Who's Afraid of Nagelian Reduction?

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Abstract

We reconsider the Nagelian theory of reduction and argue that, contrary to a widely held view, it is the right analysis of inter-theoretic reduction. For one, its purported successor, so-called new wave reductionism, turns out collapses into a sophisticated version of Nagelian reduction and hence does not provide an alternative. For another, the alleged difficulties of the Nagelian theory either vanish upon closer inspection, or turn out to be interesting philosophical questions rather than knock-down arguments.

Contents

1 Introduction

The purpose of this paper is to examine synchronic inter-theoretic reduction, i.e. the reductive relation between pairs of theories each of which describes the same phenomena and which are simultaneously valid to various extents.[1] Examples of putative synchronic inter-theoretic reductions are macro economics to micro economics, chemistry to atomic physics, and thermodynamics (TD) to statistical mechanics (SM). The latter will be our test case: what is it to say that TD reduces to SM?

The central contention of this paper is that Nagel's account of reduction essentially gives the right answer to this question. We first turn our attention to the Nagelian model of reduction and consider some of the problems that it allegedly faces. We then discuss its successor, so-called new wave reductionism (NWR), which is nowadays commonly advocated in its place. Our conclusion is twofold. First, we argue that upon closer inspection NWR collapses into a sophisticated version of Nagelian reduction, which, for reasons that will become clear as we proceed, we refer to as Nagel-Schaffner Reduction (NSR). Hence, received wisdom notwithstanding, NWR does not provide an alternative to NSR. Second, we reconsider the alleged difficulties of NSR and conclude that not only are they far from being as insurmountable as it they are often said to be; in fact most of them vanish upon closer inspection and those that don't turn out to be interesting philosophical issues rather then knock-down arguments. So NSR is alive and well and can be used as a regulative model for reductionist research programmes.

[1] There are, of course, various other types of reductive relations, most notably diachronic theory reductions an example of which is Newtonian and relativistic mechanics. See Nickles (1975). For an in-depth discussion of such cases see Batterman (2002).

2 Statistical Mechanics - A Reductionist Enterprise

SM is the study of the connection between micro-physics and macro-physics. TD correctly accounts for a broad range of phenomena we observe in macroscopic systems like gases and solids. It does so by characterizing the behavior of such systems as governed by laws which are formulated in terms of macroscopic properties such as volume, pressure, temperature and entropy. The aim of statistical mechanics is to account for this behaviour in terms of the dynamical laws governing the microscopic constituents of macroscopic systems and probabilistic assumptions.

There is a broad consensus, among physicists and philosophers alike, that SM is a reductionist enterprise. The following quotes are indicative of this:

> 'We know today that the actual basis for the equivalence of heat and dynamical energy is to be sought in the kinetic interpretation, which reduces all thermal phenomena to the disordered motions of atoms and molecules' (Fermi 1936 p.ix).
>
> 'The explanation of the complete science of thermodynamics in terms of the more abstract science of statistical mechanics is one of the greatest achievements of physics.' (Tolman 1938, 9)
>
> 'The classical kinetic theory of gases is [a] case in which thermodynamics can derived nearly from first principles.' (Huang 1963, Preface)

Further statements pulling in the same direction can be found in Dougherty (1993, 843), Ehrenfest & Ehrenfest (1912, 1), Goldstein (2001, 40), Khinchin (1949, 7), Lebowitz (1999, 346), Ridderbos (2002, 66), Sklar (1993, 3) and Uffink (2007, 923).

What is meant by reduction? That practitioners of SM do not really discuss the issue is not really a surprise; however, it should rise some eyebrows that by and large philosophers working on the foundations of SM also only rarely address this issue. So the pressing question remains: what notion of reduction is at work in the context of TD and SM?

Different statements of the reductive aims of SM emphasise different aspects of reduction (ontological, explanatory, methodological, etc.), but all agree that a successful reduction of TD to microphysics involves the *derivation* of the laws of TD from the laws of microphysics plus probabilistic assumptions. This has a familiar ring to it: deducing the laws of one theory from another, more fundamental one, is precisely what Nagel (1961) considers a reduction to be. Indeed, the Nagelian model of reduction seems to be the (usually unquestioned and unacknowledged) 'background philosophy' of SM.

One could lay the case to rest at this point if Nagel's model of reduction was generally accepted as a viable theory of reduction. However, the contrary is the case. As is well known, the Nagelian model of reduction was from its inception widely criticised, and is now generally regarded as outdated and misconceived. Representative for a widely shared sentiment about Nagel's account is Primas, who notes that 'there exists not a single physically well-founded and nontrivial example for theory reduction in the sense of Nagel...' (1998, 83).

This leaves us in an awkward situation. On the one hand, if Nagel's account really is the philosophical backbone of SM, then we have an (allegedly) outdated and discarded philosophy at work in what is generally accepted as the third pillar of modern physics alongside relativity and quantum theory! This is unacceptable. If we want to stick with Nagelian reduction the criticisms have to be rebutted. On the other hand, if, first appearances notwithstanding, Nagel's account is not the philosophical backbone of SM, what then is? In other words, the question we then face is: what notion of reduction, if not Nagel's, is at work in SM?

