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Autonomy of Theories: An Explanatory Problem

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Abstract

This paper aims to draw attention to an explanatory problem posed by the existence of multiply realized or universal behavior exhibited by certain physical systems. The problem is to explain how it is possible that systems radically distinct at lower-scales can nevertheless exhibit identical or nearly identical behavior at upper-scales (typically everyday length and time scales). Theoretically this is reflected by the fact that continuum theories such as fluid mechanics are spectacularly successful at predicting, describing, and explaining fluid behaviors despite the fact that they do not recognize the discrete (atomic/molecular) nature of fluids. A standard attempt to *reduce* one (continuum) theory to another (atomic/molecular theory), is shown to fail to answer the appropriate question about autonomy.

1. Introduction

In 1963 Clifford Truesdell declared that physicists are at a "disadvantage when facing common experience." (Truesdell 1984, p. 47) The reason for this, as he saw it, is that physicists are too focused on *fundamental theories* of the structure of matter. To a large extent they ignore phenomenological theories (such as continuum mechanics) that describe "less fundamental" or phenomenological behaviors of matter at everyday length and time scales. While 1963 was a long time ago, and we have since seen many debates about what should count as fundamental physics[1], philosophical theorizing mostly still privileges the reductionist approach that Truesdell scorns. Truesdell continues:

> The training of professional physicists today puts them under heavy disadvantage when it comes to understanding physical phenomena, much as did a training in theology some centuries ago. Ignorance commonly vents itself in expressions of contempt. Thus "physics" by definition, is become exclusively the study of the structure of matter, while anyone who considers physical phenomena on a supermolecular scale is kicked aside as not being a "real" physicist. Often "real" physicists let it be known that all gross phenomena easily could be described and predicted perfectly well by structural theories; that aside from the lack of "fundamental" (i.e. structural) interest in all things concerning ordinary materials such as water, air, and wood, the blocks to a truly "physical" (i.e. structural) treatment are "only mathematical." (Truesdell 1984, p. 47)

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The idea that "all gross phenomena easily could be described and predicted perfectly well by structural theories" is one of the core aspects of reductionism.[2] The idea that the only impediment to such reductionist predictions is mathematical serves to highlight the fact that reductionists often dismiss mathematical difficulties as *merely pragmatic* and that such pragmatic problems do not stand in the way of *in principle* derivations from fundamental theory.

Truesdell's real physicist also challenges the idea that phenomenological theories like continuum mechanics, or more generally special sciences like psychology and economics exhibit genuine autonomy from the more fundamental "structural" theories of high energy physics. If all generalizations of such phenomenological theories can really be described and predicted perfectly well by structural theories, then their apparent autonomy is just that—apparent. Exactly what I mean by "autonomy" here will become clearer as the paper develops. For the moment it suffices to think of it as referring to the relative independence of *successful* upper-scale (continuum) theories of material behavior from any specification of details about the actual molecular, atomic, or subatomic structure of the materials.[3]

In this paper I will elaborate on the notion of autonomy that is at play in this debate. While discussions about the relationships between "fundamental" or structural theories, and nonfundamental/phenomenological theories have often been framed in terms of questions of reduction and emergence, I would like to eschew such terminology to the extent that I can. In fact, I believe that the framework in which questions about reduction and emergence are usually raised actually hinders an investigation into what I believe is the most important feature of intertheory relations. This is the manifest (relative) *autonomy* of the "less fundamental" theories from "more fundamental theories." In what follows, I hope to present an argument to that effect.

To begin let us consider an old example due to Hilary Putnam (1975) about explanatory priorities.

