A Philosophical Study of the Transition from the Caloric Theory of Heat to Thermodynamics:

Resisting the Pessimistic Meta-Induction

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1. Introduction

The caloric theory of heat has attracted much historical attention but most philosophers of science seem to agree, at least implicitly, that this theory has not much philosophical interest. The reason for this is probably the widespread view that the caloric theory of heat is false: a totally misguided and mistaken attempt to identify the cause and nature of heat and heat phenomena. 'Caloric' has been taken as a paradigmatic case of a non-referential scientific term. It is normally used as a vivid example of unfortunate positing of and theorising over unobservable entities.

Most interestingly, in recent literature in the philosophy of science, the case of the caloric theory has been used in order to undermine scientific realism. In particular, Laudan has argued that the now abandoned caloric theory, together with other abandoned theories such as the phlogiston theory of combustion and ether theories in the nineteenth century, can be used to support the premisses of a pessimistic inductive argument against scientific realism.[1] This argument can be stated as follows:

> The history of science is full of cases of empirically successful theories proved to be false. It is also full of central theoretical terms featuring in successful theories which did not refer. Therefore, by a simple (enumerative) induction on scientific theories,

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[1]L. Laudan, 'A Confutation of Convergent Realism', *Philosophy of Science* **48** (1981), 19–49, reprinted in J. Leplin (ed.), *Scientific Realism* (Berkeley: University of California Press, 1984a), see p. 231; *Science and Values* (Berkeley: University of California Press, 1984b); 'Explaining the Success of Science: Beyond Epistemic Realism and Relativism', in J. T. Cushing *et al.* (eds), *Science and Reality* (Notre Dame: University of Notre Dame Press, 1984c).

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our current successful theories are likely to be false and most or many of the theoretical terms featuring in them will turn out to be non-referential.[2]

If sound, this argument undercuts scientific realism and especially the distinctively realist claim that we have reasons to believe in the likely truth of current scientific theories. Moreover, this argument, if sound, must be compelling to any naturalistically minded philosopher of science. For, it shows that substantial empirical knowledge coming from an extensive investigation of the history of science, that is to say, knowledge which relates to which and what theories turned out to be false, can be philosophically significant. This means that substantial knowledge coming from the history of science can make us form, determine and revise our attitude to important philosophical issues, such as our intellectual attitude towards scientific theories.

While keeping the naturalistic perspective intact, that is, while endorsing the view that arguments coming from the history of science are philosophically significant, I shall try to oppose the premisses of the pessimistic argument against scientific realism. I must stress at the outset that the structure of the meta-inductive argument is as follows. The argument begins by presuming that current theories are true, and then it judges past theories with respect to current ones. Then, the claim is that many past theories turned out false, because, either the entities they postulated no longer feature among our current ontological commitments, or the laws, processes and mechanisms that they postulated no longer feature in our current theories. Hence, in order to undercut the argument, it would be enough to show that many of the laws, processes and mechanisms that past theories postulated have been retained in our current scientific image of the world, and/or that many of the terms featuring in past theories are co-referential with terms featuring in current scientific theories — of course under an appropriate understanding of reference.

My philosophical study of the transition from the caloric theory of heat to early thermodynamics is meant to establish that there is a clear-cut sense in which the caloric theory of heat was approximately true, and this despite the (notorious) lack of a theory of approximate truth. I shall show that the well-confirmed content of the theory was retained in thermodynamics. In suggesting that the caloric theory was approximately true I shall challenge one of Laudan's central assumptions, namely that we have no grounds for believing that a theory is approximately true unless we also believe that all of its central terms refer.[3] The thrust of my argument will be that the approximate truth of

[2]M. Hesse has cast the same argument in a principle, the 'Principle of No Privilege', which follows from an 'induction from the history of science'. According to this Principle: 'our own scientific theories are held to be as much subject to radical conceptual change as past theories are seen to be' (cf. M. Hesse, 'Truth and Growth of Knowledge', in Frederick Suppe and Peter D. Asquith (eds), *PSA 1976*, Vol. 2 (East Lansing: Philosophy of Science Association, 1976), pp. 261–280, see p. 271, p. 264).

[3]*Op. cit.*, note 1 (1984a), pp. 227, 230.

a theory is distinct from the full reference of all of its terms, especially when the truth of the laws established by a theory turns out to be independent of assumptions involving allegedly central theoretical entities. In particular, I shall try to show that the caloric representation of heat was not as central, unquestioned and entrenched as some philosophers have argued.[4]

The study will also question and refute another of Laudan's assumptions, namely that 'part of what separates the realist from the positivist is that the former's belief that evidence for a theory is evidence for *everything* that the theory asserts'.[5] Contrary to this position I shall try to show that evidence supports differentially the several parts of a theory, that scientists are aware of this fact, and that they differentiate accordingly their degrees of belief in the several parts of a theory. Therefore, the study is going to suggest that the realists' attitude towards theories and the ontological commitments they entail is not an all-or-nothing one. Rather, it is one of *differentiated belief* in accordance with the evidence supporting each part of a theory. Towards the end of the paper I shall embark on some general thoughts concerning the general strategy followed in this paper.[6]

However, at this early stage I must make some general points concerning my use of historical evidence. Not being a historian of science, I do not pretend to give a historically cogent and complete account of the periods in which the theories studied were advocated. My approach is certainly not 'contextual'; it fails to do justice to the development of these theories as organic units of broader cultures and communities. It may also fail to conform with modern historiographical standards. For instance, it may appear as if I appeal to some extra-contextual standards to judge the scientific theories under investigation and the relevant scientific writings. All in all, it may seem as though I suffer from (excessive) Whiggism, which is both unpopular and controversial.

I dare say that using some aspects of the history of science to drive a philosophical argument may be bound to lean to Whiggism. Let me elaborate. My concern is to study how past scientific theories hooked on to the world and what was scientists' intellectual attitude towards them. Then, I try to use the results of this study in order to derive some philosophical lessons relevant to the recent debates over scientific realism. My Whiggism comes in

[4]cf. Laudan, *op. cit.*, note 1 (1984b), p. 113.

[5]*Op. cit.*, note 2 (1984a), p. 226, emphasis in the original.

[6]For general discussions of the pessimistic induction one can see W. H. Newton-Smith, *The Rationality of Science* (London: Routledge and Kegan Paul, 1981); R. Boyd, 'Scientific Realism and Naturalistic Epistemology', in Peter D. Asquith and Ronald N. Giere (eds), *PSA 1980* (East Lansing: Philosophy of Science Association, 1981), Vol. 2, pp. 613–652; J. Worrall, 'Scientific Realism and Scientific Change', *Philosophical Quarterly* **32** (1982); and E. McMullin, 'Explanatory Success and the Truth of Theory', in N. Rescher (ed.), *Scientific Inquiry in Philosophical Perspective* (Lanham: University Press of America, 1987).

when I treat scientific theories 'out of context', as, more or less, 'frozen' entities, presented in papers and relevant documents, and examine them with our current lights. However, in this paper, I am interested in the logical relations between the several parts of a theory and between theories; in the structure of demonstrations; in the assumptions, principles and methods used in the latter; in the use of evidence to support and vindicate theoretical assumptions and beliefs about underlying mechanisms and causes. I am also interested in the attitude that scientists had towards their theories and the commitments they were willing to undertake, as evidenced in what they explicitly *stated* in their writings, as well as in the principles and hypotheses they *used* in their demonstrations. Ultimately, I am interested in examining whether and the extent to which, *from our point of view*, past scientists entertained true beliefs about the world; and to what extent they seemed aware, or convinced, that they had true beliefs. These are issues relevant to the philosophical discussion of the argument from the pessimistic meta-induction, as well as to the general discussion of the possibility and viability of a realist picture of the dynamics of science. Although the foregoing issues are philosophical, my subject-matter is past scientific theories, that is historical entities. Hence, in order to substantiate my argument, I am bound to appeal to historical knowledge, as exemplified in the relevant sources. This may create a tension, and, at any rate, Whiggism seems to solve it. Probably, a contextual account of the relevant histories could also be relevant to the philosophical debates. I am not qualified to judge this though, nor subsequently, to pursue it. Moreover, I do not deny that my use of historical evidence is not neutral — what is? — but rather seen in a realist perspective. Yet, historical accuracy, as exemplified in stating (and interpreting) what scientists said, argued and wrote about their theories, is not, necessarily, threatened by adopting a specific philosophical perspective. It is one thing to interpret what a scientist said about his theory; it is quite another thing to 'cook up' history so that it yields realism!

2. The Caloric Theory of Heat

2.1. *Heat as an Imponderable Fluid or Heat as Motion?*

The central concerns of the theories of heat in the late eighteenth and early nineteenth centuries were the following: the cause of the rise and fall of the temperature of bodies; the cause of the expansion of gases when heated; change of state; and the cause of the release of heat in several chemical interactions and especially in combustion. It was in this broad problem-situation that scientists such as Joseph Black, Antoine Lavoisier and Pierre-Simon Laplace introduced the causal-explanatory model of caloric.

