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# Nagel's analysis of reduction: Comments in defense as well as critique

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ABSTRACT

Despite all the criticism showered on Nagel's classic account of reduction, it meets a fundamental desideratum in an analysis of reduction that is difficult to question, namely of providing for a proper identification of the reducing theory. This is not clearly accommodated in radically different accounts. However, the same feature leads me to question Nagel's claim that the reducing theory can be separated from the putative bridge laws, and thus to question his notion of heterogeneous reduction. A further corollary to the requirement that all the necessary conditions be incorporated in an adequate formulation of the putative reducing theory is that the standard example of gas temperature is not reducible to average molecular kinetic energy. As originally conceived, Nagel's conception of reduction takes no account of approximate reasoning and this failure has certainly restricted its applicability, perhaps to the point of making it unrealistic as a model of reduction in science. I suggest approximation can be accommodated by weakening the original requirement of deduction without jeopardizing the fundamental desideratum. Finally, I turn to briefly consider the idea sometimes raised of the ontological reducibility of chemistry.



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## 1. Introduction

Determining whether reduction has been achieved in any particular area presupposes a conception of what is meant by reduction. Reductionism can only be assessed by reference to criteria for successful reduction. This is just as important for the antagonist as for the upholder of reductionism (in some area), since it must be clear what is being denied. It might be thought that these criteria should first be determined by an analysis of the general idea of reduction before proceeding to the assessment in the particular case. On the other hand, it might be said that such general criteria can only be formulated by evaluating general principles in the light of what seems feasible in actual cases, and striving towards something like what Goodman called a reflective equilibrium. But since we have to begin somewhere, general considerations are taken up first with a view to providing a framework for the subsequent discussion rather than laying down incontrovertible principles impervious to considerations from actual cases.

What general motivation is there for reductionism? For the positivists, this was the unity of science. A radical critique challenges unity as a goal of science (Dupré, 1993). But even without going so far, unity surely does not *require* reduction, intuitively understood as the incorporation of one theory within another. Even if reduction were sufficient for unity, its justification might well call for far more than is actually to be found in an examination of the current state of science, making it a very speculative thesis. Consistency, requiring the absence of contradictions, and more generally in the sense of the absence of conflicts, tensions and barriers within scientific theory, would provide weaker, though apparently adequate, grounds for unity. An interpretation along these lines should always be borne in mind when considering whether results in a certain field really do amount to reduction.

Reduction is sometimes resisted by those worried about the autonomy of their discipline. Again, reduction may be sufficient for calling autonomy into question, but is not necessary. Autonomy would be called into question if the unity of science involved not merely consistency, but such an intertwining of theories and areas of investigation that it is impossible to disentangle the putatively autonomous areas. With the development of physical chemistry at the end of the 19th century, for example, chemistry is, perhaps, so entwined with physics that what would be left after removal of physics is but a pale shadow of modern chemistry. It is, perhaps, not even clear what the removal of physics from chemistry would amount to. So several general positions can be initially distinguished: (i) unity in virtue of reduction, with no autonomous areas, (ii) unity in virtue of

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consistency and not reduction, but still no autonomy because of interconnections, (iii) unity in virtue of consistency and not reduction, with autonomous areas, and (iv) disunity. Dupré motivates his picture of disunity by drawing largely on biology (and cookery), and the arguments may seem not to carry over to chemistry, at least those parts which stand in close relation to physics. I will say a little more about non-reductionist conceptions of the unity of science presently. For the moment, I turn to the general conception of reduction.

There is little interest in the analysis presented by Kemeny and Oppenheim (1956) based on the positivists' distinction between observation and theoretical terms and the understanding of the advance to a more comprehensive theory simply in terms of the incorporation of older observation sentences alongside new. Apart from the difficulties with the distinction between observation and theoretical terms, their explicitly instrumentalist understanding of theories is hardly consonant with the intuitive atomism underlying reductionism motivated by the idea that all that happens is ultimately determined by what happens at the microlevel. This is a realist view, according to which laws governing processes and structure at the microlevel are taken to be fundamental, and the ultimate goal of science is to reduce all other descriptions of what goes on in the world to these fundamental principles of physics. Even if antirealism is taken seriously in the realms of quantum physics, in so far as reduction is motivated by atomism in chemistry, it is the realist picture that carries the burden of the argument.

A more incisive stab at the problem of defining reduction, associated particularly with Ernest Nagel's earlier writings and especially Chapter 11 of his 1961 book, *The Structure of Science*, treats the reduced theory as derived by deduction from the reducing theory. Deductive subsumption is a stronger requirement than the minimal notion of coherent coexistence (consistency) required by unification. It led Nagel to distinguish two cases. *Homogeneous* reduction involves no new concepts in the reduced theory relative to the reducing theory. He cites the incorporation of Galileo's law of fall and Kepler's laws within Newton's theory of mechanics and gravitation as examples. *Heterogeneous* reduction arises where the concepts of the reducing theory do not include all those of the reduced theory. There is no possibility of deducing statements formulated in a foreign vocabulary without a reinterpretation establishing a connection between the conceptual apparatus of the two theories in the form of what he calls *bridge laws*. Nagel gives the example of the reduction of the Charles–Boyle gas law to the kinetic theory of gases, where the concepts of temperature and pressure, which do not figure among the mechanical concepts of kinetic theory, are connected with averaged features of the motion of the constituent molecules. He suggests that this is illustrative of a more general reduction of classical thermodynamics to statistical mechanics.

