Reduction and Multiple Realizability

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Abstract

This paper addresses the recent resurgence of Nagel style reduction in the philosophical literature. In particular, it considers the so-called multiple realizability objection to reductionism presented most forcefully by Sober in 1999. It is argued that this objection misses the point of multiple realizability and that there remain serious problems for reductionist methodologies in science.

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1 Introduction

In both contemporary physics and philosophy of science, there is a tension between reductionist and emergentist methodologies. In high energy (or so-called "fundamental") physics the dominant methodology is reductionist. It has been tremendously successful in explaining and describing various deep features of the universe. This methodology asserts that we should search for the basic building blocks of the universe and then, having found them, provide an account of the nonfundamental features of the world that we see at length scales much larger (or at much lower energies) than those investigated by particle accelerators. From this reductionist perspective, emergent phenomena, if there are any, would be those that apparently are not reducible to, or explainable in terms of, the properties and behaviors of these fundamental building blocks. And, of course, the strong form of reductionism will deny the existence of emergent phenomena.

The very talk of "building blocks" and fundamental particles carries with it a particular, and widespread view of how to understand emergence in contrast with reductionism: In particular, it strongly suggests a mereological or part/whole conception of the distinction.[1] Emergent phenomena, on this

[1] Without doing a literature survey, as it is well-trodden territory, one can simply note that virtually every view of emergent properties canvassed in O'Connor's and Wong's *Stanford Encyclopedia* article reflects some conception of a hierarchy of levels characterized by aggregation of parts to form new wholes organized out of those parts. [15]

conception, are properties of systems that are novel, new, unexplainable, or unpredictable in terms of the *components or parts* out of which those systems are formed. Put crudely, but suggestively, emergent phenomena reflect the possibility that the whole can display very different behaviors than the simple mereological sum of its parts.

While I believe that sometimes one can think of reduction in contrast to emergence in mereological terms, in many instances the part/whole conception misses what is actually most important. Often it is very difficult to identify what are the fundamental parts. While identifying the fundamental parts of a physical system can be a challenging task, it is often more difficult (and more central to understanding physical behavior) to see how the properties of those parts play a role in determining the behavior of systems at scales much larger than the length and energy scales characteristic of those parts. In fact, what is most often crucial to the investigation of the models and theories that characterize systems is the fact that there is an enormous separation of scales at which one wishes to model or understand the systems' behaviors—scale often matters, parts not so much.[2]

Despite the preference for a part/whole conception of reductive relations, traditional philosophical accounts of reduction have not been expressed in explicitly mereological terms. The next section presents the standard philosophical account of reduction. This account, due originally to Nagel, has had its ups and downs, though recently there have been a number of papers arguing that it is the correct way to think about reduction. In section 3 I present what many have taken to constitute the most important objection to Nagel reduction—the problem of multiple realizability. Next, in section 3.1 I discuss an influential argument due to Elliott Sober to the effect that this objection is misguided. I argue that Sober's rebuttal misses what is actually the relevant objection to reductionism from multiple realizability. Section 4 then presents a response to the problem from multiple realizability as I understand it. But it is not a response that is friendly to reductionism. Finally, in section 5 I consider another way of thinking about reductionism and discuss a pair of examples that fit better with actual scientific practice.

[2]Some examples, particularly from theories of optics, where one can speak of relations between theories and models where no part/whole relations seem to be relevant can be found in [3].

2 Philosophical Notions of Reduction

Philosophical ideas about the nature of reduction have their genesis in Ernest Nagel's 1961 book *The Structure of Science* [14]. He asserts that "[r]eduction ... is the explanation of a theory or a set of experimental laws established in one area of inquiry, by a theory usually though not invariably formulated for some other domain." [14, p. 338] It is important for what follows that reduction, on Nagel's view, is an *explanatory* enterprise. (See section 3.1). The following schema captures the core of Nagel's understanding of intertheory reduction:

- **N**: A theory T (explanatorily) reduces a theory T' if and only if the laws of T' are derivable from the laws of T.

Many theories that are purportedly reduced to others by Nagelian reduction contain terms (predicates) that are absent in the reducing theory. For example, a paradigmatic example of intertheory reduction in physics is the reduction of (parts of) thermodynamics to statistical mechanics. In Nagel's book he discusses the reduction of the ideal gas law $PV = nRT$ to statistical mechanics. Note in particular, that the predicate *temperature*, T, does not appear in the reducing theory of statistical mechanics. Reductions of this sort are called, by Nagel, "heterogeneous." They pose a problem for Schema **N** because straightforward logical derivation (of the type Nagel envisioned) of any law statement containing a predicate that appears nowhere in the reducing theory will be impossible. "[I]f the laws of the secondary science [the reduced theory] contain terms that do not occur in the theoretical assumptions of the primary discipline [the reducing theory] ..., the logical derivation of the former from the latter is *prima facie* impossible." [14, p. 352]

In order to perform the requisite derivation one needs so-called "bridge laws" that connect the predicate in the reduced theory (e.g., temperature) with some predicate in the reducing theory (e.g., mean molecular kinetic energy). In fact, Nagel proposed two necessary formal conditions for reduction:

- *Connectability*. "Assumptions of some kind must be introduced which postulate suitable relations between whatever is signified by 'A' [a term in the reduced theory's (T') vocabulary] and traits represented by theoretical terms already present in the primary [reducing] theory."

