Chapter 6

STRUCTURAL REALISM AND THE CALORIC THEORY OF HEAT (Draft)

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1. Introduction

The 1960s was a turning point for the scientific realism debate when Thomas Kuhn and others undermined the orthodox positivist tradition by showing that a careful study of the historical record speaks against the linear accumulation of scientific knowledge. But, as is so often the case, reaction to the admittedly naïve positivist view was disproportionate and resulted in an equally naïve and diametrically opposite view, namely that there is no significant accumulation whatsoever. Realist philosophers like Richard Boyd and Hilary Putnam were quick to reply that not everything is lost in the wake of a scientific revolution. Successive scientific theories preserve the theoretical relations and referents of earlier theories so long as both belong to a mature science. This attempt to rescue realism did not last long, for in the late seventies and early eighties a more sophisticated anti-realist argument appeared. The pessimistic induction argument, most often associated with Larry Laudan, is now widely considered to be one of the two main obstacles for realism (see, for example, Kitcher (1993: 136), Leplin (1997: 136) and Worrall (1982: 216); the other being the underdetermination of theories by evidence.[1] Put simply, the argument holds that since past predictively successful scientific theories have eventually been discarded, we have inductive evidence that our current theories will also be discarded one day. This landmark attack has precipitated a realist strategy (see, for example, Clyde Hardin and Alexander Rosenberg (1982), Philip Kitcher (1993), Jarrett Leplin (1997), Stathis Psillos (1994; 1999) and John Worrall (1989; 1994)) that is primarily concerned with showing that the historical record provides grounds for optimism. More precisely, it is argued that some theoretical components survive theory change and it is only those that are responsible for any success accrued by the rejected theories. This strategy is now the mainstream approach for scientific realists.

The aim of this chapter will be to address RP2 and SRP2. That is, I will investigate whether theoretical components, structural or not, survive scientific revolutions and, if so, whether they are indeed solely responsible for the success of abandoned theories. Of course to settle this issue in a satisfactory way is an enormous task since it would require a detailed analysis of all the relevant historical episodes. For this reason I have chosen a more tractable problem by limiting my investigation to a case study, namely the caloric theory of heat. This choice reflects three considerations:

A) The caloric theory appears on Laudan's list as one of those successful theories that together with their central theoretical terms get abandoned.

[1] The pessimistic induction argument can be viewed as a constructive demonstration of underdetermination since the new theory, despite being incompatible with the old theory, entails its empirical consequences.

B) It is a case that has not yet been investigated by structural realists.

C) It has received significant attention in the recent literature. Therefore, it will be easier to compare any structural realist claims to those of other parties in the scientific realism debate.

The chapter will unfold as follows: I will first present a historical account of the caloric theory of heat. Then, I will proceed to evaluate claims made by realists and anti-realists with regard to the caloric theory and its central theoretical terms. Following that I will examine whether epistemic structural realism can make better sense of the history of the caloric. Even though my investigation and results will be restricted to this case study I will try to extrapolate, whenever reasonable, more wide-ranging results about the history of science. Indeed, the final topic in this chapter will be a general assessment of the import of historical arguments in the scientific realism debate.

2. The Rise and Fall of the Caloric Theory of Heat

The Pre-Caloric Era

To understand adequately the history of the caloric theory it will be helpful to understand the developments that led up to the caloric era. Chemistry and the study of heat were virtually non-existent prior to the eighteenth century. The Aristotelian tradition, despite its unfruitfulness, was dominant for centuries. According to this tradition, there were four elements or substances, namely earth, water, air and fire. Not surprisingly, phenomena of heat were understood with reference to the element of fire. The Atomists and the Epicureans had roughly the same conception, viewing heat/fire as a substance with weight.[2] Thus, in both traditions 'the materiality of heat hypothesis' was central. Needless to say, the explanations furnished under either tradition were crude and qualitative in nature.

It was only with the rise of alchemy that some limited progress was achieved. Despite the magical underpinnings of their views, the alchemists developed several techniques that contributed to the advance of chemistry. They were, for example, good at distillation and the production of concentrated acids, alcohols and perfumes. They were also very good at metallurgy, especially on amalgams and on acid/metal reactions.

In the seventeenth century the conceptual atmosphere started to change for the better. Robert Boyle attacked the four elements of the Aristotelian tradition and popularised mechanical philosophy. His mechanical world-view took particles as elementary and explained their behaviour through forces such as gravity. Chemistry could not benefit from this world-view, however, since at the level of elementary particles there was not much one could predict about macroscopic phenomena, i.e. the sole domain of chemistry at the time. Nonetheless, some advances in the study of heat phenomena were made, for example, in the studies of adiabatic phenomena and the construction of instruments (see Robert Fox (1971: 41)).

[2] For more on the Atomistic and Epicurean conceptions of heat, with an emphasis on Lucretius' work, see Jesús M. Montserrat and Luis Navarro (2000).

The eighteenth century brought with it an excitement about the study of the natures of air and of combustion. The Aristotelian idea that air is one of four elements was now buried for good. Instead it was believed that air is composed of different gases and that heating turns matter into a gaseous state. Stephen Hales introduced methods for collecting and measuring the volume of gases prompting the development of pneumatic chemistry. Joseph Black, Henry Cavendish, Joseph Priestly, and Carl Wilhelm Scheele progressively identified, via experiments, various gases each with its own set of properties. Black, for example, discovered some of the properties of carbon dioxide, CO_2, and gave it the name ‘fixed air’. Cavendish did the same with hydrogen and called it ‘inflammable air’. Thus, one of the main achievements of this period was the idea that air has various manifestations, i.e. it is not just one kind of thing or substance.

Concurrent with these discoveries was the development of the phlogiston theory of combustion in the beginning of the century by Georg Ernst Stahl. Phlogiston, a Greek term which means ‘that which is set on fire’, was conceived of as the physical manifestation of heat. The tradition of subscribing to the materiality of heat hypothesis thus found a new carrier in phlogiston. It was thought to reside in all combustible objects and to be released during burning. The more heat given off by an object the more phlogiston was taken to be contained within that object. Among the theory’s main advocates was Joseph Priestly. He managed to isolate oxygen, shortly after Scheele’s independent discovery, but also to recognise its central role in combustion. As an explanation for the fact that objects were burning more vigorously in the presence of oxygen, Priestly postulated that the gas that we now call ‘oxygen’ was entirely devoid of phlogiston and was therefore more receptive of the phlogiston present in objects than ordinary air. He thus appropriately named it ‘dephlogisticated air’.

Debates over the weight of phlogiston eventually contributed to its demise. It was observed that in combustion some substances like wood lost weight whereas others like phosphorus gained weight. To accommodate these facts two contradictory assumptions would have to be made. In those cases where substances lost weight it was assumed that phlogiston had positive weight. This was a natural assumption since, qua substance, phlogiston was expected to have positive weight. It could not, however, be satisfactorily reconciled with those cases where substances gained weight and the assumption that phlogiston had negative weight was made.

Antoine Lavoisier is widely acknowledged as the person who did the most to discredit the phlogiston theory. Here’s an illuminating passage where he expresses his aversion for the ad-hoc way in which Phlogistonists tried to solve the weight and other problems:

> All these reflections confirm what I have advanced… Chemists have made phlogiston a vague principle, which is not strictly speaking defined and which consequently fits all the explanations demanded of it. Sometimes it has weight, sometimes it has not: sometimes it is free fire, sometimes it is fire combined with an earth; sometimes it passes through the pores of vessels, sometimes they are impenetrable to it. It explains at once causticity and non-causticity, transparency and opacity, colour and the absence of colours. It is a veritable Proteus that changes its form every instant ([1783] 1962: 110).

In 1772 Lavoisier conducted several experiments from which it became apparent that when phosphorous or sulphur are burnt in glass containers there is an increase in their weight and an absorption of air. More precisely, the experiments showed that in combustion the air becomes fixed, i.e. turns into carbon dioxide, leading to an increase in weight. In other experiments, he observed that heating oxide of mercury released a gas that allowed a candle to burn more brightly. This observation had already been made by Priestly who actually prompted Lavoisier's own experiment during a visit to Paris in 1774. Contra Priestly's explanation that this phenomenon was simply due to the release of phlogiston into 'dephlogisticated air', however, Lavoisier argued that it was the combination of the unknown gas with metals that produced an increase in weight. He proposed to call it 'oxigène' from the Greek terms for 'acid' and 'generate' and argued that the weight gained or lost in combustion is always equal to the weight of oxygen gained or lost. By the early 1780s Lavoisier's theory of combustion replaced the phlogiston theory. Priestly remained an unyielding advocate of phlogiston until his death in 1804.

The Caloric Theory of Heat

Lavoisier's role was pivotal in discrediting the phlogiston theory of combustion. More importantly for our aim, however, was Lavoisier's central role in proposing and developing the theory that replaced it, namely the caloric theory of heat.[3] As its name suggests, the theory's concerns were not confined to combustion.[4] Lavoisier's first steps towards the formulation of the theory can be traced to as early as 1766. However, a reasonably detailed account of his theory only appears in print about a decade later in the *Mémoires* of the French Academy of Sciences.[5] Lavoisier coined the term 'calorique' sometime before 1784 – though it was used in print for the first time in 1787 (see Fox (op.cit.: 6) – and listed caloric on the first periodic table of chemical elements.[6] The theory blossomed in the first half of the 19th century, after the death of Lavoisier, partly due to the development of more precise methods of calculation and measurement. During this period it came to dominate most of the work done in the study of heat.

Like phlogiston, the caloric is thought of as the physical manifestation of fire or heat, i.e. it is thought of as a substance. It is also thought to be an imponderable, i.e. imperceptible or at least hard to perceive, and *almost* weightless fluid. Given its

[3] For a detailed account of the relation between Lavoisier's theory and his rejection of the phlogiston theory see Morris (1972: 16-17).

[4] Morris describes the many functions of the theory as follows: "A single theoretical framework accounts for a vast array of heat phenomena including expansion and contraction, changes of state or form, the role of heat as an agent in promoting new chemical combinations, and temperature changes in chemical reactions, especially combustion and respiration" (op.cit.: 35). Fox concentrates on the caloric theory of gases, though he explains that "[f]or the period covered in [his] book it would be quite impossible to separate the study of gases from that of heat, because from the late 1770s to the 1850's acceptance of the static theory [of gases] generally implied belief in the imponderable, highly elastic fluid of heat, or caloric" (op.cit.: 2).

[5] For a thorough account of the development of the theory see Morris (1972).

[6] Etymologically, the term originates from the Latin 'calor' which means heat. It can, for example, be found in Lucretius' poem *De Rerum Natura*. Lavoisier together with Guyton de Morveau, Claude Louis Berthollet, and Antoine François de Fourcroy systematised the terminology of chemistry in their *Méthode de Nomenclature Chimique*. His *Traité Élémentaire de Chimie* offered the first periodic table of elements, many of which were to comprise the building blocks of chemistry. Lavoisier also brought about the modern notion of chemical element. Indeed, for this and other contributions he is considered one of the founders of modern chemistry.

imperceptibility how can we make any inferences regarding its presence, one may well ask. The most common answer is that it is intimately related with temperature, the latter of course being a measurable quantity. More precisely, it is thought that the addition of caloric to a body raises its temperature while its subtraction lowers it. In sum, most caloricists were in agreement that the caloric had the following properties: (1) it is an imponderable almost weightless elastic fluid, (2) it is composed of indestructible particles, (3) its particles are mutually repulsive but attracted by ordinary particles, (4) its accumulation in a body is the sole reason for the sensation of heat, (5) its combination with, or release from, ordinary matter is responsible for changes of state, (6) it is a conserved quantity.