Nagel's account of reduction has been heavily criticized on many fronts. A well-known opponent is Jaegwon Kim, who thinks it completely wrong and offers an alternative which involves "no talk of 'bridge laws' or 'derivation' of laws" (1999, p. 12). But I have elsewhere (Needham, 2009) criticized both Kim's diagnosis and his alternative, and he receives only brief mention here. A more serious criticism was, like so much of late 20th-century philosophy of science, anticipated by Duhem. Nagel's claim that the incorporation of Kepler's laws in Newton's more general theory exemplifies his conception of homogeneous reduction fails because:

> The principle of universal gravity, very far from being derivable by generalization and induction from the observable laws of Kepler, formally contradicts these laws. If Newton's theory is correct, Kepler's laws are necessarily false (Duhem, 1954, p. 193).

For according to Newton's theory, each planet moves not only under the influence of mutual gravitational attraction with the sun, which would result in elliptical orbits, but also under the influence of all the remaining mass in the universe, especially the other planets, which distort the regular elliptic path. Paul Feyerabend (1962) was later to see in essentially this same point the basis of his "meaning variance" thesis, according to which the meaning of terms in a theory is determined by the general principles of that theory, and to change the principles is to change the meaning of the terms. This "incommensurability thesis" challenged Nagel's conception of homogeneous reduction and the accumulative conception of scientific progress. But the startling conclusion that theory change entails meaning change does not follow from the explicitly formulated principle of the determination of meaning by the entire theory. Different conditions might well determine the same thing.

Duhem provides a much more sensible interpretation, to which I will return later. First I want to emphasize a positive feature of Nagel's analysis—one which has, I think, to be retained in any acceptable analysis of the concept of reduction.

## 2. Nagel's leading idea

The leading idea of Nagel's analysis is that the reduced theory is derived from the reducing theory. This entails that the reduced theory is really brought back to the reducing theory, and guarantees that it really is reduced to the reducing theory. Nagel offers no explicit argument to show that this tracing or bringing back must be understood in terms of "is logically deducible from". But it would have been obvious to him that this understanding of the reduction relation provides for properly identifying the reducing theory, and this is the feature I think has to be retained in any acceptable analysis of the concept of reduction.

In his famous analysis of Euclidean geometry, Hilbert (1902) showed that the five classical Euclidean axioms were not adequate to establish the theorems traditionally based upon them. By rigorously insisting on explicit proofs, he found that the traditional arguments contained lacunae, which could only be filled by adding further axioms. The traditional axioms did not, therefore, embody a proper statement of the theory of Euclidean geometry, and an expansion of the axioms which did threw into doubt some traditional claims about the theory. It could no longer be maintained that it was certain on the grounds that each of the axioms was so simple that its truth was obvious by the light of reason. Some of the axioms Hilbert found it necessary to add in order to fill out the gaps in the traditional arguments and provide rigorous proofs were quite long and complicated. The requirements of rigorous proof and deduction provide for the classical procedure of identifying gaps in arguments which must be filled either by better argument or reckoning with additional assumptions.

Similar points can be made about misleadingly oversimplified claims about reduction. It is said that classical thermodynamics is reducible to mechanics. But this has not been demonstrated, even if the general tenets of probabilistic reasoning are taken for granted along with logic and mathematical analysis. Substantial additional assumptions are required, as was clear to Boltzmann (see Uffink's (2004) review), and in a review of subsequent research, Sklar (1993, pp. 367–373) suggests that the only motivation for the additional hypotheses is the anticipated reduction.

When assessing a reduction claim, it is necessary to be clear about what the claim is, and this involves being clear about what, exactly, the reducing theory is, and not just what the proponent might think are the more striking postulates. There can be no tacit hidden premises; the entire theory must be brought into the light of day. A claim is no stronger than its weakest point, and may be distinctly less plausible when the full extent of the reducing theory is made clear, if for no other reason than sheer lack of independent motivation. Another reason might be circularity of one form or another. Blatant circularity in the form of a *petitio principii* is one thing. It is also common to speak of reduction of concepts, for example the reduction of macroscopic concepts to microscopic concepts. The familiar claim that temperature is the mean kinetic energy of the molecules is of this kind. There should be no circularity of concepts in the sense of a substantial use of the putatively reduced concept in the arguments justifying the official reduction. We will see that the familiar claim about temperature as mean molecular kinetic energy falls foul of this constraint.

Nagel's requirement of deducibility provides for the proper identification of the reducing theory. It is, admittedly, difficult to fulfill, but cannot just be given up without adequate compensatory precautions. Kim criticizes Nagel for being unrealistic and even trivial, and proposes to base a notion of reduction on the concept of causation rather than derivation of laws. But it is difficult to see how explicating the reduction relation in terms of causality can furnish a criterion of adequacy serving to properly identify the reducing theory. We speak of some underlying factor as causing a phenomenon, and it is of the nature of causality that the explicitly mentioned factor is but one among many others. Causation is typically used when it is impossible to account for all the relevant circumstances. But in that case, there can be no question of formulating a complete reduction claim, and the extent of the putative reducing theory remains unknown. At all events, to the extent that causality introduces a concept distinct from deduction, it raises the suspicion of resting on some not fully articulated notion which begs the question of reduction. The term "causation" is sometimes explicated in terms of deduction, and used in this sense, does not provide for an alternative to Nagel's account of reduction.