2. Pegs and Boards

Although I just said I do not want to talk explicitly about reduction, it is useful to develop the nature of the kind of autonomy in question by discussing a famous example of Putnam's and an influential response to the conclusions Putnam draws from that example. This response from Elliott Sober (1999) is framed in terms of intertheoretic reduction. Putnam asks us to consider a (reasonably stiff or rigid) board containing two holes, one square of side length 1*cm*, the other round of diameter 1*cm*. Next note that a square peg of side length .9*cm* 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 a deduction from the microstructure (atomic/molecular) of the board and peg will *not* explain this fact. (Putnam asserts that a long detailed quantum mechanical description of the board and peg is completely unnecessary to explain the macroscopic behavior of this system: "If you want to, let us say that the deduction *is* an explanation, it is just a terrible explanation, and why look for terrible explanations when good ones are available?" (Putnam 1975, p. 296))

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. (Sober 1999, p. 547)

Sober considers Putnam's claim that the macroscopic explanation is superior to to the micro/quantum explanation as a challenge to a relatively standard understanding of *explanatory reductionism*. On this view the reductionist asserts that

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. (Sober 1999, 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 be this limited, or so reductionism asserts. . . . (Sober 1999, p. 543)

Note that the apparent superiority of the macroscopic/geometric explanation can in part be taken to be an expression of the fact that the lower-scale quantum mechanical details are not that relevant. Put differently, the macro-explanation is to a certain extent autonomous from any lower-scale details. One can strengthen this intuition by appealing to a multiple realizability argument made famous by Fodor in "Special Sciences, or the Disunity of Sciences as a Working Hypothesis." (Fodor 1974) To do so, we need to consider two systems of pegs and boards made of different materials. (This is exactly what Sober has us consider.)

So 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 differences might very well affect the behavior of the pegs as they go 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. (Sober 1999, pp. 550–551) Is the choice between providing a unified or 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. (Sober 1999, p. 551)

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

The multiple realizability argument challenges the reductionist claims expressed in (i) and (ii) by highlighting the *autonomy* of the higher-level (geometric) explanation from the lower-level (*very different*) micro-based explanations stemming from the microscopic details of the iron and aluminum boards and pegs. As we see, Sober thinks this autonomy is, at best, only apparent since the higher-level singular occurrences and the higher level generalizations are reductively explainable by lower-level physics. I think that this misses the real point of the challenge of multiple realizability.[4]

I believe that the real challenge posed by the possibility of multiple realizability is to provide an answer 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?

Note the this question refers to a macroscopic *pattern* of behavior. This is important. Patterns are repeatable and relatively robust phenomena. So (**MR**) is asking for an account of a repeatable, relatively stable phenomenon. As such the question concerns the very possibility of this stability under variations in lower-scale detail.

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 overlooked an important explanatory challenge.

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 (for now) we are considering explanations to be "in principle" explanations, we can assume at this point that such a description can indeed be provided.[5] 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. All this is just to say that 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 do they provide an explanation for the common macro-scale behavior of these two peg-and-board systems by performing these in principle derivations? One might respond by claiming that providing both derivations together explains the common behavior of the two systems. But this misses the key aspect of the question (**MR**); namely, that we are interested in *understanding what is responsible for the robustness or stability* of the macro-scale pattern of behavior. The disjoint explanations, even taken together, do not address this question. 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 at smaller scales (details that tell us that the microstructure of iron and aluminum are genuinely distinct[6]), are irrelevant for the macroscopic behavior of interest. In doing this one is able to demonstrate that the macroscopic behavior is stable under changes in the microscopic details. And, with the demonstration of that stability, one can understand how it is that the macro behavior is relatively autonomous from the micro details. Neither of these derivations provide anything like such an account.

Does the upper-level unified explanation provide an answer to our question? Here too I think that the answer is "no." An appeal to geometric properties together with the rigidity of the pegs and boards 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 in terms of the reducing theory alone.

The challenge of multiple realizability to explanatory reductionism *properly understood,* concerns the ability of the *theory of the heterogeneous micro-realizers* to explain the *robustness* of the common behavior displayed by the systems at macro-scales. That is, the challenge is to explain the *autonomy* of upper-scale common behavior from lower-scale details. However, as we have seen, "disunified" explanations, while certainly telling us a lot about the behavior of individual systems, do not explain the autonomy in question. And, this is true even if we buy into the idea that someday we will have a completed physics—even if we dismiss explanatory difficulties as "merely mathematical" or as involving only "pragmatic" difficulties.

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 asserts that physics unifies because everything can be explained, and explained *completely*, by adverting to physical details. (Sober 1999, p. 561)

This message however, does nothing to answer the question in (**MR**), which, I claim is a distinct problem concerning the explanation of the autonomy of upper-scale patterns of behavior from lower-scale detail. Neither the lumping nr the splitting strategies answer (**MR**).