- *Derivability*. "With the help of these additional assumptions, all the laws of the secondary science, including those containing the term 'A,'

> must be derivable from the theoretical premises and their associated coordinating definitions in the primary discipline." [14, pp. 353–354]

Many articles have explored and attempted to establish exactly what these "suitable relations" or bridge laws are supposed to be like. Are they established by linguistic convention? Are they factual discoveries? Are they to be identities? Etc.

Recently, a number of philosophers have argued that despite many years in apparent disrepute, the Nagel account of reduction is largely correct and should, therefore, be resurrected. I am referring to articles by Butterfield [8, 7, 9], by Dizadji-Bahmani, et al., [10], and by Schaffner [18]. These authors endorse Elliott Sober's [19] dismissal of what to some seemed to be a devastating objection to Nagelian reduction—the objection from multiple realizability. [19] This argument has its genesis in work by Putnam [17] but received its most influential formulation in Fodor's "Special Sciences" paper. [11] Butterfield holds that Sober has "definitively refuted [the multiple realizability argument against reductionism] . . . without needing to make contentious assumptions about topics like explanation, natural kind and law of nature." [7, p. 942] Once we examine Sober's argument in the next section, we will see reasons to question Butterfield's claim. Specifically, Sober makes some quite strong and contentious claims about explanation.

In the following sections I will the outline the multiple realizability argument against Nagelian reduction consider and and I will critically examine Sober's response. One aim is to show that these arguments are not persuasive. Another will be to offer some positive remarks about the kind of reductionist methodology upon which philosophers ought to be focused.

3 Multiple Realizability

In the paper "Special Sciences, or the Disunity of Sciences as a Working Hypothesis" [11] Fodor argues that the possibility of multiple realizability of special science properties is a major component in an argument to the effect that the special science cannot be reduced to some lower-level, more fundamental science—ultimately to physics. Multiple realizability of a given property means that that property is realized by or implemented in a wide variety of lower-level *heterogeneous* physical states or properties.

As an example, consider a paradigm case (originally introduced by Putnam [16] and employed by Kim [13]) of the purported multiple realizability of the mineral jade. Jade, supposedly has two distinct physical realizers: jadeite and nephrite that are distinct chemical kinds. In the kind of toy "theory" that typically features in these arguments, the question is whether one can reduce a theory of Jade containing a "law"—*all jade is green*—to a lower-level theory that recognizes jadeite and nephrite as distinct chemical realizers. To do so on a Nagelian conception of reduction, will require the satisfaction of the *Connectability* requirement where the upper level predicate "jade" will be related to "traits represented by theoretical terms already present in the primary [chemical theory that concerns the properties jadeite and nephrite and their differences]." [14, p. 353]

An obvious connection (if, in fact, jadeite and nephrite are the sole realizers of jade) is provided by the following bridge law:

1. $(\forall x)(x \text{ is jade} \leftrightarrow (x \text{ is jadeite} \vee x \text{ is nephrite}))$

But Fodor argues that the disjunction of jadeite and nephrite is too heterogeneous to be a natural kind of the lower-level chemical/mineral theory. Kim strengthens this intuition by arguing that the natural kinds are individuated by their causal powers and that causal powers of upper level properties just are the causal powers of their realizers:

- *Principle of Causal Individuation of Kinds*: Kinds in science are individuated on the basis of causal powers; that is, objects and events fall under a kind, or share a property insofar as they have similar causal powers. [13, p. 17]

- *Causal Inheritance Principle*: If an upper level property is realized in a system on an occasion in virtue of a "physical realization base P," then the causal powers of that upper level property are identical with the causal powers of P. [13, p. 18]

If one accepts these principles as applying to the case of jadeite and nephrite, then the argument against reduction proceeds as follows: If kinds are individuated on the basis of causal powers, and if the causal powers of the distinct realizers for the upper level property, jade, are radically distinct/heterogeneous, then the realizations of jade on distinct occasions will be realizations of distinct kinds. Appealing to disjunctive properties as kinds in (3), for example, is just beside the point. There is no bridge-law that can respect the differing nature of the heterogeneous realizers. On Kim's view, in fact, this argument can be used to show that the upper level generalization—*all jade is green*—isn't a law at all and there is no special science for jade. Or, if we insist that there is a special science for jade, it is not autonomous from the lower-level chemical/mineral theory of jadeite and nephrite.

I do not want to go into the details of the enormous debate about the legitimacy or illegitimacy of these arguments. Instead, since Butterfield and others endorse Sober's argument to the effect that multiple realizability does not threaten reduction, I will briefly examine Sober's discussion.