It might be said that deduction is not applicable in many areas where reduction is discussed, such as biology and philosophy of mind, and causation is more appropriate in these areas. But that is not sufficient to motivate replacing Nagel's account with a causation-based one. It certainly cannot motivate replacing Nagel's account in physics and chemistry where causation is not the prominent notion it is in philosophy of mind, say, and its adoption would be tantamount to assuming lack of unity, and so throwing out one of the reductionist's goals, from the outset.[1] Disunity, especially between the fields of human endeavor and physics, may well be the right position to adopt. My point is that a reductionist in the philosophy of mind is not going to establish a convincing case, or at least which contributes to the overall unity of science, by appealing to a concept like causation which cannot provide for the complete identification of the reducing theory.

## 3. Bridge laws and heterogeneous reduction

Nagel's distinction between homogeneous and heterogeneous reduction was mentioned in the Introduction. In order for it to be possible to deduce statements involving new vocabulary, connections must be established between these new concepts and those of the reducing theory in the form of sentences that Nagel called bridge laws. In contrast to Frege's project of showing number theory to be analytic by reducing it to logic, where Frege claimed that the definitions of number-theoretic concepts in terms of logical concepts were analytic, Nagel called the connecting principles bridge *laws* by way of emphasizing that they cannot be a priori, analytically true definitions. Nevertheless, an analogy with definitions has often been retained in the form of an assumption that bridge laws take the form of an equivalence. Many classic arguments against reduction in some area depend on this assumption, disputing the presumed bridge-law equivalences. Davidson made this assumption when arguing against the so-called mind-body identity thesis as part of his argument for anomalous monism. And after devoting the first part of his paper to questioning whether classical genetics is formalizable in logical terms as required by Nagel's analysis, Kitcher goes on in the second part of his (1984) paper to question whether general bridge laws can be established by criticizing universally quantified equivalences between Mendelian genetic properties and molecular properties.

The equivalence form of bridge laws is not a requirement imposed by Nagel's scheme, however, misleading claims to the contrary like that of Kim (2003, p. 568) notwithstanding. Nagelian reduction only requires a connecting principle facilitating deduction. Often, a one-way implication suffices rather than the corresponding equivalence assumed by bridge-law critics like those just mentioned. In fact, the implication "If Reducing Theory then Reduced Theory" would clearly do the trick of facilitating deduction, although there may be little reason to accept it in the absence of more modest connecting principles which more interestingly serve this same function. There may be several alternative connecting principles each adequate to the task of facilitating the deduction, so dismissing one does not show that no other could do the job. The upshot is not merely that the reductionist's claim might be construed unnecessarily strongly, devaluing the critical argument to the rhetorical device of artificially making the claim easier to criticize. A critique of any one bridge law leaves open the possibility that others might serve equally well, and so is inconclusive.

Several authors confuse criticism of Nagel's scheme with the imposition of their own conception of what the function of bridge laws should be, thinking of them as establishing what they call identities between properties. Rather than looking for weaker bridge laws, they suggest that the material equivalence adequate for Nagel's deduction requirement is too weak! It is not strong enough to express property identity, which is seen as a failing of Nagel's general account. But this should be distinguished from criticism internal to Nagel's original concerns, and provided with a separate motivation which can be compared with that offered for the Nagelian concept. Robert Causey (1972) was one of the first to introduce this idea that predicate or property identity is required for reduction, suggesting that material equivalence merely expresses a "correlation" and needs to be strengthened to predicate identity if a reduction is to provide a genuine explanation. But Causey never states a sufficient condition stronger than material equivalence for his "property identity", only giving a necessary condition in terms of successful explanation. Whether proponents of this alternative conception of reduction have made good sense of this stronger notion of predicate identity I leave for others to determine, and return to

[1] Sarkar (1998) defends reduction in many areas of biology on the basis of an understanding of reduction in which the reduced statements describe behavior of a system that can be explained in terms of interaction between spatial parts of the system. But he is clear that because of the requirements on molecular structure they impose, the lock and key mechanisms frequently invoked in biochemistry can only be described as reduction to what he calls macromolecular physics, which by his own account does not amount to, and perhaps even precludes, reduction to chemistry and physics (Sakar, 1998, pp. 147–149).

the weaker Nagelian conception of a bridge law as what facilitates deduction in the heterogeneous case.

Nagelian bridge laws are, we saw, contingent statements containing concepts from both the putative reducing and reduced theories which facilitate the deduction of the latter from the former. But in that case they are substantial additional claims over and above those of the putative reducing theory. Accordingly, the reduction does not trace back the reduced theory merely to the putative reducing theory, but to the combined theory consisting of this together with the putative bridge laws. It is this combined theory which is the real reducing theory—the theory which is identified by the deductive requirement as being sufficient for the reduction—which might be very well integrated. There are therefore no new terms in the reduced theory which do not appear in this theory. Insistence on Nagel's leading idea, that the reduction relation be understood in terms of deduction, thus leads to the dissolution of the distinction between heterogeneous and homogeneous reduction. There is just homogeneous reduction to a completely specified reducing theory.