This dilemma is not recognised in the literature on SM, much less seriously discussed. But when raised in informal discussion – at receptions and in the corridors of conference hotels – one is usually told to embrace the second option: Nagelian reduction *is* outdated and discarded but the so-called 'New Wave Reductionism' associated with the work of Churchland and Hooker provides a model of reduction that avoids the pitfalls of Nagelian reduction while providing a viable philosophical backbone of SM. In what follows we argue that this is an empty promise.

Before delving into the discussion of different accounts of reduction, let us introduce two cases against which we test our claims: the Ideal Gas Law and the Second Law of thermodynamics. These are generally considered to be paradigm cases of reduction and hence serve as a benchmark for accounts of reduction.

Ideal Gas Law. The state of a gas is specified by three quantities: pressure p, volume V, and temperature T. A gas is ideal if it consists of particles without spatial extension (point particles) which do not interact with each other. Needless to say, there are no ideal gases in nature, but as long as the pressure is low, real gases can be treated as ideal gases to a very good approximation (since the volume of molecules is extremely small compared to the volume occupied by the gas and the inter-molecular forces are negligable). If such a gas is in equilibrium (i.e. if it is evenly distributed over V, and p and T do not change over time), volume and temperature are related to one another by the so-called Ideal Gas Law: $p\,V = k\,T$, where k is a constant. Let us call this law together with the qualifications about its scope the *thermal theory of the ideal gas.*

Consider a gas consisting of n particles of mass m confined to a volume V, for instance a vessel on the laboratory table. Each particle has a particular velocity $\vec{v}$, and its motion is governed by Newton's equations of motion. Assume, furthermore, that we are given a velocity distribution $f(\vec{v})$, specifying what portion of all particles move in direction $\vec{v}$. The exact form of this distribution is immaterial at the moment. Let us call Newtonian mechanics plus the assumptions just mentioned the *kinetic theory of the ideal gas.* The aim now is to derive the law of the thermal theory of the ideal gas from the laws kinetic theory.[2]

[2]For details see Greiner *et al* (1993, 12-15) or Pauli (1973, 94-103).

Pressure is defined (in Newtonian physics) as force per surface: $p = F_A/A$, where A is surface (for instance a section of the kitchen table) and F_A the force acting perpendicular on the surface (for instance the gravitational force exerted on the table by a glass placed on it). If a particle crashes into the wall of the vessel and is reflected it exerts a force onto the wall, and the exact magnitude of this force follows immediately from Newton's equation of motion. We now assume that all particles in the gas are non-interacting and perfectly elastic point particles. Then consider a wall in the $x - y$ plane. Some purely algebraic manipulations then show that the pressure exerted by the gas on that wall is

$$p = \frac{m\,n}{V}\int_{-\infty}^{\infty} d^3v\, f(\vec{v})v_z^2 = \frac{m\,n}{V}\langle v_z^2\rangle, \tag{1}$$

where v_z is a particle's velocity in z-direction. This equation says that the pressure exerted on a wall in the $x-y$ plane is proportional to the mean quadratic velocity in z-direction of all the particles in the gas. We now assume that space is isotropic, meaning that no direction in space is in any way special and that for this reason $f(\vec{v})$ is the same in all spatial directions. From this assumption it immediately follows that:

$$\langle v_x^2\rangle = \langle v_y^2\rangle = \langle v_z^2\rangle, \tag{2}$$

and since, by definition, $\vec{v}^2 = v_x^2 + v_y^2 + v_z^2$ we have

$$p = \frac{m\,n}{3V}\langle \vec{v}^2\rangle. \tag{3}$$

The kinetic energy E_{kin} is defined as $m\vec{v}^2/2$, and hence this equation becomes

$$pV = \frac{2n}{3}\langle E_{kin}\rangle, \tag{4}$$

where $\langle E_{kin}\rangle$ is the average kinetic energy of a particle, and hence $n\langle E_{kin}\rangle$ the average kinetic energy of the gas. Now compare Equation 4 with the Ideal Gas Law, $p\,V = k\,T$, which yields

$$T = \frac{2n}{3k}\langle E_{kin}\rangle. \tag{5}$$

The upshot of these calculations is that if we associate the temperature T with mean kinetic energy of a particle, then the Ideal Gas Law follows from Newtonian physics (here the equation of motion and the definitions of pressure and kinetic energy) and auxiliary assumptions (that the molecules are non-interacting point particles and that the velocity distribution is isotropic).[3]

Second Law of Thermodynamics. The second law of thermodynamics states that in an isolated system the thermodynamic entropy S_T cannot decrease, which is equivalent to saying that transitions from equilibrium to non-equilibrium states cannot. The aim of reductionism is to derive this law from first principles. The details of such a derivation are too complicated to be presented here, but the main ideas are the following.[4] We begin by carving up the system's state space into disjunct regions M_i which we associate with macrostates of the gas. We then define the Boltzmann entropy as $S_B = k_B \log[\mu(M_t)]$, where M_t is the region in which the system's microstate is at time t and $\mu(M_t)$ is the Lebesgue measure of that region (the Lebesgue measure is the generalisation of the 'ordinary' three dimensional volume to higher dimensional state spaces). The main challenge then is to show that the dynamics of the system is such that S_B increases and reaches its maximum when the system reaches equilibrium. Such a proof involves various assumptions about the system, most notably the so-called Past Hypothesis and some dynamical property such as being chaotic. For the sake of argument, let us assume that this can be shown (which, in fact, is a matter of controversy). It is then generally accepted that we have reduced the Second Law of TD to SM.