Caloric was a theoretical entity and 'caloric' was a theoretical term referring to a material substance, an indestructible fluid of fine particles, which causes the rise of the temperature of a body by being absorbed by it.[7] We must mention here that caloric was distinguished from heat in the sense that the latter was the observable effect of the transportation of caloric from a hot body to a cold one. For instance, Lavoisier distinguished clearly between heat (*chaleur*) (or 'the sensation of warmth') and the cause of heat, 'or the exquisitely elastic fluid which produced it'.[8] An English scientist, A. Ure, summed up the description of caloric, in his *Dictionary of Chemistry* in 1820, thus:

> CALORIC. The agent to which the phenomena of heat and combustion are ascribed. This is hypothetically regarded as a fluid, of inappreciable tenuity, whose particles are endowed with indefinite ido-repulsive powers, and which, by their distribution in various proportions among the particles of ponderable matter, modify cohesive attraction, giving birth to the three general forms of gaseous, liquid, and solid.[9]

Being a material substance, caloric was thought to be conserved in all thermal processes. This assumption was a simple consequence of the principle of the indestructibility or conservation of matter. In 1780s, Lavoisier used caloric as an important element in his anti-phlogiston system of chemistry.[10] Moreover, the assumption that heat was conserved played an important role in the development and theoretical exploitation of experimental calorimetry. Lavoisier and Laplace in their *Mémoire sur la chaleur* laid down the well-known formula by means of which the quantities of heat involved in calorimetric experiments were calculated. That is,

$$Q = cM(T_f - T_i) \tag{1}$$

where c is a constant (i.e. the specific heat of a substance), M is the mass of the body, and T_f, T_i are the final and the initial temperatures respectively.[11] Another phenomenon that was dealt with was the change of state of a substance such as freezing and melting, boiling and vaporization. In dealing with the change of state of a substance (e.g. the vaporization of water), where although a large quantity of heat was needed for the change of state, this change takes place at constant temperature, Black assumed that heat can exist in a latent

[7]cf. A. Lavoisier, *Traité élémentaire de chimie* (Paris, 1789), translated into English by R. Kerr (1790) as *Elements of Chemistry*, reprinted by Dover (1965), pp. 1–2. For a detailed account of the causal role that caloric was called to play, one can see S. C. Brown, 'The Caloric Theory of Heat', *American Journal of Physics* **18** (1950), 367–373, see p. 370.

[8]*Op. cit.*, note 7, see p. 5.

[9]A. Ure, *Dictionary of Chemistry* (London, 1820), see p. 251.

[10]cf. *op. cit.*, note 7, Part I; also S. Lilley, 'Attitudes to the Nature of Heat about the Beginning of the Nineteenth Century', *Archives Internationales d'Histoire des Sciences* **27** (1948), 630–639, see pp. 632–633.

[11]cf. P. S. Laplace and A. Lavoisier, 'Mémoire sur la chaleur', *Oeuvres complètes de Laplace*, Vol. 10 (Paris: Gauthier-Villars, 1780), pp. 149–200, see p. 156.

form too.[12] Lavoisier suggested that caloric can exist in two forms: that is, either free (*calorique sensible*) or combined. Combined caloric was thought to be that which 'is fixed in bodies by affinity or electric attraction, so as to form part of the substance of the body, even part of its solidity'.[13] So, the existence of latent heat was explained by means of caloric being in combined form.

However, a dynamical conception of heat was the rival of the caloric theory ever since the latter was put forward. According to the proponents of the dynamical theory, the cause of heat was not a material fluid but, rather, the motion of the particles that constitute a substance. In this sense, heat was nothing over and above the motion of the constituents of a body. Lavoisier and Laplace gave the following account of the dynamical theory:

> [H]eat is nothing but the result of the insensible motions of the molecules of matter. . . . According to the hypothesis we examine [i.e. the dynamical theory] the heat is the *vis viva* (*force vive*) which is the result of the insensible motions of the molecules of bodies.[14]

The dynamical representation of the cause of heat was less developed than the caloric one. But it could also explain the transmission of heat and the restoration of equilibrium between unequally heated bodies put in contact.[15] In fact proponents of the caloric theory considered the dynamical theory as a serious but, given the current evidence, less probable competitor of the caloric theory.[16]

The main reason that this account attracted the attention of scientists was that it was able to explain the production of heat by friction. H. Davy listed a series of experiments which constituted, as he said, a *reductio ad absurdum* of the thesis that heat is a material substance, since matter cannot be produced or created by motion, for instance, by rubbing two things together.[17] Hence this empirical fact undermined the claim that heat is matter, which is never created or destroyed. Benjamin Thomson (Count Rumford) took up Davy's misgivings against the caloric theory of heat and performed several experiments in which heat is produced by friction. He also suggested that the cause of heat cannot be a material substance since heat can be produced by friction in an *inexhaustible* manner and no material substance can be inexhaustible. On the contrary, he

[12]J. Black, *Lectures on the Elements of Chemistry*, ed. J. Robinson (Edinburgh, 1803). All references to this work are from the extracts in D. Roller, 'The Early Development of the Concepts of Temperature and Heat: The Rise and the Decline of the Caloric Theory', in J. B. Conant (ed.), *Harvard Case Histories in Experimental Science* (Cambridge, Mass.: Harvard University Press, 1950), pp. 19–47, see pp. 29–42, p. 37.

[13]*Op. cit.*, note 7, p. 19.

[14]*Op. cit.*, note 11, see pp. 151–152, our translation.

[15]cf. Lavoisier and Laplace, *op. cit.*, note 11, p. 152, p. 154.

[16]cf. Black, *op. cit.*, note 12, see p. 44.

[17]H. Davy, 'An Essay on Heat, Light, and the Communication of Light' (1799), in *The Collected Works of H. Davy*, Vol. II (London: Smith, Elder, and Co., 1839; New York: Johnson Reprint Corporation, 1972), pp. 1–86, see pp. 9–23.

said, if heat was motion, as the advocates of the dynamical theory suggested, then its generation by friction was easily explained.[18]

It is noteworthy, though, that most caloricists did not take Rumford's challenge seriously because, after all, only a finite quantity of heat can ever be obtained before the bodies which are used for the production of heat by friction are rubbed away. Therefore, the claim was, the production of heat by friction cannot be inexhaustible.[19] In fact, very few scientists advocated seriously the mechanical representation of heat at the beginning of the nineteenth century.[20] The mechanical representation of heat was physically and mathematically undeveloped and it did not attract any significant attention until Clausius and Thomson showed that such a representation is compatible with the Carnot–Clapeyron mathematical formalism and the basic laws of the caloric theory.[21] Sometimes, the criticisms of the mechanical theory of heat were as hard as the following one, made by Ure, who claimed that it 'graduates perhaps into the poetry of science'.[22]

Yet, the caloric representation of heat was not unassailable either. Probably the most important difficulty that this theory faced was related to the problem of the weight of caloric. According to both the critics and the advocates of the theory, if caloric were material then it ought to have mass and weight. Up to 1785, all experiments performed had shown that a heated substance did not weigh more than when it was unheated. The absence of weight of caloric was an important problem for the caloric theory. For instance, reviewing several experiments, Black stated:

> It has not, therefore, been proved by any experiment that the weight of bodies is increased by their being heated, or by the presence of heat in them. This may be thought very inconsistent with the idea of the nature or cause of heat that I ... mentioned [i.e. that the cause of heat is a material fluid]. It must be confessed that the afore-mentioned fact may be stated as a strong objection against this supposition [i.e. that the cause of heat is a material fluid].[23]

Starting in 1787 and lasting until the late 1790s, Count Rumford performed a series of experiments in order to calculate 'the weight ascribed to heat'. Rumford examined whether liquids change in weight when they loose heat by

[18] B. Thomson (Count Rumford), 'An Inquiry Concerning the Source of the Heat which is Excited by Friction', *Philosophical Transactions of the Royal Society* **88** (1798), 80–102, reprinted in S. Brown, *Men of Physics: Benjamin Thomson—Count Rumford* (Oxford: Pergamon Press, 1967), pp. 52–73, see p. 70.

[19] cf. Brown, *op. cit.*, note 7.

[20] cf. Lilley, *op. cit.*, note 10; Brown, *op. cit.*, note 7; T. Kuhn, 'The Caloric Theory of Adiabatic Compression', **49** (1958), 132–140; E. Mendoza, 'A Sketch for a History of Early Thermodynamics', *Physics Today* **14** (1961), 32–42; R. Fox, *The Caloric Theory of Gases* (Oxford: Clarendon Press, 1971).

[21] cf. S. Brush (ed.), *Kinetic Theory, Vol. 1: The Nature of Gases and of Heat* (Oxford: Pergamon Press, 1965), see p. 14; Fox, *op. cit.*, note 20, p. 308.

[22] *Op. cit.*, note 9, p. 251.

[23] *Op. cit.*, note 12, p. 45.

just cooling down. The results obtained were negative. So he concluded that the caloric theory cannot explain away the absence of weight of caloric, unless it assumes that caloric 'is so infinitely rare, even in its most condensed state, as to baffle all our attempts to discover its gravity'. On the contrary, he argued, if one adopted the theory that 'heat is nothing more than the intestine vibratory motion of the constituent parts of heated bodies', then it would be clear that 'the weight of bodies can in no wise be affected by such a motion'.[24] In other words, Count Rumford suggested that whereas the caloric theory had to do an artificial manoeuvre in order to accommodate the absence of weight of caloric, the competing dynamical theory could accommodate this fact more naturally.

2.1.1. Belief in a Material Fluid? Does the superiority of the caloric representation of heat, at this early stage, suggest that scientists were committed to or believed in it? This is, in general, a difficult question to answer. What we shall argue for is that most of the eminent followers of the theory were very cautious as regards their *intellectual attitude* to the theory. Let us consider the following points:

1. Most of the eminent proponents of the caloric theory were aware of the difficulties that this theory faced.
2. They knew the advantages of the alternative representation of heat, especially in explaining the production of heat by friction.
3. They were also aware of the shaky experimental evidence, and the inaccuracy of most of the experimental results available.

These factors made most of the eminent scientists working within the caloric theory of heat very careful in their statements and very cautious in their theoretical commitments. Probably the most illustrative example of this behaviour comes from Black. In his lectures, Black presented both contemporary theories of heat. He moreover emphasised that 'our knowledge of heat is not brought to the state of perfection that might enable us to propose with confidence a theory of heat or to assign an immediate cause of it'.[25]

He stressed that 'the supposition' that heat is a material fluid appeared the 'most probable'. But he added that

> neither of these suppositions [i.e. the material and the mechanical] has been fully and accurately considered by their authors, or applied to explain *the whole facts and phenomena* related to heat. They have not, therefore, supplied us with a proper *theory* or *explication* of the nature of heat.[26]

[24]B. Thomson (Count Rumford), 'An Inquiry Concerning the Weight Ascribed to Heat', *Philosophical Transactions of the Royal Society* **89** (1799) 179–194, reprinted in Brown, *op. cit.*, note 18, pp. 89–101, see p. 100.

[25]*Op. cit.*, note 12, p. 42.