I have followed Fodor's and Sober's lead here in framing the question (**MR**) in the context of a discussion about reduction and the special sciences. However, the key issue really concerns the fact (and it is indeed a fact) that many of our scientific theories of the behavior of systems and materials at everyday (continuum) length scales are *almost completely autonomous* of lower-scale details. This is the fact that (**MR**) asks us to explain.

3. How to Answer (MR)

Truesdell claims that "real physicists" insist the generalizations describing "gross phenomena" could easily be described and predicted by "structural"/fundamental theories. The only difficulty involved in carrying out such descriptions and predictions are mathematical. I take it that this dismissal is equivalent to the claim (expressed by Sober) that (according to explanatory reductionism) an in principle completed physics would be able to explain the singular occurrences and general laws appealed to in "higher-level" sciences. The argument above is in part meant to establish that the fact of autonomy (namely, the object of the question (**MR**)) cannot be explained by the *standard* (derivation-of-laws-from-laws) explanations to which philosophers typically appeal. That framework, as I noted above, does not allow for an answer to questions like (**MR**).

If the above argument is persuasive, then there is genuine explanatory challenge provided by the fact of multiple realizability. I believe that an answer to (**MR**) cannot be provided by the kinds of explanatory stories presupposed by the "real physicists" with whom Truesdell finds fault. It is *not* a challenge to explanatorily reduce the higher-level science to the more fundamental "lower-level" science. Instead, the challenge is to account for the the success of our phenomenological theories. How can theories of continuum scale physics (continuum mechanics, hydrodynamics, thermodynamics, etc.) work so well and be so robust when they essentially make no reference to the fundamental/structures that our foundational physical theories are about?

Philosophers and physicists often conflate *in principle* explanatory reductionism with an account of the success and robustness of our phenomenological theories. I have been arguing that this is a mistake. But it does not mean that the question (**MR**) fails to be genuine. So rather than focus on reductive explanations, I think we should focus on answering (**MR**).

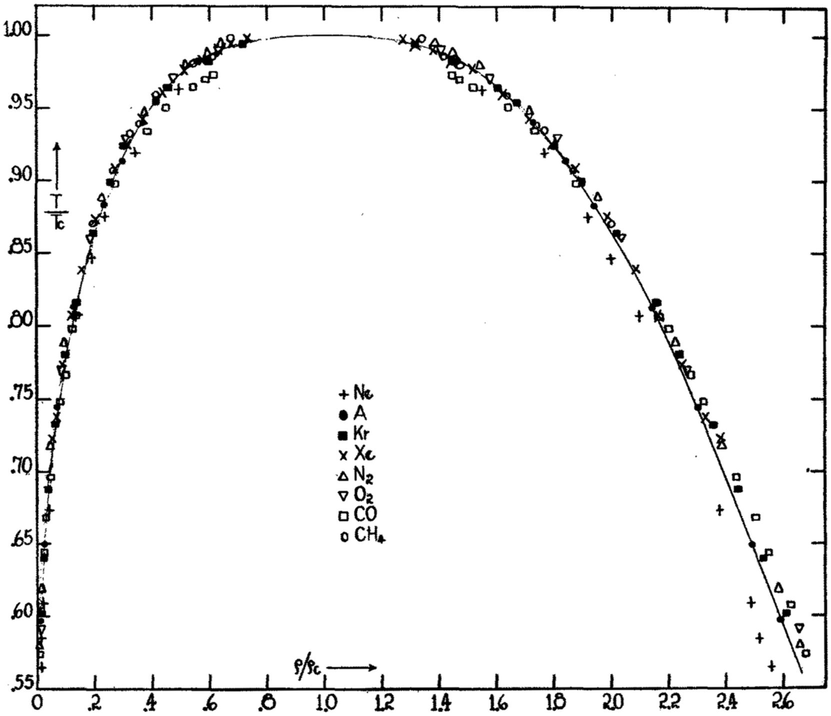

Figure 1. Universality of Critical Phenomena (Guggenheim 1945)

Let us consider a (rather famous) example of multiple realizability in physics. And, let us see how physicists (but not Truesdell's "real physicists") have tried to address the question (**MR**) in the face of this example.

