THE SUCCESS OF THE CALORIC THEORY: WHAT CANNOT BE INFERRED

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One of the most compelling recent endeavors within the realism debate has been to engage some of the classic arguments in a serious historical vein. A lovely example of this takes place between Hasok Chang, in his recent (2002) article against preservative realism, and Stathis Psillos, who argues for preservative realism in his (1999) book. Their debate is an unusual one, not only because the crux of it is tied to the largely forgotten 19th century 'caloric theory' of heat, but because these authors manage to arrive at opposite conclusions as to its bearing on the preservative realist argument.[1]

The sticking point is this: if one is to defend the preservative realist claim, that the success of a theory is a reasonable indicator of the truth of that theory, then one must account for the apparent success of the caloric theory of heat. This theory is now understood to be incorrect in so far as its central term 'caloric,' which denotes a discrete, invisible fluid responsible for the phenomenon of heat, does not actually exist. Psillos tries to solve the problem by arguing that the caloric theory "was not essentially dependent" on the existence of caloric (Psillos 199, 113). His hope is to show that the success of the theory was not driven by the "false" constituent claims, but rather by the correct ones, which have been preserved in our later theories. Chang responds by arguing that a material understanding of caloric "played a crucial role" in the success of the theory (Chang 2002, 909).

I will begin this discussion with a rigorous analysis of one particularly outstanding historical success, namely, Laplace's derivation of the sound equation. Although both Psillos and Chang offer some interesting insight into this result, in the end they take opposite sides on the question of whether or not Laplace employed incorrect assumptions in obtaining it, and neither offers conclusive evidence on the matter. I shall give what I take to be quite compelling evidence that Laplace's successful derivation was heavily motivated by assumptions about the material nature of heat which most realists now believe are wrong. This is my case example against

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[1] Preservative realism characteristically endeavors to show that most of our scientific pursuits are telescoping toward the truth, in a kind of asymptotic improvement of approximations, through the preservation of "truth constituent claims" over time. See Psillos (1999).

Psillos' claim. In the second half of this paper, I will continue to argue that we cannot actually infer the "true existence" of theoretical entities from the success of such theories. In the case of the caloric theory, it turns out that the claim "caloric exists" has little bearing on the success of the theory. Indeed, there is a way in which this claim may be consistently *asserted or denied* in the language of modern thermodynamics. How could we ever expect to infer anything more?

SUR LA VITESSE DU SON

Newton himself knew that the formulation of the wave-pulse equation in his (1687) *Principia* was inconsistent with empirical observation. So it must have been a truly striking confirmation of the caloric theory when, over a century later, Laplace (who was a calorist) finally managed to give a satisfying reformulation of the equation. Indeed, his reformulation continued to yield correct predictions for several decades afterward, even as experimental measurements became more precise, and almost certainly contributed to the powerful Laplacian influence over the direction of early 19th century physics.

Laplace is quite clear as to what he found wrong with Newton's calculation: "the difference between the observed speed of sound and the speed calculated by the Newtonian formula is due to the latent heat that the compression of air releases [develops]" (Laplace 1816, 240-241)[2]. This is easy enough to understand in hindsight: Newton mistakenly assumed that temperature remains constant across an acoustic disturbance. What is peculiar here is the occurrence of the term 'latent heat,' which Laplace understood quite specifically to refer to *caloric* in a particular chemical state. This, if anything, seems to render the assertion false. But let us not jump to conclusions; Laplace's language in his closing remarks need not reflect the assumptions that went into his successful derivation. We must dig deeper into these assumptions, and in particular into the meaning of the term 'caloric,' for crucially, it stands to be determined whether or not Laplace's derivation was logically dependent on claims which are now understood to be false.

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[2] All of the translations of Laplace given here are my own.

I must begin by briefly recounting the caloric theory itself. At its most basic level, ‘caloric’ represented the invisible cause behind the heating and cooling of a substance. Beyond this, there were actually several competing caloric theories in the early 19th century, but I will be describing the view that Laplace subscribed to, which has been called the “chemicalist” view.[3] In this tradition, Prevost (1791) notably characterized caloric as an invisible, “discrete fluid,” which flows into bodies when they are heated, and out again when they are cooled. At the time, thermometers were just beginning to become reasonably reliable, and the caloric theory offered the most viable explanation of what it was that the new thermometers were measuring:

C1. Temperature is a measure of the density of free caloric.

