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Who's Afraid of Nagelian Reduction?

Foad Dizadji-Bahmani · Roman Frigg · Stephan Hartmann

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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 intertheoretic reduction. The alleged difficulties of the theory either vanish upon closer inspection or turn out to be substantive philosophical questions rather than knock-down arguments.

1 Introduction

The purpose of this paper is to examine synchronic intertheoretic reduction, i.e. the reductive relation between pairs of theories which have the same (or largely overlapping) domains of application and which are simultaneously valid to various extents.[1] Examples of putative synchronic intertheoretic reduction are the reduction of chemistry to atomic physics, rigid body mechanics to particle mechanics, and thermodynamics (TD) to statistical mechanics (SM).

The central contention of this paper is that a Nagelian account of reduction is essentially on the right track. With some modifications and qualifications that account tells the right story about how synchronic intertheoretic reduction works.

[1] There are, of course, 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).

F. Dizadji-Bahmani (✉) · R. Frigg
Department of Philosophy, Logic and Scientific Method London School of Economics, London, UK
e-mail: f.dizadji-bahmani@lse.ac.uk

R. Frigg
e-mail: r.p.frigg@lse.ac.uk

S. Hartmann
Center for Logic and Philosophy of Science, Tilburg University, Tilburg, The Netherlands
e-mail: s.hartmann@uvt.nl

For reasons that will become clear as we proceed, we refer to this modified and qualified account as the *Generalised Nagel-Schaffner Model of Reduction* (GNS). To prime our intuitions, we start with a discussion of the reduction of TD to SM, which serves as the touchstone for our views about reduction (Sect. 2). We proceed to present a preliminary statement of GNS by first discussing Nagel's original views (1961, Chap. 11) and then introducing the amendments proposed by Schaffner and, indeed, Nagel himself (Sect. 3.1). Subsequently, we list seven (families of) problems that allegedly render this account untenable (Sect. 3.2). After briefly pointing out that these problems cannot be avoided by substituting GNS with so-called New Wave Reductionism, we reconsider the alleged difficulties of GNS. We conclude that they are not only far from being as insurmountable as they are often said to be, but that some of them vanish upon closer inspection, and those that don't turn out to be interesting philosophical issues rather than knock-down arguments (Sect. 4). The discussion of these problems leads to various important qualifications. In the last section, we clarify the relation of GNS and reductionism, the view that eventually all theories reduce to one fundamental theory (Sect. 5).

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 that 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 SM is to account for this behaviour in terms of the dynamical laws governing the microscopic constituents of macroscopic systems and probabilistic assumptions.

Although the success of the reduction of TD to SM is a matter of controversy, there is no doubt that accounting for the laws of TD in terms of laws governing the micro-constituents of systems is a reductionist enterprise.[2] But before we can assess the success of reduction, we need to know what is meant by reduction. That practitioners of SM do not really discuss the issue is no surprise; however, it should raise some eyebrows that, by and large, philosophers working on the foundations of SM also rarely, if ever, 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, Chap. 11) considered a reduction to be. Indeed,

[2] This is widely acknowledged in the literature, both in physics and philosophy. See, for instance, Dougherty (1993, p. 843), Ehrenfest and Ehrenfest (1912, p. 1), Fermi (1936, p. ix), Goldstein (2001, p. 40), Huang (1963, Preface), Khinchin (1949, p. 7), Lebowitz (1999, p. 346), Ridderbos (2002, p. 66), Sklar (1993, p. 3) and Uffink (2007, p. 923), and Tolman (1938, p. 9).

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 states that '*there exists not a single physically well-founded and nontrivial example for theory reduction in the sense of...* Nagel' (Primas 1998, p. 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 discussions, 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, Hooker, and Bickle provides a model of reduction that avoids the pitfalls of Nagelian reduction while providing a viable philosophical backbone of SM. In what follows, we point out that this is an empty promise and argue that a broadly Nagelian picture of reduction is correct.

Our methodology is to present two paradigm cases of reduction (in a pre-theoretic sense) which serve as a benchmark for any putative model of reduction. That is, some such model ought to be an abstraction that captures the salient features of the relation between the micro and macro laws in these two cases. These are the Boyle-Charles Law and the Second Law of thermodynamics.

Boyle–Charles Law. In TD, the state of a gas can be specified by three quantities: pressure p, volume V, and temperature T. Under certain conditions—low pressure and the gas initially being in equilibrium (i.e. 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 Boyle–Charles 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*.