Two points deserve attention. First, the reduction, even if successful, is only approximate. The thermodynamic entropy is static in equilibrium: once it reached equilibrium it does not change any more. The Boltzmann entropy, by contrast, fluctuates. This is generally deemed to be unproblematic because the fluctuations are very small and S_B stays close to the equilibrium value most of the time. Second, the reduction associates S_T and S_B - only when we assume that the Boltzmann entropy is the thermodynamic entropy do we obtain (an approximate version of) the Second Law. But why can we identify

[3]Notice that this argument does *not* depend on the velocity distribution being the Maxwell-Boltzmann distribution.

[4]For a discussion of the details of this derivation as well as the difficulties that occur see Frigg (2008) and Uffink (2007). Furthermore, we here only discuss Boltzmannian SM.

the two? The fact that S_B has the same formal properties as S_T – it increases in time and reaches its maximum – is not enough to justify this identification; we also need a proof that the two quantities coincide at equilibrium in the sense that they have the same values and, what is more, have the same functional dependences of other crucial variables (Emch and Liu 2002, 98-102).

3 Nagelian Reduction

We first introduce what we call the Nagel-Schaffner model. Next we present some problems it purportedly faces.

3.1 The Nagel-Schaffner Model

On Nagel's account (1961, 353-354), a theory, T_P (here TD) reduces to another theory, T_F (here SM) iff the laws of T_P can be deduced from the laws of T_F and some auxiliary assumptions.[5] The auxiliary assumptions are typically idealisations and boundary conditions. Nagel also assumed that a theory's vocabulary was neatly divided into observational and theoretical terms, and that (trivially) observational terms have meaning independently of the theoretical context. He then postulated two conditions for successful reduction. *Connectability* requires that for every theoretical term in T_P there be a theoretical term in T_F that corresponds to it. *Derivability* says that given connectability the laws of T_P can be derived from the laws of T_F plus auxiliary assumptions. In this case we call T_F the *reducing theory* and T_P the *reduced theory.*

For Nagel there are two classes of reduction: homogeneous reductions and heterogeneous reductions. In *homogeneous* reductions the two theories share the same relevant predicates. In this case the connectability requirement is trivially satisfied. The often given example of some such reduction is that of Kepler's theory of planetary motion and Newton's mechanics: the latter contains all the relevant terms of the former and therefore the deduction of Kepler's laws from Newtonian mechanics bears no conceptual complications. If the theories do

[5] The indexes 'P' and 'F' stand for 'phenomenological' and 'fundamental' respectively. This just an *aide-mémoire* and nothing depends on it.

not share predicates in this sense, the putative reduction is *heterogeneous*. In this case it is clearly not possible to derive the laws of T_P from T_F, as the laws of the latter are couched in terms of these predicates. To overcome this difficulty Nagel postulates that there be so-called bridge laws which connect the vocabulary of T_P to that of T_F by providing 'rules of translation' specifying how one language translates into the other.

The above examples make this clear. Take the Ideal Gas Law first. Volume and pressure are terms that both theories share (they are defined in the same way in both theories). But while the thermal theory of the ideal gas talks about about temperature, the kinetic theory talks about mean kinetic energy. The vocabularies of the two theories are incongruent and therefore starting with kinetic theory one cannot possibly derive Ideal Gas Law, which are couched in terms of different predicates. So we need a bridge law to overcome this difficulty. In this example the bridge law is Equation 5. From a Nagelian point of view, then, the above case can be summarised as follows (Argument 1):

> *Premise 1*: Kinetic theory – The posit that a gas is a collection of molecules obeying Newton's laws of motion, the definitions of pressure and kinetic energy, and the existence of the velocity distribution $f(\vec{v})$.
> *Premise 2*: Auxiliary assumptions – Space is isotropic, and all particles are point particles that do not interact with each other and are reflected elastically from the walls.
> *Premise 3*: Bridge law – Equation 5.
>
> ---
>
> *Conclusion*: $p\,V = k\,T$.

This model of reduction has been criticised on different grounds. The first points out that Nagel formulated his theory in the framework of the so-called syntactic view of theories, which regards theories as axiomatic systems formulated in first order logic whose non-logical vocabulary is bifurcated into observational and theoretical terms. This view is deemed untenable for many reasons, among them that first order logic is too week to adequately formalise theories and that there is no clear line to be drawn between observational and theoretical terms.[6] This, so one often hears, renders Nagelian reduction untenable.

This is too quick. While it is true that Nagel was a proponent of the syntactic view and discussed reduction within that framework, the syntactic view is irrelevant to get the account off the ground. This becomes clear from the above example: neither did we present a first order formulation of the theory nor did we even mention a bifurcation of the vocabulary into theoretical and observational (it is irrelevant wether we regard temperature as observable or theoretical), and yet we have given a Nagelian reduction of the thermal theory of the ideal gas. Its positivistic roots should not detain us from using Nagel's model of reduction: we replace first order logic with any formal system that is strong enough to do what we need it to do, and the bifurcation of the vocabulary into observational and theoretical plays no role at all.

A more serious objection emerges when we try to give a Nagelian rendering of our second example. As we have seen, it is not possible to derive the exact Second Law of thermodynamics since the Boltzmann entropy fluctuates in equilibrium, which the thermodynamic entropy does not. And this is not an exception. It is almost never the case that one can derive the *exact* laws of the reduced theory, and hence Nagelian reduction is *de facto* unrealisable.