Independent developments and ideas facilitated the birth of the caloric theory.[7] First and foremost the theory subscribes to the materiality of heat hypothesis, an idea that as we have seen predates the caloric theory by at least two millennia. The hypothesis was certainly prevalent all through the eighteenth century, taking centre stage in the works of reputable scientists such as Willem 'sGravesande, Wilhelm Homberg and Nicolas and Louis Lemery. The most marked theory advocating the materiality of heat, well known by Lavoisier's time, was that of Herman Boerhaave (1732). According to this theory, fire was composed of weightless particles that had a self-repulsive property that resulted in elasticity. These ideas eventually resurface in both the phlogiston and caloric theories.[8] Boerhaave's theory, however, departed from the caloric theory in one important respect.[9] Fire particles, held Boerhaave, are in constant motion, a motion that is ultimately responsible for phenomena of heat. More precisely, the motion of fire particles caused and sustained the motion of ordinary particles, the latter being directly responsible for phenomena of heat (see Fox (op.cit.: 12-13). Despite the popularity of this picture of heat amongst his predecessors, Lavoisier opted for a static view of caloric particles where the accrual of caloric particles alone would explain the raising of a body's temperature – property four on the above list.

Fox, justifiably in my opinion, notes that "[t]he strength of eighteenth-century belief in the materiality of fire is undoubtedly one of the most important elements in the background to the emergence of the caloric theory of heat in the 1770s" though he admits that this "does not constitute the whole story" (op.cit.: 14). Another crucial part of the story concerns the development and eventual establishing of theories of electricity, magnetism and light in the time leading up to the emergence of the caloric theory. One prominent theory, for instance, is Benjamin Franklin's theory of electricity. Theories like Franklin's postulated imponderable fluids whose properties had much in common to the caloric. Aside from the conception of electricity, magnetism and light as fluids composed of weightless particles, there was also the idea that a repulsive force dictated their behaviour.

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[7] For a succinct account of which aspects of the theory were Lavoisier's innovations and which were not see Morris (op.cit.: 30-34).

[8] According to Morris, the majority of chemists in the eighteenth century considered the self-repulsive force between fire particles on par with gravity. Apart from Boerhaave, Morris also cites Pierre Joseph Macquer as exemplifying a theorist that adopted the view of fire particles having a self-repulsive property.

[9] The idea of motion generating heat was at least partly in line with the caloric theory's main competitor in the nineteenth century, i.e. the vibratory theory of heat, to be discussed in the next subsection.

These ideas eventually led fire theorists to the view that fire itself is a fluid. Two important historical figures in this context were Bryan Higgins, a physician and chemist, and William Cleghorn, a professor of anatomy. Though working independently they proposed theories of heat that bear an uncanny resemblance to Lavoisier's theory. Higgins, for example, described fire in 1775 as an elastic fluid and claimed that its elasticity was due to repulsive forces between its particles. Cleghorn's conception of heat, found in his 1779 dissertation for the degree of MD, bears an even closer resemblance to Lavoisier's theory. In the dissertation, he describes fire as a fluid with all the essential properties of the caloric listed above.[10] A case can thus be made that at least Cleghorn had independently invented the caloric theory at roughly the same time as Lavoisier.

The exact relationship between the works of Lavoisier, Higgins and Cleghorn is not entirely clear. Fox claims that Cleghorn was definitely ignorant of Lavoisier's and Higgins' works. But there is no indication whether Higgins and Lavoisier were aware of each other's work or of the work of Cleghorn. What seems certain is that all three had knowledge of the various fluid theories available at the time. As Fox indicates, given the prominence of fluid theories during the latter half of eighteenth century, "it is inconceivable that Cleghorn (or Lavoisier and Higgins, for that matter) would not have been thoroughly familiar with them" (op.cit.: 16). Indeed, he points out that Lavoisier had acknowledged being influenced by Boerhaave and Franklin (see Fox (ibid.)). Fox also indicates that Cleghorn was familiar with the work of Boerhaave though he says nothing about Higgins's familiarity. All in all, we can say that the developments in fluid theories as well as Boerhaave's theory of fire facilitated the emergence of the caloric theory.

In addition to the six properties listed above, we also need to consider a few central concepts that had a lasting impact on chemistry. Though not his own invention, Lavoisier distinguished between combined caloric, i.e. caloric found "in bodies by affinity or elective attraction, so as to form part of the substance of the body", and free caloric "which is not combined in any manner with any other body" (1790: 19).[11] In its combined form it was undetectable but became detectable, typically via a thermometer, when it was free. One would expect to be able to detect caloric even in its combined form since caloric was heat and thus responsible for the state of the thermometer. Yet…

Lavoisier's distinction, which first appears in print in 1772, seems to have been formulated independently of Black's similar distinction put forth about a decade

[10] I could not find any indication whether Cleghorn subscribes to the sixth property, viz. the conservation of fire particles. It is worth pointing out, however, that the second property, i.e. that caloric particles are indestructible, together with the assumption that there is a fixed number of caloric particles guarantees their conservation.

[11] Morris indicates that "[t]he idea that heat matter can exist in two distinct states, free or combined, was used by his predecessors and contemporaries in explaining various phenomena" (op.cit.: 31). In a footnote on the same page he cites these as being Macquer, Gabriel François Venel, and Guillaume François Rouelle. It is also interesting to note that in the posthumously published *Mémoires de Chimie*, written in the years 1792-3, Lavoisier takes the free and combined states of caloric to represent the ends of the spectrum, everything in between being represented by a mixed third state that he calls 'the *adherent* state' (see Morris (op.cit.: 25).

earlier.[12] Black's distinction was prompted by observations that, contrary to common-sense, melting ice maintains the same temperature. To explain this, he distinguished between *latent* and *sensible* forms of heat. According to Black, when the ice melts the caloric is converted, i.e. not destroyed, into a state that cannot have an effect on the thermometer, i.e. latent heat.[13] By contrast, *sensible* heat is conceived of as being able to affect the thermometer. Thus, both Black's distinction and Lavoisier's one make use of the idea that one state of heat affects the thermometer while the other one does not. The only difference between the two conceptions is that at least until 1772 there is no indication that Black thought of latent heat as somehow matter of heat that is combined with ordinary matter.[14]

Another important concept that was to have a lasting effect on chemistry is that of heat capacity. As he notes in his manuscripts, Black started thinking about the concept of heat capacity at about 1760.[15] Black first noticed that, contrary to the mainstream view, "the quantities of heat which different kinds of matter must receive, to reduce them to an equilibrium with one another, or to raise their temperature by an equal number of degrees, are not in proportion to the quantity of matter in each" (1803: 79). In considering experiments conducted by Boerhaave, Gabriel Daniel Fahrenheit and George Martine, Black realised that different substances require different quantities of heat for their temperature to be raised by the same number of degrees. From this he argued that different substances have different capacities for heat. More exactly, for any two substances that have unequal capacities for heat, when starting from the same temperature and heated to increase their temperature by the same degree, the one with the lesser capacity will require less heat. In his own words:

> We must, therefore, conclude that different bodies, although they be of the same size, or even the same weight, when they are reduced to the same temperature or degree of heat, whatever that may be, may contain very different quantities of the matter of heat; which different quantities are necessary to bring them to this level, or equilibrium, with one another (op.cit.: 83).

In 1780 Jean Hyacinthe de Magellan coined the term 'chaleur spécifique', 'specific heat' in English, to convey the concept of heat capacity. Today we distinguish the concept of specific heat from that of heat capacity. The former is defined as the quantity of heat needed to raise the temperature of one gram of a substance by 1°C. The latter is defined as the quantity of heat required to raise the temperature of a substance by one degree, i.e. without particular reference to mass. Even today, the terms 'heat capacity' and 'specific heat' are used interchangeably in some contexts.

The view that caloric could be found in two forms, i.e. latent and sensible, soon became a major point of contention that would lead to a fracturing of the theory's supporters. Lavoisier and Pierre Simon de Laplace led those favourable to the distinction while William Irvine, a student of Black's in Glasgow who helped him

[12] According to Morris (op.cit.: 27-8), it seems that Lavoisier's distinction was not based on Black's since there is no indication that Lavoisier was familiar with Black's work prior to his postulation of the distinction. Chang mentions also "the independent contribution of Johan Carl Wilcke" to the discovery of latent heat (forthcoming: 2).
[13] Notice that this provides tacit support to the conservation of matter (including the matter of heat).
[14] See Morris (op.cit.: 27-28).
[15] See his posthumous (1803). Niels H. de V. Heathcote and Douglas McKie (1935) have a comprehensive account of the discovery of the concept.

perform experiments on latent heat, and Adair Crawford, a physician who developed Irvine's views, led those against it. Irvine thought that there was only one state of caloric and that the amount of caloric in a given object was the product of its absolute temperature, "the relative quantities of heat contained in equal weights of different substances at any given temperature", and heat capacity. He explained away the state of latent heat by arguing that phenomena associated with it were merely due to variations in the heat capacity of a substance. An analogy that is often employed to make sense of Irvine's explanation involves a bucket of water: As the bucket widens the level of water goes down and thus more water is required just to keep the water on the same level. Similarly with latent heat phenomena, when ice melts into water there is an increase in the heat capacity leading to more heat being required just to maintain the same temperature.

Chang (forthcoming-b: 4) notes that although Irvine's theory of heat capacity did not remain a serious contender beyond the early 19th century, his legacy was reflected in the subsequent debates.[16] One important aspect of this legacy was his work on specifying an exact relationship between heat and temperature capable of generating quantitative, albeit inaccurate, predictions.[17] Prior to this work quantitative predictions were virtually unheard of in theories of heat. Lavoisier, for example, claimed that the quantity of heat contained in aeriform state was *more than* that contained in liquid state and *even more than* that contained in solid state (see Morris (op.cit.: 15, 34)).

As indicated above another eminent figure who advocated the caloric theory was Laplace. Together with Lavoisier they favoured a Newtonian foundation to the caloric theory. This was reflected in their demand that caloric particles repelled one another but attracted ordinary matter. That is, a universal repulsive force was conceived between the caloric particles much like Newton's law of gravitation. The following picturesque description by Stephen Brush and Gerald Holton gives a few more details of what exactly was involved:

> If heat is applied to a material object, the caloric may be pictured as diffusing rapidly throughout the body and clinging in a shell or atmosphere around every one of the corpuscles. If these corpuscles (whose caloric shells repel one another) are free to move apart, as indeed they are in a gas, they will tend to disperse, and more strongly so the greater their crowding or the heat applied… If, however, a heated object is in solid or liquid form, the mutual attraction among the corpuscles themselves (considered to be a gravitational one) so predominates that the caloric atmospheres can provide mutual repulsion sufficient only for the well-known slight expansion on heating. Or at any rate, the attraction predominates until enough caloric has been supplied for eventual melting or vaporisation (op.cit.: 235).

Under this Newtonian framework, rough explanations could now be given. For example, the expansion of body with temperature could now be explained as the result of the repulsive force between caloric particles. Moreover, the transfer of heat could be explained by appeal to the idea that the caloric particles attracted particles of ordinary matter. In sum, the caloric theory became closely associated with this Newtonian picture, an association that, as we will shortly see, would mean that the latter's eventual demise would hasten the demise of the former.

[16] Chang (ibid.) lists John Dalton, Sir John Leslie, and John Murray as notable Irvinists.
[17] For more on this see Chang (op.cit.: 3-5), Fox (op.cit.:27-39) and Morris (op.cit.: 13)).

The Vibratory Theory of Heat[18]

The idea that heat is due to the motion of particles can be traced to the Atomists and Epicureans. Whether it survived through the centuries or was reinvented is not clear.[19] What we do know is that it started gaining prominence again in the sixteenth century. Francis Bacon, for example, remarked that 'heat itself, its essence and its quiddity, is motion and nothing else'. So spoke Galileo, Boyle, Hooke, and Newton. Daniel Bernoulli had proposed a vibratory theory of gases in 1738 but it went almost completely unnoticed for a century or so. More popular in the eighteenth century were hybrid theories, combining elements from both material and vibratory accounts. These postulated that heat phenomena were due to particles of heat in constant motion. We have already seen an example of this type of theory in Boerhaave.