## 4. Duhem's scheme: unity without reduction

What does the realization that the reducing theory cannot be regarded as doing without the concepts of the reduced theory, so that principles connecting, say, microscopic concepts with macroscopic concepts are part and parcel of the overall reducing theory, imply about the prospects for reduction and scientific unity? Duhem was opposed to the prevailing opinion of his day that progress and understanding in physics should be pursued by reducing phenomena to mechanics, and championed the view that thermodynamics should be understood as incorporating mechanics rather than being reduced to it:

> ...If the science of motion ceases to be the first of the physical sciences in logical order, and becomes just a particular case of a more general science including in its formulas all the changes of bodies, the temptation will be less, we think, to reduce all physical phenomena to the study of motion. It will be better understood that change of position in space is not a more simple change than change of temperature or of any other physical quality. It will then be easier to get away from what has hitherto been the most dangerous stumbling block of theoretical physics, the search for a mechanical explanation of the universe (Duhem, 1894, pp. 284–285).

Put another way, mechanics is expanded by the addition of concepts and principles from thermodynamics, and the whole new body of knowledge is integrated in a systematic new theory. All the old mechanics is incorporated, but in addition there is provision for the explanation of new phenomena. This includes phenomena involving temperature, heat and chemical reactions which could not even be expressed within the mechanical framework, but also mechanical effects requiring non-mechanical explanations. For example, pressure is a typical mechanical feature, but cannot be explained purely mechanically when arising as osmotic pressure, which calls for a distinction of chemical substances. Whereas mechanics only recognizes mass as the measure of the amount of matter, thermodynamics divides the mass into amounts of different substances making it up, and treats energy not simply as a function of mass, but as a function of the amounts of the different substances making up the mass of a body. This provides the materials for a thermodynamic explanation of osmotic pressure.

Concepts from traditional mechanics and the new ones from thermodynamics are intertwined in the theory. Pressure is identified with the negative derivative of energy with respect to volume. Energy and volume are typical mechanical concepts. But energy is treated, not just as a function of volume, but of entropy and the amounts of the various substances present in the system under consideration. Thus, pressure as the negative derivative of energy with respect to volume is related to the new thermodynamic concepts of entropy and chemical substance.

At a more general level, the relation between chemistry and physics can be considered from this point of view. Again, Duhem provides a good illustration. In the 19th century a distinction was drawn between chemical and physical properties. Properties like melting and boiling points were considered to be physical properties which could be used to distinguish substances (for example, isomers produced in a particular reaction) and perhaps even identify them (provide a general necessary and sufficient condition for being the substance in question). But these were regarded as chemical epiphenomena not essentially connected with the reactivity of a given substance with other substances, which was regarded as the specific domain of chemistry. Marcelin Berthelot appealed to this distinction in defense of the principle of maximum work, which had been proposed in the middle of the century by Julius Thomsen. In his first book, generally taken to be his ill-fated first doctoral thesis of 1884, Duhem formulates the principle as follows:

> All chemical action accomplished without intervention of extraneous energy tends towards the production of the system of substances which release the greatest heat. This principle entails the following consequence: of two conceivable reactions, the one the inverse of the other, and releasing heat while the other absorbs heat, the first alone is possible.
>
> The heat released by one reaction without bringing into play any external work is the decrease to which the *internal energy* of the system is subject by the effect of the modification. Consequently, in accordance with the rule proposed by Berthelot, the possibility of a reaction presupposes that this reaction produces a decrease in energy. The stability of a chemical equilibrium is therefore assured if the equilibrium state corresponds to the smallest value that the energy of the system can take. In a word, this rule has it that energy plays the role in chemical statics that the potential plays in statics proper (Duhem, 1886, p. ii).

Although there are many cases which conform with the principle, there are exceptions. It could only be saved by what Duhem held to be an illegitimate demarcation between chemical phenomena, alone subject to the law, and changes of physical state which Berthelot held to be exempt from the law:

> Sulphuric acid, for example, combines with ice and this combination produces cold. In order to bring this exception within the rule, the reaction must be divided into two phases: one part being the fusion of ice, a *physical* phenomenon which absorbs heat, and the other part, the combination of liquid water with sulphuric acid, a *chemical* phenomenon which releases heat. But it is by a purely mental conception, and not as a representation of reality, that it is possible to thus decompose a phenomenon into several others. Moreover, accepting that chemical phenomena obey the law of maximum work while physical changes of state would be free is to suppose that there is between the mechanism of these two orders of phenomena a line of demarcation which the work of Henri Sainte-Claire-Deville has removed (Duhem, 1886, pp. ii–iii).

The question was squarely placed within the general issue of the unity of science, and Duhem argues that Berthelot's appeal to the distinction between physical and chemical processes in defense of the principle of maximum work is ad hoc.