3.1. Universality

Physicists use the term "universality" essentially in the way philosophers use the term "multiple realizability." (Batterman 2000, 2002) Michael Berry (1987, p. 185) has said that asserting that a property is a universal property of a system, is "the slightly pretentious way in which physicists denote the identical behaviour in different systems. The most familiar example of universality from physics involves thermodynamics near critical points." Consider Figure 1 from a famous paper by Guggenheim from 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 (dimensionless) critical temperature of the fluids where the value 1.0 means that a system's temperature is the critical temperature. The curve in Figure 1 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 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, 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? What, in other words, is the answer to (**MR**) for this particular case?

3.2. Renormalization group

It wasn't until the 1970's that there was a satisfactory answer to how universal behavior is possible. That answer came out of work by Leo Kadanoff, Michael Fisher, and Kenneth 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.[7] The basic physical idea is that the universal upper-scale thermodynamic behavior of systems near criticality is dominated by fluctuations in various crucial quantities, and that the range of the fluctuations is enormous in comparison to the range of the intermolecular forces governing the interactions between molecules. As a result the fluctuations are remarkably insensitive to the detailed nature of the intermolecular forces. The renormalization group gives one a way to exploit this fact and, in certain limits, to demonstrate that differences in intermolecular forces *play essentially no role* in the upper-scale behavior. Another way to put this, is that one can change the "essential molecular features" of, say, methane into those of neon, without affecting the upper-scale behavior (that is, without changing the shape of the curve in Figure 1). This reflects a kind of *stability under changes* of the very nature of the systems. The renormalization group makes the metaphor of "morphing" one system into another mathematically precise.

It does this by first constructing 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. That is to say one replaces specific interactions among a cluster of nearby molecules with averages. Then very roughly, one considers the averages to be new "molecular components" that interact with others in a new "coarse-grained" cluster or block. Figure 2 gives an idea of how

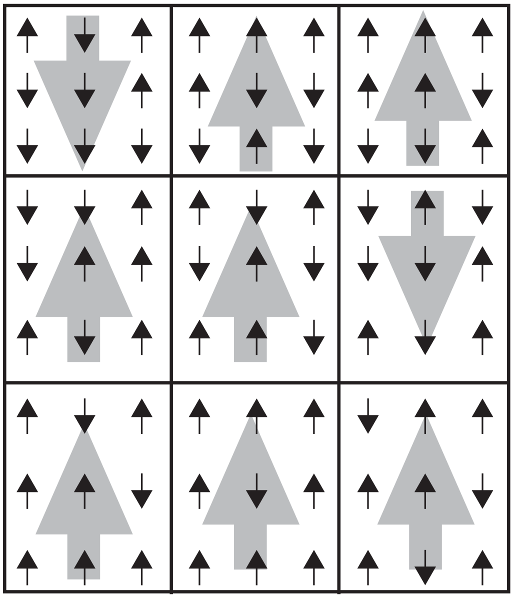

Figure 2. Blocking and averaging to yield a new (coarse-grained) system (Kadanoff 2013, p. 172)

this works. For ease of visualization, the figure shows this for for a model of spins on a lattice, but something similar will work for fluid molecules. In fact, the renormalization group argument allows us to see that this spin model (Ising model) is in the same universality class as the fluids in the Guggenheim plot! This provides a justification for our use of simple, minimal models to study behaviors of real fluids. (Batterman and Rice 2014)

The idea exploits the fact that near the critical point systems exhibit the property of self-similarity. Thus one can replace the original degrees of freedom with averages as exhibited in Figure 2 because the self-similarity guarantees that the new system will look like the old one as one coarse-grains or zooms out. One then rescales the system in an appropriate way that takes the original system to a new (likely nonactual) system/model in the space of 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. These are points which when acted upon by the transformation yield the same point.[8] A fixed point is a property of the transformation itself and all details of the systems that flow toward that fixed point have been eliminated.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.[9]