While this is true, the above example also indicates that *exact* derivability is too strong a requirement for successful reduction. To reduce the Second Law it was enough to derive a law that looks in essential ways very much like the Second Law. And the same is true in other cases: it suffices to deduce laws that are only approximately the same as the laws being targeted. Once this is realised, we can reformulate the model so that all leading intuitions are preserved whilst avoiding this particular objection. Indeed such a revision has been suggested by Schaffner (1967, 1976) and, indeed, by Nagel himself (1974). We call this the Nagel-Schaffner model of reduction (NSR).

We make room for a certain mismatch between the two theories, by requiring not that T_P itself, but rather a 'corrected' version of T_P

[6]See for instance Suppes (1977) for critical discussion of the syntactic view.

can be derived from T_F. More specifically, the proposal is that T_F reduces T_P iff there is a corrected version T_P^* of T_P such that, (a) T_P^* is derivable from T_F given that the terms of T_P^* are associated via bridge laws with terms of T_F, and (b) that the relation btween T_P^* and T_P is one of *strong analogy* (Schaffner 1967, 144).[7] This is illustrated in Figure 1.

Fig 1. The Nagel-Schaffner Model

Two points deserve elaboration. The first is the introduction of T_F^*. This is an aid to make more perspicuous that *de facto* the derivation consists of two steps: we first derive a special version of T_F, T_F^*, by introducing auxiliary assumptions and then replace the relevant terms by their 'correspondents' using bridge laws, which yields T_P^*. Of course this is equivalent to saying that we derive T_P^* from T_F plus auxiliary assumptions and bridge laws. Consider again the above examples. In the case of the Ideal Gas Law we first deduce a 'kinematic version' of the law from the kinetic theory, namely Equation 4. In the language of the above diagram this equation is the law of T_F^*. We then use the bridge law – Equation 5 – to substitute T for $\langle E_{kin} \rangle$ and obtain $p\,V = k\,T$, which is T_P^* and T_P in one since in this simple case strong analogy means identity. In the case of the Second Law we start with SM and deduce, together with the auxiliary assumptions that the system is chaotic and the past hypothesis holds, a law saying that the Boltzmann entropy increases and only fluctuates a bit once it has come close to equilibrium. This law, also referred to as 'Boltzmann's

[7]A much rehearsed criticism of Nagel's approach is that we can reduce false theories, but modus tollens then requires us to reject the reducing theory, which is self-defeating because it has to be assumed to be true (see, for instance, Bickle 1998, 24). Once the requirement that T_P be derived from T_F is replaced by the requirement that T_P^* be derived this problem vanishes. For a discussion of this point see Endicott (1998, 60- 62).

Law', is the central law of T_F^*. We now introduce a bridge law postulating that $S_T = S_B$, i.e. we associate the thermodynamic entropy and the Boltzmann entropy. This gets us from T_F^* to T_P^*, which says that the *thermodynamic* entropy fluctuates only mildly once the system has reached equilibrium. T_P^* then is strongly analogous to T_P in the sense that fluctuations are very small and the entropy stays close to the equilibrium value most of the time.

The second point is that the notion of strong analogy is obviously vague and hard to pin down. Critics might argue that it is so vague that any account based on it must be untenable (cf. Nickles (1975) Churchland (1987)). We disagree. It is a mistake to require that an account of reduction come with a general account of strong analogy. What is meant by strong analogy depends on the case at hand. It is true that in any *given* case of reduction we need to specify what exactly we mean by strong analogy, but this is a question that needs to be settled either in the relevant scientific discipline itself or the special philosophy of it, and not by a general philosophical account of reduction. The above example of the derivation of the second law makes this clear. That Boltzmann's law is strongly analogous to the Second Law in a way that underwrites reductive claims does not follow from some philosophical theory of analogy; it is the result of a careful analysis of the case at hand. Callender (1999, 2001) has argued, in our view convincingly, that the unrestricted Second Law is too strong and that we can accept Boltzmann's law without contravening any known empirical fact, which is why we can regard these laws as strongly analogous. Indeed, we should expect the same to be the case with almost every putative case of reduction: it is the particular science at stake that has to provide us with a criterion of relevant similarity in the particular context.[8]

[8]Schaffner (1967, 144) also requires that T_P^* corrects T_P in the sense that T_P^* makes more accurate predictions than T_P. This is the case in our example since experiments show that entropy fluctuates as predicted by T_P^* (and *ruled out* by T_P). However, it seems too strong a requirement to impose on *all* cases of reduction and so we leave it open whether, in any given case, improved predictive accuracy should be considered to form part of the strong analogy relation.

3.2 Problems for the Nagel-Schaffner Model

The Nagel-Schaffner model faces three problems.

Problem 1: The Content of Bridge Laws. One of the main objections against the Nagel-Schaffner model is that bridge laws play an essential role in it, yet they are problematic in many ways. There are two main issues.[9] The first concerns the content of bridge laws: what kind of statements do bridge laws make? Nagel considers three options (1961, 354-355): they can be claims of meaning equivalence, conventional stipulations, or assertions about matters of fact. The third option can be broken down further, since a statement connecting two quantities could be assert the identity of two properties, the existence of a nomic connection between them, or a presence of factual correlation. Depending on how this issue is resolved, the question arises: how do we get to know bridge laws and how are they established? This is the second issue. Nagel (*ibid.* 356) points out that this is a difficult issue since we cannot test bridge laws in the same way as we test other laws. The kinetic theory of gases can be put to test only *after* we have adopted Equation 5 as a bridge law, but then we can only test the 'package' of the kinetic theory and the bridge law, while it is impossible to subject the bridge law to independent test. This is not a problem if one sees bridge laws as analytical statements or as mere conventions, but it is a serious issue for those who see bridge laws as making factual claims.