The chemicalists characteristically believed that caloric could exist in different chemical states, and that ‘free’ or ‘sensible’ caloric was the state in which caloric could affect a sensible change in temperature. Thus, a rising or falling thermometer meant that the density of free caloric was increasing or decreasing, respectively.

Note that C1 is not so much a claim about the behavior of caloric as it is a kind of “bridge,” between an empirically observable quantity (temperature) and an entity whose existence is posited by the caloric theory (caloric). One might further choose to read this claim as containing the implicit assumption that caloric actually *exists*, but this addition would not play a significant role in the theory itself. As Lavoisier, one of the fathers of the chemicalist tradition, writes: “we are not even obligated to suppose that caloric is a real substance; it is sufficient... that it be any kind of repulsive cause which separates the molecules of matter, allowing us to imagine its effects in an abstract and mathematical way” (Lavoisier 1789 [1864], 19)[4]. Indeed, as we shall see, Laplace’s claims about the *behavior* of caloric are what gave his theory its predictive power; the existence of the substance itself was somewhat of a side-note, and a logical necessity only in so far as it served to link theory and observation.

In contrast, the following was a more substantial claim about the behavior of caloric as a material substance:

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[3] The term ‘chemicalist’ may be found in Fox (1971), as well as Chang (2004).

[4] The translation here is my own.

C2. The particles of free caloric are self-repelling.

Claim C2 offered some understanding of what caloric is composed of (discrete particles), as well as how it behaves, which produced a certain amount of explanatory power when combined with C1. For example, one could now explain why a hot cup of tea on the kitchen table slowly cools: free caloric is dense inside the hot water, so that over time, the particles are repelled away from each other into the air, where caloric is sparse.

Even more explanatory power came with two more strong assertions about the conversion of caloric into a 'latent' state:

C3. Caloric, when chemically combined with the molecules of a substance, becomes 'latent,' and occupies only volume while *leaving temperature unaffected.*

C4. The amount of free and latent caloric in a closed system is constant.

This allowed chemicalists to understand, for one, the expansion of a gas under heat. As the temperature of a gas rises, Laplace writes, "only one part of the caloric received actually produces this effect; the other part, which becomes latent, produces an increase in volume" (Laplace 1816, 238). Similarly, if we compress a gas, latent caloric is effectively "squeezed free," increasing the density of free caloric; this explains the observed rise in temperature of a gas as it is compressed.[5]

Finally, we must take note of the fact that Laplace had, by this time, begun to develop a significant extension of the caloric theory, in part by adding the following assumption:

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[5] The description of latent caloric as being "squeezed out" under compression is due to H. L. Callendar (1910, 154). In addition to this startlingly coherent explanation of gas behavior, assumptions C3 and C4 also offered an apparent solution to many of the most important challenges to the caloric theory, such as Count Rumford's famous (1798) experiment in boring canon. The determined calorist could surmise that the apparent production of heat by motion in certain cases (as through friction) was actually just latent caloric dislodging from a substance; thus, experiments like Rumford's only showed that there was indeed a great amount of latent caloric contained in certain substances.

C5. Particles of free caloric (in addition to particles of latent caloric) exist *inside* the molecules of a substance, although they may be transferred across space when that substance is heated or cooled.[6]

The utility of C5 is slightly more opaque, but let us examine how Laplace made use of it. Suppose that the air around a passing sound wave was hotter than Newton expected. Laplace understood this to imply a greater density of free caloric in the air (by C1), and thus a greater repulsive force (by C2), inside the molecules of air (by C5). These were the assumptions that allowed Laplace to conclude that an increase in temperature will result in a greater repulsive force between molecules, and thus "must increase the speed of sound, since it increases the elasticity of air" (Laplace 1816, 239).