At the end of the eighteenth century, the main proponent of the vibratory theory is Sir Benjamin Thomson, who is better known as 'Count Rumford'. The main tenet of the vibratory theory is that the motion or vibration of ordinary matter particles produces heat. Yet the exact nature of the mechanism remains unspecified. Rumford admits as much when he says "I am very far from pretending to know how, or by what means, or mechanical contrivance, that particular kind of motion in bodies, which is supposed to constitute heat, is excited, continued and propagated" (1798: 99). In an effort to justify this lack of knowledge, he cites Newton as being in an analogous situation. That is, not knowing its ultimate mechanism, did not bar Newton from articulating the law of gravity.

Rumford's sustained attack on the caloric theories was to last three decades and was often backed up by experiments. One of his most famous observations came in 1798 while overseeing the boring of cannons in Munich. Rumford noticed that the metal chips produced in the boring had a high temperature. The explanation according to the caloric theory would have been that caloric was being squeezed out of the cannon in the boring process. That is, the chips were gaining heat from the cannon and so the latter should be losing heat and, consequently, temperature. Against this explanation, he found that the cannon was also very hot. It thus seemed to him that more caloric was being released than could have been contained. Indeed, it seemed to him that the heat produced would have to be inexhaustible. Rumford concluded:

> It is hardly necessary to add, that any thing which any *insulated* body, or system of bodies, can continue to furnish *without limitation*, cannot possibly be a *material substance*: and it appears to me extremely difficult, if not quite impossible, to form any distinct idea of any thing, capable of being excited, and communicated, in the manner the heat was excited and communicated in these experiments, except it be MOTION (1798: 99) [original emphasis].

For the caloric theory to hold, Rumford claimed, a limitless outpouring of caloric would be required. No material substance is limitless so the cause of heat could not be a material substance. By contrast, if heat was the result of the vibration of particles then the heating up of both cannon and boring instrument could be more easily

[18] The vibratory theory is also referred to as the 'mechanical', 'dynamical' or 'kinetic' theory of heat. Just like with the caloric theory, there was more than one vibratory theory.

[19] Given the pervasive propagation of ancient Greek and Roman texts through the history of western philosophy it seems more probable that the view of heat as motion was merely inherited, i.e. not reinvented, by philosophers of the sixteenth century.

accommodated and explained. It was only necessary to assume that the motion was transferred from the borer to the boring instrument and from there on to the cannon.

One problem with Rumford's reasoning is that it is based on the false claim that heat can be produced by friction *inexhaustibly*. Obviously, friction between two or more bodies has to stop after some finite period of time due to the diminution of the bodies involved. This fact notwithstanding, Rumford's claim was not entirely without merit since a great deal of heat would still be produced in such experiments. The question then becomes whether the caloricists could explain the presence of such quantities of heat in a non-ad-hoc manner, a question whose answer remains unclear. Rumford's argument that, contra what a caloricist would expect, both the cannon and the boring instrument were gaining heat seems more effective. Yet, even against this argument, the claim could be made that, for some reason, caloric was squeezed out of both cannon and boring instrument. Whether a claim like this could be explained in a non-ad-hoc way is again unclear. The fact is that Rumford's two arguments were not knockdown arguments.

Unaware of the experiments performed by Rumford, Humphry Davy conducted his own experiments and reached more or less the same conclusion. According to Sir Harold Hartley, a modern historian, Davy was "anxious to decide between the rival theories, that heat consists of an elastic fluid called caloric, or is due to a peculiar motion of the particles of matter" (1971: 94). One of his most famous experiments involved the rubbing of two plates of ice. It was expected that not enough caloric was present to melt the ice yet the friction alone resulted in the production of sufficient heat. In another experiment, he melted wax in a vacuum using heat produced by the friction of a wheel rubbing against some metal. The apparatus sat on top of a block of ice, maintaining a temperature of zero degrees Celsius. He argued that no heat could enter the system and concluded that heat was the result of the vibration of ordinary matter particles.

Davy reported the results in his first publication, resolutely concluding that "it has just been experimentally demonstrated that caloric, or matter of heat, does not exist" (1799:??). Yet, as numerous authors have pointed out Davy's conclusion was a non sequitur.[20] Just like with Rumford's experiments the evidence did not conclusively refute the caloric theory. The caloricist might not have expected the ice to melt under such circumstances but it was hardly the kind of evidence that was irreconcilable. All that the caloricist needed to dispute was how well the experimental set up was insulated. In other words, the caloricist could always claim that caloric was leaking into the apparatus. As before, whether this claim could be explained in a non-ad-hoc way is not altogether clear.

Among the many experiments Rumford performed, some dealt with conduction and convection.[21] By conduction he understood the transmission of heat via direct contact between particles while by convection he understood the transmission of heat via the motion of particles in a fluid. Indeed, Rumford thought that heat in air and water, as well as probably other liquids and gases, was transmitted only via convection, i.e. not via conduction. His justification was that the molecules of liquids and gases are

[20] Brush and Holton (op.cit.: 237). Roller (1950: 86-87) argues that Davy's rubbing plates of ice experiment was flawed.
[21] See Chang (op.cit.: 12-14).

constantly moving and so would not be able to sustain the transmission of heat via direct contact between molecules. Among the experiments Rumford employed to support his view, one involved the heating of the surface of a vat of water. Through this experiment, he showed that there was no detectable increase of temperature below the surface. If conduction could operate in liquids then the heat on the surface would be propagated below via direct contact between the molecules.

More precise experiments revealed a slow conduction of heat in liquids.[22] These results were compatible with the caloric theory since for caloricists conduction just meant the flow of caloric between molecules. That is, there was nothing in their theory that precluded conduction in liquids. Rumford must have seen the acceptance of conduction in liquids as giving tacit support to the caloric theory. In what was likely an ad-hoc move to avoid this indirect support for the caloric theory, he argued that the phenomena presumed to be due to conduction in liquids were actually due to the radiation of heat.

Phenomena of radiant heat, however, were more of a hazard than a refuge for the vibratory theory. The reason was the perceived relationship between heat and light at this time. Many scientists in the first half of the nineteenth century thought of light and heat as qualitatively identical entities. A consequence of this conception was the view that the nature of heat depended on the nature of light. Independent experiments carried out by William Herschel, Macedonio Melloni, and James Forbes suggested that radiant heat exhibits all the properties of light, i.e. properties such as reflection, refraction, and polarization.[23] Through the independent work of caloricists Marc-Auguste Pictet and Pierre Prévost, whose experiments and explanations on radiant heat in the 1790s attracted great prominence, the caloric theory gained the upper hand in this domain of phenomena.

The Demise of the Caloric Theory

The current view in the historiography of science is that Rumford's and Davy's experiments did little to overturn the caloric theory of heat.[24] Historical facts notwithstanding, one needs to consider whether the evidence presented at the turn of the nineteenth century was strong enough to refute the caloric theory or any material theory for that matter. It is certainly true that the caloric theory together with auxiliary assumptions was in conflict with the friction experiments since it rejected the idea that heat is merely due to motion.[25] This evidence was not sufficient to refute hybrid theories, like Boerhaave's, since they could employ explanations that ultimately relied on the motion of fire particles. In other words, to the extent that the vibratory theory could explain such phenomena hybrid theories could too. More importantly, even the

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[22] Chang (op.cit.: 13) notes that the caloricist John Leslie, among others, rejected Rumford's claims. Dalton is another good example (see his (1798)).

[23] Brush and Holton indicate that at the time there was no distinction between radiant heat and other forms of heat and "it was therefore believed that any conclusion about the nature of radiant heat would be valid for the nature of heat in general" (op.cit.: 238). By transitivity, since the nature of radiant heat and the nature of light do not seem all that different, it was assumed that the natures of heat and light must not be all that different.

[24] See, for example, Fox (op.cit.: 4) and Morris (op.cit.: 33). The older view that saw Rumford's experiments as crucial was propagated by Tyndall (1863).

[25] It is worth reminding ourselves of Duhem's point here about the inability of testing theories in isolation. The friction experiments cannot be conclusive, i.e. crucial experiments, against the caloric theory unless we can establish the innocence of all the auxiliaries accompanying it.

caloric theory's inability to square itself with such observations did not make a strong case for its abandonment since the vibratory theory also faced several anomalies. For example, the vibratory theory could not yet explain phenomena that involved the conservation of heat in mixtures.[26] This was partly due to the vibratory theory's undeveloped state which meant that it was unable to, as an alternative theory, take the upper hand. Overall, we can say that, at least until after the first quarter of the nineteenth century, there were not sufficiently strong reasons to abandon the caloric theory.

The question as to why the caloric theory was eventually abandoned has not been answered in a satisfactory way.[27] Various explanations that are neither exclusive nor exhaustive have been put forward through the years. Fox, for example, notes that the rejection of the Laplacian approach to science after 1815, which was based on Newtonian principles and advocated belief in imponderable fluids, was "a major cause of the discrediting of the caloric theory" (1971: 2). Another development that cast doubt on the caloric theory was the electrochemical theory of Jön Jacob Berzelius. His theory explained phenomena of heat that arise in chemical reactions as having an electrical origin. For the first time the hegemony of the caloric theory in providing explanations for this domain of phenomena was challenged. Berzelius' theory vied with the caloric theory for the provision of explanations of phenomena involving chemical heat.

As we have seen, an altogether different explanation for the desertion of the caloric theory invokes the close relationship attributed to light and heat. When in the beginning of the century the Newtonian particle theory of light, which took light as a substance, was favoured, it was easier to think of heat as a substance too. Eventually, however, the tables were turned. In the second quarter of the nineteenth century, Fresnel's successful wave theory of light started to take a hold, replacing the particle theory. A consequence of this change was the abandonment of the view that light is a substance, thereby making it easier to espouse a non-substance theory of heat.[28] This can be seen, for example, in Sadi Carnot's posthumously published notes, where the acceptance of the wave theory of light is taken as evidence in support of the vibratory theory of heat.

Whatever the exact reasons, the outcome was certainly fatal. Fox summarises the attitude of scientists at the dusk of the caloric theory's life nicely:

> …the result in the 1820's was not a sudden turning towards our modern vibrational theory but a period of generally acknowledged agnosticism with regard to the nature of heat, a period that lasted until the caloric theory was finally abandoned about 1850 (ibid.: 3-4).

[26] See Brusch and Holton (op.cit.: 238).

[27] Fox, whose historical study of the caloric is the most definitive, offers only a patchwork of reasons for the theory's demise.

[28] According to Fresnel's theory, light is transmitted through vibrations in an all-pervading elastic medium, the ether. Once this story was accepted it was easier to accept the idea that radiant heat was, in a manner similar, the result of vibrations of ordinary matter particles. One need to recognise, however, that this simile between light waves and radiant heat could be also be accommodated by a hybrid theory by supposing that it is the fire particles that are transmitted through the ether.

During this period the research potential of the caloric theory continued to decline until it was relegated to a merely pedagogical role. The advent of energy conservation, supplanting heat conservation, dealt the final blow to the caloric theory. Experiments performed by James Prescott Joule confirmed the principle of energy conservation by illustrating the interconvertibility of heat and work. These experiments eventually paved the way for the vibratory theory's coming to dominance in the 1850s. By that time there was no credible resistance offered by the caloric theory.

3. Scientific Realism and the Caloric Theory

Recall that Laudan criticises the connection realists make between success on the one hand and approximate truth and reference on the other. That is, he criticises the inference from the explanatory and predictive success of a theory to its approximate truth and referential success. Laudan believes he has shown that there are plenty of past theories that invalidate that inference.[29] More pertinent to the historical context of this chapter, Laudan argues that the caloric theory, along with other fluid theories of the nineteenth century, is an example of a successful theory whose central theoretical concept turned out to be non-referring (op.cit.: 26-27). In this section I will examine the realist reactions to Laudan's claim.

Is 'Caloric' a Referential Term?