Problem 2: The meaning of terms. The rationale in invoking bridge laws is to connect the vocabularies of two theories to each other. Feyerabend (1962) argued that such a move is impermissible. The meanings of the central terms of a theory are fixed by the role they play in the theory, and terms become meaningless when taken out of their theoretical context. For this reason terms in different theories have different meanings (and even where two different theories seemingly share theoretical terms, for example 'mass' in Newtonian Mechanics and Special

[9] A further criticism focuses on the formulation of bridge laws. Nagel took bridge laws to be universally quantified bi-conditionals in first order logic, which, so the objection continues, are too weak to express the content of a law. As we have pointed out above, the use of first order logic in Nagel's original presentation of his model is inessential, and there is no reason to adhere to it.

Relativity, this is merely a sharing of *names* but not of *concepts* since the terms have different meanings in each context). But, so the argument goes, one cannot associate terms with different meanings with each other. But since the meaning of a term is determined by its theoretical context, it is impossible to associate terms from different theoretical contexts with each other, which makes Nagelian reduction impossible, since bridge laws in effect express such associations. Feyerabend illustrates this with the case of temperature. In thermodynamics temperature is defined in terms of Carnot cycles and is used in the definition of the thermodynamic entropy ($S_T = \int dQ/T$), which is governed by the strict, non-probabilistic, Second Law. The bridge law used when reducing TD to SM identifies S_T with a quantity that has a very different theoretical context, one in which Carnot cycles play no role and the central notion is probability. This, Feyerabend thinks, makes no sense.

Problem 3: Multiple realisability. A T_P-property is *multiply realisable* if it can correspond to a number of different T_F-properties. The standard example of multiple realisability is that of pain: pain can be realised by different brain states. Likewise, temperature can be realised in a variety of different ways in different media (Sklar 1993, 352). It is often suggested that the multiple realisability of T_P-properties shows reduction to be untenable.[10]

Why is this? Unfortunately the point is frequently asserted but rarely argued. Yet, the main driving force behind it seems to be the view that (a) bridge laws are genuine laws of nature, and that (b) proper laws of nature cannot have a disjunctive form (i.e. saying something like 'Temperature is either ... or ... or ...') (*cf.* Fodor 1974, 108). Multiple realisability then seems to preclude bridge laws from being genuine laws.

These difficulties have been regarded by many as so severe that avoid-

[10]This argument is commonly attributed to Fodor (1974). It is worth noticing, however, that the multiple realisability argument, at least per Fodor, is not an argument against NSR *per se*, but an argument against what he calls *reductivism*, the view that 'all true theories in the special sciences should reduce to physical theories' (*ibid.*, 97, *cf.* 110). Whilst the argument may not be aimed at NSR, it still might, *de facto*, be detrimental to it, which is the reason to consider it here.

ing them appeared to be a better strategy than trying to address them. This what the approach known as New Wave Reductionism purports to achieve: explaining the reduction of one theory to another while avoiding these difficulties. We discuss this approach in the next section and conclude that it is unsuccessful. In Section 5 we return to these problems are argue that they are by no means as devastating as the opponents of the Nagel-Schaffner model have made them out to be.

4 New Wave Reductionism

The approach to reductionism that has later become known as New Wave Reductionism (NWR) has first been prosed by Churchland (1979, 80-88), and has then been developed by Churchland (1985, 1987) and Hooker (1981), and later Bickle (1996, 1998).

Churchland invites us to consider two theories T_o and T_n, an old and new one (e.g. the thermal theory and the kinetic theory of gases). He then states two desiderata for reduction (1979, 81):

> 'First, it provides us with a set of rules – "correspondence rules" or " bridge laws" [...] – which effect a mapping of the terms of the old theory (T_o) onto a subset of the expressiosn of the new or reducing theory (T_n).'
>
> 'Second [...], a sucessful reduction ideally has the outcome that, under the term mapping effected by the correspondence rules, the central principles of T_o [...] are mapped onto general sentences of T_n that are *theorems* of T_n. Call the set of such sentences S_n. This set is the image of T_o within T_n.' (original emphasis)

Churchland is quick to point out that this is an 'ideal or maximally smooth' case (*ibid.*). In general the situation will be more involved in two ways (*ibid.*, 83-84). First, the image of T_o within T_n may not be a direct consequence T_n *alone*; it may be deducible only from an 'augmented theory $T_n^{'}$ comprising within T_n plus auxiliary assumptions. In the case of the Ideal Gas Law, for instance, we added the assumptions that particles are point particles and that space is isotropic. Second, we may not be able to derive a wholly faithful image of T_o within $T_n^{'}$

(let alone T_n), and may have to rest content with deriving a modified or corrected version $T_o^{'}$ of T_o. In this case we refer to S_n as the 'corrected image of T_o'. We then require that the corrected theory $T_o^{'}$ and the original theory T_o be *closely similar* (*ibid.*, 83) or *analogous* (Hooker 1981, 49). Putting these pieces together we obtain the picture illustrated in Figure 2.