[26]*Ibid.*, p. 44, first emphasis added, the rest in the original.

In other words, Black was sceptical with regard to the caloric theory — in fact with both representations of heat available at his time, because neither of those could adequately explain all the then known phenomena of heat. He went on to say that most of the ways that caloricists followed in order to develop their theory in the light of recalcitrant experience were *ad hoc*. Black gave an excellent account of *ad hoc* modifications, in the following remark:

> Many have been the speculations and views of ingenious men about this union of bodies with heat. But, as they are all hypothetical, and as the hypothesis is of the most complicated nature, being in fact a hypothetical application of another hypothesis, I cannot hope for much useful information by attending to it. *A nice adaptation of conditions will make almost any hypothesis agree with the phenomena.* This will please the imagination, but does not advance our knowledge.[27]

This careful attitude towards the caloric representation of heat, which was tantamount to a suspension of judgement until better evidence comes in, was not just Black's idiosyncratic behaviour. Lavoisier and Laplace, after presenting both current theories of heat, suggested that the theory of experimental calorimetry was independent of the considerations concerning the nature of heat. Let us read through their nice statement:

> We will not decide at all between the two foregoing hypotheses [i.e. material vs mechanical theory of heat]. Several phenomena seem favourable to the second [i.e. the mechanical theory of heat], such as the heat produced by the friction of two solid bodies, for example; but there are others which are explained more simply by the other [i.e. material theory of heat] — perhaps they both hold at the same time. So, . . . one must admit their common principles: that is to say, in either of those, *the quantity of free heat remains always the same in simple mixtures of bodies*. . . . The conservation of the free heat, in simple mixtures of bodies, is, then, independent of those hypotheses about the nature of heat; this is generally admitted by the physicists, and we shall adopt it in the following researches.[28]

The foregoing remark is important since it suggests two things: on the one hand, the principle of conservation of heat was not considered as a consequence of heat's being a material substance, but rather as a theoretical generalisation stemming from the experiments in calorimetry; on the other hand, since calorimetric laws were independent of considerations about the nature of heat, they could not be used to test either of the theories about the nature of heat.

Lavoisier repeated his reservations about the caloric representation of heat in his monumental *Traité élémentaire de chimie*, in 1789. Although in this work he put forward the material theory of heat as a candidate for the cause of heat phenomena, he was careful to qualify his commitments:

> Strictly speaking, we are not obliged to suppose this to be a real substance [i.e. caloric]; it being sufficient, as will more clearly appear in the sequel of this work, that

[27] *Ibid.*, p. 46, emphasis added.
[28] *Op. cit.*, note 11, pp. 152–153, our translation.

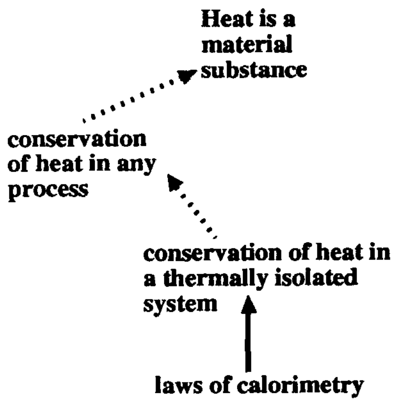
> it is considered as the repulsive cause, whatever that may be, which separates the particles of matter from each other.[29]

So, Lavoisier stressed that he could well be wrong about the real agent of the repulsive power of heat, and hence that he had to water down his commitment to caloric. We suggest then that the scientists of this period were not committed to the truth of the caloric representation of heat.[30] Therefore, caloric was not as central an ontological commitment as Laudan, for instance, has suggested.[31] Or, equivalently, the whole theory of heat did not revolve around the unquestioned belief that caloric was the cause of heat phenomena. In fact, most scientists' cautious attitude was the product of some important methodological considerations: first, the theory faced anomalies which it could not explain easily; second, an alternative theory was available, which could account for some of the anomalies that the caloric theory faced; third, the support of the caloric representation of heat came mainly from its explanatory role within the theory and not also from independent considerations; fourth, the modifications that the caloric theory was susceptible to were rather artificial and *ad hoc*; fifth, most of the work in experimental calorimetry was independent of any theory of heat and hence it did not urge full commitment to any of those theories.

Was the attitude towards the caloric theory an instrumentalist one? This does not follow from our study. Instead, using current philosophical terminology, we can claim the following: semantically, the community's attitude towards the theory was realistic. That is to say, 'caloric' was a putative referring term which stood for the cause of the rise of temperature of bodies when heated and, in particular, for a material fluid. Epistemically, their attitude was one of cautious belief, that is a belief which extends as far as the evidence supports the theory.

2.1.2. Laws of Experimental Calorimetry and the Materiality of Heat. As a way of highlighting some philosophically interesting points, let me give a short account of the evidential relations between the laws of experimental calorimetry and the explanatory hypothesis that heat is a material substance. Although the hypothesis that heat is a material substance explained the conservation of heat in calorimetric set-ups, the explanation offered was not good. For the fact that heat is conserved in calorimetric set-ups is only a consequence of the system's being thermally isolated. Hence, there was no need to have recourse to the materiality of heat in order to explain the success of experimental calorimetry. In other words, given that the system under consideration is thermally isolated, the laws of calorimetry were

[29] *Op. cit.*, note 7, p. 5.
[30] cf. also Lilley, *op. cit.*, note 10, p. 631.
[31] cf. note 4.



Schema 1.

independent of any assumption about the nature of heat, as Lavoisier and Laplace were apt to make clear.

Conversely, experimental calorimetry did not directly support the materiality assumption. For all that the experimental calorimetry really supported is that heat is conserved in a thermally isolated system, where no heat is lost to the surroundings and no work is produced. As it was realised by the advocates of the materiality of heat, most notably by Black, the hypothesis that heat is a material substance was in need of further independent evidence, if it were to be sustained. For instance, we saw Black suggesting that the hypothesis that heat is a material substance was modified in an *ad hoc* way in order to accommodate recalcitrant experience, such as the null weight of heat. These considerations resulted, as we said, in not taking the materiality assumption as beyond reasonable doubt. Generally, we may represent the evidential links between the hypothesis of the materiality of heat and the laws of calorimetry as in Schema 1. (Dotted lines present a way in which the discovered laws might have been taken to support the materiality assumption. The solid lines display what they really supported.)

2.2. The Velocity of Sound and the Law of Adiabatic Change

In 1816 Laplace published a memoir in which he suggested that the transmission of sound takes place in an adiabatic way, thereby predicting the correct speed of sound. This was an amazing success, for he corrected Newton's mistake in the calculation of the speed of sound in air. Unlike Newton, who had assumed that the expansions and contractions of a gas as sound passes through it take place isothermally, Laplace suggested that the propagation of sound is an *adiabatic process.* He assumed that there is some quantity of latent heat which is released from the compression of the air. This quantity of heat is normally diffused in the gas. But, for Laplace, 'since this diffusion takes place very slowly relative to the velocity of the vibrations, we may suppose without sensible error that during the period of a single vibration the quantity of heat remains the same between two neighbouring molecules'.[32]

He then approximated the process of sound propagation by the following two steps: (a) an isothermal compression of the gas, and (b) heating of the gas at constant volume.

Laplace suggested that Newton's mistake was that he had not calculated the effect of the second process on the pressure (or elasticity) of the gas. For Laplace, 'it is clear that the second cause [i.e. the second process] should increase the velocity of sound since it increases the elasticity of the air'.[33]

Laplace showed that the speed of sound is represented by the formula

$$v^2=(c_p/c_v)\,\mathrm{d}P/\mathrm{d}\rho \qquad (2)$$

where c_p is the specific heat of air under constant pressure, c_v is the specific heat under constant volume, P is the pressure and ρ the density of air.[34] The result obtained was 345.18 m/s.[35] Laplace attributed the difference from the experimental value to 'the uncertainty in experimental measurements'.[36] In fact, he was right, since he took $\gamma(=c_p/c_v)=1.5$ based on the quite off-the-mark calculations by Delaroche and Berard.[37] This successful and novel prediction lent more credence to the caloric theory.[38]

The interesting feature of Laplace's explanation of the propagation of sound and the correct prediction of its speed in air is that it did not explicitly rest on any particular representation of heat, although Laplace was an advocate of the caloric theory. It is also noteworthy that Laplace's explanation of the propagation of sound in terms of an adiabatic process was correct and was retained in the subsequent theoretical accounts of heat.

In 1823, Poisson established by theoretical means the law that governs adiabatic processes, that is,

$$PV^{\gamma}=\text{constant} \qquad (3)$$

[32]P. S. Laplace, 'Sur la Vitesse du Son dans l'air et dans l'eau', *Annales de Chimie et de Physique* 3 (1816), translated by R. B. Lindsay, in Lindsay R. B. (ed.), *Acoustics: Historical and Philosophical Development* (Stroudsburg, Pa: Dowden, Hutchinson and Ross, 1972), see p. 181.

[33]*Ibid.*

[34]As he put it: 'The real speed of sound equals the product of the speed according to the Newtonian formula [i.e. $c=\sqrt{\mathrm{d}P/\mathrm{d}\rho}$] by the square root of the ratio of the specific heat of the air subject to the constant pressure of the atmosphere at various temperatures, to its specific heat when its volume remains constant' (cf. *op. cit.*, note 32, p. 181).

[35]The experimentally measured velocity of sound had been 337.18 m/s whereas the calculations according to Newton's formula yielded about 288 m/s.

[36]*Op. cit.*, note 32, p. 182.

[37]Later on, in 1822, Laplace calculated again the velocity of sound but this time based on the much better specification of γ by Gay-Lussac and Welter (cf. Laplace 'Sur la Vitesse du Son' (1825), in *Ouevres Complètes de Laplace*, Vol. 13 (Paris, 1904), pp. 303–304. The result was 337.7 m/s.