Attempts to argue that theoretical terms employed in the past can be interpreted as referring to entities posited by current scientific theories originate with Putnam (1975; 1978). These attempts go hand in hand with causal theories of reference. According to the latter, although scientific theories come and go, some theoretical terms latch onto real entities, properties and relations by virtue of causal chains stretching back to the original dubbing of the object, regardless of whether the descriptions employed were correct.[30] As Boyd (2002) indicates many, if not most, scientific realists now accept a causal theory of reference that incorporates descriptive elements (see, for example, Kitcher (1993), Papineau (1979), and Psillos (1999)). These hybrid accounts go by the name 'causal-descriptivism'.

Realising that the rampant disregard for descriptions can only lead to trouble, Putnam augmented the causal theory of reference with a principle of charity (a.k.a. principle of benefit of the doubt). In short, the principle allows us to brand an old theoretical term referential if the descriptions associated with it do not diverge unreasonably from those of its modern counterpart. Are realists of this variety charitable enough to include the caloric in their list of referential terms? Putnam does not directly answer this question but implies that it is not unreasonable to take 'caloric' as a referring term when he says, speaking also on behalf of Boyd, "[W]e do not carry [the principle of the benefit of doubt] so far as to say that 'phlogiston' referred" (1978: 25). Also adhering to the principle of charity but being more resolute Hardin and Rosenberg say "If one is to draw such a line in chemistry, for example, it would most plausibly come with the publication of Lavoisier's *Elements of Chemistry* and thus would exclude phlogiston theory as a counterexample" (op.cit.: 612).

[29] In fact, he construes the inference as two inferences, i.e. one from explanatory and predictive success to approximate truth, the other from explanatory and predictive success to referential success.
[30] Of course, the dubbing in these cases cannot be performed indexically since the terms purportedly refer to unobservables. It is assumed, however, that the dubbing can be performed through the effects unobservables have.

That the approach resulting from the combination of the causal theory and the principle of charity is far-fetched and suffers from several difficulties has been pointed out by many (see, for instance, Laudan (1984), Cumminskey (1992) and Worrall (1994)).[31] I will here only briefly mention the most serious one. If we allow reference fixing in the above way, many past terms can, with a little help, qualify as referring to entities postulated by current theories. That is, if at least one description associated with a past term is partially correct by our lights, referential success or failure depends on how reasonably close people think that description is from the aggregate of descriptions associated with the current term. Needless to say that opinions vary on what is partially correct and reasonably close. As I pointed out in the previous paragraph, Putnam leaves open the issue whether 'caloric' refers, whereas Hardin and Rosenberg are convinced that it does refer. Indeed, even 'phlogiston' can be made to refer under some interpretations. Phlogiston was thought of as the cause of combustion, a role afterwards assumed by oxygen. If one takes the description 'cause of combustion' as reasonably close to the descriptions associated with the term 'oxygen' today, it could be argued that 'phlogiston' referred all along to the element oxygen. The problem is that it is certainly a desideratum of an adequate account of reference and scientific theory change to able to provide *unambiguous* answers to questions of referential failure or success.

Given the above specification of the properties ascribed to it by its advocates, I cannot see how one can reasonably maintain that the term 'caloric' refers to any currently accepted entity. Not a single one of its essential properties (see the list in subsection 'The Caloric Theory of Heat' above) has survived to the present. Unless we help ourselves to an unrealistically charitable understanding of continuity of reference, the caloric must be accepted as a paradigmatic case of reference failure. I do not think existing accounts of reference, including causal-descriptive ones, offer persuasive reasons why the term 'caloric' refers to a type of entity that we today call 'heat energy'. The term 'caloric' simply does not seem to refer to anything. Indeed, as we shall soon see some realists accept the caloric's reference failure and seek other ways to protect their turf from Laudan.

Is Caloric Central to the Caloric Theory?

Hardin and Rosenberg (op.cit.) were among the first to argue that realists need not tie the approximate truth of a theory to referential success. More recently, Psillos has given a new twist to this approach by arguing that not all instances of abandoned terms within successful, and presumably approximately true, theories should be alarming to the realist, for at least some of them are simply not central to those theories. He thus strives to separate referential success from a theory's predictive and explanatory success by arguing that the latter need not entail the former. Psillos' extensive study of caloric (see (1994), (1999:ch.6)) and his attempt to undermine Laudan's argument makes him a prime target for this sub-section of the chapter.[32]

For Laudan's argument to have any impact on the realists, holds Psillos, we must examine whether the abandoned theoretical terms were really central to the theories they are customarily associated with. If they were not central then their eventual

[31] Worrall's article is particularly relevant for he rejects Hardin and Rosenberg's claims in the context of defending structural realism.
[32] Psillos (1999: ch.6) is simply a shorter but revised version of Psillos (1994).

abandonment is inconsequential to the preservation commitments of the scientific realist, for their referential failure does not undermine the success, and presumably the truth content, their theories enjoyed. What makes a term central? A term is central, says Psillos, if it satisfies the following three conditions:[33]

(CT1) It appeared in a genuinely successful theory.
(CT2) Its descriptions *were* indispensable in the derivation of predictions and explanations of phenomena.
(CT3) It was thought of by the supporters of the theory as denoting a natural kind.

Caloric, argues Psillos, is not a central term for it fails on account of CT2 and CT3. According to him, though the caloric theory is indeed successful, i.e. CT1 is satisfied, the caloric posit is neither indispensable in the derivation of predictions and explanations nor thought of as denoting a natural kind by the main advocates of the theory.

To substantiate his claim that caloric is not a central term, Psillos presents a brief history of the transition from the caloric theory to thermodynamics. That condition CT3 is not met by the caloric, argues Psillos, is obvious when one looks at the epistemic attitude of eminent scientists most closely associated with the caloric theory. As evidence, he cites these admittedly sceptical passages from leading scientific figures:

> 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… It must be confessed that the afore-mentioned fact [i.e. that, contra the caloric theory, heating does not bring about an increase in weight] may be stated as a strong objection against this supposition [i.e. the caloric theory] (Black (1803:45)).[34]

> We will not decide at all between the two foregoing hypotheses [i.e. the caloric theory vs the vibratory theory]. Several phenomena seem favourable to the second [i.e. the vibratory theory], 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. the caloric theory] – perhaps they both hold at the same time (Laplace and Lavoisier (1780: 152-3)).[35]

> 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 [i.e. the caloric theory], and it should be said that this foundation does not appear to be of unquestionable solidity (Carnot (1824: 46/100-101)).

From these passages he concludes that "the scientists of this period were not committed to the truth of the hypothesis that the cause of heat was a material substance" and that "[t]herefore, caloric was not as central a posit as, for instance, Laudan has suggested" (op.cit.: 119).[36]

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[33] See his (1999: 129). Notice that the conditions are relative to the time period when the theory under consideration was reigning. This is a contentious issue that I plan to return to later on in this section.
[34] For more on Black's sceptical attitude see de V. Heathcote and McKie (op.cit.: 27-30).
[35] Psillos also quotes from Lavoisier (1790: 5).
[36] The vibratory theory of heat, says Psillos, was similarly not adhered to, the reason being that, unlike the caloric theory, it was still not sufficiently developed.

Kyle Stanford (forthcoming) has rightly criticized Psillos for presenting a biased reading of the history of caloric.[37] To be specific, Stanford holds that the passages Psillos quotes are unrepresentative of their authors' attitudes since they are drawn from isolated remarks made about the caloric and also the ether.[38] In the case of the caloric, he elaborates, Psillos does not take into account the fact that Black was simply exhibiting a widely shared aversion towards theorising that was prevalent amongst Scots in the eighteenth century. Even more problematic for Psillos' account, argues Stanford, is the fact that Black dismissed the vibratory theory as incoherent on the basis of his own discoveries of latent heat. Thus, Stanford concludes, insofar as Black does take an epistemic stance, it is one of endorsing the caloric conception of heat as the more probable of the two.

Fox would, without doubt, agree with this assessment given his comments on the relationship between Black and the caloric theory:

> There is certainly a danger of being misled by Black's public show of caution into underestimating the closeness of the relationship between his work and the development of the material theory of heat. Black, as I have argued, did a great deal to further the theory, however indirectly or unwittingly; and it is also hard to believe that he himself thought of heat as anything but a substance when he was arriving at and elaborating the concepts of specific and latent heat (op.cit.: 25).[39]

The same bias, says Stanford, is found in the textual evidence Psillos cites from Lavoisier and Laplace. The point of the *Mémoire sur la Chaleur*, he claims, is to present the ice-calorimeter, an instrument measuring the quantity of heat in relation to the weight of ice melted. Since the measurement techniques of the calorimeter were compatible with both the caloric and vibratory theories of heat, it is not surprising, Stanford speculates, that Laplace and Lavoisier attempted to address the widest possible audience by taking a neutral stance on the nature of heat. More convincingly, he indicates that despite their initial remarks in the *Mémoire*, the rest of the book finds Laplace and Lavoisier unequivocal in their commitment to the caloric theory.[40] This is especially so in their explanatory accounts of various phenomena. What is more, Stanford says, Lavoisier's repeated endorsements of the theory and its posit through the years, coupled with his appeal to the caloric for explanations and his view that it is confirmed by evidence, should be enough to dispel the idea that his attitude towards it was agnostic.

Historians of science, like Fox and Morris, agree that Lavoisier was undoubtedly committed to the caloric conception of heat.[41] Morris, for instance, points out that

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[37] It is important to note that Stanford does not address Psillos' claims about Carnot.

[38] In the case of the ether, Stanford concedes that Psillos correctly identifies sceptical attitudes towards particular models of the ether. He complains, however, that Psillos ignores the general conviction of scientists that there must be *some* mechanical medium through which light propagates even though no one was wholeheartedly committed to any particular model.

[39] Fox (op.cit.: 51) indeed makes similar remarks about Pictet arguing that even though he had doubts about the nature of heat he nonetheless worked on the basis of the caloric theory.

[40] Morris agrees with this point, saying that "If Lavoisier wavered in his view, he did so only in the first half of this memoir" (op.cit.: 31n).

[41] Fox (op.ci.t.: 30) lists a number of other historians who agree on Lavoisier's loyalty to the caloric, namely E.M. Lémeray, G. Bachelard, C.C. Gillispie, and J.R. Partington.

“[a]lthough in the 1783 joint memoir on heat the authors stated they would avoid a commitment to a particular theory of heat, subsequent explanations of specific phenomena reveal Lavoisier’s commitment to the concept of heat as a material substance” (op.cit.: 30-1). Indeed, he goes on to argue that even in those texts where Lavoisier sounds more sceptical he ultimately employs the material conception of heat in his explanations of various phenomena (op.cit.: 31). It is only Laplace’s commitment that is genuinely brought into question. Yet, as Fox (op.cit.: 30) notes, even Laplace’s scepticism eventually waned so that by 1803 he was as committed to the caloric conception of heat as Lavoisier had been.[42]

Stanford’s critical remarks about the bias in Psillos’ textual evidence are reasonable by any standards. Psillos might object that the fact that scientists like Black, Lavoisier, Laplace and Carnot had any doubts is in and of itself sufficient to undermine the significance of the caloric. It seems to me, however, that this dispute is merely a red herring. Any effort to reconstruct the epistemic attitudes of past scientists often relies on tenuous speculations about the extent to which each of them was committed to a given posit. In his contribution to a symposium on Psillos’ book, Redhead rightly complains, “the discussion looks not so much like philosophical analysis, but rather involves peering into the psychology and/or private notebooks to ascertain what scientists really meant by terms like ‘ether’ or ‘phlogiston’ ” (op.cit: 344). Whether or not a scientific community *sees* a term as central is not important if the term is really indispensable for the predictions and explanations of phenomena. After all, the scientific community may well have an epistemic attitude that is inconsistent with the indispensability status of a theoretical term. For example, they may not yet realise its indispensability. Alternatively, they may think it is indispensable in cases where it is not. The main point that I am trying to raise here is that condition CT3 can be dropped since it is merely parasitic on condition CT2.[43]

Before I turn to Psillos’ evidence for the claim that the caloric does not satisfy condition CT2, I want to make some preliminary remarks on his choice of this condition. While CT3 is a criterion specific to Psillos’ defence against Laudan’s arguments, CT2 conveys a familiar idea in the realist camp. In order to save scientific realism from prima facie damning historical evidence, the realists seek to drive a wedge between those parts that are responsible, essential or even indispensable for the explanatory and predictive success of theories and those that are not. In other words, realists try to show that we should not expect all components of theories to be preserved through theory change but only those that are genuinely supported by evidence.