Fig 2. New Wave Reductionism

If one theory can be obtained from another merely by substituting terms by their bridge law doppelgäners Churchland calls the two theories 'relevantly isomorphic' (1985, 10), in which case one is a 'relevantly adequate mimicri' (*ibid.*) or 'equipotent image' (1979, 82) of the other. So S_n is an equipotent image of T_o in the case of a perfectly smooth reduction, and to $T_o^{'}$ in the general case. The smoothness of a reduction is a matter of degree, ranging from 'ideal' to 'bumpy', and it depends on two factors (1979, 83-84): on how realistic the auxiliary assumptions of $T_o^{'}$ are, and on how close $T_o^{'}$ is to T_o. The literature on NWR is by and large silent about the relation between $T_o^{'}$ and T_o, but what we have said in the last section about strong analogy can also be said about the relation between $T_o^{'}$ and T_o and so we don't think that there is a serious problem here.

When looking at Figure 2 a question springs to mind immediately: in what way is NWR different from NSR? It seems that if we substitute T_F for T_n, T_F^* for S_n, T_P for T_o, and T_F^* for $T_o^{'}$ we are back to NSR. Nevertheless, proponents of NWR insist that theirs is an entirely different model of reduction. They highlight two points. First, NWR and NSR are said to differ in what gets deduced. NWR insists that what is deduced is not T_o (or $T_o^{'}$) but the (corrected) *image* S_n of T_o. Churchland makes this point explicit by urging us to give up 'the idea that what gets *de*duced in a reduction is the theory to be *re*duced' (1985, 10), because

> 'a reduction consists of the deduction, within [T_n], not of [T_o] itself, but rather of a roughly equipotent *image* of [T_o], an image still expressed in the vocabulary proper to [T_n]. The correspondence rules play no part whatever in the *de*duction.' (1985, 10, original emphasis)

And, what is more, not only do correspondence rules play no role in the *deduction*, they also play no role in the *reduction per se*:

> 'it is important to appreciate that cross-theoretical identity claims, even if they are justly made, are not a part of the *reduction proper*, and they are not essential to the function it performs.' (1979, 83, emphasis added; *cf.* Bickle 1998, 27)

It is not entirely clear what is meant by 'reduction proper', but if the suggestion is – which it seems to be – that bridge laws play no role (or are unimportant) in reducing one theory to another one, then this cannot be right. It is true that the deduction of S_n is the centre piece of a reduction and it is, of course, also true that this deduction is *internal* to T_n and entirely couched in the language of T_n, but this does not imply that bridge laws are unimportant. It is crucial to a reduction that S_n is an equipotent image of T_o (or $T_o^{'}$) and that it is just any old collection of theorems. But being an equipotent image of T_o is a relational property, and one that obtains if, and only if, there is a mapping of T_o-terms onto T_n-terms, and this mapping is defined by bridge laws. So bridge laws play a crucial role in defining what the aim of the deduction is: they single out which set of theorems we should try to derive. Given this, how could they possibly be irrelevant? Consider the example of the ideal gas. In this case S_n consists of Equation 4. We only know that this formula is relevant to the reduction once we have bridge laws telling us to associate pressure and volume in the kinetic theory with pressure and volume in the thermal theory, and mean kinetic energy with temperature. Many other theorems can be derived from the kinetic theory, but they are not relevant to a reductionist project not because they are uninteresting, but because there are no bridge laws linking them to a theorem of the thermal theory.

A more realistic description of what happens in a reduction would be to say that it consists in two steps: the intra-theoretic deduction of S_n within T_n and the inter-theoretic mapping of T_o into S_n using bridge laws.[11] We agree with that, but the difference to NSR now is one of emphasis or presentation and not of substance. Saying that we *first* derive S_n from T_n and *then* substitute the terms in S_n to retrieve $T_o^{'}$ (which we must do in order to establish that S_n is an equipotent image of $T_o^{'}$) is equivalent to deriving $T_o^{'}$ from T_n since the bridge laws can always be added to the premises of the deduction which, trivially, yields $T_o^{'}$. In sum, once it is acknowledged that bridge laws are important in establishing S_n's status as an equipotent image, NWR collapses into NSR, in this respect.

The second alleged difference between NWR and NSR has to do with the content of bridge laws. Witness Churchland explaining the concept of a bridge law invoked in NWR:

> The correspondence-rule pairings need not be construed as identity claim, nor even as material equivalences, in order to show that T_n contains an equipotent image of T_o. In fact, we can treat each correspondence rule as a mere ordered pair of expressions [...] and we will then need only the minimal assumption that the second element of each pair truly applies where and whenever the first element of each is normally *thought* to apply. (1979, 83; *cf.* 1985, 10).

On this understanding of bridge laws, Equation 5 neither asserts the identity of mean kinetic energy and temperature, nor claims that they are nomically connected; all that is stated is that they apply in the same situation.[12] The passage quoted is preceded by a critical mention of Nagel's account of reduction, which suggests that Churchland thinks that on Nagel's view bridge laws must be identity statements and that it is the advantage of NWR that it is not committed to such

[11] In fact, Churchland (1985, 11) could be read as suggesting exactly this.