[38]cf. Mendoza, *op. cit.*, note 20, p. 34; Kuhn, *op. cit.*, note 20, p. 139.

where γ is the ratio of the two specific heats of a gas under a certain temperature.[39] Here again, the interesting thing is that Poisson showed that this law is *independent* of any specific hypothesis about the nature of heat (caloric). His argument rested only on the hypothesis that the quantity of heat absorbed or released by a body was a *state function* of three macroscopic properties of the body, namely pressure, temperature and volume.[40] Or, as he put it in a previous paper, 'the volume, pressure and temperature being again . . . the same as they were before the expansion of the mass of air, the quantity of heat communicated to it is necessarily equal to what it has lost'.[41]

The empirical law, which was known as the *equation of state* of a gas, provided a correlation between the three properties. That is,

$$p = a\rho(1 + \alpha T) \tag{4}$$

where α and a are constants, ρ the density of a gas and T the temperature. Therefore, heat was taken as a function of any two of the three macroscopic properties of a gas, i.e. $Q = f(P, V)$. Poisson analysed the foregoing function in terms of partial derivatives of P and $\rho(=m/V)$ with respect to the temperature, and then derived the law of adiabatic change by integration.[42]

I must stress that Poisson's assumption that the quantity of heat involved in a process is a state function of two macroscopic parameters can be taken as the fundamental hypothesis of the advanced caloric theory. In particular, if such a function of heat did exist, it would imply that, in a complete cycle from (V_1, T_1) back to (V_1, T_1), the quantity of heat absorbed was equal to the quantity of heat released, irrespective of the way that the changes took place; that is, it would imply that heat is a conservative quantity. However, the hypothesis that heat is state function does *not* imply anything about the supposed *material* nature of heat: a quantity may be conservative without being material. (As we shall see in Section 4, this is what Clausius showed *vis-à-vis* the internal energy of a substance.) It is then in this sense that Poisson's hypothesis was independent of the assumption that heat is a material substance.

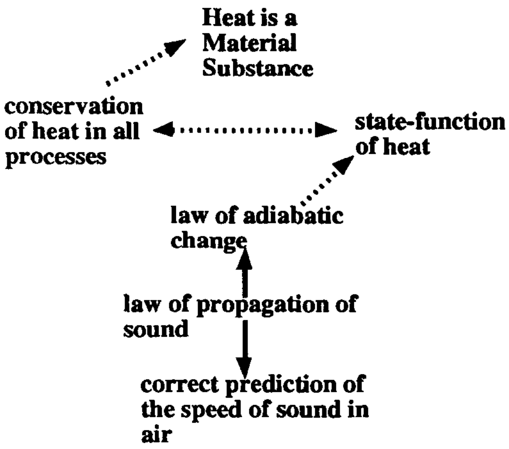
After Clausius's work in thermodynamics, it was established that heat is *not* a state function of the macroscopic properties of a gas. On the contrary, the quantity of heat released or absorbed by a body depends on the way that the process happens. In order to see that, we have to take into account the work produced in a thermal cycle. Then, the quantity of heat involved in a process does not uniquely depend on the initial and final states in which the substance

[39] S. D. Poisson, 'Sur la Chaleur des Gaz et des Vapeurs', *Annales des Chimie et de Physique* **23** (1823), translated by J. Herapath, *Philosophical Magazine* **62** (1823), 328–338, see pp. 328–329.

[40] cf. Fox, *op. cit.*, note 20, p. 177, and C. Truesdell, *The Tragicomical History of Thermodynamics 1822–1854* (New York: Springer, 1980), see p. 41.

[41] Poisson, 'Sur la Vitesse du Son', *Annales de Chimie et de Physique* **23** (1823), 5–16, quoted by Truesdell, *op. cit.*, note 40, p. 37.

[42] cf. Poisson, *op. cit.*, note 39, pp. 329–330.



Schema 2.

undergoing the changes is found. That is, heat is not conserved in all thermal processes.

However, Poisson's derivation of the theoretical law of adiabatic change, despite the fact that it rested on a wrong hypothesis that heat is a function of state, is approximately correct. The reason for this is the following. Although heat is *not* a function of the state of a gas, we can *approximate* infinitesimal changes of the quantity of heat of a gas, such as those occurring in an adiabatic process, by the method employed by Poisson, that is by analysing an infinitesimal change of heat in terms of the partial derivatives of two macroscopic parameters.[43]

2.2.1. The Law of Adiabatic Processes and the Materiality of Heat. Let me give a short account of the evidential relations between the law of adiabatic change and the explanatory hypothesis that heat is a material substance. As I said, in the advanced caloric theory the hypothesis that heat is a material substance was concretised by the hypothesis that heat can be mathematically represented as a state-function. However, Laplace's account of the propagation of sound did not, explicitly, depend on the hypothesis that heat is a material substance. Moreover, the theoretical derivation of the law of adiabatic change was approximately correct despite the fact that it rested on the mathematical representation of heat as a state-function. Thus, we may depict the evidential links between the hypothesis that heat is material and the law of adiabatic change as in Schema 2. (Dotted lines present a way in which the discovered laws might have been taken to support the materiality assumption. The solid lines display what they really supported.)

[43]cf. E. Fermi, *Thermodynamics* (New York: Dover Publications, 1936), p. 20 and pp. 25–26.

2.3. Carnot and Caloric[44]

Carnot devoted his *Reflections on the Motive Power of Fire* to the theoretical study of the work that can be produced by a gas undergoing specific changes so that it returns to its initial state (i.e. traverses a complete — and reversible — thermal cycle). In particular, Carnot's problem situation was the theoretical study of whether steam-engines (and other thermal machines) can improve their work capacity indefinitely.[45]

In his theoretical account of the motive power of heat, it seems as though Carnot had accepted the principle of the conservation of heat and the existence of a state-function of heat. For instance, he wrote (although in a footnote of his text) that:

> [t]his fact [i.e. the conservation of heat] has never been called in question. It was first admitted without reflection, and verified afterwards in many cases by experiment with the calorimeter. To deny it would be to overthrow the whole theory of heat to which it serves as a basis. [p. 19/p. 76][46]

As it is suggested by the foregoing quote, Carnot, like many of his contemporary scientists, tended to derive the principle of conservation of heat in any process from its observed conservation in calorimetric processes. We saw earlier that the latter conservation was correctly established, and, in fact, it followed from the very set-up of calorimetric experiments. But the unrestricted generalisation that heat is conserved in *any* process was not warranted nor supported by the experimental findings in calorimetry. It is more natural to suggest that Carnot's seeming conviction that heat is a conservative quantity sprung from the admission that heat is represented by a state-function.

However, Carnot seemed aware of the difficulties faced by the unrestricted generalisation to the effect that heat is conserved in any process whatever. He questioned, even in his published paper, the soundness of the supposed central axiom of the caloric theory. He remarked:

> The fundamental law [i.e. that heat is a state-function] which we proposed to confirm seems to us however to require new verifications in order to be placed beyond doubt. It is based on the theory of heat as it is understood today, and it should be said that

[44]In my study of the development of caloric theory I left aside Laplace's account of caloric and its interaction with matter, as it was presented in his monumental *Traité de Mécanique Céleste, Livre XII* (1823) and in a series of articles in the *Connaissance des Temps* for 1824 and 1825. However, this does not affect the argument of the paper. I deal with Laplace's account of the advanced caloric theory in my paper 'Laplace and the Caloric Theory of Heat: A Case of *Ad Hoc* Modifications', presented in the 19th International Congress of History of Science.

[45]cf. P. Lervig, 'On the Structure of Carnot's Theory of Heat', *Archive for the History of Exact Sciences* **9** (1972/73) 222–239, see p. 224.

[46]Henceforth, the references to Carnot's paper will be given in the text by two numbers referring to the relevant pages in both of the English translations of Carnot's memoir, i.e. S. Carnot 'Reflections on the Motive Power of Fire' (1824), in E. Mendoza (ed.), *Reflections on the Motive Power of Fire by Sadi Carnot and other Papers on the Second Law of Thermodynamics by É. Clapeyron and R. Clausius* (New York: Dover Publications, 1960), and R. Fox (ed.), *Reflections on the Motive Power of Fire: A Critical Edition with the Surviving Manuscripts* (Manchester: Manchester University Press, 1986).

> this foundation does not appear to be of unquestionable solidity. New experiments alone can decide the question. Meanwhile, we can apply the theoretical ideas expressed above, *regarding them as exact*, to the examination of different methods proposed up to now for the realisation of the motive power of heat. [p. 46/pp. 100–101, emphasis added][47]

I shall now concentrate on Carnot's work on the motive power of heat. Carnot stated that the work produced in a steam-engine is due to the *redistribution of caloric* among the parts of the engine.[48] In other words, he thought that the steam produced in the boiler of an engine was used to transport caloric to the condenser, thereby producing mechanical work, without any quantity of heat being consumed in this process. It is cogent that the hypothesis that heat is a material substance entailed the foregoing thesis: if caloric was a substance then it had to be indestructible; then it could produce work in a heat engine without being consumed, but by its mere redistribution. In fact, Carnot used the analogy of a waterfall in order to state that heat is not consumed in producing work. He likened the motive power of heat with the motive power of a waterfall, where no water (i.e. material substance) is lost (cf. p. 15/p. 72).[49]

However, I must stress that Carnot did not commit himself to the hypothesis of conservation of heat. In order to support this claim let us study the demonstration of the theorems relating to the well-known Carnot cycle. Carnot considered two bodies A and B kept at different, but constant, temperatures, T_1 and T_2 respectively. In particular, $T_1 > T_2$ (cf. Fig. 1, adapted from Carnot's original paper). The working substance was a gas contained in a tank *abcd*, whose one side *cd* is movable with a piston. Carnot's process consisted of four steps (cf. pp. 17–19/pp. 74–76):

(1) The gas was brought in contact with the body A, at the constant temperature T_1, and was slowly left to expand, at a constant temperature T_1, to the position *ef* (i.e. isothermal expansion from V_1 to V_2).

(2) The body A, then, was removed from the gas, and the latter was left to expand from the position *ef* to the position *gh*, where its temperature became equal to that of the body B, i.e. T_2 (i.e. adiabatic expansion from T_1 to T_2).

(3) Then, the gas was brought in contact with the body B, at a constant temperature T_2, and compressed from *gh* to *cd*, at a constant temperature T_2 (i.e. isothermal compression from V_2 to V_1).