3.1 Sober on Reduction and Multiple Realizability

In "The Multiple Realizability Argument Against Reductionism" [19], Sober adopts a Nagel-like position about reductionism. I say "Nagel-like" because he doesn't explicitly endorse Nagel's view but he does endorse the Nagelian idea (quoted earlier) that reduction involves *explanation.* According to Sober there are two claims that form "at least part of what reductionism asserts:

i. Every singular occurrence that a higher-level science can explain also can be explained by a lower-level science.

ii. Every law in a higher-level science can be explained by laws in a lower-level science." [19, p. 543]

He follows these claims with the following rider:

> The "can" in these claims is supposed to mean "can in principle," not "can in practice." Science is not now complete; there is a lot that the physics of the present fails to tell us about societies, minds, and living things. However, a completed physics would not this be limited, or so reductionism asserts .... [19, p. 543]

I will have more to say about the use of "in principle" claims below.

Sober considers Putnam's famous peg and board example to assess the plausibility of (i) and (ii). We are asked to consider a board containing two holes, one square of side length $1cm$, the other round of diameter $1cm$. Next note that a square peg of side length $.9cm$ will fit through the square whole but not the round, circular hole. Why? Putnam claims that the macroscopic geometric properties of the peg and board system explain this fact and that an appeal to the microstructure (atomic/molecular) of the board and peg will *not* explain this fact. (A long detailed quantum mechanical description of the board and peg seems completely unnecessary to explain the macroscopic behavior of this system.) If he's right about this and (i) and (ii) characterize relevant features of reductionism, then reductionism will fail.

Sober counters that intuitions can pull one in different directions and that Putnam's claim about the explanatory priority of the macroscopic regularity is illusory.

> Perhaps the micro-details do not interest *Putnam*, but they may interest *others*, and for perfectly legitimate reasons. Explanations come with different levels of detail. When someone tells you more than you want to hear, this does not mean that what is said fails to be an explanation. There is a difference between explaining too much and not explaining at all. [19, p. 547]

Sober asks us to consider two peg-and-board systems. For the sake of argument, let us assume that the first board and peg are made of a ferrous material, like iron; and that the second system is made of some non-ferrous material, such as aluminum. (These difference might very well effect the behavior of the peg as it goes through the square holes as there may be magnetic effects in the iron peg-and-board system absent in the aluminum system.) If we adopt Putnam's macro explanation, then we will have the same explanation for the pegs' behavior in the two cases. This has the advantage of providing a "unified" explanation of the different systems behaviors. On the other hand, if we opt for a micro-explanation, then, since the pegs and boards are different, the micro details and hence the micro explanations will likewise be different. In such a case we will have a less unified or a "disunified" explanation. [19, pp. 550-551] Is the choice between providing a unified vs. a disunified explanation of the pegs' behavior an *objective* choice between two genuinely competing explanations? Sober says "no."

> ...I am claiming that there is no objective reason to prefer the unified over the disunified explanation. Science has room for both lumpers and splitters. Some people may not be interested in hearing that the two systems are in fact different; the fact that they have the same macro-properties may be all they wish to learn. But this does not show that discerning differences is less explanatory. Indeed, many scientists would find it more illuminating to be shown how the same effect is reached by different causal pathways. [19, p. 551]

We see that Sober again counters by claiming that the choice between the unifying and disunifying explanation is a pragmatic choice.

However, I think that his response here *reflects a confusion about exactly what is to be explained.* Sober is concerned with the question of whether every singular occurrence and every law of a higher-level science can be reductively explained by appeal to fundamental physics. But there is another question we can ask, and I think it provides the real challenge to reductionism from the existence of multiply realized higher-scale patterns. The challenge of multiple realizability with respect to explanatory reduction is to provide an answer to the following question:

- (**MR**) How can systems that are heterogeneous at some (typically) micro-scale exhibit the same pattern of behavior at the macro-scale?

Now we can ask the following: Do the "disunified" explanations actually provide an answer to this question? For that matter, does the "unified" explanation actually provide an answer to this question? I contend that neither do. And so, Sober and those who endorse his argument, have really missed the crucial challenge to reductionism.

Consider the two micro-explanations of the pegs' behavior relative to the boards'. The first peg, call it "A" needs to be described in all of its quantum mechanical glory. Since we are considering explanations to be "in principle" explanations, we can assume at this point that such a description can indeed be provided. Next, A's state description serves as input or initial data in the appropriate dynamical equation (the Schrödinger equation) from which we are to imagine we can derive its trajectory through the square hole in the first board. Similarly, a different state description of the second peg, "B," serves as initial data for determining the behavior of B's trajectory through the square hole in the second board. Of course, we are going to need extreme micro-descriptions (quantum descriptions) of the two boards as well. Given the differences in materials (iron vs. aluminum), these descriptions will, likewise, be very dissimilar—the macro behavior of the two systems is multiply realized by heterogeneous realizers.

These distinct derivations are completely disjoint. The derivation of A's behavior tells us nothing about the behavior of B, and *vice versa*. In what sense have we provided an explanation for the common macro-scale behavior of these two peg-and-board systems by performing these in principle derivations? Recall the problem is to answer (**MR**). I suggest that the only way to answer this is to provide an account of why the *details* that genuinely distinguish these systems from one another (details that tell us that the microstructure of iron and aluminum are genuinely distinct), are irrelevant for the macroscopic behavior of interest. Neither of these derivations provide such an account.