[12] Bickle (1998, 28) denies that bridge laws say even that much and claims that the two terms in a pair need not be co-extensional. This is wrong, and Churchland is right in stipulating, in effect, that the two notions of the pair have to be co-extensional (1985, 10-11). Bridge laws typically have the form of mathematical equations, and these, when interpreted as factual claims, are false if the terms on both sides of the equality sign fail to apply on the same occasions.

an understanding of bridge laws.

This claim is wrong. As we have seen above, Nagel himself thought that it was an open question how bridge laws ought to be understood and suggested that they could be construed as conventions, statements of synonymy, or factual claims. An understanding of bridge laws as claims of co-extensionality falls within the scope of the third option, and hence clearly is an option Nagel regarded as acceptable. This is of course not to say that this is the right analysis of bridge laws (we come back to this question below); the claim merely is that an understanding of bridge laws as a co-extensionality claim is clealy compatible with NSR, and hence does not serve to distinguish NWR and NSR.

So we conclude that upon closer analysis NWR and NSR turn out to be equivalent.[13]

5 Nagelian Reduction Reconsidered

In order for NSR to be an acceptable account of reduction, we need to address the problems mentioned in Section 3.2. Since all three of them in one way or another have to do with bridge laws, it is important to first get clear on the exact nature of bridge laws.

There is one perennial confusion about bridge laws that has hampered a discussion of their contents: the view that bridge laws identify the basic entities of T_P and T_F with each other. For instance, the identity of light and electromagnetic radiation, electric currents and the flow of electrons, and gases and swarms of atoms (see, for instance, Sklar, 1967, 120). Hence NSR is committed to an understanding of bridge laws as identity statements. However, so a standard line of criticism continues, this is untenable because we can reduce essentially false theories to true ones, and hence NSR is untenable.

[13] A similar conclusion has been reached by Endicott (1998). However, our claim is more radical than his since he grants that NWR and NSR differ in what gets deduced, which we deny (*cf.* our first point). For further discussions of NWR, with a special focus on issues in the philosophy of mind, see Endicott (2001), van Eck, De Jong and Schouten (2006) and Wright (2000).

This argument misconstrues the nature of bridge laws. Basic reductive claims are not what bridge laws are about. In fact, these claims are part of the reducing theory T_F. It is the basic posit of the kinetic theory of gases that gases are swarms of atoms, and it is the basic posit of statistical mechanics that the systems within the scope of thermodynamics have a molecular constitution and that the behaviour of molecules is governed by the laws of mechanics – none of this is the subject matter of a bridge law. Let us call a basic identity claim of this kind the 'background reduction' of the (potential) intertheoretic reduction of T_P to T_F. Background reductions can, of course, be false, but if they are it is the reducing theory that is false, and not a bridge law connecting T_F to T_P. Bridge laws enter into the picture only once these basic identities have been established, and they then assert that the T_P-properties of a system thus identified stand in a relevant relation to the T_F-properties of that system, and that and that the magnitudes of these properties stand in a relevant functional relationship.

Before turning to the question of what this relevant relation is, let us address Feyerabend's criticism, that reduction is impossible because in order to associate two terms with each other they must have the same meaning, which, however, is never the case if the terms occur in two different theories. Whether this argument is cogent depends on what one means by 'meaning'. Feyerabend associates the meaning of a term with the role the term plays in a theoretical framework; the meaning of the term 'temperature' as it occurs in thermodynamics, for instance, is determined by everything we say about temperature in the language of thermodynamics. Given this conception of meaning it is clear that terms occurring in different theories must have different meaning. But this is irrelevant when it comes to reduction. As Chruchland rightly remarks, 'it is not meaning that is preserved in intertheoretic reduction. Indeed, the pairings effected therein standardly *fail* to preserve meaning.' (1979, 81; cf. 86) When meaning is framed in this way, what matters is not meaning equivalence. What matters is whether the properties that the terms in the bridge laws *refer to* stand in a relevant relation to each other.[14]

[14] For those subscribing to the so-called direct reference view of meaning (roughly the view that the meaning of term is its referent) this conclusion would be reversed: meaning equivalence would play an essential role in reduction.

What, then, is the relevant relation between the properties picked out by the terms paired up in a bridge law? This question can be understood in two different ways. First, the issue may be what requirements need to be imposed in order to make NSR fly. The answer to this is: none. As we have seen above, Nagel contemplates different options, and all of them are acceptable *as far as NSR is concerned.* So no further constraints need to be imposed to get NSR *in itself* off the ground.

However, there is a temptation to further restrict the class of allowable bridge laws. The second reading of the question, then, is what these restrictions should be. At this point we would like to take a non-committal stance. We agree that bridge laws are not claims about meaning equivalence: as we have just seen, terms correlated in bridge laws characteristically don't have the same meaning. We also agree that they cannot be mere conventions. If one wants to maintain (as we do) at least a minimum of realism about unobservables, then there clearly is right or wrong in theoretical associations: it is true that the temperature of gas correlates with $\langle E_{kin} \rangle$, but it is false that it correlates with $\langle E_{kin} \rangle^2$.[15] So bridge laws are factual statements. They express the fact that two properties are correlated in the sense that whenever one is instantiated the other is instantiated as well, and that their magnitudes are related to one another in the way described by the bridge law: Equation 5 says that whenever a gas has a temperature it also has a mean kinetic energy and that the value of T varies linearly with $\langle E_{kin} \rangle$, where the proportionality constant is $2n/3k$. This leaves open the question whether T and $\langle E_{kin} \rangle$ are *merely* correlated (as a brute matter of fact), whether there is a nomic connection between them, or whether they are identical (and the terms co-referential).