The next step in correcting Newton's equation then follows naturally, since Laplace's assumptions about latent caloric already suggest how the air around a passing sound wave might be hotter than Newton assumed. An oscillating sound wave compresses the air through which it travels, thus momentarily decreasing the volume. On Laplace's understanding, Newton failed to realize that this decreases the quantity of latent caloric in the air (by C3), thereby increasing the amount of free caloric (by C4)[7]. Hence, as Laplace asserts, "the heat which is released [developed] by the approach of two neighboring molecules of a vibrating arial fiber elevates their temperature" (Laplace 1816, 239). This newly released free caloric would not have time to dissipate in the period of a single vibration (that is, the vibration is adiabatic), and thus, Laplace realized, the air should be hotter than Newton expected, and the speed of sound faster. In order to avoid the irrelevant details of Laplace's final calculation (which involved a handful of additional assumptions about the behavior of caloric particles), I shall simply note Laplace's final result, that the speed of sound should be faster than Newton predicted by a factor of $\surd(c_\rho/c_v)$, where c_ρ and c_v represent the specific heat of air under constant pressure and constant volume,

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[6] Laplace's (1816) article on sound constitutes an early expression of this claim, although it is easy to miss, hidden in his assumption that an increase in temperature amounts to a greater elasticity of air. Laplace later stated C4 more explicitly, and proceeded laboriously to derive a number of gas laws and other thermal results. See, for example, Laplace (1821).

[7] Indeed, it would have been very difficult for Newton to draw this conclusion, being sympathetic to the view that heat is something of the nature of motion.

respectively. This correction finally placed the speed of sound squarely in the range of reasonable error with respect to contemporary measurements.

At this point, it is not too difficult to see how Laplace's derivation achieved such resounding success: by our current understanding, it is *true* that an acoustic disturbance increases the temperature of air, and it is *true* that an increase in the temperature of air increases its elasticity. These are assertions may be defined purely in terms of the operations by which they are measured, and they are in clear agreement with empirical observation. Indeed, this agreement very likely influenced Laplace's derivation. Nevertheless, logically, much of Laplace's theoretical derivation came prior to these assertions, and relied heavily on specific expressions about the nature and behavior of heat that are now considered to be downright *false*. As described in C1 through C5, caloric is supposed to be an entity whose density determines temperature, whose constituent particles are self-repelling, etc. But in modern thermodynamics, it is clear that this understanding of the term 'caloric' does not refer to anything that actually exists. This fact, together with the long-lasting success of Laplace's caloric theory, is a result that is potentially deadly for the preservative realist argument. Let us now examine further why the argument fails.

THE ROLE OF ONTOLOGY

One might be tempted to focus only on Laplace's "true" assertions, that an acoustic disturbance increases the temperature of air, and that an increase in the temperature of air increases its elasticity. They were largely responsible for Laplace's success, and were in a sense "preserved" in modern theories. But these assertions were in fact *predictions* of Laplace's caloric theory, not starting assumptions! So the preservative realist is still stuck making sense of the success of these predictions, because the Laplace's theoretical assumptions in deriving them were certainly *not* preserved. Yet it is an understandable reaction to want to throw out the undesirable ontology wrapped up in Laplace's theory and just keep the results that we take to be true. Such a tendency should lead us to wonder: does ontology really play a role in the success of a theory?

We may immediately note it must play *some* role, since there is at least one ontological principle entailed by the notion of success itself. Chang discusses this principle in his excellent (2004) work on temperature, where he calls it the 'principle of single value': "a real physical property can have no more than one definite value in a given situation" (Chang 2004, 90). To take Chang's own example, "[i]t would be nonsensical to say that a given body of gas has a uniform temperature of 15° and 35°C at once," not because it is a *logical* impossibility, but because it goes against this fundamental assumption about the nature of the physical world (ibid). Therefore, if we are going to make claims about 'successful predictions' at all, we must require that these predictions yield precisely one value per given situation, in accordance with this axiom.

However, insightful though the principle of single value may be, it is also extremely basic, and we are still left to wonder if some more complex ontological claims might play an essential role in any of our successful predictions. In keeping with the discussion, it seems only proper to consider this question with respect to the caloric theory and its victorious successor, the kinetic theory of heat. This turns out to be a particularly interesting case, as there appears to be a naïve kind of structural equivalence between the latter theory and a more advanced formulation of the caloric theory, which began to blossom for a few decades, just before its demise. The possibility for this equivalence was first argued by the British physicist H. L. Callendar, in his 1911 presidential address to the Physical Society of London.