Does the upper level unified explanation provide an answer to our question? Here too I think that the answer is "no." The appeal to geometric properties does explain why peg A can proceed through the square hole and not through the round hole. Similarly, for the behavior of peg B. Does this explain *how multiple realizability is possible* according to the theory that distinguishes the realizers? No. Rather, it describes the behavior to be explained in non-fundamental terms. It appeals to the fact that the diagonal of the peg is greater than the diameter of the round hole. If we are interested in why pegs and boards exhibit this exclusionary behavior *despite the fact that they have different microstructures*, we don't have an answer. Compare this with an account of why a particular mineral is green: It is jade and all jade is green. But if we are interested in why jade's greenness is realized by the distinct mineral/chemical structures of jadeite and nephrite, this doesn't provide an answer.

The challenge of multiple realizability to explanatory reductionism *properly understood,* concerns the ability of the *theory of the heterogeneous micro-realizers* to explain the common behavior displayed by the systems at macroscales. But as we have seen, "disunified" explanations, while certainly telling us a lot about the behavior of individual systems, do not explain the common behavior. And, this is true even if we buy into the idea that someday we will have a completed physics.

Sober's take home message is that reductionists should

> build on the bare proposition that physics in principle can explain any singular occurrence that a higher-level science is able to explain. The level of detail in such physical explanations may be more than many would want to hear, but a genuine explanation is provided nonetheless, and it has a property that the multiple realizability argument has overlooked. For reductionists, the interesting feature of physical explanations of social, psychological, and biological phenomena is that they use the same basic theoretical machinery that is used to explain phenomena that are nonsocial, nonpsychological, and nonbiological. . . . The special sciences unify by abstracting away from physical details; reductionism as-

> serts that physics unifies because everything can be explained, and explained *completely*, by adverting to physical details. [19, p. 561]

Note that throughout his argument, Sober assumes that reduction is *explanatory reduction*—an assumption Butterfield denies, holding instead that it is just "definitional extension." He also assumes that having an explanation is an *in principle* claim about a *completed ideal physics*. Both of these assumptions are, contrary to Butterfield's assertion, quite contentious. Sober's argument simply doesn't apply to a view of reduction that doesn't aim at explanation. After all, his very definition of reduction expressed above in (i) and (ii) refers to an explanatory project. Finally, whether it even makes sense to talk of an ideal complete physics is a matter of contention.

For the sake of argument, let us grant momentarily that physics "in principle can explain any singular occurrence that a higher level theory is able to explain." I have been arguing that this is actually not relevant to the real problem posed by the multiple realizability of the higher level science. One can grant that the singular occurrence of peg A's passing through the square hole may in principle be explained by physics while denying that that explanation (or even that explanation in conjunction with the explanation of B's behavior) explains how the different realizers can exhibit the same macro-scale behavior.

We need to distinguish between two types of explanatory why-questions:

I. Why does an individual system display an instance of macro-scale pattern?

II. Why, in general, are such macro-scale pattern to be expected or even possible? [3, pp. 23-25]

The challenge of multiple realizability, expressed by (**MR**), demands that the second question be answered. But the proposition that physics can explain any given instance or "singular occurrence" asserts only that questions of type (I) can be answered and does not in any way guarantee that questions of type (II) are answered by the same derivations that answer type (I) questions.

At a minimum, I believe this shows that a very different kind of explanation is required to answer type (II) questions. Further, as Sober does not recognize the difference, this opens up the possibility that one can have an explanation of how multiple realizability is possible *without* having a Nagel type intertheoretic reduction. In order to establish this, I will need to briefly present an argument scheme that does answer type (II) questions and argue that it really is quite distinct from the kinds of explanatory derivations envisioned by Sober and the new Nagelian reductionists mentioned earlier.

4 Explaining Multiple Realizability or Universality

As noted, a response to question (**MR**) requires demonstrating that the micro details that genuinely distinguish the heterogeneous realizers of some macro-scale pattern of behavior are irrelevant for that behavior's occurrence. In several places I've argued that such a demonstration is explanatory and has a character quite distinct from standard deductive-nomological strategies favored by philosophical reductionists of Nagelian stripe. [3, 1, 2, 5] The paradigm example of such an explanatory demonstration is provided by the renormalization group explanation of universal behavior in condensed matter physics.

Physicists use the terms "universality" and "universal behavior" to refer to identical behavior displayed by different systems. In [2, 3] I've argued that one should think of this notion of universality as being the same as the philosophers' conception of multiple realizability—different systems with very different micro details exhibiting the same macroscopic behavior. Thus, if renormalization group arguments can explain universality, then they can provide explanations of how multiple realizability is possible. In other words, we should look to these arguments as providing at least one way to answer question (**MR**).