It is worth mentioning, even briefly, two prominent realists who employ this strategy. Kitcher, for example, has argued in favour of drawing a similar distinction between *working posits* and *presuppositional posits*. In his own words:

[42] Morris suggests that Lavoisier’s material conception of heat may have been due to Laplace (see (op.cit.: 9).
[43] Leplin raises a similar point arguing that Psillos confuses “the question of what entities scientists believe in with the question of what entities they needed to get predictive success” (2000: 981-2). Worrall has brought to my attention another point against Psillos’ arguments. A notion might still be central to a theory, despite the fact that scientists have doubts about the theory and the notion itself. The point thus separates commitment to truth and centrality, a distinction overlooked by Psillos.

> Distinguish two kinds of posits introduced within scientific practice, *working posits* (the putative referents of terms that occur in problem-solving schemata) and *presuppositional posits* (those entities that apparently have to exist if the instances of the schemata are to be true) (1993:149).

The reply to Laudan's argument, holds Kitcher, should be that only the presuppositional are suspect posits. The realist can reject these with impunity. According to Kitcher, the ether and the caloric were such posits therefore their rejection should not be seen as grounds for pessimism. After all, large parts of their corresponding theories got preserved. Unfortunately, Kitcher's thoughts on the caloric are limited to these few remarks so I must leave my discussion of his ideas at that.

Devitt (1984) has criticised Laudan's arguments from a variety of angles, one of which is rooted on a similarly drawn distinction. The realist, he claims, is committed only to 'necessary' posits, thereby implying a category of unnecessary ones. Devitt does not apply this idea to any historical cases. His comments, thus, remain merely suggestive as he himself notes: "Perhaps many of the historical cases of elimination the radicals [i.e. Laudan, Kuhn, Feyerabend, etc.] produce are of posits which scientists did not really make" (ibid.: 144).

Having made these initial remarks on the prevalence of CT2 in the realist literature, I return to Psillos' evidence for why the caloric is not a central term. That CT2 is not met by the caloric can be seen through an investigation of three cases that enjoyed success during this period, claims Psillos. These are: 1) the laws of calorimetry, 2) Laplace's prediction of the speed of sound in air, and 3) Carnot's cycle. Let us take each one in turn.

Calorimetry is concerned with measuring changes in the amount of heat. Psillos correctly points out that the laws of calorimetry were developed independently of the caloric conception of the nature of heat. Although the theoretical basis for these laws consisted of two assumptions closely associated with the caloric conception, namely (1) the conservation of heat in the mixing of two substances where no chemical reactions occur and (2) a concept of specific heat, this basis, according to Psillos, was sufficiently minimal to maintain independence between calorimetry and the caloric theory.[44] Thus, Psillos concludes, "since calorimetric laws were independent of considerations about the cause of heat, they could not be used to test either of the theories of the cause of heat." (op.cit.: 118-9).

The second case on the list, argues Psillos, is similarly independent of any deep theory. Laplace's prediction of the speed of sound in air was both successful and novel, yet it did not rely on the hypothesis of the materiality of heat. In making this prediction, Laplace had corrected Netwon's theoretical calculation, which was based on the assumption that the expansions and contractions of a gas occur isothermally, i.e. at a constant temperature. Against this assumption, Laplace suggested that the propagation of sound occurs adiabatically, i.e. without loss or gain of heat. The main point that Psillos tries to raise here is that the adiabatic conception of the transmission

[44] Psillos claim, at least with regard to the principle of the conservation of heat, seems to be vindicated by Laplace and Lavoisier's view that the principle could be upheld without commitment to any theory about the nature of heat.

of sound was not dependent on the caloric conception despite the fact that it has been preserved to the present.

The third case also supports the idea that the caloric was dispensable in predictions and explanations of phenomena, says Psillos. He notes that although Carnot adopted the principle of the conservation of heat, a principle that was closely associated with the caloric theory but certainly not necessitating it, he purposely avoided reference to the principle in his famous demonstration of what is now known as 'the Carnot cycle'.[45] Psillos indicates that in his description of the cycle as a four step process, Carnot "never explicitly said that the quantity of heat released by body *A* was absorbed by body *B*", a statement that would amount to the conservation of heat (op.cit.: 124). Moreover, Psillos cites Clausius (1850:133-4) who noted that Carnot's theorems did not depend on the assumption that no heat is consumed in a Carnot cycle, i.e. did not depend on the assumption of the conservation of heat. From this, Psillos concludes that Carnot's cycle did not depend on the hypothesis that heat is conserved (op.cit.).

Having presented his case for why the caloric is not a central term of the caloric theory, Psillos turns his attention to Laudan's claims on the relation evidence has to theories. He accuses Laudan of interpreting realists as confirmational holists, i.e. as committed to the idea that the observational evidence for a theory confirms the whole theory equally. According to Psillos, Laudan's view comes from a misreading of Boyd (1981), who claims that evidence confirms not only observational claims but also theoretical ones. Yet Laudan, Psillos asserts, mistakenly takes this to suggest that *all* theoretical claims are equally justified by the evidence. Against Laudan, he argues that it is possible to 'localise the relations of evidential support', identifying those claims of the theory that are supported by the evidence and to what extent. More precisely, he argues that "realism requires and suggests a *differentiated attitude to*, and *differentiated degrees of belief in*, the several constituents of a successful and mature scientific theory" (op.cit.: 126).

Naturally, those theoretical claims deemed essential to the prediction-making and explaining of phenomena inspire greater degrees of belief in their truth/approximate truth than those deemed inessential. Psillos takes the laws of calorimetry, Laplace's adiabatic process, and Carnot's cycle, as inspiring high degrees of belief in the truth/approximate truth of the caloric theory, independently of the caloric posit, which he takes as inspiring no belief at all. He summarised this point in the following passage:

> …the laws of the caloric theory can be deemed to be approximately true independently of the reference failure of 'caloric', i.e. irrespective of the absence of a natural kind as the referent of the term 'caloric'. So, a point worth highlighting is that when the laws established by a theory turn out to be independent of assumptions involving allegedly central theoretical terms, it can still make perfect sense to talk of the approximate truth of this theory (ibid.: 127).

[45] The reader will notice that Psillos' comments here are inconsistent with his earlier comments. In discussing the first case, i.e. the case involving the laws of calorimetry, he claims the independence of the principle of conservation of heat from the caloric theory. Here he emphasises their dependence in order to depict Carnot's avoidance of the principle as a general avoidance of the caloric theory.

Chang (forthcoming-a) argues that none of the above three cases lend any support to Psillos' conclusion. More exactly, he argues that there are two major problems affecting the three cases:

(D1) The predictively and explanatorily successful laws, beliefs, and practices of this era were developed independently of the caloric theory. Hence, any that were preserved would not count as supporting the view that parts of the caloric theory were preserved.

(D2) Although certain assumptions about caloric made indispensable contributions to the successes of this era, these assumptions were not subsequently preserved.

According to Chang, the first and third of Psillos' cases suffer from D1 while the second case suffers from D2. With D1 Chang challenges the view that any part of the caloric theory was preserved. With D2 Chang challenges the view that successful parts of theories always get retained through scientific revolutions.

Let us look more closely at Chang's analysis of each of Psillos' three cases. The first case, Chang argues, suffers from D1. He agrees with Psillos that the laws of calorimetry are independent of the caloric conception of heat but reaches a different conclusion from him. To remind the reader, Psillos concludes that the independence is evidence that the caloric theory properly construed, i.e. devoid of the metaphysical statements about the nature of heat but encompassing such things as the laws of calorimetry, deserves to be credited with the predictive success drawn from the laws of calorimetry and hence to be considered approximately true. Contra Psillos, Chang argues that the laws of calorimetry cannot be seen as evidence for the preservation of the caloric theory of heat precisely because they are independent of it.[46] The dispute thus hinges on whether independence from the caloric conception of the nature of heat means independence from the caloric theory altogether.

The second case, claims Chang, suffers from D2. In the years after Laplace's prediction, Poisson derived a general law of adiabatic expansion of gases that Laplacians then used to underpin Laplace's original prediction. Chang points out that Poisson's derivation was carried out by appeal to ontological assumptions central to the caloric theory, namely (a) that the caloric is a discrete fluid and (b) that it consists of point-like particles that repel one another but attract particles of ordinary matter.[47] Despite the eventual abandonment of these ontological assumptions, Chang argues, they were indispensable (in Psillos' sense of the term) in the derivation of the correct law of adiabatic processes. According to Chang, we have a counterexample to the link between indispensability and successfulness on the one hand and preservation and approximate truth on the other. That is, a theoretical component may have been essential for the production of some successful prediction yet this component was neither preserved nor approximately true.

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[46] There is, naturally, still the issue of something being preserved and we will deal with Chang's views on this soon.

[47] In a perplexing manoeuvre, Psillos (1999: 120-1) brings up the issue of Poisson's derivation and its dependence on assumptions about the caloric but does not attempt to square this historical fact with his own account.

Still in the province of the second case, Chang argues that the 'explanatory rationale' behind Laplace's correction of Newton's theoretical value for the speed of sound in air rests on assumptions about caloric. More precisely, he argues that Laplace understood adiabatic heating as the result of a mechanical compression leading to the release of caloric. Since at the time no other theory offered a plausible explanation for adiabatic heating, says Chang, it is reasonable to suppose that the caloric-based explanation offered by Laplace was indispensable (again in Psillos' sense of the term) for the success enjoyed by Laplace's prediction of the speed of sound in air. Just like above, the main point is that this serves as a counterexample to the link between indispensability and successfulness on the one hand and preservation and approximate truth on the other.

Psillos's third case, Chang argues, also suffers from D1. The first thing to note, he claims, is that Carnot's work was not central to the caloric tradition. His work came at the dusk of the caloric era and most of the leading caloricists paid little attention to it. Indeed, according to Chang, Carnot had already forsaken Laplacian microphysics and was working within a 'macroscopic-phenomenalistic' framework. This was evident, he claims, in Carnot's view of the adiabatic law as an empirical regularity devoid of any reference to Laplacian microphysics. Where Carnot did employ the caloric theory, argues Chang, the results he derived from it were subsequently abandoned or at least considerably revised. He cites as a case in point Lord Kelvin's revision of Carnot's theory, replacing heat conservation with energy conservation. In addition, he cites the abandonment of the picture of the production of mechanical work in a heat engine where caloric merely gets redistributed amongst the engine's parts. What got preserved, Chang claims, were elements unrelated to the caloric theory; elements like the principle of maximum efficiency which anticipated the second law of thermodynamics.

Three issues arise from the above discussion. The first concerns the independence of a successful theoretical component from a given theory. Though Psillos and Chang agree that the first and third cases are examples of components independent from the caloric, their claims about the extent of this independence are quite different. Psillos takes the independence to be between the laws and the caloric conception of heat as a material substance. Chang, on the contrary, takes the independence to extend to the caloric theory as a whole. In effect, they disagree on what the caloric theory really was. They thus draw different conclusions. Whereas Psillos presents these cases as supporting the view that the caloric theory was approximately true despite the caloric's referential failure, Chang thinks that they undermining it.

In my opinion, this dispute misses what is of value. No matter whether one chooses to classify the laws of calorimetry and the Carnot cycle as components of the caloric theory, the fact of the matter is that these components have survived into the twenty-first century. That in itself lends some credence to the view that scientific knowledge is to a certain extent a cumulative enterprise. Of course, the fact that the laws of calorimetry and Carnot's cycle have been preserved is not sufficient to establish that *all* or even *most* successful posits get preserved but, at least, they are a firm step in that direction.[48]

[48] Devitt (1984) argues that the realist does not need to establish that *all* past posits will be preserved. He does not, however, say anything as to whether all *successful* posits must be preserved.