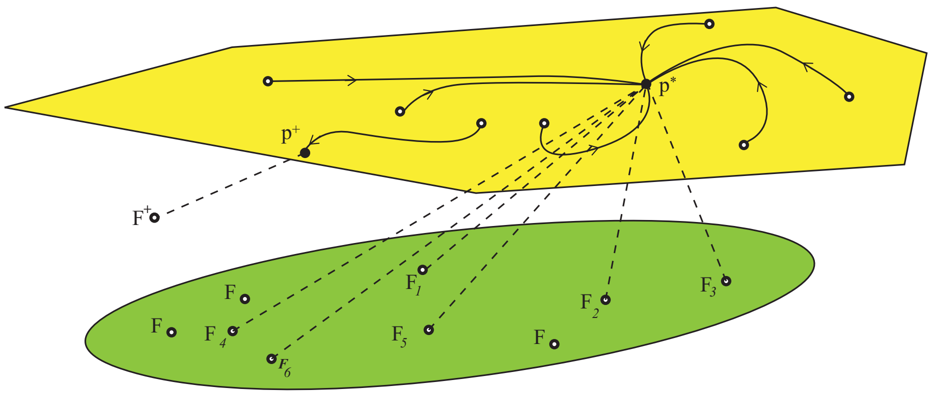

Figure 3. Fixed Point in Abstract Space and Universality Class

That macro-behavior can be determined by an analysis of the transformation in the neighborhood of the fixed point.

In Figure 3, 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. The trajectories here represent the "morphing" of one system into another under the coarse-graining operation exhibited in Figure 2. Note that another system/model, F^+ fails to flow to the fixed point p^* and so that system/model is not in the universality class.

The argument just sketched, by which one can delimit the class of heterogeneous systems all exhibiting the same macro-behavior, is not at all like the kind of derivation from initial data and fundamental equation of the kind Sober sees in the disunified explanations he discusses.

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**).

4. Examples and Generalizations

One might, I suppose, think that answering (**MR**) is not something scientists and applied mathematicians really worry very much about. I think this attitude is mistaken. Many problems in applied science start by recognizing that behaviors at one scale are radically different than those at some other scale (both spatially and temporally). Furthermore, it is necessary to model such behaviors in different ways. In this section I want, first, to present a very simple example that points to some profound methodological consequences of considering seriously the relations between models of a single system at different scales. I will argue that these

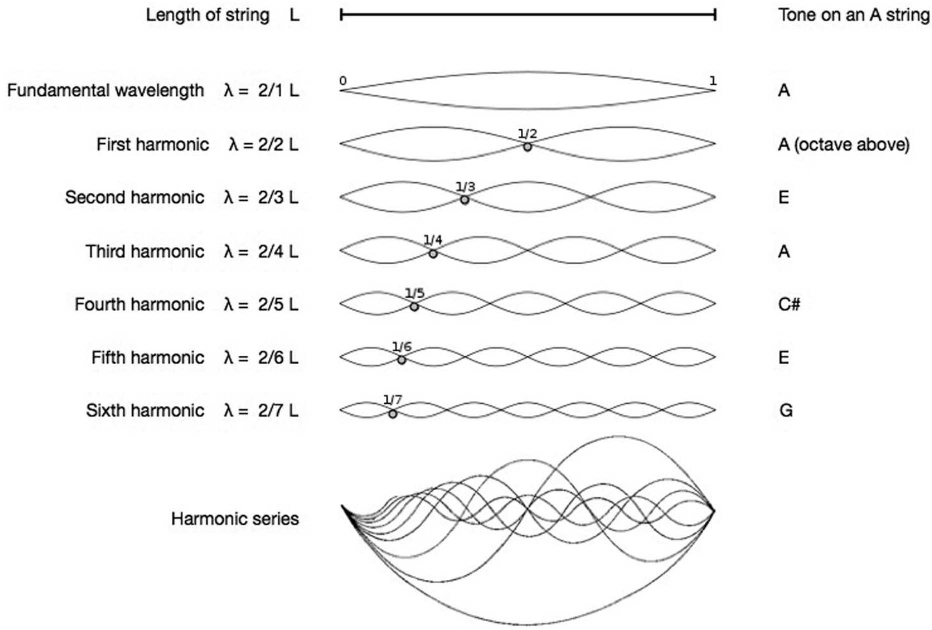

Figure 4. Harmonic Series for an A-String

behaviors at different scales require different explanations, and that they cannot be related to one another in a way that privileges a lower-scale, "more fundamental" level of explanation.