Just to make things a little more concrete, consider figure 1 from a famous paper by Guggenheim 1945. This figure plots the temperature vs. density of eight different fluids in reduced (dimensionless) coordinates. Values on the x-axis below 1.0 represent the density of vapor phase of the fluids the values above 1.0 represent the density of the liquid phases of the fluids. Thus at 1.0 the densities of the different phases are the same. The y-axis plots the critical temperature of the fluids where the value 1.0 means that a system's temperature is the critical temperature. The curve is called a coexistence curve and it provides the various densities of liquid and vapor phases at different temperatures. The remarkable thing about this plot is

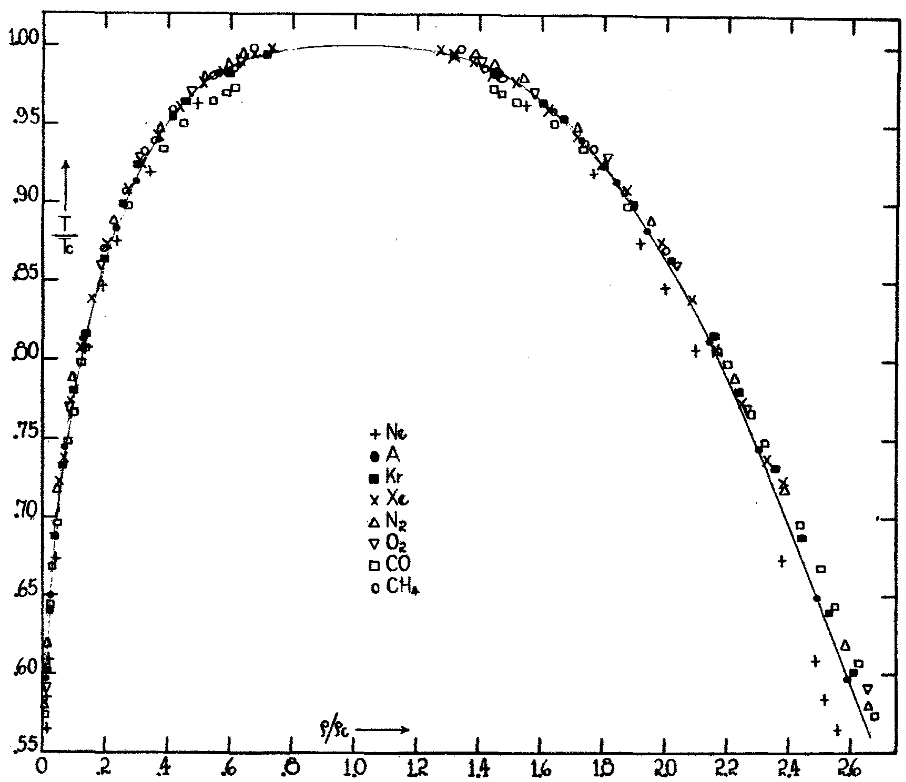

Figure 1: Universality of Critical Phenomena [12]

the fact that it shows the shape of the coexistence curve to be *the same for each fluid at its critical value for density and temperature*: Every different fluid represented has a different molecular make-up. For example, Neon, Ne, and methane, CH_4 have very different microstructures. As a result of these different microstructures the actual critical temperatures and critical densities of each fluid will be different. Nevertheless, the fact that when one plots the behaviors of these different systems in reduced coordinates, one can see that each system exhibits identical behavior near their respective critical points—*the shape of the curve is identical for each system.* This is a paradigm example of universality/multiple realizability. Each molecularly distinct system exhibits the same macro behavior represented by the fact that the data for each system all lie on the same curve. How is this remarkable multiply realized pattern possible?

Again, if we provided a detailed derivation from the quantum mechanical state for a particular neon fluid we might be able to show that the coexistence curve for neon has this shape. But that derivation will be different from one that would demonstrate that a particular methane fluid also realizes the same shaped coexistence curve. The "disunified" explanations will not answer the relevant question of the form (**MR**).

It wasn't until the 1970's that there was a satisfactory answer to how this universality is possible. That answer came out of work by Leo Kadanoff, Michael Fisher, and Ken Wilson. Wilson won the Nobel prize for finalizing the technique that enables one to demonstrate that the (molecular) details that genuinely distinguish the different fluids from one another (that genuinely allow us to see, for example, that each has a different critical temperature and critical density) are *irrelevant* for the common macro-scale behavior of interest (that they all have coexistence curves of the same shape). This mathematical argument is called the renormalization group explanation of the universality of critical phenomena.

Let me very briefly and non-technically outline the explanatory strategy. One constructs an enormous abstract space each point of which might represent a real fluid, a possible fluid, a solid, etc. Next one induces on this space a transformation that has the effect, essentially, of eliminating degrees of freedom by some kind of averaging rule. The idea exploits the fact that near the critical point systems exhibit the property of self-similarity. This allows one to the degrees of freedom in the or original system with the averages. One then rescales the system in an appropriate way that takes the original system to a new (possibly nonactual) system/model in the space of