Berthelot's rule supposes that a chemical reaction produces a reduction in internal energy of the reacting material, and thus that a stable state of chemical equilibrium corresponds to the lowest possible value of energy of the system, just as does the stable state of a mechanical system. The failure of Berthelot's rule shows that energy alone cannot serve as the criterion of chemical equilibrium, and if the analogy with mechanical systems is to be upheld, a generalization of mechanics is required. Something other than energy must be found to play the role analogous to that which the potential plays in mechanics. Duhem goes on in his Introduction to the 1886 book to show how work in thermodynamics by Massieu, Horstmann, Helmholtz and Gibbs had led them to a better appreciation of the conditions governing chemical equilibrium, and proceeds to further develop the thermodynamic potentials, which were adequate to cover all cases of chemical equilibrium without ad hoc distinctions, in the body of his book.[2]

Duhem was concerned with the interrelation between mechanics and thermodynamics, and did not take atomic theories seriously. But the general idea of integration by expansion can incorporate microscopic theory, even if Duhem did not take this step, and this does seem to be how the relation between thermodynamics and statistical mechanics is generally understood. To illustrate, consider the case of temperature. This is a macroscopic concept which is given a definite meaning as an intensive magnitude measurable on a ratio scale with an absolute zero in thermodynamics, where it is equated with (or in some formulations, defined as) the derivative of the energy with respect to the entropy. Philosophers seem have taken from Nagel the thesis that temperature is the average kinetic energy of the molecules of the body concerned, at least where this is a gas. But having a certain average molecular kinetic energy cannot be a sufficient condition for a gas having a temperature. The molecules of a gas in any condition will have an average kinetic energy, but the macroscopic requirement for a body having a temperature is that it is specifically at equilibrium, and not just in any condition. So only for a gas at thermodynamic equilibrium is the temperature equal to the average molecular kinetic energy. There is a microscopic condition corresponding to the macroscopic condition of being at thermodynamic equilibrium, namely that the energy has the Boltzmann distribution, according to which the number of particles, $n_i$, with energy $U_i$ in relation to the number of particles with the minimal energy, $n_0$, is $n_i = n_0\, e^{-U_i/kT}$. Here $k$ is Boltzmann's constant and $T$ is the temperature. As a definition, then, the claim that the absolute temperature of a gas is the average kinetic energy of the gas molecules provided they exhibit the Boltzmann distribution of energy is circular, since the Boltzmann distribution presupposes the temperature.

The relation between thermodynamics and statistical mechanics is not normally understood to be vitiated by circularity, however, which suggests that it is a mistake to construe this relation as one of reduction. It is rather a case of the one complementing the other. This becomes clearer by considering the derivation of the Boltzmann distribution. The usual argument applies Legendre's method of undetermined multipliers, which introduces two terms which have to be determined. Just considering the last stages of the argument, $n_i$ has been expressed in the form $n_i = n_0\, e^{\beta U_i}$ and it remains to determine $\beta$. From Boltzmann's statistical interpretation of entropy in terms the number of ways the particles can be distributed over the energy levels without changing the total energy, an expression for the entropy, $S$, is derived (with the help of Stirling's approximation). This expression contains $\beta$ and expresses $S$ as a function of the total energy, $U$, from which it follows that the expression can be differentiated with respect to the total energy, $U$. The result is

$$\left(\frac{\partial S}{\partial U}\right)_V = -k\beta. \tag{1}$$

The hypothesis about the microscopic conditions takes us this far. In order to finally determine $\beta$, classical macroscopic thermodynamics is now called upon in the form of the identity mentioned above involving the temperature, or rather the inverse of this, namely

$$\left(\frac{\partial S}{\partial U}\right)_V = \frac{1}{T}. \tag{2}$$

The right-hand sides of (1) and (2) can then be identified, implying $\beta = -1/kT$ and yielding the expression for the Boltzmann distribution given above.

Not only does the expression of the Boltzmann distribution refer to the temperature; its derivation calls on the fundamental condition governing temperature in the macroscopic theory. Clearly, what this illustrates is not part of a reductionist program, but the integration of macro- and microtheory in a joint venture which shows that temperature is clearly related to the distribution of energy over the constituent molecules in a gas. There are many other such connections. The application of thermodynamics requires equations of state specifying functional relations characteristic of specific systems. Traditionally, these were provided by empirical methods which delivered the gas laws, Henry's law relating the solubility of a volatile constituent of a solution to its partial pressure in the gas phase over the solution, Raoult's law relating the vapor pressure of the solvent to its concentration, and so forth. Statistical mechanics complements thermodynamics by providing an alternative route to equations of state often not easily accessible by the traditional experimental methods.

From the Duhemian perspective, there is no preconceived notion of "fundamental law" or "basic property". Microscopic principles complement macroscopic theory in an integrated whole, with no presumption of primacy of the one over the other. From the long historical perspective, the body of systematic theory has been seen to grow. If, as seems reasonable, it will continue to do so in the future, who knows what additions may accrue? There is no telling whether, or to what extent, Duhem's dream of a unified science will be realized. Perhaps it just will not prove possible to bring the various threads together into a consistent, systematically unified whole. We might introduce the term "physical property" for whatever properties feature in this future science, be it unified or not. But there is no telling what the term "physical property" might or might not cover. The so-called closure principle favored by reductionists has no real force. In Kim's formulation, the claim that "If a physical event has a cause at $t$, it has a physical cause that occurs at $t$" (Kim, 2005, p. 43) is at best an empty tautology, to the effect that whatever eventually yields to systematic theory eventually yields to systematic theory.