The second issue concerns Psillos' second case, namely Laplace's adiabatic conception of the transmission of sound in air. While Psillos wants to maintain the independence of this conception from the caloric conception of heat, Chang wants to show that the conception was very much embedded within the caloric tradition. On the basis of Psillos' own criteria of what counts as indispensable, namely the explanatory and predictive power of the descriptions associated with a term, Chang seems to win the argument. It is certainly undeniable that: (i) Laplace's *explanations* of the adiabatic propagation of sound and (ii) Poisson's derivation of the general law of adiabatic processes was based on the caloric conception of heat.

Let us consider these two cases one at a time. Allegedly following Psillos' definition of a successful theory or component as one that gives better explanations when compared to its contemporaries, Chang cites as the real successes of the caloric theory a string of assumptions about the nature of the caloric that helped provide various explanations, which were relatively uncontested at the time, including Laplace's explanations of the adiabatic propagation of sound.[49] To cite a few of these assumptions: heat is a self-repulsive substance, temperature is simply the density of the caloric, caloric is a chemical substance, etc. Indeed, Chang makes much of the fact that these were later abandoned to support the view that even components that are responsible for the success of a theory can be, and often are, in due course discarded. That is, he uses this claim to undermine the view that successful components get preserved through time.

Do we want to call the items on Chang's list 'the real successes of the caloric theory' just because no alternative explanations were as 'plausible' at the time? None of these assumptions had any predictive-making merits of their own. All they had going for them was prima facie plausibility as explanations. But even that evaporated in time. It is true that we can call these 'the real successes of the caloric theory' under Psillos' account of a successful theory, but it is also true that Psillos' account is not adequate. For a scientific statement to be successful it does not suffice to have some plausibility as an explanation. We must demand that these assumptions have their own predictive-making merits, i.e. that they are independently confirmed.[50] Here I must side with Psillos. The three cases he considers seem to be the best examples of successful components of the caloric *era* for they have independent confirmation. It is worth noting that Chang never explicitly denies that Psillos' three examples are real successes of the caloric *era*, though he contests that the first and third examples are successes of the caloric *theory*.

The Poisson case is similarly unsupportive of Chang's argument. Just because a law is derivable on the basis of certain premises, does not mean that the success of the law must rub off onto the premises. One of the lessons about confirmation in the preceding chapter was that if evidence e confirms a hypothesis H it does not necessarily follow that it confirms any theory that entails H. In this context, we can say that even though there is confirmatory evidence for the adiabatic law, that evidence need not confirm the premises Poisson used to derive it. A similar point can be made without resorting to confirmation theory but simply by way of deductive logic. We have all been taught in elementary logic that a valid argument with a true

[49] I say 'allegedly' for I have not found the relevant passage where Psillos makes such a claim.
[50] The notion of independent confirmation that I employ here might be a bit unorthodox for some.

conclusion need not have a single true premise. In other words, even if Poisson's law has been validly inferred, as Chang maintains, the premises employed in the derivation need not be true. Indeed, given their lack of independent confirmation we now think that the premises under consideration are false.

Whether a set of sentences partakes in the confirmation obtained by any of its entailments depends, among other things, on whether that set can be independently confirmed. The premises employed by Poisson in the derivation of the adiabatic gas law, i.e. that the caloric is a discrete fluid and that it consists of point-like particles that repel one another but attract particles of ordinary matter, had no independent confirmation.[51] Today the adiabatic law is derived from different premises. It is derived from the first law of thermodynamics, i.e. that energy is conserved, plus certain other assumptions. The first law, unlike the caloric assumptions, has the benefit of independent confirmation. That, of course, is no guarantee of the truth/approximate truth or even preservation of the first law but it is certainly good reason to maintain its superiority over the caloric assumptions.

The third issue regards Psillos' requirement of indispensability, as it appears in CT2. Psillos formulates CT2 so that the indispensability of a term is relative to the period when the theory in question reigns. That is, the term must be indispensable to the scientists of that era, though, presumably, it can become dispensable for a later one. Chang shows that the caloric posit and its properties were indispensable, if we follow Psillos' formulation, to Poisson's derivation of the adiabatic law during the era in question, i.e. early in the nineteenth century.

Indispensability must surely be something fixed by the relationship between the theory, the relevant auxiliaries and the evidence. It must not be something dependent on the whims of scientists of a particular era, as Psillos maintains. Had Psillos opted for a period and scientist-free criterion of indispensability, he would have been able to distinguish between the original and modern derivations of Poisson's law. He could thus reply to Chang that there is more support for the modern premises than for the original ones since the modern ones enjoy considerable independent confirmation.

4. Structural Realism and the Caloric

So much for the history of the caloric theory and the various attempts to reconcile it with scientific realism. It is now time to turn to epistemic structural realism in order to investigate whether it can offer a more justifiable account of the successes and failures of the caloric era.

Let us begin with the successes, which, as we have seen, can, without gross inaccuracies, be identified with Psillos' three cases. To remind the reader these are: 1) the laws of calorimetry, 2) Laplace's prediction of the speed of sound in air, and 3) Carnot's cycle. I presume that by the first one of these, Psillos (but also Chang) means the following two principles:

(L1) $Q = m \cdot c_p \cdot (T_f - T_i)$

[51] Indeed, post-caloric theoreticians of heat would argue for something stronger, namely that these premises have been disconfirmed by evidence.

(L2) $Q_a = - Q_b$

The first principle states that the heat change *Q* in a simple mixture of substances that does not undergo any other, especially chemical, reactions will be equal to the product of the mass *m*, the specific heat at constant pressure c_p, and final T_f minus initial T_i temperature of the mixture. The second principle simply states that, on the assumption that the calorimeter is a closed system – an obvious idealisation –, the heat lost by object *a* must be gained by object *b*.[52]

Laplace's prediction of the speed of sound in air, i.e. the second case on Psillos' list, was derived from the following formula:

(L3) $v^2 = (c_p/c_v)\ dP/d\rho$

Where *v* is the velocity of sound, c_p the specific heat at constant pressure, c_v specific heat at constant volume, *P* the pressure, and ρ the density of air. The general law employed to buttress Laplace's derivation is Poisson's adiabatic gas law typically written thus:

(L4) $P \cdot V^{\gamma} = \text{constant}$

It states that for a fixed mass of gas, which is thermally insulated, the product of its pressure *P* and volume *V* will be constant; $\gamma = c_p/c_v$ is the ratio of the two specific heats, i.e. specific heat at constant pressure c_p and specific heat at constant volume c_v, of a gas at a given temperature.

Finally, Carnot's cycle, Psillos' third case, gives rise to what is hailed as 'Carnot's principle' or 'theorem':[53]

(L5) $\eta = 1 - T_{low}/T_{high}$

This states that a heat engine operating between two different temperatures, where T_{low} is the low temperature and T_{high} is the high temperature, will have a maximum efficiency η, i.e. given a certain input of heat it will have a limit on how much of that heat is converted into work. Indeed, the maximum amount of work, i.e. the maximum efficiency, is achievable only by an ideal Carnot engine. Such ideal engines are also called reversible engines or processes for it is assumed that no loss occurs in the quantity of heat after a complete cycle. Indeed, if such an ideal engine produces mechanical work in the process of heat transfer, it would be able to produce perpetual motion. As Carnot notes:

> … this would be not only perpetual motion, but an unlimited creation of motive power without consumption of either caloric or of any other agent whatever. Such a creation is entirely contrary to ideas now accepted, to the laws of mechanics and of sound physics. It is inadmissible. (1824: 21)

[52] This statement is idealised in at least two ways: 1) a calorimeter is not a closed system and 2) some heat will be converted to work in the process of transfer. See below for more on the relationship between heat and work.

[53] The mathematical expression of maximum efficiency is due to Emile Clapeyron's mathematization and reformulation of Carnot's views.

It is still inadmissible in the physics of the twenty-first century. We still think that all actual engines are irreversible though some approximate reversibility. The principle of maximum efficiency implies that no engine can produce more work than a reversible engine since that would allow the production of an unlimited amount of work. It is considered the cornerstone of the second law of thermodynamics.

These equations may not all have been postulated within the caloric tradition but they have certainly survived through the scientific revolutions of the nineteenth and twentieth century. They can now be found in modern textbooks on thermodynamics (see, for example, Kondepudi and Prigogine (1998)). What can the structural realist make of them?

Recall that Worrall argues that the preservation of mathematical equations, as opposed to the ontological interpretations underlying them, is to be expected from the structural realist perspective.[54] His case study, as we have seen, involves the survival of Fresnel's equations into Maxwell's theory, ontologically reinterpreted to avoid reference to the elastic-solid ether. The obvious way to extend Worrall's argument here is to simply say that the above equations should be seen as relations devoid of any ontological interpretation. In other words, Worrall would say that the caloricists got something right, namely the structure of some heat processes.[55] Given that these structures have been preserved through two hundred years of theory upheaval, whereas the ontological assumptions about the nature of heat associated with them have not, they provide prima facie evidence for structural realism.

The Phenomenological Character of L1-L5

In a certain sense to be specified shortly, L1-L5 are phenomenological laws.[56] All the terms appearing in L1-L5 denote *broadly construed* observable quantities. By this I do not have in mind that the terms' whole meaning can be captured observationally. Rather, what I have in mind is that each of them is tied to one or more observational tests which fully and completely determine their numerical value. More precisely, the values of these terms are determined either through direct instrument measurements or through a calculation that relies on such measurements.[57] For example, measuring mass and temperature typically involves taking direct measurements from instruments like the triple beam balance and the thermometer. To determine heat change Q in a simple mixture of substances requires first measuring mass, specific heat at constant pressure, and temperature, and second calculating a value from these. In either case, reading the results of measurements is an act of observation and in this sense the

[54] It is worth pointing out that McMullin has expressed views that are broadly sympathetic to the historically motivated version of structural realism though there is no indication that he accepts the Ramsey-sentence approach. In a discussion on how to overcome the pessimistic induction he says: "I would argue that these [i.e. ethers and fluids] were often, though not always, interpretive additions, that is, attempts to specify what 'underlay' the equations of the scientist in a way which the equations (as we now see) did not really sanction" (1984: 17).

[55] Notice, I don't say 'there is something in the caloric *theory*' that was right. I am concentrating on the fact that some successful structural components made their appearance during that *era*, not on whether or not these components were indispensable parts of the caloric theory.

[56] Chang also stresses the phenomenological character of the principles of calorimetry, i.e. L1 and L2, but does not make a similar judgement about L3-L5.

[57] One may complain that under this definition no term would count as unobservable.

relevant terms should be thought of as broadly construed observational. Consequently, L1-L5 should be thought of as phenomenological laws.

That the terms in L1-L5 denote observable quantities can be demonstrated by looking at the units employed in measuring them. In the International System of Units (SI), mass, thermodynamic temperature, length, time, electric current, amount of substance, and luminous intensity make up the *base quantities*. These are measured in *base units*, namely the meter, the kilogram, the second, the ampere, the kelvin, the mole and the candela respectively. The base units are the elementary units upon which all other units are defined. My claim that L1-L5 are phenomenological laws is endorsed by the fact that each of the quantities in L1-L5 is either one of the SI base units or defined in terms of these. To be precise, the five equations identify seven quantities, namely velocity, heat, specific heat, pressure, density, volume, and maximum efficiency, that are defined in terms of the first four base units, mass, temperature, length and time.[58]

Here's a break down of how the seven quantities at hand are defined in SI:[59] 1) *Velocity* is measured in terms of meters per second. To give a value to velocity, all one needs to do is measure the distance covered and the time elapsed. 2) The units with which we measure the quantity of *heat*, namely the joule, the calorie and the BTU, are interdefinable. Thus, I need only state the relationship expressed by one here: One British Thermal Unit, or BTU, is the quantity of heat required to raise the temperature of one pound of water by one degree Fahrenheit. As before, the quantities in this definition, i.e. temperature and mass are directly measurable. 3) Similarly with *specific heat*, measured as a combination of units of heat, mass, and temperature. One combination of these units sees specific heat as the quantity of heat in calories required to raise the temperature of one gram of some substance by one degree Celsius. The calorie, as I just mentioned is interdefinable with the BTU. The other two quantities are simply temperature and mass. 4) *Pressure* is measured in the pascal unit, defined as one newton per square meter. The newton is itself defined in more basic units as the force required to produce an acceleration of one meter per second per second to a mass of one kilogram. Acceleration is ultimately reducible to a relation between the measurable quantities of time, direction and length. Notice that all of the terms in the definition of the newton are directly measurable quantities, namely length, time, and mass. 5) The definition is simpler with the units of (mass) *density*, being specified in kilogram per cubic meter. 6) The same level of simplicity follows *volume*. It is measured in units of cubic meter. 7) Finally, *maximum efficiency* is measured in terms of the joule, which, as I have just pointed out, is interdefinable with the BTU.