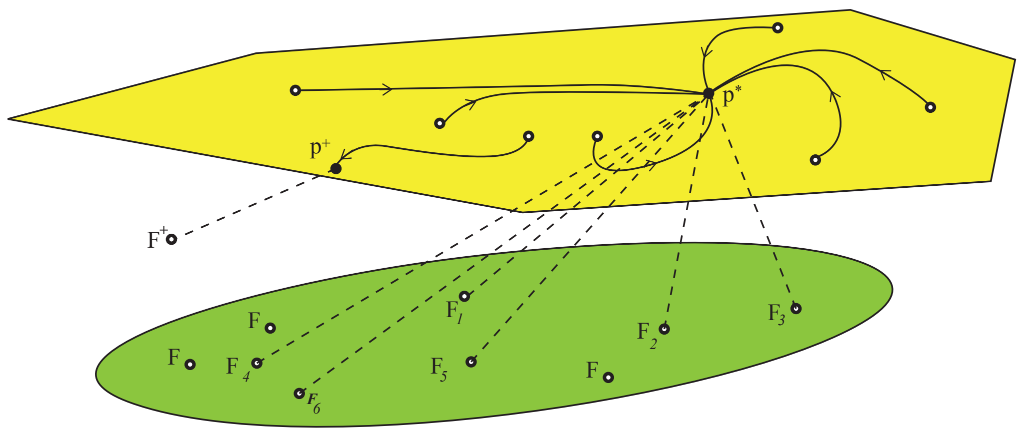

Figure 2: Fixed Point in Abstract Space and Universality Class

systems that exhibits macro-scale behavior similar to the system one started with. This provides a (renormalization group) transformation on all systems in the abstract space. By repeatedly performing this operation, one eliminates more and more detail that is irrelevant for that macro behavior. Next, one examines the topology of the induced transformation on the abstract space and searches for fixed points of the transformation. (If τ represents the transformation and p^* is a fixed point we will have $\tau(p^*) = p^*$.) Those systems/models (points in the space) that flow to the *same* fixed point are in the same universality class—the universality class is delimited—and they will exhibit the same macro-behavior.[3] That macro-behavior can be determined by an analysis of the transformation in the neighborhood of the fixed point.

In figure 2, the lower collection represents systems in the universality class delimited by the fact that these systems/models flowed to the same fixed point, p^*, under the appropriate (renormalization group) transformation τ in the upper abstract space. Note that another system/model, F^+ fails to flow to the fixed point p^* and so that system/model is not in the universality

[3] To put this another way: The universality class is the basin of attraction of the fixed point.

class.

The argument just sketched, by which one can delimit the class of heterogeneous systems all exhibiting the same macro-behavior, is not remotely like the kind of derivation from initial data and fundamental equation of the kind Sober sees in the disunified explanations he discusses. In fact, it is difficult to consider this story in all of its mathematical complexity as a derivation of the kind Nagelian's appear to demand for reductive explanation. Nevertheless, it is an *explanation* of how the universal/multiply realized common macro-behavior is possible from the point of view of a theory that genuinely distinguishes the realizers from one another.

Note that we have neither explained a single occurrence of a higher-level property nor a higher-level law. We have provided, instead and answer to the question (**MR**). Sober's way of framing the problem simply misses this difference.

I contend that this kind of strategy can provide an explanation for universal/multiply realized behavior without satisfying the criterion of derivability that is essential for Nagelian reduction. This means that, *pace* Sober, when properly understood, the argument from multiple realizability—answering question (**MR**)—does pose a serious challenge to Nagelian reductionism.

5 Another Look at Reductionism

As we have seen, Schema **N** treats the reduction of one theory to another as a matter of derivation in which the reduced theory's laws are derived from the laws of the reducing. Typical examples are the reduction of parts of thermodynamics to statistical mechanics, the reduction of classical mechanics to quantum mechanics, etc. It is typical that the older, less encompassing or *coarser*, and perhaps less fundamental theory is said to reduce to the newer, more encompassing or *finer*, and perhaps more fundamental theory. In the physics literature, however, one often finds claims of reduction going in the other direction. It is sometimes said that Statistical mechanics reduces to thermodynamics in the limit in which the number of particles goes to infinity. Similarly, physicists tend to assert that quantum mechanics reduces to classical mechanics in the limit in which Planck's constant can be said to be small. In general, the idea is that the finer theory (T_f) reduces to the coarser one (T_c) as the limit of some parameter, ϵ appearing in that finer theory approaches some value (typically 0 or ∞):

$$\lim_{\epsilon \to 0} T_f = T_c.$$

I have argued that in many instances, pairs of physical theories are best investigated by paying attention to the nature of the limiting behavior between them. [3, p. 78–80] One reason for this is that there can be different kinds of limiting relations. Sometimes there are smooth or regular relations between equations of different theories. But most of the time, the limits are *singular*. A limit is singular as opposed to regular if the behavior of the equation as the limit is being approached (no matter how small ϵ is, though greater than zero) is qualitatively distinct from the behavior when ϵ is identically equal to zero. These qualitative differences are often indicative of interesting and novel behavior.

Rather than rehearsing arguments about theories (or relations between equations in different theories) I've already given, I want to present an example where one needs to consider relations between behaviors displayed by a *given system* at different scales. I will argue that these behaviors require different explanations and that they cannot be related to one another in a way that privileges the lower-scale, "more fundamental" level of explanation. As such, I think this example—and myriads of others like it—pose a serious challenge to bottom-up reductionist methodologies.