Now 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 is located at a position $\vec{x}$ and has a particular velocity $\vec{v}$, and its motion is governed by Newton's equations of motion. Assume that the gas is ideal in the sense that it consists of point particles and that they only interact elastically. Assume, furthermore, that we are given a velocity distribution $f(\vec{v})$, specifying what portion of all particles move with $\vec{v}$ (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 of the kinetic theory.[3]

Pressure is defined (in Newtonian physics) as force per surface: $p = F_A/A$, where A is surface and F_A the force acting perpendicular to the surface. 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. We now assume that all particles in the gas are perfectly elastic point particles. Then, consider a wall in the x–y plane. Some purely algebraic manipulations show that the pressure exerted by the gas on that wall is

$$p = \frac{mn}{V} \int\limits_{-\infty}^{\infty} d^3 v f(\vec{v}) v_z^2 =: \frac{mn}{V} \langle v_z^2 \rangle, \tag{1}$$

where v_z is a particle's velocity in z-direction, and $\langle v_z^2 \rangle$ the average of the square of the velocity (which is defined by the integral in the equation). 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, the components of $f(\vec{v})$ are the same for 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

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

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

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

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

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

The upshot of these calculations is that if we associate the temperature T with mean kinetic energy of a particle (multiplied by a constant), then the Boyle–Charles 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 point particles, that they collide elastically, and that the velocity distribution is isotropic).

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

[3] For details, see Greiner et al. (1993, pp. 12–15) or Pauli (1973, pp. 94–103).

cannot occur. The aim of reduction 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 properties of the dynamics 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 has 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. In fact, this association performs the same function as Eq. 5 in above case: Only if we associate the two can we derive the Second Law of TD from SM.

3 Nagelian Reduction

We first introduce what we call the *Generalised Nagel-Schaffner model of Reduction*, and then present some problems it purportedly faces.

3.1 The Generalised Nagel–Schaffner Model

On Nagel's original account (1961, pp. 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. More specifically, two conditions for successful reduction are postulated. *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*.

[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; with the Gibbsian framework, the question poses itself in a different way.

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

For Nagel, there are two classes of reduction. In *homogeneous* reductions, the two theories share the same relevant predicates. In this case, the connectability requirement is trivially satisfied. Examples of this kind of reduction are the reduction of Kepler's theory of planetary motion to Newton's mechanics, and the reduction of classical rigid body mechanics to classical particle mechanics because in both cases the latter theory contains all the relevant terms of the former. If the theories do not share the relevant terms, the putative reduction is *heterogeneous*. In this case, it is not even possible to derive the laws of T_P from T_F. 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.

An obvious difficulty for this model is that, often, it is in fact not possible to derive the *exact* laws of T_P. For instance, we have seen in the last section that 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. Thus *exact* derivability is too stringent a requirement: It suffices to deduce laws that are approximately the same as the laws being targeted. This revision of the original model has been developed in a string of publications by Schaffner (1967, 1976, 1977, 1993, Chap. 9), and, indeed, by Nagel himself (1974). 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 that (b) the relation between T_P^* and T_P is one of, at least, *strong analogy* (sometimes also 'approximate equality', 'close agreement', or 'good approximation').

It is worth pointing out that the derivation of T_P^* involves 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, but for the following discussion it is helpful to clearly distinguish the two steps.)[6] This can be seen in the above example: We first deduce a 'kinematic version' of the law from the kinetic theory, namely Eq. 4, which is T_F^*, and then use the bridge law—Eq. 5—to obtain $p\,V = k\,T$ (which is T_P^* and T_P in this simple case).

In sum, reduction is the deductive subsumption of a corrected version of T_P under T_F, where the deduction involves first deriving a restricted version, T_F^*, of the reducing theory by introducing boundary conditions and auxiliary assumptions and then using bridge laws to obtain T_P^* from T_F^*. This is illustrated in Fig. 1. We call this the Generalised Nagel-Schaffner model of reduction (GNS).[7]

Bridge laws are crucial to this picture of reduction. While Nagel himself remains relatively non-committal about the exact form and nature of bridge laws, Schaffner

[6] Note that this ordering is a reconstruction; in actual practice it may well be the case that people work 'from both directions'.

[7] This schema is sometimes also referred to as the g*eneralized reduction-replacement* model (GRR); see e.g. Schaffner (1993, Chap. 9). However, GRR is often taken to also incorporate Schaffner's view of bridge laws, which we follow in spirit but not in detail (see below). To avoid confusion as regards bridge laws we use 'GNS' rather than 'GRR'.

Fig. 1 The Generalised Nagel–Schaffner model of reduction

(1976, pp. 614–615; 1993, pp. 411–477) offers a concise characterisation of bridge laws, which he calls *reduction functions*. For Schaffner, a reduction function is a statement to the effect that a term t_P of T_P^* and a term t_F of T_F (or T_F^*; both theories contain the same terms) are coextensional. For example, the terms 'temperature' and 'mean kinetic energy' are coextensional when applied to a gas (we come back to this qualification below). At least in physics, properties usually have magnitudes: A gas does not have a temperature *simpliciter*, it has a temperature of so and so many degrees Kelvin. Thus, a bridge law not only establishes coextensionality; it also specifies the functional relationship between the magnitudes of the terms. Formally, the bridge law contains a function f such that $\tau_P = f(\tau_F)$, where, respectively, τ_P and τ_F are the values of t_P and t_F. The latter condition is not redundant: it does not follow from the fact that 'temperature' and 'mean kinetic energy' are coextensional, that the functional relation between their magnitudes is the one specified by Eq. 5. In fact, coextensionality could be true and yet the functional relation between the two be completely different. For this reason, a bridge law is incomplete without a specification of the functional dependence of magnitudes. So we can give the following tentative definition of bridge laws (we will qualify this statement below): A bridge law is a statement to the effect that (1) t_P applies if, and only if, t_F applies, and (2) $\tau_P = f(\tau_F)$.