4.1. Vibrating string

Consider a violin string of length *L*. See Figure 4.[10] Suppose we are interested in determining the harmonic behavior of the string. In order to determine the harmonic modes[11] we need to solve a wave equation—a partial differential equation. In order to solve it we must impose so-called boundary conditions. To derive the harmonics exhibited in Figure 4 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*, we will be completely unable to derive the harmonic structure.

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 conditions required for our continuum explanation of the harmonic structure.[12] 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 (2008, pp. 184–192) calls this suppression of details "physics avoidance."

Physics avoidance might seem to be required but only 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 reductionist explainers always insist is possible in principle. We've seen that Sober, as well as Truesdell's "real" physicist, thinks it makes sense to appeal to "in principle" derivations from an ideal completed physical theory. (Sober 1999, p. 543) (Truesdell 1984, p. 47) 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 is in part because the equations of molecular dynamics and those of wave physics have completely different mathematical structures. The former are ordinary differential equations while the latter are partial differential equations. The claim that *in principle* these limiting derivations can be performed *without some attempt to say how that can happen* is as philosophically empty as the claim that there will someday be a completed physics that will tell us about "societies, minds, and living things." (Sober 1999, p. 543) Furthermore, as we will see below, even sophisticated multi-scale modeling at best only provides *bounds* on upper-scale properties, not exact values.

How does this relate back to the discussion of (**MR**)? That question, recall, concerns the relative autonomy of upper-scale behavior of systems from lower-scale details. The harmonic structure—the energy modes of a vibrating violin string—will be realized despite widespread differences in the microstructural makeup of the violin. It doesn't really matter at all what the violin is made of, whether its bridge is some kind of wood or metal, etc.[13] The separation of scales that justifies a continuum description of the harmonics and a molecular/atomic description of the interaction between string and bridge provides an understanding of the autonomy of the continuum account from the molecular/atomic details.

4.2 Multi-scale modeling of materials

To stress the point that multi-scale modeling is necessary, let me briefly consider another macro vs. micro problem that is a paradigm of 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 continuum equation is, in the context of the current discussion, remarkably autonomous from lower-scale physical details.) 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 mesoscale structures (inhomogeneities) present in the beam. But determining the connection between these lower-scale structures and the potential values for the constitutive parameters is a difficult mathematical problem known as "homogenization."[14] At best homogenization enables one to establish a fictitious, effective model equivalent in bending behavior to the actual steel beam.

One cannot determine the values for the material/constitutive parameters (or even bounds within which the values will be found) by atomic/lattice scale modeling alone. The mesoscale structures within the beam play an essential role in determining the macro/continuum behavior of the beam. 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 two central aims of continuum micromechanics are the bridging of length scales and describing the structure–property relationships of inhomogeneous materials.
>
> The first of the above issues, the bridging of length scales, 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 sequences must be described. Homogenization (also referred to as "upscaling" 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. . . . Since homogenization links the phase arrangement at the microscale to the macroscopic behavior, it can provide microstructure–property relationships. 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. (Böhm 2016, p. 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 ideal of *in principle* derivation of behaviors of systems (or laws, or theories) from from more "fundamental" lower-scale details (or more fundamental laws or 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 this ideal 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 evidence that it exists. Furthermore, the way scientists actually do attempt to answer questions of the form (**MR**) looks nothing like the kind of in principle derivations to which philosophers often appeal.[15]

5. Conclusion

This paper aims to draw attention to an explanatory problem posed by the existence of multiply realized, or universal, behavior exhibited by certain physical systems. The problem is to explain how it is possible that systems radically distinct at lower-scales can nevertheless exhibit identical or nearly identical behavior at upper-scales (typically everyday length and time scales). I think the philosophical literature has by and large missed the fact that this is an interesting question to ask.[16] However, when philosophers consider issues relating to multiple realizability, they almost always focus on questions of reduction. Specifically, they adopt the stance taken by Clifford Truesdell's "real physicist" and by Elliott Sober at least in (Sober 1999). Namely, they argue that upper-scale behavior—behavior that is often characterized very nicely by the equations of continuum physics—is completely understandable and explainable in terms of *fundamental/atomic/subatomic* physics. They claim that the only obstacle to actually providing explanations of such behavior is purely "mathematical" or pragmatic. Sober, as we've seen, expresses this claim in terms of the possibility of "in principle derivability": *in principle,* "[e]very singular occurrence that a higher-level science can explain also can be explained by a lower-level science" and that *in principle,* "[e]very law in a higher-level science can be explained by laws in a lower-level science." (Sober 1999, p. 543)