If the testable aspect of L1-L5 brings out their phenomenogical character, as it is here suggested, how can any version of realism gain support from their preservation? More pertinently, where does this leave the structural realist? Ramsey-style ESR does not

[58] Strictly speaking, it is thermodynamic temperature measured in the Kelvin unit that is taken to the most basic. However, it is interdefinable with other units of temperature such as the Celsius and the Fahrenheit, which, under SI, are taken to be derived units. Also, I have left γ, the ratio that represents the two specific heats of a substance, out of this list simply because it expresses a relation between items on the list, i.e. two types of specific heat.

[59] To pre-empt any objections similar to those directed at the Logical Positivists, it is not my intention here to present these as *exhaustive definitions* for these quantities but rather to show that the value of the terms denoting these quantities can be fixed using measurements.

seem able to benefit from such laws. Ramseyfication takes theoretical terms and replaces them with variables, existentially quantifying over them. The above equations, as we have construed them here, presumably have no theoretical terms but only observational ones. Hence, there is nothing that needs Ramseyfying. This does not mean that L1-L5 count against Ramsey-style ESR. It does, however, mean that they do not lend any support to it since their preservation seems to be evidence for the accumulation of observational/phenomenal/empirical structure, not of any theoretical structure. Indeed it is this kind of evidence that van Fraassen uses to support SRP4, namely that there is continuity of structure through theory change but only of the structure of phenomena not of the structure of unobservables.

One possible reply for the advocate of Ramsey-style ESR would be to dispute the claim that *all* of the above quantities are observable. Obviously, Ramseyfication is no longer redundant once some of the above terms are thought of as theoretical.[60] Another possible reply would be to argue that, if there is no theory, there's no question of theory preservation arising. Yet another possible reply would be to say that it is not the equations that must be Ramseyfied but the whole theory. Since the caloric theory makes claims about theoretical entities, i.e. employing theoretical terms like 'caloric', there is, after all, something to be Ramseyfied.

Though all of these solutions are plausible, other difficulties faced by the Ramsey sentence approach to ESR, uncovered in chapter four, avert me from pursuing it further here. Instead, I want to say a few words about how L1-L5 fit with the Russellian approach to ESR. First, since ESR à la Russell is not expressed via the Ramsey sentence approach, it avoids the problem of redundancy. Second, far from posing a problem, the phenomenological character of L1-L5 lends credence to it. According to Russell-style ESR, the rational reconstruction of scientific knowledge starts with concrete observational/phenomenal structures. As I pointed out at the onset of chapter two, laws, among other categories of scientific statements, set up structures. Thus, L1-L5 specify phenomenological structures of the kind needed to advance Russell's programme. The next step is to extract the abstract from the phenomenological structures. The last and final step involves an appeal to H-W and MR in order to establish the claim that the physical causes of the phenomena have the same abstract structure as the phenomena.

As an illustration of how the Russellian treatment functions, I will here consider L1 in some more detail. L1 is a concrete observational structure that can be written down as follows: $S_O = (U_O, R_O = (m(x_i, t_i) = m_i, c_p(x_i, t_i) = c_i, T(x_i, t_i) = s_i, Q(m_i, c_i, s_i) = q_i)$, where U_O is the (finite) domain of observable objects and R_O is a class of functions defined on that domain.[61] Function $Q(m_i, c_i, s_i) = q_i$ represents L1. Now, according to the Russellian picture, from S_O we can extract abstract structure $S_A = (U_A, R_A)$, where U_A is the class of all sets equinumerous to U_O and R_A is the class of all relations isomorphic to the relations in R_O. In other words, S_A is an isomorphism class that contains S_O as one of its members. By appeal to H-W and MR we hypothesise that S_O has the same abstract structure S_A as the concrete physical structure whose members

[60] A similar reply would be to argue that, if there is no theory, there's no question of theory preservation arising. I do not take this reply seriously since I think that L1-L5 can be supportive of

[61] A function is a special kind of relation. They can thus be represented by sets of ordered n-tuples. For example, a function $f(x)=y$ is a set f of ordered pairs such that whenever $<x,y> \in f$ and $<x, z> \in f$ then $y=z$. In the above example I employ functions instead of relations for the sake of expediency.

stand in causal relations with the members of S_O. We can call this concrete physical structure 'S_P'. In other words, the claim is that S_P is isomorphic to S_O. Trivially, S_P is another member of the isomorphism class S_A. The Russellian claim then is that there is one concrete physical structure S_P which is causally responsible for concrete observational structure S_O. It is worth reminding the reader that under Russellian ESR, S_P can only be specified up to isomorphism.

The Russellian structural realist can find hope in the preservation of phenomenological laws since she assumes that between any relation of observables/phenomena there is a relation of unobservables with the same abstract structure. For Russell, if you recall, the unobservables are the external world causes of the observables. The phenomenological laws signify underlying relations between unobservables that are isomorphic to the relations between the phenomena. Provided one accepts the H-W and MR principles, this result is surely realist in spirit for it says something about the world. Of course, the traditional way of understanding the scientific realist debate dictates that phenomenological laws are not supportive of realism in any way. However, the unorthodox way in which the Russellian sets up realism means that this particular type of realism derives its force from phenomenological laws. There are plenty of objectionable ideas here but, as before, I reserve their discussion for the next and final chapter.

5. Structural Realism and the History of Science

Is Everything Structural Preserved?
The answer to this question must be obvious by now. Not all that is structural will be preserved. Examples of structural components that never made it past a scientific revolution are plentiful. In the historical context examined in this chapter, one can cite the following: (a) that the quantity of heat absorbed or freed by a given body is a state function of its properties of pressure, volume, and temperature, (b) the Irvinist equation for the determination of the absolute zero point of temperature,[62] (c) that specific heat is constant under temperature change, etc. These statements are structural for they state relationships between different quantities, yet they have been discarded as false.

This fact is by no stretch of the imagination lethal to the epistemic structural realist. The preservation claim must simply be qualified to reflect a more realistic picture of the development of science. One suggestion is the following:

(MSSS) Not all structures may survive, but *most predictively successful* elements that do survive are structural.

The ESR-ist can concede that some structures, especially those with little or no predictive power, are left behind in the wake of a scientific revolution. Moreover, the ESR-ist can concede this and still claim that most elements that enjoy genuine predictive success and survive scientific revolutions are structural.[63] In the previous

[62] See section six below. For a more thorough account see Chang (forthcoming-b: 11).
[63] That not all surviving components need be structural will be made clear in the next subsection. There, it will be pointed out that preservation sometimes happens for reasons other than truth/approximate truth.

subsection, I indicated that those components that are generally acknowledged as the caloric era's lasting contributions to our knowledge of heat have all been structural.

Does Structure Always Survive Intact?
Worrall's study of Fresnel's theory of light and my study of the caloric theory of heat exemplify cases where structural components survive intact. That is not always so. Critics of ESR have complained that, more often than not, old equations reappear only as limiting cases of new equations. As Worrall points out, this fact can be accommodated by structural realism when appeal is made to the correspondence principle. According to Heinz Post's well-received formulation of the correspondence principle, "this is the requirement that any acceptable new theory L should account for its predecessor S by 'degenerating' into that theory under those conditions under which S has been well confirmed by tests" (1993: 16).[64] What Post has in mind is a correspondence between mathematical structures. Indeed, given that the principle applies solely at the level of mathematics, its applicability, Worrall notes correctly, "is not evidence for full-blown realism – but, instead, only for structural realism" (1996: 161).

The challenge is to spell out exactly what this correspondence involves while at the same time avoiding a trivialisation of the relationship between old and new structures. Without entangling myself into a lengthy discussion of the correspondence principle, I would like to offer one familiar type of correspondence that can be spelt out more precisely and that finds support from the historical record. Here's my own rendition of it:

(NC): A structure L' that has a predecessor L becomes isomorphic to, or approximates, L when a parameter in L' is neutralised.

The neutralisation of a 'parameter in a structure' just means that the relevant relation in the structure is redefined from an n-tuple to an (n-1)-tuple, since one of the terms is neutralised, i.e. for all intents and purposes dropped. Typically, the neutralisation process in equations involves one of either two things: (1) setting the parameter to zero (as, for example, in cases where the value of the parameter is to be added to some other values) or (2) setting the parameter to one (as, for example, in cases where the value of the parameter is to be multiplied by some other values).

Three examples that feature centrally in their respective theories will authenticate the validity of NC. Take momentum in the special theory of relativity (STR) and in classical mechanics (CM) first.[65] In STR we express momentum with the formula $p = m_0 v / \sqrt{1-v^2/c^2}$. In CM we express it as $p = m_0 v$. In the limit as $v \to 0$, the

[64] Though the correspondence principle is customarily attributed to Niels Bohr, its spirit has been around at least since Newton pronounced that he had derived his theory from Kepler's laws (see Zahar (2001: 118)).
A recent *festschrift* on Post's contributions to our understanding of the correspondence principle reveals diverse manifestations of these correspondence relations (see French and Kamminga (. Stephan Hartmann (2002) summarises these and argues in favour of a pluralistic view. He raises doubts about the existence of a universal correspondence principle. It is worth noting that nothing in ESR stipulates the need for a universal correspondence principle.
[65] This example is taken from Hartmann (2002). The use I make of this example varies from Hartmann's.

denominator of the STR equation for momentum is neutralised and the classical formula p = $m_0 v$ is recovered.

The second example concerns the relation between Minkowskian and Galilean space-times. Minkowski space-time allows for a non-singular metric which is represented by the matrix diagonal (1, - 1/c^2, - 1/c^2, - 1/c^2), where *c* is the speed of light in a vacuum. Since the metric is non-singular the above matrix diagonal has an inverse, namely (1, -c^2, -c^2, -c^2). If we let c=∞, that makes 1/c = 0, and the metric becomes singular (1, 0, 0, 0), allowing no inverse. By doing so, relativity of simultaneity disappears and we recover Galilean space-time. As in the case above, neutralising the term 1/c allows the recovery of the old structure. Unlike the first example, the structure is recovered intact.

The third example concerns the relation between the Poisson bracket formulation of classical mechanics and the Moyal bracket formulation of quantum mechanics. The latter introduces non-commutative multiplication for phase space functions. If we set Planck's constant *h* to zero, thereby neutralising it, commutativity is recovered and so is the Poisson bracket formulation of classical mechanics. As in the second example, the structure is recovered intact after the neutralisation process.

That some old equations can be recovered whole or approximately whole simply by neutralising one of (often many) parameters of new equations cannot be dismissed as mere mathematical trickery. N-correspondence is quite difficult to meet. To test this, we can use a computer program that generates random pairs of equations. Because a great many different equations are possible, the odds of getting a pair that N-corresponds are very small. Indeed, without a computer's finite limitations the odds are next to nothing. This result holds even if we allow for the most liberal understanding of approximation acceptable in science. In other words, N-correspondence is not only supported by historical evidence but it is also not trivially satisfiable.