Schaffner's presentation of bridge laws suggests that he takes it to be the case that, in a successful reduction, (a) *every* term of T_P^* is connected to a term of T_F, and that (b) a term of T_P^* is connected to exactly one one term of T_F (see, for instance, 1967, pp. 139–140). We take neither of these conditions to be necessary for a successful reduction. Our reasons to deny (b) will become clear when we discuss multiple realisability (in Sect. 4). The reason to deny (a) is that we want to allow for partial reductions. If *all* terms of T_P^* are connected to terms of T_F and all laws (or central statements) of T_P^* can be deduced from T_F plus bridge laws under the *same* auxiliary assumptions, then we have a complete reduction of the entire theory T_P. If only some terms are connected and we can deduce only some laws (or central statements), then only the laws that can be derived are reduced, but not the entire theory. In this case, we speak of a partial reduction of T_P^*.[8]

These are the main tenets of GNS. We now list a number of criticisms, which we address in Sect. 4. The discussion of these criticisms leads to important qualifications of GNS. Finally, we make a point of nomenclature: When we talk about 'Nagelian Reduction', we refer to GNS. This is justified, since GNS is the best

[8] Sometimes this is couched as the difference between theory-reduction and law-reduction. When understood in this way, there is no fundamental difference between the two, and theory-reduction is simply complete law-reduction.

match between the central ideas of Nagel's (1961) original theory and the needs of scientific practice.

3.2 Problems for GNS

The Nagel-Schaffner model faces a number of criticisms; some of them are puzzles requiring a solution, others are purported refutations. Most of these have been put forward against Nagel's original views rather than against Schaffner's, or even GNS. However, since GNS is equally open to most of these objections, they need to be tackled.

Problem 1: The Syntactic View of Theories 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, one of them being that first-order logic is too weak to adequately formalise theories and that the distinction between observational and theoretical terms is unsustainable.[9] This, so one often hears, renders Nagelian reduction untenable.

Problem 2: The Meaning of Terms. The rationale for 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. 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 Relativity, this is merely a sharing of *names*, but not of *concepts*, because the terms have different meanings in each context). But, it is argued, one cannot associate terms with different meanings with each other. 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.

Problem 3: The Content of Bridge Laws. There is a question about what kind of statements bridge laws are. Nagel considers three options (1961, pp. 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 assert the identity of two properties, the presence of a (merely) de facto correlation between them, or the existence of a nomic connection. Although the issue of the content bridge laws is not per se an objection, it is a question that has often been discussed in ways that suggests that this issue has no clear cut answer and gives rise to various objections, in particular in connection with multiple realisability (Problem 4), to which we turn now.

Problem 4: Bridge Laws and Multiple Realisability. The issue of multiple realisability (MR) is omnipresent in discussions of reduction. A T_P-property is *multiply realisable* if it corresponds to more than one different T_F-properties. The standard example of a multiply realisable property is that of pain: Pain can be

[9] See for instance Suppe (1977) for critical discussion of the syntactic view.

realised by different physical states, for instance in a human's and in a dog's brain. The issue also seems to arise in SM because, as Sklar points out (Sklar 1993, p. 352), temperature is multiply realisable. MR is commonly considered to undermine reduction. There seem to be four different (groups of) arguments for the conclusion that MR undermines reduction.[10]

The first argument from MR is that, in order to reduce T_P-phenomena to T_F-phenomena, T_P-properties must be shown to be 'nothing over and above' T_F-properties. That is, it must be shown that T_P-properties do not exist as something extra or in addition to T_F-properties: There is only one group of entities, T_F-properties. Showing this requires the *identification* of T_P-properties with T_F-properties. But a multiply realisable T_P-property is not identifiable with a T_F-property. This undercuts reduction.

The second argument takes as its starting point the observation that certain T_P-properties are not only multiply realisable, but that, on top of that, their realisers at the T_P-level are also of disparate kinds. Dog brains differ vastly from human brains and gases of particles have little in common with crystals or spin systems, and yet they can exhibit the same macro properties, namely pain and temperature. This puts us in the awkward situation that T_P-properties are homogeneous in kind and yet have a variety of very different realisers. This, so the argument goes, cannot be. A homogeneous T_P-property can only be reduced to a homogeneous T_F-property. So, at the very least, one would have to require that all realisers of a given T_P-property share some important feature in common for the association to count as reduction. But in just those putative cases of MR, this unity amongst the T_F-properties is lacking.