Consider a violin string of length L. See figure 3.[4] Suppose we are interested in determining the harmonic behavior of the string. In order to determine the harmonic modes[5] one needs to solve the wave equation—a hyperbolic partial differential equation. In order to solve it one needs to impose so-called boundary conditions. To derive the harmonics exhibited in figure 3 the boundary conditions demand that the two ends of the string remain fixed. Strictly speaking we require that they be zero dimensional or point boundaries. That is to say, they don't wiggle at all as time progresses. Physically these mathematical boundary conditions correspond to the string's not moving at the bridge of the violin and at the nut. Without these strict conditions, one cannot derive the harmonic structure.

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[4]Thanks to Julia Bursten for the figure and for discussions about this example.

[5]These are the overtones associated with the fundamental vibrational length of the string. Tone-based musical instruments have harmonic modes for each fundamental pitch or chord, and like varying volumes of members of a choir, the relative strength of the harmonic overtones determines the particular timbre and character of an instrument's sound.

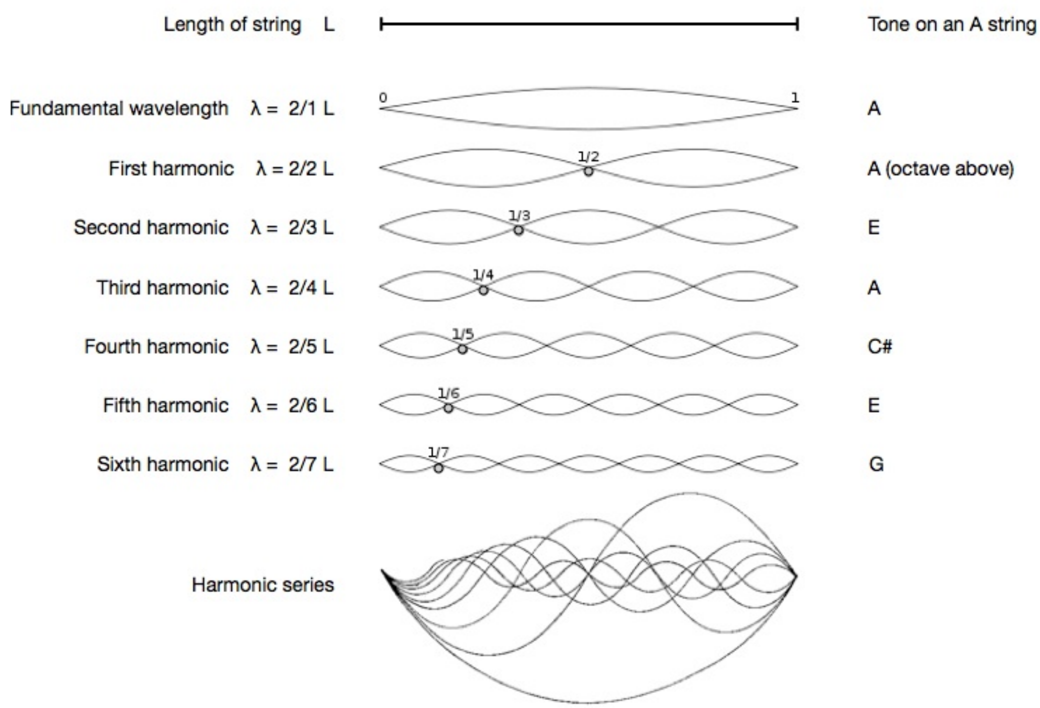

Figure 3: Harmonic Series for an A-String

On the other hand, if the string is really fixed and immovable at the two ends, then, physically, we would not be able to hear the violin! After all, the sound box of the violin amplifies the sound. But if the string is genuinely fixed and rigid at the endpoints, there will be no transfer of energy to the violin's sound box and no sound will come out of it.

If we want to be able to explain and understand how we hear the violin when it is played, we need to model the actual interaction between the string at the bridge and nut. But this involves *completely shifting scales and requires that we engage in molecular modeling of the interactions at the strings endpoints*. Of course, if we do this, we lose the boundary condition required for our continuum explanation of the harmonic structure.

To explain the harmonic structure we must suppress the detailed lower scale physics by crushing all of that detail to a mathematical point. Mark Wilson calls this suppression of details "physics avoidance." [20, pp. 184–192]

Physics avoidance might seem required on purely practical or pragmatic grounds. One might claim: "*In principle*, we can explain the harmonic structure of the violin string by appeal only to lower scale atomic and molecular details." In fact, this is exactly the kind of strategy reductionists always insist is possible. We've seen that Sober, too, thinks it makes sense to appeal to "in principle" derivations from an ideal completed physical theory. [19, p. 16] But it is hard indeed to see how one can derive continuum wave behavior from purely atomic and molecular considerations. Appeals to the possibility of *in principle* derivations rarely, if ever, come with even the slightest suggestion about how the derivations are supposed to go. At the very least one needs to consider limiting relations between discrete models and continuum models of the kind that say "let the number of molecules/atoms be infinite." This, however, doesn't involve "derivation" in the sense typically understood by Nagelian reductionists. The latter, as we've noted, typically refer to derivation from "laws" usually understood as equations of motion (in this case molecular dynamical equations of motion.) The mathematics of these equations are quite different in form from the continuum wave equation that we need to solve in order to determine the harmonic structure of the string. The claim that *in principle* these limiting derivations can be performed *without some attempt to say how that can happen* is philosophically empty.