Before we examine the details of this argument, let us briefly consider what such a curious structural equivalence might imply about the necessity of ontology in these theories. Realists of the structuralist disposition might be inclined to assert that there is "structural truth" to them both. However, this claim is not really warranted here; even if the caloric and the kinetic theories do turn out to account equally well for the same sets of observations, the most that we are really entitled to conclude is the structural equivalence of their data sets. In other words, all we really know is that their predictions match *over some domain*. Such a base level of equivalence between two models is not such a strange idea; the mathematical logician, for example, is quite accustomed to the notion that a consistent, satisfiable theory can have an *infinite* number of (non-isomorphic) models.[8]

Now, if there is really no compelling reason to choose one model over the other, then it seems possible that the actual existence claims of either model play *little role at all* in the success of a theory, beyond the very basic level previously discussed. One might cite other, perhaps pragmatic grounds for choosing a model; however, in the language of thermodynamics, Callendar actually argues that the caloric theory is in many respects preferable to the kinetic theory. Both of Callendar's arguments, for the structural equivalence and for the practical utility of the caloric theory, are very compelling, and they are worth discussing here in more detail.

The caloric theory that Callendar discusses is the one developed by French physicist Nicolas Léonard Sadi Carnot. The caloric paradigm was already well established when Carnot published his monumental (1824) *Reflexions on the Motive Power of Fire*, due to the work of giants like Lavoisier and Laplace. Nevertheless, he did not continue to develop his work after this publication, and later rejected the caloric theory altogether, as he makes clear in his posthumously published notes: "[w]hen a hypothesis is no longer adequate for explaining phenomena, it must be abandoned. This is the case with the hypothesis according to which caloric is considered as matter, as a subtle fluid" (Carnot 1824 [1986], 185).

Part of why so many of his results turned up in modern physics is perhaps that Carnot's reasoning hardly ever included the kind of weighty ontological claims that Laplace employed. His justification of the Carnot cycle consisted of little more than the assumption that perpetual motion is impossible.[9] Indeed, Psillos and Chang both seem to agree that in this sense, Carnot was not much of a calorist, and thus that he does not really have a place in the preservative realism debate.[10] However, this conclusion may be a little premature, for Callendar clearly disagrees, even going so far as to assert that Carnot actually offered "the correct final solution to the problem of the caloric theory" (Callendar 1910, 168).

The essential observation is that Carnot's expression,

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[8] For example, take any first-order theory T that has a countably infinite model. Then, by the "downward" Löwenheim-Skolem theorem, there exist models of T of *every* transfinite cardinality, none of which can be isomorphic.

[9] This fact is observed by Callendar (1910, 161), and Psillos (1999, 123) also gives a beautiful illustration of it.

[10] See Psillos (1999, 123) and Chang (2002, 905).

$$F'(t) = dW/dQ$$

when taken with assumptions C3 and C4 of the caloric theory, leads to a correct model of temperature and work *in a reversible process*. Here, dW/dQ is the change in work W with respect to a quantity of heat Q[11] over a derived function of time F, the latter being independent of the material substance involved.

To see how, first notice that Laplace's assumptions C3 and C4 imply the following claim, which Callendar describes as a "fundamental postulate" unique to the caloric theory: the quantity of caloric (latent and free taken together) required to change the volume and density of a body by a given amount, "is the same, in what ever way the change is effected" (Callendar 1910, 166). The important difference between Callendar's claim and C3+C4 is that the former is largely stripped of ontology; it is a claim about the measurable quantities of volume and temperature, rather than a direct statement about the nature and behavior of caloric as a substance. Now, taken together with the assumption that the specific heat ratio at a constant volume is independent of pressure, this implies that the function $F(t)$ is constant, and we may denote it A. Thus, through integration over an interval of temperature (T_0, T), we obtain that:

$$W = AQ(T - T_0).$$

This equation is less general than the first one, since it assumes the conditions for what is now called a *perfect gas*, and that a quantity of heat is measured as caloric. But, as Callendar notes, "[a]dmitting these assumptions the solution is obviously correct" (Callendar 1910, 168). If Carnot had been more inclined to pursue this result, he may have realized that "caloric" is not *equivalent to motive power*, but merely *capable of performing work* under certain conditions. The pesky assumption that caloric is conserved, and hence that every processes is reversible, need not be applied at all.

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[11] The quantity of heat here need not be measured in terms of energy; this notion has not yet been introduced, as Carnot arrives at this equation almost entirely through his discussion of the impossibility of perpetual motion, and then taking the derivative to extend the result to the infinitesimal.