The third argument takes issues with disjunctive laws. If a T_P-property B is multiply realisable, then the associated bridge law has to be a disjunction of the form $B = A_F^1 \vee A_F^2 \vee$, where A's are the T_F-property realisers of B. What is worse is that this bridge law is not only a disjunction, but it is also (at least potentially) open-ended. However, it is claimed that a law of nature cannot have the form of an disjunction, let alone an open-ended one: Laws cannot be disjunctive. For this reason, bridge laws are not laws when there is MR, and thus Nagelian reduction is untenable.

The fourth worry is that MR undercuts the explanatory value of a reduction: If a T_P-property is multiply realizable at a lower level, then the lower level science is not able to explain phenomena at the higher level which the higher level science explains well.

Problem 5: The Epistemology of Bridge Laws. How are bridge laws established? Nagel (*ibid*., p. 356) points out that this is a difficult issue since we cannot test bridge laws independently. The kinetic theory of gases can be put to test only *after* we have adopted Eq. 5 as a bridge law, but then we can only test the entire 'package' of the kinetic theory and the bridge law, while it is impossible to subject the bridge law to independent tests. While this is not a problem if one sees bridge

[10] For a discussion (but not necessarily endorsement) of the first see Kim (2008), the second and the third Richardson (2008), and the fourth Sober (1999). These themes can, in one way or another, bet traced back to Fodor (1974), which is *locus classicus* for arguments against reduction based on MR.

laws as analytical statements or mere conventions, it is an issue for those who see bridge laws as making factual claims.

Problem 6: Spurious Reduction. Auxiliary assumptions play an essential role in the derivation of T_F^* from T_F, and T_F^* is essential to the reduction because it is T_F^* that connects to T_P^*. Two worries pertain to this. The first is that, if T_P^* can be deduced only with the help of additional assumptions, then it is not true that T_P has been reduced to T_F, and hence the reduction of T_P fails. If anything, T_P has been reduced to T_F *plus auxiliary assumptions*, but this is not what we were aiming for. The second and more pressing worry is that, as long as no restrictions are placed on what assumptions are allowable, reductions are cheap, if not trivial, because we can always write down assumptions that imply T_F^*. In fact, we could simply add T_F^* as an auxiliary assumption, and then trivially derive it. This, however, would certainly not amount to a reduction of T_P to T_F.

Problem 7: Strong Analogy. Strong analogy is essential to GNS. This raises three issues. The first is that the notion of strong analogy is too vague and hard to pin down to do serious work in a reduction. It is a commonplace that everything is similar to everything else, and hence saying that one theory is analogous to another one is a vacuous claim. Second, even if one is not going as far as regarding analogy as arbitrary, there remains the worry that there does not seem to be a general characterisation of the strong analogy required in a reduction. What counts as an analogy is context dependent and can be decided only case-by-case, which is a problem for a view that aims to be a general account of reduction (*cf*. Sarkar 1998, p. 173). The third worry is that, since T_P^*, rather than T_P itself, is deduced from T_F, it is illegitimate to say that T_P has been reduced. What really has been reduced is T_P^*, and T_P has simply been lost, or replaced, on the way, and so there is no reduction of T_P.

These difficulties are regarded by many as so severe that avoiding Nagelian reduction altogether seems to be a better strategy than addressing them. This option appears to be particularly attractive because a viable alternative seems to be readily available: the position know as *New Wave Reductionism* (NWR).[11] This position is often recommended as a substitute that can do all the work that Nagelian reduction was meant to do, while not suffering from any of its problems.[12] This is mistaken. In fact, Endicott (1998, 2001) has argued that NSW collapses into Nagelian reduction and leaves the intellectual landscape largely unchanged. We endorse Endicott's arguments (modulo some minor points that are inconsequential for the overall argument) and conclude that replacing Nagelian Reduction with NWR does not solve any of the problems that attach to intertheoretic reduction.

[11] The position has first been prosed by Churchland (1979, pp. 80–88), and has then been developed by Churchland (1985, 1989), Hooker (1981), and Bickle (1996, 1998). The term 'New Wave' is due to Bickle, and therefore the label 'New Wave Reductionism' is sometimes reserved for Bickle's view. It has become customary, however, to use it broadly and take it to denote the entire tradition starting with Churchland.

[12] This view is hard to pin to down in print, but it has been put to us in discussion on countless occasions.

4 Nagelian Reduction Reconsidered

Given that we can't avoid the problems of GNS by simply replacing it with another view of reduction, these problem have to be addressed. This is the task of the present section.

Problem 1 The objection that GNS is based on the syntactic view of theories and therefore untenable is mistaken. Although Nagel was a proponent of the syntactic view, there is no textual (or other) evidence that he took the syntactic view to be an essential part of his model of reduction; and Schaffner makes no assumptions about the correct analysis of theories when presenting his theory of reduction. This is for good reasons, because the syntactic view is unnecessary to get GNS off the ground, as is clear from the above examples: Neither did we present a first-order formulation of the theories, nor did we even mention a bifurcation of the vocabulary into theoretical and observational terms. Where first order logic is too weak, we can replace it with any formal system that is strong enough to do what we need it to do. The bifurcation of the vocabulary plays no role at all.