[47]It is noteworthy that the foregoing quotation replaced the following statement, favourable to the fundamental hypothesis, which appears in the draft of Carnot's memoir: 'The fundamental law which we proposed to confirm seems to us to have been placed beyond doubt. . . . We will now apply the theoretical ideas expressed above to the examination of different methods proposed up to now for the realisation of the motive power of heat' (cf. Carnot in Mendoza, *op. cit.*, note 46, p. 46). For other doubts concerning the 'fundamental hypothesis', see Carnot, *op. cit.*, note 46, p. 19/p. 76.

[48]cf. Carnot, *op. cit.*, note 46, p. 7/p. 65.

[49]For comments on Carnot's simile of the waterfall, see R. Fox 'Les *Réflexions sur la Puissance Motrice du Feu* de Sadi Carnot et la Leçon le Leur Édition Critique', *La Vie des Sciences, Comptes Rendus* **5** (1988), 283–301, see pp. 290–292.

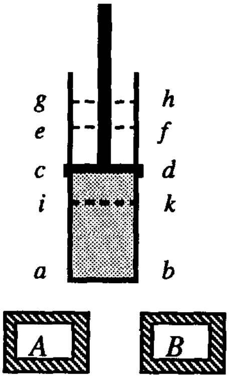


Fig. 1. Carnot's cycle.

(4) The body B was removed, and the gas was compressed from *cd* to *ik*, its final temperature being again T_1. Then, the gas is brought in its initial state *abcd* by contact with the body A (i.e. adiabatic compression from T_2 to T_1).

The process could be repeated indefinitely, by repeating the four steps in the same order: (1), (2), (3), (4). . . .

Using his cycle, Carnot was able to demonstrate the following theorems:

(1) The *maximum quantity of work* can be produced when and only when a substance undergoes transformations in a Carnot cycle (cf. p. 19/p. 76).

The demonstration of this theorem is most interesting since Carnot appealed to independent, well-established background knowledge.

Suppose, Carnot said, that more work W' is produced in cycle C' than the amount of work W produced in a Carnot cycle C. If this were so, he remarked, it would be possible to create perpetual motion by starting with cycle C', then directing the excess motive power $W' - W$ from the condenser to the boiler, and finally applying the operations of the Carnot cycle C. But 'this would be not only perpetual motion, but an unlimited creation of motive power without consumption of either caloric or of any agent whatever. Such a creation is entirely contrary to ideas now accepted, to the laws of mechanics and of sound physics' (p. 12/p. 69).

So, Carnot established that $W' - W$ must be negative in order to avoid perpetual motion. Hence, W is the maximum work that can be produced in a reversible cycle.[50] Incidentally, we can see at this point how realists are justified in saying that mature scientific theories operate in a background of sound theoretical knowledge. Carnot's theoretical calculation of the maximum work produced in a thermal cycle could only be established as reflecting the

[50]cf. P. Lervig, 'What is Heat? C. Truesdell's Views on Thermodynamics: A Critical Discussion', *Centaurus* **26** (1982), 85–122, see p. 88.

sound physical principle of the impossibility of perpetual motion of the first kind.

(2) The work produced in a cycle is independent of the substance used and, for a given quantity of heat, depends only on the difference in temperature of the bodies between which the cycle works.[51]

Carnot realised, correctly, that the crucial factor in the whole process of generating mechanical work was the difference in temperature between the boiler and the condenser of a steam engine. Hence, he correctly suggested that the work produced in a cycle is independent of the working substance involved. Carnot also suggested that the work produced in a cycle is *a function of the quantity of heat* transferred from body A to body B, during the process. That is, the work produced in a complete cycle C was

$$W(C)=g(Q^{tr}, T_f - T_i). \tag{5}$$

The demonstration of the second theorem appears tied up to the incorrect hypothesis that heat is conserved in a Carnot cycle. For, despite his doubts concerning the conservation of heat, we saw that Carnot assumed that the work produced in his cycle was due to the redistribution of caloric between bodies A and B. This can be taken to mean that the quantity of heat Q_A released from body A during step (1) is equal to the quantity of heat Q_B absorbed by body B during step (3) and, therefore equal to the quantity of heat transferred from A to B; that is

$$Q_A = Q_B = Q^{tr}. \tag{6}$$

This is a conservation statement, and it is arguable that Carnot was committed to this.[52] Yet, Carnot was again very cautious. In presenting his cycle he did not say explicitly that the quantity of heat released by body A is *absorbed* by body B. In the crucial step (4) of his cycle, Carnot only mentioned that 'the compression is continued till the air acquires the temperature of the body A' (p. 18/p. 75). This by no means entails that $Q_A = Q_B = Q^{tr}$. Hence, we may not fail to notice that Carnot did not explicitly appeal to any assumptions about the conservation of heat in order to establish his law.[53]

[51]In Carnot's own wording: 'The motive power of heat is independent of the agents employed to realise it; its quantity is fixed solely by the temperatures of the bodies between which is effected, finally, the transfer of the caloric' (*op. cit.*, note 46, p. 20/pp. 76–77).

[52]cf. P. Lervig, *op. cit.*, notes 45 and 50.

[53]For a similar point see M. Klein, 'Closing the Carnot Cycle', in *Sadi Carnot et L'Essor de la Thermodynamique* (Paris: Edition du Centre National de la Recherche Scientifique, 1976), pp. 213–219, see pp. 216–217, p. 219. A careful examination of the *Reflections* reveals that in at least two points, where variants of the Carnot cycle were presented, Carnot used something like $dQ=0$, which would commit him to the conservation of heat (cf. *op. cit.*, note 46, pp. 35–37/pp. 85–86 and 92–93; also T. Kuhn, 'Carnot's Version of "Carnot's Cycle" ', *American Journal of Physics* 3 (1955), 91–95, see p. 93).

In order to see this point more clearly, we must jump slightly ahead and see Clapeyron's account of Carnot's cycle. Clapeyron was the first to put Carnot's theory into analytic form. In particular, he drew the now famous diagrammatic representation of Carnot's cycle. But during the crucial step (4), where the gas, after being compressed isothermally in contact with the refrigerator body *B*, is allowed to compress adiabatically, he stated that 'the compression continued till the heat released by the compression of the gas and absorbed by the body *B* is exactly equal to the heat communicated by the source *A* to the gas, during its expansion in contact with it in the first part of the operation'.[54] This is a clear conservation statement. In other words, Clapeyron demanded that $Q_A = Q_B = Q''$ — that is, he demanded that heat is conserved in a Carnot cycle.

Given the analysis of Carnot's theorems and their demonstrations, we may suggest that Carnot was not committed to the hypothesis that heat is conserved in his cycle. As we have already shown, he did not use this hypothesis in the proofs of the first theorem and he did not explicitly adopt such a hypothesis in proving his second theorem.

Carnot had also already stated the second law of thermodynamics rather clearly in his published article. He stated that '[t]he production of motive power is then due . . . [to the] *transportation [of caloric] from a warm body to a cold body*'. And then, '[i]t follows from this principle that, in order to create motive power, it is not enough simply to produce heat. Cold is also essential; without it, the heat is useless' (p. 7/p. 65). This statement suggests that one cannot produce motive power by just cooling a body, or a set of bodies, below their temperature. Two bodies, or a system of bodies, are needed, kept constantly at different temperatures, if work is going to be produced. This fundamental insight was later on elevated to the status of Second Law of Thermodynamics by Thomson and Clausius, both recognising that Carnot had grasped this fundamental law in his *Reflections*.[55]

In his posthumously published notes, Carnot suggested that the caloric theory could not account for this fundamental principle and, therefore, he had to abandon the theory. He stressed that within the caloric theory, it 'would be difficult to say why, in order to develop motive power by heat, a cold body is required; why motion cannot be produced by consuming the heat in a heated body'.[56] In other words, Carnot suggested that the hypothesis of the conservation of heat broke down when it was called forth to explain the production

[54]E. Clapeyron, 'Memoir on the Motive Power of Heat', in Mendoza, *op. cit.*, note 46, see pp. 76–77.

[55]W. Thomson (Lord Kelvin) 'On the Dynamical Theory of Heat', *Transactions of the Royal Society of Edinburgh* (1851), reprinted in his *Mathematical and Physical Papers*, Vol. 1 (Cambridge: Cambridge University Press, 1882), see p. 179, and references to Clausius appearing there.

[56]cf. S. Carnot 'Notes on Mathematics, Physics and Other Subjects', in Fox (ed.), *op. cit.*, note 46, see p. 187.

of work by heat.[57] He also stressed that the caloric theory of heat was undermined by a series of experimental results, mostly related to the production of heat by friction.[58]

The discovery that the sound law of the impossibility of perpetual motion (of the second kind) runs counter to the caloric theory was, probably, decisive in turning the balance against the caloric theory in Carnot's mind. From his posthumously published notes, one can also see that some time between 1824 and his early death in 1832, Carnot countenanced a mechanical theory of heat.[59]

2.3.1. Carnot's Theory and the Materiality of Heat. Here again, let me give a summary of the evidential relations between Carnot's theory and the explanatory hypothesis that heat is a material substance. As I explained in detail, Carnot did not explicitly *use* the principle of conservation of heat in the formulation and derivation of his theorems despite the fact that such a principle lurks in the *Reflections*. Carnot actually came to disbelieve such a hypothesis in virtue of the fact that it was at odds with the impossibility of perpetual motion (of the second kind). As I shall show later, Clausius and Helmholtz consolidated the claim that Carnot's theorems can be demonstrated by appealing only to the impossibility of perpetual motion of the first kind. So Carnot's theorems did not support the assumption that heat is material. Thus, we may state the evidential links between the hypothesis that heat is material and Carnot's laws as in Schema 3. (Dotted lines present a way in which the discovered laws might have been taken to support the materiality assumption. The solid lines display what they really supported.)

