertibility of heat into mechanical effect although it appears a necessary consequence of the dyn[amica]l the[ory] was not noticed by Sir H[UMPHRY] D[AVY], was contradicted by CARNOT in the statement of an axiom "which had never been doubted; to deny which would be to overturn the whole theory of heat of which it is a fundamental principle" accompanied by an admission that experimental facts may make [it necessary *del.*] this overthrow of generally received opinions necessary. Mr. JOULE was the first to assert the mutual convertibility of heat and mechanical effect and so to complete the fundamental principles foundation of the dynamical theory. But it is only by the general confirmation of the theory that heat is motion afforded by his experiments that this assertion is supported.*

[* foot-note facing pages twenty one and twenty two]
Especially by his discovery that heat is always generated by the friction of fluids in motion. Mr JOULE applied the following argument, which to me was perfectly convincing, to show that steam expanding & doing work must issue with less than the total heat it carries away from the boiler. Let the work it produces be spent on fluid friction. Then if it does not issue with precisely as much less heat as it drew from the boiler as the equivalent of work it has performed, there would ultimately be no thermal agency & more or less than the total heat. The demon[stratio]n was comm[unicate]d to me or at least suggested to me by Mr. JOULE but has nowhere so far as I am aware, been published. Of Reasoning on the same principles a very beautiful example is to be found in Mr. J[OULE]'s pub[lishe]d paper on the heat developed by the comp[ressio]n of air – I wrote a letter to J[OULE] which has been pub[lishe]d, pointing out the effect of fric[tio]n in the orifices. CLAUSIUS has objected – his obj[ectio]ns require no other answer than that were it not for fric[tio]n the steam would rush into the air containing in its motion part of the work & having comm[unicate]d to the air the rest of the work, due to its pr[essure]-h.p. to atm[ospheric] p[ressure]; & it would have lost the heat due to this work.

[page twenty one]
The author considers that as yet no experiment can be quoted which directly demonstrates the disappearance of heat when mechanical effect is evolved; but he considers it certain that the fact has only to be tried to be established ex-

perimentally, having been convinced of the mutual convertibility of the agencies by Mr. JOULE's able arguments.
The only other writers who have as yet published researches in the dynamical theory of heat [advocated distinctly the dynamical theory *del.*] are Mr. RANKINE & CLAUSIUS; whose nearly contemporaneous & certainly independent writings memoirs on the subject were published last year. Some of the most important conclusions

[page twenty two]
of these two authors are identical and the memoir of CLAUSIUS contains a most satisfactory & nearly complete working out of the theory of the motive power of heat by CARNOT's peculiar method of reasoning [which is the object of the 1st part of the present paper *del.*] but hypothesis is so mixed with sound theory that the general effect is lost. The most important new proposition of the dynamical theory contained in the present paper was given by CLAUSIUS – Without making any claim to priority, the author stated that all the conclusions at present communicated to the R[oyal] S[ociety] were obtained by him without any knowledge of CLAUSIUS' paper; some of them before and some of them after he became acquainted with that of Mr. R[ANKINE]. Both these authors work out the dyn[amica]l th[eory] on certain hypotheses equivalent to this; that the "innere arbeit" of air is not altered by compression & ∴ that the heat evolved is the equiv[alen]t of the work.

[page twenty three]
REGNAULT's experimental data make any *hypoth[esis]* of the kind unnecessary; and, as far as they can be depended upon even betw[een] the limits 0 & 100° Cent[igrade], they show that in reality that hypothesis is very appreciably at fault. In the present paper the practical part is worked out without any hypothesis.
Conclusions –
1st. If $M dv + N dt$ denote the quantity of heat which must be added to a mass of any substance (for instance to a mass of air, or of steam and water, in a closed vessel) to raise its temperature from t to $t+dt$, while its volume is increased from v to $v+dv$; M and N will be functions of v and t which satisfy the following equation:

$$\frac{dM}{dt} - \frac{dN}{dv} = \frac{\frac{dp}{dt}}{J}.$$

Indicated on p. 551 of my "Account".
where dp denotes the augmentation of pressure due to an elevation of temperature from t to $t+dt$, unaccomp[anie]d by any change of volume; and J denotes an absolute constant being the mechanical equivalent, discovered* by JOULE, of a thermal unit.

*[foot-note facing page twenty three]
I consider MAYER's idea of an equivalent to be arbitrary & really false. JOULE at one time shared that idea; at least so I thought, as I could not I thought get him to see that we cannot expect an equivalent in the heat evolved by the compression of air *left compressed*. But JOULE made out from magneto electricity & the friction of fluids in motion the equivalent on true principles.

[page twenty four]
Between (2) and (3). Take JAMES' reasoning regarding the freezing point, comparison of μ's &c. ....
Conclusion (3). A Quantity Q of heat enters a perfect thermodynamic engine at temp[eratu]re S; a remainder (waste) is rejected at temp[eratu]re T into the refrigerator. The quantity of work performed will be $J\left(1-\varepsilon^{-S\int_t^s}\frac{\mu\,dt}{J}\right)$ & the remainder of heat lost,

[End of Draft]

Acknowledgements. I wish to express my thanks to Dr. P. M. HEIMANN, Professor D. S. L. CARDWELL, and Professor M. J. KLEIN for their invaluable suggestions and criticisms of earlier drafts of this paper.

Unit for the History, Philosophy
and Social Relations of Science
University of Kent
Canterbury, Kent, England

(Received July 1, 1976)