Problem 2. Feyerabend's criticism is 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. Thus, the meaning of the term 'temperature' as it occurs in thermodynamics 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 meanings. But when meaning is framed in this way, meaning equivalence is immaterial to reduction; what matters is whether the properties that the terms in the bridge laws *refer to* stand in a relevant relation to each other. Feyerabend's imposition that only terms with the same meaning can be associated with each other is unmotivated, unnecessary, and foreign to GNS.[13]

Problem 3. What is the status of bridge laws? The first two options Nagel considers are meaning equivalence and convention. These can be discarded. That bridge laws cannot be claims of meaning equivalence follows from our discussion of Problem 2. Neither can they be mere conventions. Conventions are arbitrary and all that matters is that they be respected after a choice has been made. We can choose to drive on the right or on the left hand side of the road; neither choice is better, or more justified, than the other. What matters is that everybody respects the choice once it has been made by the group. Bridge laws are not like that. Clearly, there is right and wrong in theoretical association. It is true that the temperature of a gas is proportional to $\langle E_{\text{kin}} \rangle$, but it is false that it is proportional to $\langle E_{\text{kin}} \rangle^2$. Furthermore, often a process of painstaking research was necessary to make such

[13] In fact, Nagel himself (1961, p. 352) denied that meaning has to be preserved in reductions. 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.

associations. That does not sit well with an understanding of bridge laws as conventions.

For this reason, bridge laws are factual claims. This, however, leaves open the question whether bridge laws express mere correlations (or Humean regularities), nomic connections involving certain necessities, identity statements, or yet other metaphysical relations. There is a strong push in the literature to first come to a general answer to this question and then settle for identity.

To assess this tendency, we have to distinguish between two different kinds of bridge laws: The first kind associates basic entities of T_P and T_F with each other; they identify, for instance, light and electromagnetic radiation, electric currents and the flow of electrons, and gases and swarms of atoms (see, for instance, Sklar 1967, p. 120). We refer to this kind of bridge laws as *entity association laws*. The second kind of bridge laws enter the scene once the basic entities of T_P and T_F are associated with each other and then assert that the T_P-properties of a system stand in a relevant relation to the T_F-properties of that system, and that the magnitudes of these properties stand in a relevant functional relationship. Let us call these *property association laws*.

Entity association laws are different from property association laws both in content and in origin. Entity association laws indeed express identities: gases *are* swarms of molecules, genes *are* strings of amino acids, etc. The same does not hold for property association laws; these laws can, but need not express identities. We will argue for this claim shortly. The second difference is that, while property association laws are external to T_F, entity association laws are internal to T_F. It is the basic posit of the wave theory of light that light is an electromagnetic wave; 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.[14] Entity association laws can, of course, be false; but if they are, it is the reducing theory that is false. By contrast, property association laws are external to T_F. For instance, there is nothing in the kinetic theory of gases *per se* that tells us to associate mean kinetic energy with temperature. This raises questions both about their content and form.

Problem 4. The question about the content of property association bridge laws is best discussed in the context of arguments against reduction based on MR. Unlike entity association laws, which clearly have to be identities, property association laws could, at least in principle, also be mere regularities, lawlike connections, or express yet another relation. However, there is a long tradition of arguing that *all* bridge laws have to establish identities. Hence, property association laws have to establish identities between properties because everything less than identity is insufficient for a genuine reduction.[15]

[14] For this reason, there is even a question whether calling these laws bridge 'laws' is a appropriate. We would prefer to refer to them as the 'background reduction of T_F'.

[15] Causey (1972) was one of the first to introduce this line of argument. Sometimes the argument is put as a criticism of bridge laws: it is assumed that bridge laws only express extensional equivalence and then it is concluded that bridge laws are insufficient for reduction because reduction requires identity.

As per the first argument, the driving force behind the requirement that bridge laws express identities is the view that, for a reduction to be successful, it has to be shown that T_P-properties are nothing over and above T_F-properties. We believe this to be mistaken. Whether or not the establishment of strict identities is a desideratum for a reduction depends on what one wants a reduction to achieve. If metaphysical parsimony or the defence of physicalism are one's primary goals, then identity may well be essential (although, even then, less than identity might be sufficient; we return to this issue when discussing explanation). But in science neither of these are very high on the agenda. Reductions are desirable first and foremost for two other reasons: consistency and confirmation. That is, T_F and T_P have to be consistent, and evidence confirming T_F also has to confirm T_P and vice versa. Further items can be added to this list, explanation being the most obvious addition (the condition that T_F explain T_P, we come back to this below). However, these additions are not essential: Reductions that achieve nothing but consistency and confirmation are *bona fide* reductions. These aims, and this is the crucial point, can be achieved without bridge laws being identity statements. In fact, mere de facto correlations between properties are all that is required for the needs of reduction, and we can remain agnostic about the question of whether bridge laws express anything beyond mere correlation.