One way to justify the legitimacy of NC as a correspondence principle is to think of neutralisation as a process of idealisation. That is, we can think of *L* as an idealised version of *L´*. When we neutralise a term in *L´* we sacrifice a certain degree of predictive accuracy and, by extension, concreteness. Conversely, the move from *L* to *L´* can be thought of as one of de-idealisation, i.e. concretization. This view is common amongst scientists. It is also shared by some philosophers of science, notably those of the Poznań School (see, for example, Krajewski (1977)).

To repeat, I am not here suggesting that all cases of correspondence take this form. Rather, given that NC is not easy to come-by, the existence of structures that exhibit NC-correspondence lends some credence to the view that some robust structural continuity exists even where the surviving structures are not intact.

In light of the necessity to employ correspondence principles, I suggest that we modify MSSS thus:

(MSSS′) Not all structures may survive, but *most predictively successful* elements that do survive, either intact or suitably modified (for example, according to NC), are structural. Indeed, most, if not all, predictively successful structures survive.[66]

A measure of vagueness in MSSS′ is unavoidable. I just indicated one way in which new structure can, with reasonable modification, be made to correspond to old structure. That, of course, is not sufficient to fix the meaning of the clause ‘suitably modified’. More needs to be done in order to show the overall history of science bears the mark of MSSS′. My intuition is that once a theory gets on the road to mathematization and starts producing accurate results, it becomes less likely that non-structural components survive and more likely that those components that do survive, in some form or other, are structural.[67]

The Criterion of the Maturity of Science

Realists who seek to establish some continuity between past and present theories rely on a criterion of the maturity of science. The rationale behind it is to relegates those theories and terms that feed Laudan’s cause to immature science, thereby avoiding the need to justify why these theories and terms have been abandoned. This ploy is true even of structural realists. Worrall’s solution to Laudan’s challenge incorporates such a criterion. More precisely, his criterion requires that a science be branded mature only when it predicts *novel* types of phenomena. Worrall makes use of this criterion to distinguish between those sciences from which we should expect structural components to be preserved and those from which we should not. He is thus indirectly telling us that structural components from immature theories will, most probably, not be preserved.

This seems like overkill to me. Theories that arise in sciences deemed immature on Worrall’s account may still contain structural components that are preserved for good reasons, i.e. for their ability to make accurate predictions though not obviously for their ability to predict novel types of phenomena. That is, a structure may be able to accurately predict existing types of phenomena due to its having latched onto the world, without being able to make predictions about novel types of phenomena. Worse yet, we can easily imagine a scenario where a postulated structure, which can predict novel types of phenomena, eventually gets thrown away because no one realises that it can. If this sounds fictional, recall that it was not Fresnel who realised that his theory entailed the occurrence of a bright spot at the centre of the shadow of an opaque disc lit from a single slit but Poisson. This consequence of the theory is not at all obvious. It could have been missed altogether. Nothing guarantees that we can see all the consequences of a particular theory, not least because it is unlikely that we have all the relevant auxiliaries required to test a theory at hand. Another reason that could prohibit scientists from realising the potential of a theory for novel predictions concerns the unavailability of the required technology to build test instruments. We would certainly not want to exclude structures that may otherwise be predictively successful from our evidence of the cumulative development of science. Yet, if we follow Worrall’s account, we would have to.

[66] That is, it is not just that most predictively successful elements that survive are structures, but also that most predictively successful structures survive.

[67] Mathematization on its own is not sufficient. This is evident if one looks, for example, at econometrics, where there is a high level of mathematization and comparatively very little predictive power.

In general, it seems to me that criteria of maturity offer quasi-arbitrary divisions of the history of science that are unable to impute any epistemic benefits to those who use them. The epistemic structural realist can thwart anti-realist claims, i.e. that successful theories have nonetheless been abandoned, by simply appealing directly to the preservation of structure. Whether a structure comes from a mature or immature science or theory, or, even, no theory at all, makes absolutely no difference.[68] What makes a difference is whether the structure survives in some recognisable form and is directly responsible for some predictive success.

6. What the History of Science Cannot Teach Us

The scientific realism debate has been dominated by historical arguments for the last four decades. In one corner, anti-realists argue that there is no theoretical preservation taking place. That, they claim, should be indicative of the falsity of theories past, and the likely falsity of theories present and future. In the opposite corner, many realists argue that at least some theoretical components get preserved, a detail that, they claim, should be indicative of their approximate truth. Both corners thus agree that the historical record is essential in settling the debate.

Without doubt, the realist needs to provide a rejoinder to the anti-realist's historical arguments. Yet, the expected returns from a realist-friendly interpretation of the history of science have been overestimated. Realists seem to behave as though realism will defeat its foes on the basis of establishing historical continuity. Yet, on a strict reading, that would require belief in the view that the preservation of a component *X* is a necessary and sufficient condition of *X*'s approximate truth/truth. No realist, I hope, would be happy to adopt such a strong claim. Indeed, it can easily be shown that the preservation of a theoretical component through theory change is neither a *necessary* nor a *sufficient* condition for its truth or approximate truth.[69] Realists should not even adopt the weaker, though still strong, claim that preservation is either a necessary *or* a sufficient condition for the approximate truth/truth of what gets preserved.

It is *not a necessary condition* because even though a component may be true/approximately true, its preservation is not guaranteed. Suppose a scientist postulates a law that is actually true or approximately true in its domain of phenomena. Many reasons, quite a few of which are social/cultural, could transpire to make the general scientific community cast the law aside. For example, if the law seems incompatible with well-established theories, there will be no guarantee to adopt it. This will especially be the case when the predictive accuracy of the law cannot yet be fully tested – as when the instruments to perform such measurements are inexistent, unreliable or inaccurate. An example of, at least temporary, unreliability/inaccuracy in the current historical context involves the Irvinist equation for the determination of the absolute zero point of temperature, $c_i x + L = c_w x$, where c_i is the heat capacity of ice, c_w the heat capacity of water, L the latent heat of fusion, and x the absolute temperature of ice/water at the melting point. The equation was contested at first but the issue could not be settled due to a lack of reliable and

[68] In support of this point, I can point the reader to a recent collection of essays *Models as Mediators* (edited by Morgan and Morrison (1999)) where authors (for, example, Nancy Cartwright and Margaret Morrison) argue that many structures in science are significantly autonomous from theory.
[69] That it is not a sufficient condition is a point that has been made also by Chang (2003)

accurate measurements. Eventually the accuracy and reliability of the measurements improved sufficiently to tell against the equation. This fact notwithstanding, the point here is that there is no guarantee that we will be always be able to construct instruments that can assess the predictive power of theories. For this and other reasons there is no guarantee that true/approximately true theoretical components will be preserved.

A potential realist reply may take the following form: Had the scientific community tested the law, they would have discovered its wonderful predictive powers, making its rejection difficult, if not completely out of the question. In other words, the predictive success enjoyed by the law should guarantee that scientists, following the canons of rationality, would preserve it for posterity. Though this may largely be true, notice that now it is the predictive success of the law that takes centre stage, not its preservation. In fact, the issue of preservation becomes parasitic on the issue of predictive success. Preservation becomes superfluous.

It is *not a sufficient condition* because the mere survival of a given theoretical component does not guarantee that it has latched onto the world. Various reasons may be responsible for a component's survival. It may be a convenient feature of scientific practice or it may be a useful tool that has no power of representation. Plenty of examples can be drawn from the history of science to make plain that the preservation of a theoretical component is an insufficient condition to its truth/approximate truth. In the case study presented in this chapter, we traced the hypothesis of the materiality of heat for at least two millennia until its wholesale rejection in the middle of the 19th century. Its long preservation guaranteed neither its survival nor its truth/approximate truth.

No realist, I believe, would take preservation as a necessary or sufficient condition for a theory's truth/approximate truth. But then what role exactly does preservation play in the scientific realism debate. The most telling, though admittedly not conclusive, test for which components have latched onto the world is whether they have independent confirmation. Testing this can be done independently of any historical considerations, and, therefore, makes the requirement that a component be preserved superfluous.

I advise the realists to focus more on elaborating such prediction-based criteria.[70] I am not claiming here that we should completely dismiss the importance of history. Given that one of science's chief aims is to procure accurate predictions, any elements that are preserved will likely have predictive merits. Indeed, as the demands for predictive accuracy increase it will be reasonable to assume that so is the preservation of predictively powerful, as opposed to merely convenient, elements. It is, nonetheless, the ability to make predictions that is their ultimate judge. Preservation can be a rough indicator of this ability. Although neither a necessary nor a sufficient condition for truth/approximate truth, it can still play a role, albeit a lesser one, in the scientific realism debate.

[70] Of course, if radical underdetermination holds not even prediction-based criteria can save realism.

7. Conclusion

The primary aim of this chapter was to evaluate how scientific realism and structural realism perform when confronted with the caloric theory of heat.

In assessing scientific realism, two main strategies were considered. The first tries to establish that the caloric is a referential term. Like many others before me, I argued that this strategy fails to justify in a principled way which terms can be thought of as referential and which cannot. The second strategy, Psillos' own, tries to establish that the caloric was not a central term in the theory. Employing, among other things, objections from two critics of Psillos, i.e. Stanford and Chang, I indicated that Psillos' arguments by and large fail. On the basis of Psillos' own definition of what counts as central, the caloric posit was neither doubted by leading figures of the theory as central nor was it entirely dispensable in deriving explanations.

Indeed, what Psillos hoped to achieve by this strategy is unrealistic. First, scientists' epistemic attitudes towards a given theoretical term cannot always be trusted. A glaring example from the given historical context is the trust scientists placed – or, should I say misplaced – on the hypothesis of the materiality of heat. A more reliable factor seems to be that the term is really indispensable in producing predictions and explanations. Second, by being relative to a given epoch, Psillos' criterion of indispensability is vulnerable to Chang's objection that the caloric posit and its properties were indispensable-at-the-time.

In assessing structural realism, I surmised that Psillos' three cases of successful components are structural. The three cases involve five equations that I listed as L1-L5. What seemed as prima facie support for structural realism, however, had to be re-evaluated when the phenomenological character of L1-L5 was revealed. Worrallian-style ESR does not seem to benefit from such laws, since they have no theoretical terms to replace with variables and quantify over as Ramseyfication prescribes. Though there are plausible solutions to this problem none were pursued in detail because Ramsey-style ESR faces other difficulties that make it unattractive.

I believe to have shown that Russellian-style ESR can more easily derive benefit from L1-L5. These laws are the concrete observational/phenomenological structures from which the abstract structure of the physical world is derived. In their capacity as concrete observational structures, they are, of course, supportive of constructive empiricism too. The crucial difference again depends on whether or not one is willing to accept principles H-W and MR. These principles allow the believer to cross the boundary from anti-realism to realism but they do so at a price. Whether that price is worth paying remains an open question.

It has been pointed out that belief in ESR does not indiscriminately commit one to the belief that all structures will be preserved. Some structures never make it past scientific revolutions. Moreover, when they do survive, structures are not always found intact. More often than not, old structures reappear only as a limiting cases of new structures. I suggested, following Worrall and many others, that this fact can be accommodated by ESR when appeal is made to the correspondence principle. I offered one concrete version of the correspondence principle, what I called 'NC', arguing that it is corroborated by some well-known cases in the history of modern physics. I believe to have showed that NC is difficult to satisfy, i.e. it is non-trivial, and that therefore should not be taken lightly. In the end, I conceded that more work needs to be done to establish whether the history of science corresponds to MSSS´, i.e. that "Not all structures may survive, but *most predictively successful* elements that do survive, either intact or suitably modified (for, example, according to NC), are structural."

The criterion of the maturity of science is one of the last issues I took up. My conclusion was that the structural realist does not need to draw quasi-arbitrary distinctions between mature and immature science. Instead, what matters is whether a structure survives in some recognisable form. Indeed, even this last claim is not strictly speaking correct since preservation seems parasitic on the predictive power of structures. For this reason, I urged the participants in the scientific realism debate to not overestimate what can be achieved by historical continuity.