Another aspect of the long historical perspective is that as theories are elaborated, their internal structure is ever more deeply investigated. In the course of this process, it may well turn out that concepts turn out to be definable and whole theories prove reducible. Duhem resisted reduction to mechanics as a general criterion of explanation, and maintained on the contrary that mechanics is subsumed as a special case of general thermodynamics in which the entropy is constant. He was wary of adopting at the outset a commitment of reduction to some preconceived base, which he took to be foreign to the enterprise of empirical science. The primary point is that adequate tools have to be introduced to deal with phenomena, and reduction in terms of

[2] This view of the integration of physics and chemistry is further elaborated in Needham (2008).

preconceived notions of what is understandable should not be allowed to obstruct this process. But certainly with the introduction of new concepts and principles, logical streamlining may prove possible. This brings us back to Nagel's concept of reduction.

## 5. Approximation

As we saw in the Introduction, Duhem was well aware that when scientists speak of the derivation of a law or theory from another theory, it is rarely deduction that they have in mind, but argument by approximation. Newton's laws might be so contrived that Kepler's laws are deduced from them—as a textbook exercise under the pretense that each planet moves only under the influence of the sun. But as Duhem points out in the passage quoted in the Introduction, the fact that there are several planets means that this contrivance cannot be sustained on the basis of Newton's laws, according to which all masses are sources of gravitational attraction. The fact that the planets, which would have elliptical orbits if they were each moving solely under the influence of the sun, do not move solely under the influence of the sun and therefore cannot have elliptical orbits according to Newton's theory does bring out a shortcoming of Nagel's account of reduction. But it does not involve the nonsense of "meaning variance" and "incommensurability" which have been taken to challenge Nagel's conception of homogeneous reduction and the accumulative conception of scientific progress. It requires that the role of approximation in science be recognized.

Feyerabend's primary target was the account of cumulative progress based on Nagel's conception of reduction. He objected that, strictly speaking, the laws of Galileo and Kepler do not follow from Newton's, but stand in contradiction with them. In the case of Kepler's laws, since the planetary orbits are not perfect ellipses because of the perturbations on the solar attraction of any one planet by the others, Feyerabend who took the view that the meaning of the central expression in terms of which the laws are expressed is determined by those laws, concluded that the same expressions mean different things in the context of the different theories of Kepler and Newton. Nagel's reductionist thesis presupposes, he says, the meaning invariance of these expressions, and this prerequisite is not satisfied. Accepting Newton's theory would therefore mean abandoning Kepler's laws. Duhem, on the other hand, argues that Kepler's laws are only approximately true according to the criterion established by Newton's laws. Whatever support there is for Newton's theory from the motion of the planets results from calculating the perturbations on the orbit of any given planet induced by the others on the basis of Newton's laws, and comparing these with observation. There are two aspects to the use of approximation which might be distinguished here.

First, a particular hypothesis cannot be considered independently of those factors delimiting the degree of approximation with which it is purportedly upheld at a given point in history. Duhem argued against Poincaré that in interpreting formulas as statements of physical theory, we should not draw consequences by importing to them a more definite determination than experiment can support (Needham, 1998, pp. 48–50). Moreover, in confining interpretation within these limits, we must take into account all the considerations leading to the interpretation which observation does sustain. Such considerations are drawn from the whole body of knowledge at the scientist's command; they are not easily delimited and enumerated, but cannot reasonably be ignored. Accordingly, reduction should be formulated so as to accommodate facts such as that although Kepler's laws are based on the hypothesis of elliptic orbits, it was the increased precision of Tycho Brahe's observations that allowed Kepler to distinguish an elliptical form of Mars' orbit from that ascribed by his predecessors:

> Since many of Tycho's determinations of the angular distances between stars were checked by two other observers, it was known that his results could generally be trusted to within two or three minutes of arc. Both Ptolemy ... and Copernicus speak of a 10-minute error as being tolerable for observational purposes. Without Tycho's reduction of the expected error Kepler's discoveries could not have been made (Wilson, 1972, pp. 93–94).

Kepler constructed a curve which fitted within a narrower margin of error, but well understood that his own elliptical hypothesis could be justifiably upheld only within certain limits, albeit narrower than those within which Ptolemy and Copernicus believed their claims justified. By parity of reasoning, observations conforming to yet narrower limits of error might exclude an elliptic orbit.

It is unreasonable to interpret Kepler's claims, as Feyerabend does, without regard to the limits of experimental error within which he confined himself. When interpreting formulas as statements of physical theory rather than claims of pure mathematics, we should not draw consequences by importing to them a more definite determination than experiment can support. Once this is taken into account, the idea that earlier theories are separated from later ones by an abyss which cannot be bridged by rational argument, making the views of the respective theorists incommensurable, is no longer plausible, and no longer poses a threat to the accumulative conception of science.