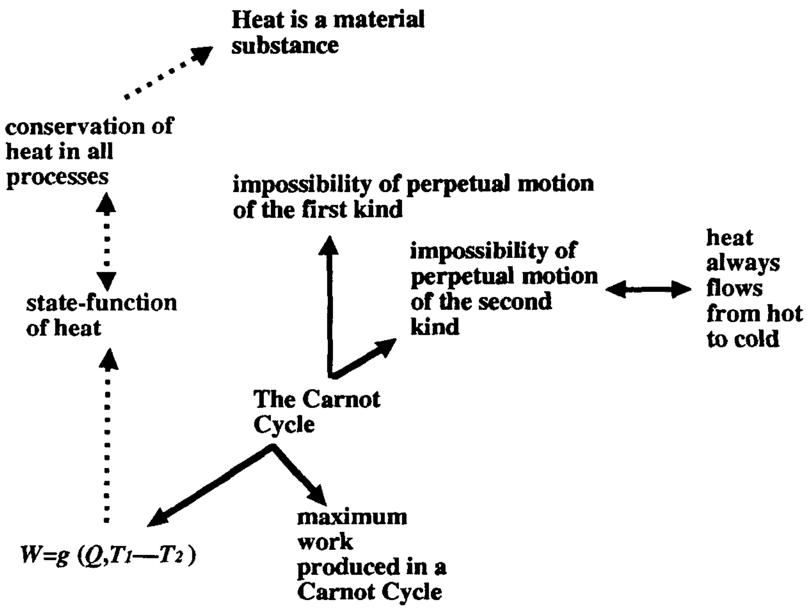
3. Localising Relations of Evidential Support

The foregoing study of the stages of the development of the caloric theory of heat suggests that the evidence to be explained may not support strongly an

[57]Already in his published paper, Carnot, right after he utilised the principle of the impossibility of perpetual motion, stated that perpetual motion would amount to 'an unlimited creation of motive power without *consumption* either of caloric or of any other agent whatever (*op. cit.*, note 46, p. 12/p. 69, emphasis added). It is difficult to establish that this statement was anything more than a slip. But, it can be seen as implying that the sound law of the impossibility of perpetual motion yields that heat must be consumed during a thermal cycle in which work is produced. So it can be seen as suggesting that the impossibility of perpetual motion (of the first kind) is at odds with the principle of conservation of heat. In fact, this line of thought was used by Clausius in his demonstration of Carnot's theorems.

[58]*Op. cit.*, note 56, pp. 185–186.

[59]*Op. cit.*, note 56, p. 191. Investigating Carnot's manuscripts, E. Mendoza has suggested that 'it seems that many of the notes were written at virtually the same time as the *Réflexions*. . . . In fact, by the time he came to correct the proofs (or to write the very final draft if there was one) he had begun to lose confidence in all that he had written. The surprising thing is that he published his book at all' (E. Mendoza, 'Contributions to the Study of Carnot', *Archives Internationales d'Histoire des Sciences* **12** (1959), 377–396, see p. 389).



Schema 3.

explanation at hand, that is, it may not warrant a belief that this explanation is likely. In particular, it suggests that an explanation must not be believed if the evidence does not warrant, at least to a high degree, that had the explanatory hypothesis been different, the phenomena would be inexplicable. Stated in an anachronistic way, it seems to me that the belief that underwrote scientists' attitude to the caloric theory of heat, in the light of the well-founded laws of the experimental calorimetry, the law of adiabatic change and Carnot's theory, was this: the probability of these laws given the truth of the hypothesis that heat is a material substance was not high, and moreover it was not overwhelmingly greater than the probability of these laws given the falsity of the hypothesis that heat is a material substance.

The foregoing account of the development of the caloric theory of heat has rested on the premiss that it is both in principle and in practice possible to *localise* the relations of evidential support, and show which parts of a theory are supported by the evidence at hand, or at any rate, which parts are better supported than others. In other words, we suggested that it is possible to locate which theoretical beliefs are likely to be true given the evidence, and, in a comparative spirit, which beliefs are more likely to be true than others.[60] It

[60]This localism is, I think, an instance of the localism put forward by Glymour in his *Theory and Evidence* (Princeton: Princeton University Press, 1980). I take it that the spirit of Glymour's bootstrapping account of confirmation is that empirical evidence may support some theoretical claims made by a theory better than others, that is that the evidence reaches the several parts of a theory in a non-uniform way (p. 110). However, I shall leave showing how Glymour's bootstrapping machinery could be applied in the case of the caloric theory for another occasion.

follows from this consideration that a realistically minded scientist differentiates her degrees of belief in the several portions of a theory in the light of the supporting evidence. In particular, we showed that most of the eminent theorists of the caloric theory had precisely this differentiated attitude towards the several claims of the theory, and a very cautious attitude towards the hypotheses that did not enjoy strong evidential support.

Contrary to this view, Laudan has argued that a realist must be holist in matters confirmational, for otherwise a realist cannot claim that the deep-structural claims of a theory are well-supported.[61] Laudan also seems to think that the realist must be committed to the view that observational evidence for a theory is evidence for *everything* that a theory asserts.[62] However, Laudan's contention about the alleged realist commitments seems to rest on the following misleading account of the support enjoyed by the theoretical claims of a theory: empirical evidence cannot support straightaway some theoretical claims; instead empirical evidence supports a theory as a whole and therefore it supports its theoretical claims, again as a whole.

Laudan's view of the link between theoretical claims and empirical evidence rests partly on a bad reading of Boyd's important claim that the support which empirical evidence lends to a theory goes all the way up to the deep-structural claims of the theory.[63] Correct though it is, Boyd's position was meant to block the positivist contention that the empirical evidence supports only the parts of the theory which deal with observable phenomena. In this connection Boyd was right to say that evidence for the empirical adequacy of a theory is evidence for the truth of a theory as a whole, and in particular for the truth of its theoretical claims.

However, Boyd's position does not commit a realist to a holistic confirmation of a theory. All it says is that the confirmation of a theory confers support not merely on its observational consequences, but also on its theoretical claims. Yet, there is no reason to think that empirical evidence cannot lend different credence to the several theoretical claims made by the theory. Nor is there any reason to think that all parts of a theory are equally supported by the evidence that confirms the theory. Empirical evidence may well go all the way up to the theoretical claims of a theory, and yet support some of its theoretical claims better than others, or remain silent about some other theoretical claims. As our study showed, in actual scientific theories, there are some deep-structural claims which are warranted by the evidence at hand and some others which are unwarranted or less supported by it.

Let me highlight some ways in which the evidence supports some theoretical claims of a theory only weakly.

[61] *Op. cit.*, note 1 (1984a), pp. 226–227.
[62] *Op. cit.*, note 1 (1984a), p. 226.
[63] cf. Boyd, *op. cit.*, note 6.

- Some piece of evidence may be in conflict with some particular theoretical claims.
- In the light of recalcitrant experience, some theoretical claims are modified in an *ad hoc* way in order to square up with the new unfavourable evidence.[64]
- Some theoretical claims are independent of the evidence, in that the evidence does not make them more likely than alternative and incompatible claims.
- Some theoretical claims are 'neutral' with respect to sound background beliefs, in that the latter do not increase their probability of being true.

Generally, not all deep-structural claims of a theory play the same role in the derivation of predictions and in providing well-founded explanations of observable phenomena. Some theoretical claims may be used centrally in the derivations of predictions and explanations of the phenomena, some others may be 'idle'; some theoretical claims may be mere visualisations of underlying causes, unable to generate testable predictions, or at any rate, unable to specify circumstances under which they can be thoroughly tested. Thus, one would expect that not all theoretical claims made by a theory are equally compelling.

There is no *a priori* warrant that all theoretical beliefs are supported to an equal degree by the evidence. On the contrary, given that experience shows us that deep-structural beliefs may be supported in different degrees by evidence, ranging from highly likely to rather unlikely, it is a good empirical constraint to any confirmation theory to localise the praise and the blame for the successes and the failures of a theory and differentiate our degrees of belief in the theoretical claims made by a theory accordingly. Hence, it is entirely consistent to stress that empirical evidence sends its support all the way up to theoretical claims, but it does not do so indiscriminately and without differentiation.

Generally, the realist answer to Laudan's challenge about holistic confirmation would be this: if we enjoy some theoretical beliefs, it is because empirical evidence, together with other sound background beliefs, shows that these beliefs are likely. This answer leaves space for a localist theory of confirmation. For empirical evidence surely goes all the way up to the deep-structural claims endorsed by a theory, in the sense that it does not stop at the observational consequences of the theory. But on its way up, it confirms differentially the several theoretical claims made by the theory. The realist need not commit herself to unwarranted theoretical claims; yet she has good reasons to commit herself to theoretical claims, insofar as the latter are well-supported by evidence and other sound background beliefs.[65]

[64]For the notion of ad hocness that I appeal to, one can see J. Worrall, 'Scientific Discovery and Theory Confirmation', in J. C. Pitt (ed.), *Change and Progress in Modern Science* (Dordrecht: Reidel, 1985), especially p. 302, pp. 311–314.

[65]A realist can also suggest that other epistemic virtues lend support to theoretical claims. For instance, as M. Friedman has argued, the ability of a theoretical structure to unite hitherto unrelated phenomena provides extra confirmation and further reasons to believe that such an underlying structure exists (cf. Friedman, *Foundations of Space–Time Theories* (Princeton: Princeton University Press, 1985), see pp. 242–243).

I conclude that a realist need not be a holist in matter confirmational. She need not be committed to the position that *all* deep-structural assumptions that scientific theories put forward are equally likely to be true, or equally likely to be false. She need not commit herself to the position that evidence confirms one's theoretical beliefs as a whole to an equal degree. Evidence can be such that shows which theoretical claims are likely to be true, and which we must discard or suspend our judgement about. The scientific realist need not commit herself to believing a theory as a whole packet. Instead, realism requires and suggests a *differentiated attitude*, and a *differentiated degree of belief* in the several parts of a successful and mature scientific theory. The degree of belief in a theory is, in general, a function of its support by the evidence at hand. Since different parts of a theory are supported in a different degree, the realist should place her bets for the truth of a theory accordingly.