We believe that the needs of reduction do not force us to settle this question. This claim is controversial. In fact, there has been a strong trend in the literature on reduction to try to settle exactly that, and, more specifically, require that bridge laws express identities.[16] We

[15] Nagel (1961, 355-358) points out, rightly, that there is no in principle way to decide between conventions and factual statements, and that which way one goes depends on one's realist committments.

[16] cf. references in Klein (2009) p. 43, footnote 5. Interestingly, Hooker seems to have

agree with Klein (2009) that these attempts are mistaken. Nothing over and above correlation is needed for reduction *per se*. The driving force behind further demands on bridge laws are not the needs of reduction, but other philosophical commitments, in particular views about explanation and laws of nature.

To see this, consider the third criticism mentioned in Section 3.2, multiple realisability, as an example. The claims is that laws cannot contain disjunctive properties. The reason for this is that laws are thought to relate natural kinds, and disjunctive properties cannot be natural kinds. However, both the nature of natural kinds and laws is a highly contentious matter, and hence it is neither clear what the assertion amounts to nor does it follow that we have to accept the conclusion. In fact, there are conceptions of laws of nature – for instance Cartwright's (1983) or Giere's (1999) – which make no appeal to natural kinds at all. So given all this it seems as if reduction turns on one's conception of laws of nature.

This is, however, mistaken. As Klein (2009, 49-50) points out Nagelian reduction simply does not require bridge laws to be laws of nature in any substantial sense. The primary goal of bridge laws is the coordination of the vocabulary of two theories, and they do so by claiming that whenever one term in the bridge law applies the other one applies as well; or in terms of properties bridge laws say that whenever the property referred to by one term is instantiated, then the property referred to by the other term is instantiated as well. Hence, bridge laws are purportedly true generalisations, but they need not count as laws of nature in any thick sense for them to successfully back up a reduction.[17]

It is true, however, that more than this 'thin' nature of bridge laws may stand in conflict with other aims of science. Most notably explanations seem to demand law-like connections which are stronger than mere statements of correlation (this is the gist of most well known

conflated the stance that a further restriction of a bridge law is not needed (which is also our stance), with the claim that there is no difference between some such further restrictions (viz. correlations, nomic connections and identities). cf. Hooker (1981 202).

[17]This is also implicit in Sklar's discussion of the case of temperature, when he insists that the the multiple realisability of temperature is no impediment to reduction (1993, 352-354).

counter-examples to the DN-model of explanation). There are two ways to go at this point. Either we argue that the *particular* bridge laws used in a certain context in fact express identities or nomic connections, or we have to scale down the explanatory claims we base on a reduction (Klein 2009, 51). The latter may well be undesirable for various reasons, but it does not undercut reduction *per se*.

In light of the above, we suggest that neither of the problems mentioned in Section 3.2 is a persuasive argument against NSR.

Let us finally address the issue of the epistemology of bridge laws: how do we come to know bridge laws and how do we test them? The problem is that we cannot independently test bridge laws, and Nagel's discussion of them does not make it clear where we get them from. Proponents of NWR have repeatedly complained that we do not, as Nagel suggests, *start* with T_F, *then* write down a bridge law, *and finally* deduce T_P^*. In terms of the above figures, we rather start at both ends and work our way to the middle, meaning that we start deducing certain theories T_F^* from T_F, then try to come up with a corrected version of T_P, and then, if we see a mapping between the two emerge, we decided which terms to pair up. The same point is made even more explicitly by Ager, Aronson and Weingard (1974, 119-122), who argue that the right analysis of the ideal gas case is not Argument 1 in Section 3.1, but the following (Argument 2):

Premise 1: Equation 3
Premise 2: $pV = kT$

Conclusion: Equation 5

In other words, it is not the Ideal Gas Law that we derive from the kinetic theory plus a bridge law (Equation 5); it the the bridge law that we derive from the Ideal Gas Law and the kinetic theory. We then take the bridge law seriously if the reduction is sufficiently smooth (in the sense explained in Section 4); in other words, the smoothness of the reduction is taken as evidence for the factual correctness of the bridge law.[18]

[18]In fact, proponents of NWR argue that smoothness supports the claim that the bridge law is an identity claim (Churchland 1995, 11). We think that this is too strong, but the main idea, namely that smoothness supports factual correctness, seems valid.

We agree with that point, which is an important insight into how we establish bridge laws. But we don't think that there is substantial controversy here. Nagel's is a rational reconstruction of a reductive argument and as such does not reflect the actual course of events, which is much closer to what NWR and Ager, Aronson and Weingard say. So the proponent of NSR can take this as a friendly amendment without having to change anything in his position.

6 Conclusion

We have argued that NSR is alive and well, and scientist involved in a reductionist research programme do the right thing if they take NSR as a regulative ideal. This, however, should not be taken to support *reductionism*, the (much stronger) claim that ultimately all sciences are reducible to one basic science (usually physics). This may or may not be true; but nothing in NSR forces this view upon us. We have pointed out what it would *mean* to reduce a theory to another one; whether any given theory can actually can be reduced to another theory, or even whether theoretical reduction can be achieved across the board, is, in our view, a factual and not a philosophical question. All we can do is wait and see whether a reduction is forthcoming.

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