I have been arguing that these two claims are essentially vacuous. Those who make such claims need to provide at least some idea of how such derivational explanations are supposed to proceed. In addition, if one is interested in answering questions of the form (**MR**), even if one could provide such in principle derivational explanations, they would not constitute an answer. To answer (**MR**) requires a demonstration of the stability of the upper-scale (continuum) behavior under the perturbation of lower-scale (molecular/atomic/subatomic) details. Neither of these in principle derivations (even if possible) do this. So at the very least, if one thinks (**MR**) is a legitimate scientific question, one needs to consider different explanatory strategies. The renormalization group and the theory of homogenization are just such strategies. They are inherently multi-scale. They are *not* bottom-up derivational explanations.

Notes

[1] Phillip Anderson's (1972) famous paper, "More is Different," represents the beginning of a backlash against such reductionist thinking at least by physicists.

[2] Truesdell's use of "structural theories" will strike contemporary philosophers as odd. But the contrast with those theories that describe "gross," "bulk" or "ordinary" phenomena/materials is clear. By "structural" he is referring to atomic or fundamental theories.

[3] In other words, our theories of "ordinary materials such as water, air, and wood" work remarkably well despite completely ignoring any structure of those materials at scales below centimeters. This relative independence from lower-scale details is at the heart of the notion of autonomy at play in the paper.

[4] This is not to say that Fodor, Putnam, or Sober actually addressed the challenge I am posing. In fact, I think they haven't, and that is one reason why the debates about multiple realization and reduction still continue in the literature.

[5] Let me be clear that I believe that ultimately this assumption is not justifiable. More about that below.

[6] For example, these details are relevant for determining the distinct magnetic properties of iron and aluminum.

[7] I have talked about this strategy in many places over the years. See (Batterman 2000, 2002, 2011).

[8] If τ represents the transformation and p^* is a fixed point we will have $\tau(p^*) = p^*$.

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

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

[11] 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.

[12] This is something quite analogous (though less fraught, I believe) to the claims of complementarity that were prevalent in early (Bohr's) interpretations of quantum mechanics.

[13] Molecular/atomic models would need to pay attention to those details, but the continuum behavior doesn't care.

[14] In fact, the mathematics involved is related quite intimately to the renormalization group arguments discussed in section 3.2.

[15] An anonymous referee for this journal has claimed that this argument against in principle claims of derivability lacks force. The referee writes: "[T]he paper does not contain an argument that a true completed physics is not possible." Furthermore, the referee claims that the renormalization group explanation for autonomy presupposes that "there is no true completed physics." Regarding the latter claim, the renormalization group is completely agnostic about the possibility of a true completed physics. In fact, one of the amazing aspects of the renormalization group strategy is that it allows one to construct a hierarchy of effective theories even though we are completely ignorant of the physics at the highest energies (presumably where the completed theory will lie). There may very well be a true complete physics. But whether that is so, is irrelevant for the explanatory story. Regarding the former claim that there is no argument that there cannot be a true completed physics, the referee is completely correct. But, I do not see the force of this. It seems to me that the burden of proof regarding this question should be on the individual claiming that a completed physics, if it exists, will be able to answer the explanatory question at issue. If "completed" entails being able to answers to questions like (**MR**), then that is question begging. If "completed" does not entail this, then one would like to have some idea of how such an explanation might begin to be formulated. And, I know of no place where this has been attempted.

[16] There are exceptions. Fodor (1997, 160–161) asks the question and says there is no answer. Papineau (1993, p. 43) asks the question and assumes that the only response that isn't "incredible" is the reductive one I've been challenging. For a response to Papineau, see (Batterman 2000, section 7).

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