The history of science shows vividly that not all posited causal mechanisms which purport to display the nature of a natural kind, like heat, are well-supported by the evidence at hand. In the light of this fact as well as the fact that providing causal explanations in terms of fully-articulated natural kinds is a *sine qua non* of theory-making, does the foregoing careful attitude towards theoretical commitments mean that extrapolations of causal mechanisms have, or ought to have, a small degree of belief? I think that it does not. For a start, the history of science does not suggest that no theoretical claims concerning deep-structural mechanisms can be supported to a high degree by evidence. Moreover, on purely probabilistic grounds, it follows that one's theoretical claims have a probability less than or equal to the probability of the observational consequences they entail. But this latter fact does not entail that the probability of one's theoretical claims must be, or necessarily is, small. The probability of a theoretical claim depends on the evidence and the sound background beliefs that support it. The latter may be such that they render the probability of a deep-structural theoretical claim high. In other words, background beliefs and evidence may be such that they determine a theoretical belief as very likely.[66] Nothing can prevent us from entertaining high degrees of belief in deep-structural theoretical commitments provided that we seek for, and find, evidence which can sustain these commitments; and that we commit ourselves to them only insofar as this evidence obtains.

Careful historical studies can reveal more about the nature of deep-structural, causal, explanations as well as about plausible and reliable methods for advancing and assessing them. For instance, what our case study seems to

[66]For an excellent study of the determination of theories by evidence, applied to the case of quantum discontinuity, one can see J. Norton 'The Determination of Theory by Evidence: The Case of Quantum Discontinuity 1900–1915', *Synthese*, **97** (1993), 1–31.

reveal is, on the one hand, that *ad hoc* explanations are not reliable and, on the other hand, that if these explanations are used only as visualisations and representations of underlying causes, they are not well-founded. In other words, a good methodological advice to scientists would be not to commit themselves to *ad hoc* and untested assumptions whose only role is to visualise the causes of certain phenomena. Moreover, careful historical studies can reveal which theoretical assumptions and beliefs were *used* in the derivation of the laws and in theoretical generalisations. So, they can reveal which parts of past theories were really central and indispensable, in the sense that they were used in the workings of a theory, and which played a subsidiary role, that is, did not exemplify real commitments. In brief, let us just emphasise that belief can be the right intellectual attitude towards the scientific theories, but belief can be partitioned in degrees; and hence belief in a theory, and the theoretical beliefs it entails, can be a matter of degree.

4. From Caloric Theory to Thermodynamics: Clausius and Carnot

I have already argued that none of the three stages in the development of the caloric theory of heat really supported the belief in a material representation of heat and that working scientists were aware of this. The laws of experimental calorimetry, the law of adiabatic change and Carnot's theorems concerning the 'motive power of heat' are approximately true independently of the flaws in the hypotheses concerning the materiality of heat. This hypothesis was not as central, entrenched and unquestioned as sometimes it has been taken to be. My suggestion was that the aforementioned laws are approximately true independently of the referential failure of 'caloric' — that is, irrespective of the absence of a natural kind standing for the referent of the term 'caloric'.

The existence of a significant truth-content in the caloric theory is not a conclusion that we draw with hindsight. I shall now turn my attention to Clausius, one of the founders of modern thermodynamics, in order to see the sense in which the caloric theory of heat was thought to be approximately true in the eyes of the proponents of the new theory of thermodynamics.[67]

R. Clausius concentrated his attention on the capacity of heat to produce work. He realised two things. (1) Joule's experimental principle of the equivalence of heat and work, that is, the principle that a certain quantity of heat must be consumed in the production of a proportional amount of work, strictly contradicts Carnot's 'subsidiary statement' that no heat is lost in a thermal cycle where work is produced. (2) Joule's principle is strictly compatible with

[67]I must stress that in this study I do not intend to present the historical development of thermodynamics. Rather, I want to focus on its relation with the caloric theory, as seen by its founders.

Carnot's 'essential principle' that *heat always flows from a warm to a cold body*.[68]

According to Clausius, during the production of work it may be the case that both a quantity of heat is consumed in the generation of work *and* a quantity of heat passes from the warm to the cold body, so that both quantities stand in a definite proportional relation to the work produced. In other words, in the place of the one hypothesis of the caloric theory which, as such, contradicted Joule's experimental findings that heat is consumed for the production of work, Clausius established two distinct but compatible principles which constitute the two laws of thermodynamics. In particular, Clausius disposed of the assumption that heat is conserved in the Carnot cycle. Instead, he showed that the work produced in the cycle is equal to the mechanical equivalent of the heat consumed during the isothermal expansion and the isothermal compression of the working substance.

Analysing the Carnot cycle, Clausius introduced the new concept of 'internal energy' of a gas, which 'has the properties which are commonly assigned to the total heat, of being a function of V and T, and of being therefore fully determined by the initial and final conditions of the gas' (p. 122). The internal energy of a gas is a function of the macroscopic parameters of the gas and therefore is conserved in a complete cycle, where the gas returns to its initial state. In other words, the properties that Laplace, Poisson and Carnot thought to be assigned to caloric, are the properties of the internal energy of a gas. More specifically, Clausius suggested that the so-called 'total quantity of heat' absorbed by the gas (or the working substance in general) is, in fact, separated into two parts: (i) the *internal energy* of the gas with the properties that the advocates of the caloric theory erroneously attributed to the 'total quantity of heat' and (ii) the *quantity of heat consumed for the generation of work*, the amount of which depends on the course of changes that the gas undergoes. So, it is important to notice that according to Clausius 'caloric' is a partially referring term. It did not refer to any material substance, but under its mature formulation, it can be seen as referring partially to the internal energy of a substance.

Clausius then derived the first law of thermodynamics which asserted that the quantity of heat received by a gas during a very small (infinitesimal) change of volume and temperature is equal to the increase of the internal energy U of the gas plus the heat consumed for the work done by the gas.

Clausius observed that, despite Carnot's being far from proving the first law of thermodynamics, his theorems were independent of the faulty assumption that no heat is lost in a Carnot cycle (cf. pp. 133–134). On the contrary,

[68]R. Clausius, 'On the Motive Power of Heat, and the Laws which can be Deduced from it for the Theory of Heat' (1850), in Mendoza (ed.), *op. cit.*, note 46, see p. 112. Henceforth all references to Clausius will be given in the main text, stating the relevant page number of his article.

they followed from the physical impossibility of perpetual motion.[69] Clausius therefore concluded that:

> It seems therefore to be *theoretically* admissible to retain the first and the really essential part of Carnot's assumptions [i.e. that 'the equivalent of the work done by heat is found in the mere transfer of heat from a hotter to a colder body'] . . . and to apply it as a second principle in conjunction with the first [i.e. the first law of thermodynamics]; and the correctness of this method is, as we shall soon see, established already in many cases by its *consequences* [p. 132, p. 134, emphasis in the original].

By showing that Carnot's theorems are sound, Clausius derived, in the new science of thermodynamics, all the established laws that appeared in the works of Carnot and Clapeyron.[70] For Clausius it was obvious that since heat is lost, there is no need for postulating an indestructible substance like caloric to account for the transmission of heat. As we saw, this was also apparent to Carnot by the time he published his memoir. Besides, Clausius thought that Joule's principle is explained better if it is admitted that heat is a kind of motion of particles in a body. Clausius stated this clearly but refrained from any specification of this motion 'further than to assume in general that the particles are in motion, and that their heat is the measure of their *vis viva* . . . (p. 112). As he stated later, '[i]n my former memoirs I intentionally avoided mentioning this conception [i.e. the nature of the motion which constitutes heat], because I wished to separate the conclusions which are deducible from general principles from those which presuppose a particular kind of motion . . .'.[71]

W. Thomson (Lord Kelvin), who quite independently of Clausius stated the two laws of thermodynamics, arrived at similar conclusions. Thomson pointed out that Carnot's principle of the flow of heat from the warm to the cold body can be arrived at without using the false assumption that no heat is lost. In

[69]Elsewhere Clausius stated: '[Carnot's] proof of the necessity of such a relation [i.e. the maximal efficiency of a Carnot cycle] is based on the axiom that it is impossible to create a moving force out of nothing, or in other words, that perpetual motion is impossible'. And, he added: 'Nevertheless I did not think that Carnot's theory, which had found in Clapeyron a very expert analytical expositor, required total rejection; on the contrary, it appeared to me that the theorem established by Carnot, after separating one part and properly formalising the rest might be brought into accordance with the modern law of equivalence of heat and work, and thus be employed together with it for the deduction of important conclusions' (cf. R. Clausius, *Die Mechanische Warmetheorie* (1867), extracts appear in S. Sambursky (ed.), *Physical Thought from the Presocratics to the Quantum Physicist* (London: Hutchinson, 1974), see p. 406, p. 407). Helmholtz arrived at similar conclusions in 1847. Referring to the general importance of the principle that perpetual motion is impossible he stated: 'By this proposition [i.e. the impossibility of perpetual motion] Carnot and Clapeyron have deduced theoretically a series of laws, part of which are proved by experiment and part not yet submitted to this test, regarding the latent heats of various natural bodies' (cf. H. Helmholtz, 'The Conservation of Force', in Brush (ed.), *op. cit.*, note 21, pp. 89–110, see p. 93).

[70]For instance the law that the difference of the two specific heats of a gas is constant (p. 130), the law of adiabatic change (p. 131), and the Carnot–Clapeyron equation (p. 134).

[71]R. Clausius, 'The Nature of the Motion which we call Heat' (1857), in Brush (ed.), *op. cit.*, note 21, see p. 111.

general, Thomson dealt with what *modifications* should be done to Carnot's theory when the dynamical hypothesis of heat is adopted.[72] In the 1850s the science of thermodynamics started its own route as a result of a *synthesis* of the laws of experimental calorimetry, other sound laws of heat phenomena, Carnot's theory of maximum work, the principle of the inter-convertibility of heat and work and a mechanical representation of the nature of heat.[73]

The reader may object to the last part of my study that Clausius' derivation of Carnot's theory rests on a distinction between an *essential* part of Carnot's theory and a *subsidiary* one. If the subsidiary one, i.e. that no heat is lost, is overthrown then Clausius' derivation goes through. But, the question arises, what is the justification for this distinction?