Let us discuss consistency and confirmation in more detail. No rational person should hold contradictory beliefs. Hence, given two (self-) consistent theories T_1 and T_2, these ought to be consistent with each other (T_1 and T_2 are required to be consistent because no one should hold an inconsistent theory to begin with). If the two theories use completely different languages *and* are about a different target domain, then this requirement is satisfied trivially; there does not seem to be a problem about the consistency of algebraic quantum field theory and costly signaling theory in evolutionary biology. Things become more involved if the two theories' target domains are identical (or have significant overlap), in which case consistency does not come for free (i.e. not merely as a result of the theories not sharing any non-logical vocabulary). Theories like SM contain what we have above called entity association laws and so SM and TD are not consistent merely on the grounds that they use different vocabulary; they make claims about the *same* systems and the question arises whether these claims are consistent with each other.[16] Establishing a reductive relation between SM and TD ensures the consistency and hence co-tenability of the two accounts, because, trivially, if one consistent theory can be deduced from another consistent theory the two are consistent.[17] All that is needed for such a deduction is that there be conditionals saying 'for all x, x is t_F if and only if it is t_P'.[18] It simply does not matter whether this conditional expresses an identity, a

[16] Instrumentalists may require only the consistency of claims about observables; realists may also require consistency of theoretical claims. But there is a consistency issue no matter where one stands on the question of scientific realism.

[17] In fact, what is established is the consistency T_F and T_P^* rather than T_P. T_P and T_F may remain inconsistent, strictly speaking, because, as seen, T_P^* is usually (only) strongly analogous with T_P. However, all we really need is that T_F be consistent with a 'near enough' cousin of T_P, and because T_P^* and T_P are strongly analogous this is indeed the case.

[18] Strictly speaking it is not even necessary that the right-to-left implication holds.

nomic necessity or a mere de facto correlation; all we need for the deduction is that whenever t_F applies, then t_P applies.

Next in line is confirmation. Consider again two theories whose target domains are identical (or have significant overlap). We then would expect evidence confirming one theory to also confirm the other theory, and we expect confirmation to 'travel' both ways (though not necessarily with the same strength). This, however, can happen only if the two theories are connected to one another, and the connection postulated by GNS fits the bill.[19] Assume, first, that we have evidence supporting T_P^* and the bridge laws. On GNS, this theory is a deductive consequence of T_F (plus auxiliary assumptions) and the bridge laws, and on every credible account of confirmation, a general theory receives some boost in confirmation if one of its consequence bears out (although different accounts of confirmation analyse the basic idea in different ways). Conversely, if we have evidence supporting T_F and the bride laws, then T_P^* receives confirmatory support because a deductive consequence of a hypothesis inherits the confirmation of the hypothesis itself. As in the case of consistency, all that matters for confirmation is that there be sentences connecting terms from one theory to terms of the other so that the deduction becomes possible, but it is immaterial to the deduction whether these sentences express mere Humean regularities or some strong metaphysical relation. So, again, no commitment to an identity reading of bridge laws is forced upon us.

The second argument is that reduction is incompatible with there being a diverse set of realisers for one T_P-property: There must be something that binds together, or unifies, all the realisers or a T_P-property over and above merely being realisers of that particular T_P-property. This demand is unjustified. In fact, the second argument is just the identity view in disguise. While it admits that there can be different realisers, it requires that they all share something in common and then the implicit assumption is that what T_P-property is *really* reduced to is this common feature. We have already argued that identity is unnecessary for reduction, and so we also reject this argument. There simply is no reason to think that, say, 'temperature' for gas being co-extensional with mean kinetic energy precludes it from being co-extensional with a completely different micro-property in other systems.

The third argument from MR is that bridge laws cannot be genuine laws where multiply realisable properties are involved because multiply realisable properties require disjunctive bridge laws but genuine laws of nature cannot be disjunctive. It is hard to see why this should be so, and we can only share Sober's 'sense of incomprehension and mystery' at why the word 'or' should undermine the aims of reduction (1999, p. 553). First, as Sober points out, it is not clear where to draw the line between disjunctive and non-disjunctive laws, since what is non-disjunctive in one formulation could turn out to be disjunctive in another one and *vice versa*. Second, even if it is true that 'proper' laws of nature (whatever these are) cannot be disjunctive, there is no need for bridge laws to be laws of nature in that sense. Bridge laws can be of a different kind and have to satisfy less stringent demands than other laws of nature. All we require from bridge laws is that they serve the purposes of reduction (which, on our view, are consistency and confirmation), and

[19] In our (2011) we show that this is the case if we adopt a Bayesian framework.

disjunctions pose no problem for these (even if they are open-ended). Third, it is not clear why laws of nature cannot have a disjunctive form. What seems to lie in the background are worries concerning natural kinds and spurious confirmation. But it is not clear whether these worries are conclusive, and the burden of proof lies with those who argue against disjunctive laws.[20]