But if approximation saves Nagel from Feyerabend's attack, it also calls for a relaxing of the deduction condition, and this is the second aspect of approximation to be reckoned with. The perturbations of the elliptic orbits were not deduced from Newton's laws, but calculated "with all the high degree of approximation that the constantly perfected methods of algebra involve", as Duhem (1954, p. 193) puts it. What, on the Newtonian theory, is the proper statement of the circumstances governing the motion of a planet involves a many-body problem in the form of a differential equation which has no analytic solution. Numerical methods must be used, whose closeness to the exact solution can sometimes be estimated. But as a term describing what is involved in tackling differential equations, "approximation" covers more than this. Often, further assumptions will be introduced, perhaps by appeal to a model of the situation of interest, resulting in a simpler equation. This is the rule in science, and the application of a straight deduction is definitely the exception, so unless Nagel's account can accommodate reasoning by approximation, it will be left on the sidelines as a largely inapplicable ideal.

Being an ideal cannot detract from the value of providing a criterion for the proper identification of the reducing theory, which is embodied in the deductive requirement. But this virtue could be retained if the notion of argument connecting reducing theory with reduced theory were relaxed to include argument by strictly numerical methods. The incorporation of further assumptions by appeal to models and other sources of relevant information about the situation of interest must be clearly distinguished as such, and understood as adding to the reducing theory rather than forming part of the general logical and mathematical apparatus determining the validity of argument. The derivation of the Boltzmann distribution discussed above, for example, appeals to the Stirling approximation, which as a purely numerical argument involving no additional physical assumptions. The criticism of the reduction of temperature found no fault with this aspect of the argument. But statements about the restricted range of validity, which the use of approximative arguments frequently involves, should be included in the additional assumptions, however. The solution of the equation for the simple harmonic motion of a pendulum, for example, is based on the approximation that $\sin\theta \approx \theta$, which only holds good for small angles. This may well have implications for the hierarchy often associated with reductionist claims. Successful reduction of A to B and of B to C may not entail the successful reduction of A to C if the reductive arguments are approximative and involve different ranges of validity.

Woolley's criticism of the reduction of molecular structure to quantum mechanics can be understood in terms of the additional assumptions introduced by approximation, his point being that the argument involving appeal to the Born–Oppenheimer approximation should be understood as involving an assumption which should be counted as part of the reducing theory. But then, as in the case of temperature, the construal of the argument as a reduction can be criticized on grounds of circularity. Molecular structure "is ... not securely founded in quantum theory" (Woolley, 1978, p. 1077), but is introduced "by hand" (Woolley, 1988, p. 56) when the Born–Oppenheimer approximation is employed in solving the Schrödinger equation applied to molecules. Sutcliffe and Woolley (unpublished) still find that "it still remains to justify the treatment of the nuclei that [a satisfactory account of chemical structure] involves by full quantum mechanical means".

## 6. Ontological reduction

In the philosophy of chemistry literature an unhappy distinction is often made between epistemological and ontological reduction (see, for example, Le Poidevin, 2005; Lombardi & Labarca, 2005; Vemulapalli & Byerly, 1999). This is unfortunate because although a distinction can be drawn between what the relation itself involves and knowing whether these conditions obtain, the possession of such knowledge does not amount to a kind of reduction. A more suitable term for what seems to be contrasted with ontological reduction in this literature, suggested (cf. Hendry & Needham, 2007) by speaking of reducing and reduced theories, is "intertheoretic reduction". Bearing in mind that the reduced theory might take the form of a single sentence, however, and that an ontological claim is an existence claim, formulated by an existential sentence, then ontological reduction would be a special kind of reduction and not something standing in contrast with intertheoretic reduction. But what people seem to have in mind when speaking of ontological reduction is an eliminative idea, sometimes based on reduction of primitive terminology by definition, sometimes expressed as the claim that something is "nothing but" something else, whatever that amounts to.

Hempel saw bridge laws as a vehicle for the definitional elimination of terms from the primitive theoretical vocabulary:

> Full reduction of concepts in this strict sense would require, for every term of [the reduced theory], a connective law of biconditional form, specifying a necessary and sufficient condition for its applicability in terms of concepts of the [reduced theory] alone. Such a law could then be used to "define" the [reduced theory] term and thus, theoretically, to avoid it (Hempel, 2001, p. 198).

We have seen that connecting laws need not take the form of equivalences. In fact, since the equivalences are stronger than the one-way conditionals which often suffice to facilitate deduction, the definitions would require supplementation with entirely superfluous additional information, which is contrary to what talk of elimination might suggest. But with the disappearance of heterogeneous reduction we can see that there can be no question of eliminating concepts from the reduced theory by reduction. Any candidates for elimination by reduction must be part of the reducing theory, and acknowledged if the reducing theory is.

Reducing to primitive terminology by definition simplifies metalinguistic proofs by induction on the length of formulas, but it should not be inferred from this that the concepts are eliminated. Consider the notion of entropy, which Clausius *introduced* by showing how it could be defined once he had formulated the second law of thermodynamics. True, he never used the concept in his development of thermodynamics, eliminating energy and entropy as soon as possible in favor of the original thermodynamic concepts of heat and work (Klein, 1978, pp. 331–332). It was Gibbs who first put the concept of entropy to serious use in characterizing systems entirely in terms of functions of state, taking a great stride forward in illuminating the structure and potential application of thermodynamics and showing it to be a theory of systems at equilibrium. The concept is not eliminated just because it is defined. More recent formulations of thermodynamics take the extensive magnitudes of energy and entropy as primitive. It would be equally inappropriate to suggest that the definition of temperature that is possible in these latter formulations shows that the concept of temperature is eliminated. Choosing a formulation of the theory does not amount to recognizing just the primitive concepts of that particular formulation and rejecting the defined concepts.