I shall not repeat what I already said about the alleged centrality of the assumption that heat is a material substance. Rather, the point I want to make is that the relevant scientific community has the authority to draw such a distinction between an essential and a subsidiary part of a theory. Hence, by pointing to the reasons for the community's upholding this distinction we can see why it was thought that this distinction was justified. Let us see these reasons in our case:

1. The, according to Clausius, essential bit of Carnot's theory was also the bit best supported by the evidence.
2. Von Helmholtz, Clausius and Thomson showed that the disputed principle of the conservation of heat was unnecessary in the derivation of Carnot's law.
3. The shared desideratum in the community was to keep as much as possible of Carnot's and Clapeyron's neat mathematical machinery and successful predictions. It was easily observed then that, if a hypothesis of Carnot's theory was overthrown, the rest of the theory fitted perfectly well with Joule's important experimental findings.[74]
4. The sound laws that had been established within the caloric theory were readily deduced and accounted for in the new theoretical framework of thermodynamics.
5. No alternative to the effect of a total rejection of Carnot's theory was ever produced.

These reasons reflect nothing more than the methodological desiderata and theoretical concerns of the relevant scientific community. Hence, we may conclude that Clausius's distinction between essential and subsidiary principles in Carnot's theory was justified because it reflected the *theoretical and methodological desiderata* of the scientific community.

[72]cf. Thomson, *op. cit.*, note 55, p. 176, p. 179.

[73]For a brief account of the development of thermodynamics and its relation to the kinetic theory of gases, see P. Clark, 'Atomism versus Thermodynamics', in C. Howson (ed.), *Method and Appraisal in the Physical Sciences* (Cambridge: Cambridge University Press, 1976).

[74]cf. Clausius, *op. cit.*, note 68, p. 112; Thomson, *op. cit.*, note 55, p. 81.

Having completed our account of the transition from the caloric theory to thermodynamics we may stress a last point. The development of the dynamical representation of heat was constrained by the successes of the caloric theory. That is to say, the latter were such that any alternative account of heat should have to have been able to accommodate them. Not only did the dynamical representation of heat after 1850 provide the correct account of the causal mechanisms involved in the thermal processes, but it also succeeded in accommodating the sound parts of the previous theory within the bounds of the new account of the causal nature of heat. In particular, Clausius, Thomson and Von Helmholtz showed conclusively that the sound parts of Carnot's theory and the laws of calorimetry were strictly compatible with, and deducible from, thermodynamics. The important point here is that this was suggested by the main scientists working in the field. In fact they located and preserved the truth-content of the caloric theory of heat by replacing the erroneous hypothesis of conservation of heat by two independent and compatible laws and by keeping the rest of the sound laws.[75]

Hence, it was *known* to the scientists of the period that there is a sense in which the caloric theory was approximately true, despite the referential failure of 'caloric'. We may even suggest that if the term caloric was not so loaded it could have been retained in order to refer to the internal energy of a substance. As we saw, the latter, like caloric, is a function of the macroscopic properties of a substance even within the new theory of heat, and hence there is a sense in which 'caloric' may be seen as referring to the internal energy.[76] At any rate, we may argue that the success of a theory is independent of the full reference of all of its 'central' theoretical terms. But it is not independent of the approximate truth of its laws. The approximate truth of its laws is suggested by the laws being supported to a high degree by the evidence together with sound background beliefs; it is consolidated by the laws being deducible from a broader and truer theory. In particular, a scientific realist has nothing to fear from the referential failure of caloric insofar as the success of the theory is explained by the truth of its laws and their embedding into a broader, and truer, theory.[77]

[75]I take this point to support Putnam's position that scientists try to preserve the mechanisms of the earlier theory as often as possible, and this strategy has led to important discoveries (cf. Laudan, *op. cit.*, note 1, 1984a, p. 235). Hence the foregoing study meets Laudan's challenge that no historical study has sustained this retentionist attitude as an evaluative strategy in science (cf. Laudan, *ibid.*).

[76]cf. P. Churchland, *Scientific Realism and the Plasticity of Mind* (Cambridge: Cambridge University Press, 1979), see p. 19.

[77]I take it that this latter point vindicates the claim — often attributed to Sellars — that every satisfactory theory must explain why its predecessor was successful insofar as it was successful (cf. Laudan, *op. cit.*, note 1 (1984a), p. 240). The vindication comes from the fact that it is a constraint for the advancement of a new theory to incorporate and explain — within the new framework — the successes of the superseded theory. If the successor theory does not do this, it *risks* losing the substantial explanatory and well-confirmed content of its predecessor.

5. Conclusions

I began this study with Laudan's argument from the pessimistic induction and I promised to show that the caloric theory of heat cannot be used to support the premisses of the meta-induction on past scientific theories. I tried to show that the laws of experimental calorimetry, adiabatic change and Carnot's theory of the motive power of heat were (i) independent of the assumption that heat is a material substance, (ii) approximately true, (iii) deducible and accounted for within thermodynamics.

I stressed that results (i) and (ii) were known to most theorists of the caloric theory and that result (iii) was put forward by the founders of the new thermodynamics. In other words, the truth-content of the caloric theory was located, selected carefully, and preserved by the founders of thermodynamics.

However, the reader might think that even if I have succeeded in showing that Laudan is wrong about the caloric theory, I have not shown how the strategy followed in this paper can be generalised against the pessimistic meta-induction. I think that the general strategy against Laudan's argument suggested in this paper is this: the empirical success of a mature scientific theory suggests that there are respects and degrees in which this theory is true. The difficulty for — and real challenge to — philosophers of science is to suggest ways in which this truth-content can be located and shown to be preserved — if at all — to subsequent theories. In particular, the empirical success of a theory does not, automatically, suggest that *all* theoretical terms of the theory refer. On the contrary, judgements of referential success depend on which theoretical claims are well-supported by the evidence. This is a matter of specific investigation. Generally, one would expect that claims about theoretical entities which are not strongly supported by the evidence, or turn out to be independent of the evidence at hand, are not compelling. For simply, if the evidence does not make it likely that our beliefs about putative theoretical entities are approximately correct, a belief in those entities would be ill-founded and unjustified. Theoretical extrapolations in science are indispensable, but they are not arbitrary. If the evidence does not warrant them I do not see why someone should commit herself to them. In a sense, the problem with empiricist philosophers is *not* that they demand that theoretical beliefs must be warranted by evidence. Rather, it is that they claim that no evidence can warrant theoretical beliefs. A realist philosopher of science would not disagree on the first, but she has good grounds to deny the second.

I argued that claims about theoretical entities which are not strongly supported by the evidence must not be taken as belief-worthy. But can one sustain the more ambitious view that loosely supported parts of a theory tend to be just those that include non-referring terms? There is an obvious excess risk in such a generalisation. For there are well-known cases in which a theoretical claim was initially weakly supported by the evidence and yet it involved genuinely referring terms.[78] More generally, one must not demand that a theoretical claim be successful on the spot. However, I think that there are some good reasons to believe that, within an empirically successful theory, *persistent weak support* can be associated with claims that involve non-referring terms; or, equivalently, that claims which involve referring terms will get evidential support. We argued that empirical support does not stop at the level of observational phenomena, but it extends to theoretical claims of a theory. In particular, some theoretical claims enjoy strong support. That is, the evidence is such that it makes these claims likely. Why does the evidence support some claims but it does not support some others? Is this a spurious phenomenon? I think that, *ceteris paribus*, a plausible general explanation of the fact that some theoretical claims enjoy weak empirical support, while others do not, is that the former claims are false; that is the causal mechanisms they posit do not exist. The *ceteris paribus* condition is crucial, since it is possible that, for instance, the weak evidential support is a matter of not having advanced and accurate experimental set-ups to test this claim, or of not employing suitable auxiliary assumptions. But if we view a theory diachronically, as an evolving entity, persistent weak evidential support for a claim, together with advancement of alternative theories, led to the rejection of this claim. On the contrary, there are good reasons to believe that a theoretical claim that involves referring terms will overcome an initial weak support. For such a claim has a *potential* of being successful if certain conditions obtain. That is, they can be refined, or embedded in broader theories, so that they can be tested and get confirmed.[79] So, I think, it is plausible to argue that persistent weak evidential support for a theoretical claim gives a warrant that this claim involves non-referring terms.

If I am right in my suggestions, then the pessimistic meta-induction is no serious threat for a scientific realist. For, the latter can claim that it is possible to locate the truth-content of a past theory and show respects and degrees in which the truth-content of past theories was retained in later theoretical frameworks. Studies similar to the one undertaken in this paper show that an

[78]I have in mind here Prout's hypothesis, dating from 1815, that all atoms are compounds of hydrogen atoms, and in particular that the atomic weights of all pure chemical elements are whole numbers. It is well known that this hypothesis, despite its truth, was not successful for almost a century until Rutherford and his colleagues established it experimentally.

[79]Prout's hypothesis provides, again, a nice example. Despite the initial weak evidential support, the truth of this hypothesis endowed it with a potential to be successful; the success came when Prout's hypothesis was incorporated in broader theories and when suitable experimental procedures were available. In fact the anomalies that this hypothesis faced were mostly due to experimental limitations and to the lack, before the beginning of the twentieth century, of a broader chemical theory of the constitution of elements to which Prout's hypothesis was finally embedded.

investigation of past successful yet false theories reveal respects and degrees in which they were approximately true.[80]

But there is something in Laudan's argument which makes it still important, although mistaken: Laudan provoked an all-important discussion about the connections between realism and success, and he taught us that we must be cautious in what we believe and in what we commit ourselves to. Cautious belief is a lesson that every scientific realist must be taught. This attitude does not entail disbelief. Rather, it entails differentiated commitments, in the light of evidence at hand.

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[80]C. Hardin and A. Rosenberg, 'In Defence of Convergent Realism', *Philosophy of Science* **49** (1982), 604–615, and S. Psillos, 'Conceptions and Misconceptions of Ether', published in the proceedings of the International Conference 'Physical Interpretations of Relativity Theory', London, September 1992, ed. M. C. Duffy.