The last argument is that MR undercuts the explanatory power of reductions. We want to resist this argument for two reasons. First, rife doctrine nonewithstanding, reductions do not ipso facto have to double as explanations. The two core aims of reduction— consistency and confirmation—can be had without adding further items to the list, and reductions are desirable even if they do not serve any other purposes. Explanation, in particular, is nice to have where it can be had, but it is not a *sine qua non* of reduction.[21]

Second, it is not clear to us why MR should undercut reductive explanation. Kim (2008, p. 94) characterises a reductive explanation as one that shows that a particular T_F phenomenon constitutes 'an underlying mechanism' whose 'operation' yields a T_P phenomenon and which makes the T_P phenomenon 'intelligible in the light of the underlying phenomena and mechanisms'. It is not clear why MR should undercut reductive explanations in this sense. We explain why gases have temperature by appeal to the dynamical properties of its constituents. If this explanation is successful, then it is so irrespective of whether other kinds of systems can have temperature, too. Assume that gases were the only kind of objects that had temperature, and that we had a successful explanation of why gases have temperature in terms of the molecular motion of gas molecules. Why would this explanation no longer be an explanation once we realise that other systems also have temperature? There is no reason to believe that what used to be an explanation suddenly loses its status as an explanation. It has just become a more local explanation, because it does not cover all cases of temperature, but local explanations are still explanations.

Problem 5 How do we establish bridge laws? The alleged problem is that we cannot test them independently. In fact, it is not the case, as Nagel seems to suggest, that we *start* with T_F, *then* write down a bridge law (which we know to be correct!), *and finally* deduce T_P^*. Rather, what happens is that we begin with T_F and T_P and then try to find bridge laws that (modulo small corrections) make T_P derivable from T_F (*cf.* Ager et al. 1974, pp. 119–122). So the correct analysis of how the two theories relate should be

Premise 1: T_F
Premise 2: T_P

Conclusion: bridge law

[20] Often, the point is simply asserted. Kim, for instance, asserts that a multiply realisable property is 'unfit to figure in laws, and is thereby disqualified as a useful scientific property' because of its 'causal/nomic heterogeneity' (1999, p. 18). Needham (2009) is right to point out that this view is wrong: There is a good theory essentially involving temperature, namely TD, and MR is certainly no reason to deny TD its status as a scientific theory!

[21] Additions like explanation may or may not require a commitment to a stronger notion of bridge laws. In fact, Klein (2009) argues that we can have reductive explanations without committing to a view of bridge laws which sees them as expressing metaphysical relations.

In the above example, it is not the Boyle–Charles Law that we derive from the kinetic theory plus a bridge law (Eq. 5); it is the bridge law that is derived from the Boyle–Charles Law and the kinetic theory.

We agree with this point, but deny that it is a problem for GNS. In fact, this is just an instance of the Duhem problem: We are often unable to confirm hypotheses independently because we can only put entire packages (consisting of theories and auxiliary assumptions) to test. That the Duhem problem crops up in Nagelian reduction is hardly a cause for celebration, but given that this is a widespread problem in many (if not all) parts of science, it hardly is a reason to give up Nagelian reduction. As is well known, there is no royal route around the problem and arguments vary from case to case. So the conclusion to be drawn from this is simply that, in any given case of a purported reduction, we have to think carefully about what evidential support we have for the bridge laws we use. Sometimes we may take the bridge law seriously because we have good evidence for both T_F and T_P, and the reduction is sufficiently smooth.[22] In other cases, we may have other reasons to take the bridge laws seriously. Asking for a universal account of evidential support for bridge laws is a mistaken demand, and not one the GNS has to meet.

Problem 6 Let us begin with the second problem, namely that GNS is too liberal. GNS, so the objection goes, allows for auxiliary assumptions that are so strong that they are doing all the work, and, in fact, render T_F itself an idle wheel. Yet, it still forces us to say that T_P has been reduced to T_F, which is implausible. This is a fair concern, but not one that poses an insurmountable problem. Our proposal is to impose the following two conditions on auxiliary assumptions: First, T_F must be used in the deduction of T_P^*; that is, T_P^* must not follow from the auxiliary assumptions *alone*. We call this the *condition of non-redundancy*. Second, the auxiliary assumptions must belong to the paradigm of T_F; i.e. auxiliary assumptions cannot be foreign to the conceptual apparatus of T_F. This is the *condition of immanence*. These two restrictions successfully undercut spurious reductions.

Let us illustrate this with the example of the Second Law. Trivial self-deduction is ruled out by the first condition: we cannot simply write down the Second Law as an auxiliary assumption and then deduce it. But our two conditions also deal correctly with less trivial cases. Assume for the sake of argument that Boltzmann's programme has been completed successfully and a derivation of (a close cousin of) the Second Law of TD from the apparatus of Boltzmannian SM and the auxiliary assumption that the system is ergodic has been given. In our view, this would be a successful reduction, because ergodicity is part and parcel of classical mechanics, which is central to Boltzmannian SM. The auxiliary assumption merely restricts the class of allowable phase flows to ones that are ergodic, but it does not introduce anything into the theory that is in principle foreign to it. By contrast, consider the research programme known as *stochastic dynamics*.[23] The leading idea of this approach is to replace the Hamiltonian dynamics of the system with an explicitly probabilistic law of evolution.