Marshall Spector (1978) stresses the idea of replacement in his discussion of reduction, in the light of which he interprets the widely accepted reduction of temperature to the average molecular kinetic energy as involving elimination:

> From the theoretical point of view, there is no further need for the ontology of thermodynamics.... For example, we may now speak of the *mean kinetic energy of the molecules* instead of the *temperature* of a sample of gas. This may be put more forcefully by saying that temperature doesn't exist, *meaning thereby* that classical dynamics plus "atom theory" has been found to be fully adequate for the description of those phenomena for which the concept of temperature had previously been used—the old domain of thermodynamics (Spector, 1978, p. 60).

Interestingly, Spector must have been aware of the facts known since the early years of the 20th century, that atoms consist of electrons and a nucleus, yet displayed no inclination to deploy the same kind of argument against the existence of atoms and molecules. As mentioned in the Introduction, reduction is often motivated by a realist view of the ontology of the putative reducing theory, which is arbitrarily excepted from this sort of ontological reduction.

It would seem that the claim that "there is no further need for the ontology of thermodynamics" confuses what has happened to phlogiston, caloric, and suchlike, which have been eliminated with the progress of science, with theories that have been reduced with the progress of science. Theories of the latter category have, by contrast with phlogiston and caloric, been retained, albeit sometimes in more precisely articulated form where the reducing argument is approximative and experimental error has been improved. Were it true that thermodynamics had been reduced, it certainly would not have been eliminated with the progress of science. It has not even been adjusted, for "Gibbs's work [in thermodynamics]", as a contemporary assessment has it, "... has required no correction since it was published, and remains to this day the foundation for the study of phase separation. The underlying principles are few, and rigorous" (Sengers, 2002, p. 43).

Another ground for elimination might be found in considerations which have led some to recognize the multiple realizability of concepts and others to reject the common concept supposedly multiply realized. Kim is a representative of the latter school of thought, holding quite generally that "because of its causal/nomic heterogeneity, [such a common concept] is unfit to figure in laws, and is thereby disqualified as a useful scientific property" (Kim, 1999, p. 18). Thus, gas temperature, radiation temperature, the temperature of a body undergoing a solid–liquid phase change and the temperature of the nuclear spin system of lithium nuclei in a lithium chloride crystal subject to a magnetic field, for example, lacking a common realiser (there is no average kinetic energy to play the role in the latter two cases that it supposedly does in the first, and the increase in average kinetic energy in the third does not correspond to an increase in temperature), would not really exemplify a common feature of temperature. But Kim is wrong. The common notion of temperature rests on the general theory of thermodynamics, which is independent of the particular features distinguishing one temperature-bearing entity from another, and the qualification "absolute" is intended to signify the independence of this paradigmatic scientific concept of any specific features of realization.[3]

## 7. Conclusion

An analysis of reduction is important for the advocate and opponent of reduction claims alike. But recent literature is ambivalent. Nagel's analysis is usually presupposed when a reduction thesis is criticized. Thus, although Kitcher (1984) objects to reduction in biology by first arguing that biology cannot be formalized as Nagelian reduction would require, he goes on to argue against Nagelian bridge principles. On the other hand, arguments in support of a reduction thesis usually call upon some other analysis. Unless the pros and cons of reduction are argued by appeal to the same criterion, however, protagonist and antagonist will be arguing at cross purposes, and specific reductions will not contribute to the erstwhile aim of the overall unity of science.

Nagel's analysis, modified to accommodate argument by approximation, has the virtue of providing for the complete identification of the reducing theory, which genuine alternatives fail to do. This provides a safeguard, which must surely be incorporated in any adequate account of reduction, against the promotion of reduction claims on the unacceptable basis of incomplete description of the reducing theory. The condition that the reducing theory really includes all that is necessary for the inference to the reduced theory furnished grounds for rejecting the philosopher's traditional claim that temperature is the average kinetic energy of the molecules constituting the body in question because equilibrium is essential. It also led to the collapse of Nagel's distinction between heterogeneous and homogeneous reduction. The putative bridge principles are part of the reducing theory, perhaps simply as ungainly additions, although the connections may be so well integrated into the theory that they are less easily distinguished. But now if the reducing theory must be seen as treating both microscopic and macroscopic concepts, for example, within the framework of the same theory, it is natural to understand historical developments in terms of the putative reduced theory being extended by, rather than reduced to, the putative reducing theory in a common theory construction, much as Duhem proposed. This certainly seems to correspond better than the reductionist's account with how the relation between thermodynamics and statistical mechanics is understood in the standard textbooks.[4]

[3] Marras (2002, pp. 241–243) makes a good case for the same point with respect to significant psychological properties.

[4] A referee notes that in a dialogue with Thomas Kuhn, Werner Heisenberg made the point that the discovery of Relativity brought about incorporation of Newtonian Mechanics into a larger theoretical structure—rather than replacing or negating it, as is often supposed.

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