The equivalence between the two theories then begins to fall nicely into place. Callendar defines a measure of caloric in terms of the equation above, as “ the work done per degree fall in a Carnot cycle” (Callendar 1910, 173). This provides us not only with a reasonable unit of caloric, which Callendar calls a "carnot,” but with a rule of conversion between carnots and joules: “The ‘carnot’ is that quantity of caloric which is capable of producing one joule of work in a Carnot cycle per 1°C fall on the scale of a perfect gas” (ibid). So defined, this caloric theory does indeed give all the results of the kinetic theory, in so far as they describe reversible processes.

Callendar proceeds to calculate several thermal results given by the kinetic theory from this new perspective, as well several interesting computations of caloric, which I do not have time to discuss here. But one should note that Callendar’s derivations are more than just theoretical thumb-twiddling. There is clearly some virtue in this caloric theory’s direct statement of the principle of conservation of motive power; as Callendar observes, “caloric possesses the important property, essential to the natural measure of heat quantity, of remaining constant in reversible exchanges (which the energy measure does not)” (Callendar 1910, 184). This might facilitate the student’s understanding of these phenomena, or even the understanding of physicists themselves, especially with regard to elaborate definitions like that of entropy. If entropy is “merely caloric under another name,” as Callendar claims, then it might make sense for simplicity’s sake to define it in terms of the equation $W = AQ(T - T_0)$, instead of the integral by which it is usually defined.

Might we still be concerned by the fact that caloric, on this understanding, is a material substance which is not conserved? Callendar responds by relating caloric as a theoretical concept to the parallel concept of *current* in electrodynamics. Both may be ultimately defined in terms of energy, and both may be generated under certain circumstances; however, their essential utility is in their potential to simplify and aid in our understanding of the situations in which they apply. So defined, ‘caloric’ refers to a quantity that *need not exist*, just as the terms ‘current’ or ‘energy’ need not refer to any particular material substance.

However, had this understanding been apparent in the mid-19th century, when the caloric theory was being vigorously discarded, one wonders if some of the theory’s more stubborn realist sympathizers might have pursued a stronger interpretation.[12] Perhaps it is equally reasonable (though admittedly bold) to continue to posit that 'caloric' refers to a kind of matter—really!—and that it is indeed *created* under certain circumstances, as by motion or electricity. It is important to note that this claim does not change our understanding of the behavior of this newly defined 'caloric' as a term in our theories; rather, it is a claim about the *existence* of a certain kind of entity in the real world, and its correspondence to a theoretical term, just like Laplace's assumption C1. The success of the theory does not depend on whether or not one decides to commit to this claim.

CONCLUSION

We have seen, in the first section, that the spirit of Laplace's successful derivation of the sound equation appealed to assumptions about the behavior of caloric that are now considered incorrect. But Laplace's success here was largely due to the fact that these notions implied some preliminary conclusions which turned out to be fundamentally independent of ontology, and which were consistent with empirical observation. We may similarly understand Callendar's ironic result, that "the only defect of the caloric theory as developed by Carnot lay in the tacit assumption that the ordinary calorimetric units were units of caloric" (Callendar 1910, 182). By applying Carnot's method of abstracting away from as many ontological assumptions as possible, one may construe this later caloric theory as not only consistent with, but a fruitful *subset of* modern physics.

Furthermore, the importance of whether or not caloric actually *exists* seems to depend on who you ask: to Laplace, 'caloric' referred to a substance which was ontologically well-defined, while Carnot preferred to deemphasize this role. Both physicists achieved lasting success. So a more instructive conclusion, in light of Callendar's resurrection of Carnot's caloric theory, is that we may consistently claim either that caloric exists, or that it does not. In short: the success of

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[12] For a number of reasons, Carnot's work was vastly underappreciated in his time, which may account for some of the enthusiasm with which his theory was abandoned. His work was so little read, that a number of results identical to his own were given independently in the years after the publication of *Reflexions*. This neglect was probably due as much to Carnot's own dismissal of his theory, which I discussed earlier, as it was to the many incorrect expressions and experimental challenges which afflicted his theory in the years after his death.

the caloric theory says nothing about whether or not caloric “really" exists. Perhaps this is evidence that the success of our theories does not warrant any canonical inference as to whether or not their fundamental machinery actually refers. Perhaps this negative ontology is all the ontology that is required.

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