[22] In fact, proponents of NWR argue that smoothness supports the claim that the bridge law is an identity claim (Churchland 1985, p. 11). We think that this is too strong, but the main idea, namely that smoothness supports factual correctness, seems valid.

[23] For a discussion of this programme, see Uffink (2007, pp. 1038–1063).

Characteristically, this is done by coarse-graining the phase space and then postulating a probabilistic law describing the transition from one cell of the partition to another one. The Second Law is then derived from this probabilistic dynamics. In our view, this is not a successful reduction of TD to SM, because the Second Law follows from the auxiliary assumptions alone (contra non-triviality), and the probabilistic transition laws are entirely foreign to classical mechanics. Unless one could somehow derive the probabilistic laws from the Hamiltonian equations of motion governing the system, these probability laws violate immanence.

The two conditions also offer a straightforward solution to the first worry: Given that the auxiliary assumptions have to belong to the paradigm of the reducing theory, there is nothing wrong with saying that T_P has been reduced to T_F.

Problem 7. The first criticism is that the notion that two theories be analogous to each other seems hopelessly vague and that therefore an account of reduction based on this is a non-starter. At least in the context of GNS, not anything goes, however. There are two conditions that T_P^* must satisfy. First, we require that the two theories use the same conceptual machinery: T_P^* must share with T_P all essential terms. Consider again the Second Law. T_P^* is couched in the same terms as T_P, namely entropy, and differs only in how the properties vary, namely that in the former entropy fluctuates. Second, Schaffner (1967, p. 144) 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 because experiments show that entropy fluctuates as predicted by T_P^* (and *ruled out* by T_P). While Schaffner's requirement sits well with the example of the Second Law, it may be too restrictive in general. So we propose a slightly weaker requirement, doing the same work without running the risk of ruling bona fide reductions. The requirement is that T_P^* be *at least equally empirically* adequate as T_P. These two conditions undercut any attempt at playing fast and loose with analogies in such a way as might.

There is a further worry that there is no *general characterisation* of 'strongly analogous', but such a characterisation is an essential part of a workable theory of reduction. Therefore, the criterion that T_P^* and T_P be strongly analogous is empty and GNS is not a definite position at all. We disagree with this conclusion. Being strongly analogous is a contextual relation, and we should not expect there to be a general theory of analogy. Whether or not T_P^* is strongly analogous to T_P has to be decided either in the relevant scientific discipline itself or the special philosophy of it. The above example of the derivation of the Second Law makes this clear. That a close cousin of the Second Law of TD allowing for fluctuations is strongly analogous to the strict 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 a watered down version 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.

The third worry is that, unless the analogy is identity, T_P has in fact been *replaced* rather than reduced, and so we should not longer speak about reduction; in fact, T_P^*, not T_P, has been reduced. This is a matter of definition. If the term 'reduction' is reserved for cases of *exact* derivation, then T_P is not reduced. However, we see no reason to regiment language in this way. As we have just seen, GNS imposes strict conditions and what counts as a strong analogy is by no means arbitrary. As long as it is understood that reduction involves an analogy of this kind, we can see no harm in calling the GNS procedure 'reduction'.

5 Reduction and Reductionism

We have argued that GNS is alive and well, and that scientists involved in a reductionist research programme do the right thing if they take GNS 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). What we have presented is an analysis of what a successful reduction would look like, and, as such, it does not prejudge whether or not there are such reductions. Whether any given theory can actually 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. But this does not render GNS superfluous; the question of whether or not a purported reduction is a successful reduction can only be answered against the background of a presupposed conception of reduction, and it is this conception that GNS provides.

This 'wait and see' attitude does not conflict with our claim that reduction is to great extend driven by the desire for consistency. Consistency, so one might argue, is absolutely necessary and once one sees reduction as driven by the quest for consistency there better be reductions across the board. Therefore, so the argument goes, we are committed to reductionism after all. This is wrong. We are committed to the claim that *if* we have a situation of the kind described above (in which the two theories have an overlapping target domain), *then* one must have a reduction.[24] However, we are not committed to the claim that the situation described in the antecedent is ubiquitous. Whether there are such overlaps is an empirical question, and unless one can somehow make it plausible that such overlaps are ubiquitous, the view on reduction we advocate does not force reductionism upon us.

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[24] We assume here that we want to keep the reduced theory and that elimination is not a possibility. Then reduction seems the only way to establish consistency. Of course, if there is another way to achieve consistency (whilst keeping the reduced theory), then we would not be forced to claim that there has got to be a reduction, after all.

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