To stress this point let me briefly consider another macro vs. micro problem that is a paradigm of cutting-edge research in the physics of materials.

The bending behavior of a steel beam, say, is remarkably well described and explained by a continuum equation (the Navier-Cauchy equation) that was derived long before there was any empirical evidence for atoms. Naturally, it makes no reference to any structure in the beam, treating it as completely homogeneous at all scales. The only empirical input to the equation comes in the form of various constitutive parameters such as Young's modulus that in effect define the material of interest. Values for Young's modulus are determined typically through table-top measurements of how much a material extends upon being pulled and shortens upon being squeezed. These values are clearly related somehow to the actual atomic and lower scale structures (inhomogeneities) present in the beam. But determining the connection between these lower scale structures and the values for the constitutive parameters is a difficult mathematical problem knowns as "homogenization."[6]

In fact, one cannot determine the values for the material/constitutive parameters (or even bounds within which the values will be found) by purely atomic/lattice scale modeling. Structures within the beam at scales in between the micro and macro play a critical role in determining the macro/coninuum behavior.[7] To bridge the gap between models at the scale of atoms and models at the scale of meters requires information being passed both upward (as reductionists demand) and downward (as emergentists typically demand). The mathematics of homogenization plays a crucial role in these interactions between models at various scales. Here is a passage from a primer on continuum micromechanics that supports this (philosophically) nonstandard point of view.

> The "bridging of length scales", which constitutes the central issue of continuum micromechanics, involves two main tasks. On the one hand, the behavior at some larger length scale (the macroscale) must be estimated or bounded by using information from a smaller length scale (the microscale), i.e., homogenization or upscaling problems must be solved. The most important applications of homogenization are materials characterization, i.e., simulating the overall material response under simple loading conditions such as uniaxial tensile tests, and constitutive modeling, where the responses to general loads, load paths and loading se-

[6]In fact, the mathematics involved is related quite intimately to the renormalization group arguments discussed in section 4.

[7]For more details than I can go into here see [4].

> quences must be described. Homogenization (or coarse graining) may be interpreted as describing the behavior of a material that is inhomogeneous at some lower length scale in terms of a (fictitious) energetically equivalent, homogeneous reference material at some higher length scale. On the other hand, the local responses at the smaller length scale may be deduced from the loading conditions (and, where appropriate, from the load histories) on the larger length scale. This task, which corresponds to zooming in on the local fields in an inhomogeneous material, is referred to as localization, downscaling or fine graining. In either case the main inputs are the geometrical arrangement and the material behaviors of the constituents at the microscale. [6, pp.3–4]

It is crucial here to note the role of what Böhm calls "downscaling." We need information about the material natures of structures at small scales, but we get this by "[inference] from the loading conditions ... on the larger length scale."

Thus I think the reductionist ideal of in principle derivation of behaviors of systems (or laws of theories) from from more "fundamental" lower scale details (or more fundamental theories) is largely mistaken. Any examination of the actual practice of scientists interested in modeling systems at different scales will reveal nothing as simple as the kind of derivation that proponents of Nagelian reduction believe is possible. The appeal to a completed ideal physics—the main feature that underwrites these in principle claims—is purely aspirational and speculative. We have no idea what such a physics would look like, nor do we have any real evidence that it exists.

6 Conclusion

In this paper I have argued that multiply realizability, *when properly understood*, does indeed pose a serious objection to reductionism. The objection, properly understood, demands an answer to question:

- (**MR**) How can systems that are heterogeneous at some micro-scale exhibit the same pattern of behavior at the macro-scale?

In effect, this is a request for an account of the relative autonomy of the macro-scale pattern from the micro-scale details. After all, if the micro-scale details were relevant to the macro pattern, then the pattern would not persist as we change the micro details.

I argued that many reductionists who accept Sober's critique of the multiple realizability do so mistakenly, because he (and they) do not fully understand the challenge. I then outlined very briefly how one can, at least in some important cases, answer question (**MR**). This involves mathematical techniques that do not look anything like reductionists' conception of derivation or deducibility. These techniques allow one to show how details that genuinely distinguish realizers from one another are irrelevant to the existence of the pattern.

Finally, in section 5 I discussed two examples—understanding why a violin string exhibits particular harmonic tones and understanding how a steel beam behaves under elastic loading—that present severe challenges to traditional philosophical views about reduction.

The positive message to be gleaned from these examples is that one *can* sometimes bridge between macro- and micro-scales. Applied mathematicians, material scientists, and physicists have begun to develop means for such multiscale modeling. Philosophers of science need to pay more attention to the subtleties involved in these attempts. The debate between reductionists and emergentists has too long been framed as an absolute, all-or-nothing affair. In fact, multiscale modeling is a very complex enterprise. Focusing on this applied work will provide us with a much better, more nuanced understanding of the scientific method.

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