Mixed-grain Property Collaboration: Reconstructing Multiple Realization after the Elimination of Levels

Rob Rupert (U. of Colorado, Boulder)

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I. Introduction and Methodological Remarks

What is it for a property, state-type, or process-type to be multiply realizable (MR)? The intuitive idea seems clear enough. The same kind of thing can be constructed or the same kind of process can be implemented in a variety of ways: there are many ways to build a mousetrap, many ways to leave your lover, and many ways to structure physical matter so that it performs modus ponens. The attempt to flesh out and regiment such intuitions, however, invites a host of difficult questions, including "By virtue of what are two token states that seem distinct by some measures genuinely of the same higher-level type, when they are?" and "What is the metaphysical status of multiply realized processes, properties, or kinds, if there be such things?"

As philosophically compelling as those questions are, perhaps it's best to begin by clarifying what's at stake in debates about MR. Explorations of the topic have often aimed at a deeper understanding of the relations between various sciences, especially (but not exclusively) the relation between physics, on the one hand, and the so-called nonfundamental, or special, sciences, on the other.[1] Of particular importance have been questions about the reduction of nonfundamental sciences to fundamental physics and, in contrast, the form of autonomy special sciences have, if they do not so reduce – and also questions about the form of autonomy special

[1] Throughout I use 'nonfundamental sciences' and 'special sciences' interchangeably, although I think there is, in the literature, some tendency to limit the category of special sciences only to those nonfundamental sciences that are more obviously removed from fundamental physics, in particular, to those nonfundamental sciences that do not appear to reduce to fundamental physics. Chemistry, for instance, is a nonfundamental science while not being an exemplar of a special science. Such niceties play no role in the arguments of the paper, however, and thus are ignored hereafter.

sciences have even if they do reduce to physics.[2] The category and property terms of many nonfundamental sciences do not find a natural home in physics, which might seem enough to establish a methodological autonomy for such sciences, thereby revealing something important about the structure of the scientific enterprise as a whole, even if there is nevertheless some indirect way or undemanding sense in which such sciences reduce to or otherwise are accounted for by physics. Scientific reduction is also thought to be an ontological issue, concerning what ultimately exists, and thus discussion of it reaches beyond questions about the structure of scientific enquiry and the theories it delivers. Is fundamental physics all there really is? Or, does the appearance of MR in nonfundamental sciences demonstrate that, although all that exists depends on the physical, the universe is populated by other sorts of genuine properties and kinds – economic (*being money*, *undergoing inflation*), sociological (*being married, having a gender*), biological (*being a predator*, *being a cell*), and so on. Note, too, that some discussions of MR focus less on the relations between scientific categories (or vocabularies) and more on *a prioristic* metaphysics, as, for instance, part of an attempt to understand what the doctrine of physicalism really amounts to and what would be required for its truth (Melnyk 2006). In a similar vein, the mind-body problem has often taken center stage, typically in the form of an MR-based attempt to establish that mental states, properties, and processes are, or at least can be, aspects of the physical world while retaining their distinctively mental nature, a nature revealed either by the cognitive sciences or by folk practice, commonsense understanding, introspection, or conceptual analysis (Putnam 1967, Lewis 1972).

I'm interested primarily in the place of mental (psychological, cognitive – take the equivalence of these three categories as read hereafter) states and processes in the actual, natural,

[2] Perhaps it goes without saying that the resolution of such issues depends heavily on the assumed standard of reduction.

(presumably physical) world and, specifically, what the sciences of the mind (cognitive science, broadly construed) reveal about them. Strongly naturalistic commitments motivate this approach, and thus general questions about the relation between nonfundamental sciences and fundamental ones are never far from view. Thus, we learn how mental states fit into the natural world by investigating the relation between cognitive science and fundamental physics (or between the cognitive sciences and sciences we take to be more closely related to fundamental physics than cognitive science is). The prosecution of such an investigation naturally encompasses broader methodological considerations. If scientific processes and results are to inform our metaphysics of MR and of intertheoretic relations, then methodological matters deserve our careful attention. And, of course, for those who eschew metaphysical talk, methodological matters – concerning how successful scientific enquiry and modeling actually proceeds – are the main event. As a consequence, this essay frequently focuses on methodological issues, sometimes for their own sake, because we are directly engaged in a discussion of the structure of the scientific enterprise and its products, but also with an eye on the metaphysical question, because, as a naturalist, I'm inclined to think our best metaphysics of the properties investigated by the sciences – the properties that do causal-explanatory-predictive work in the actual world – should be derived from our best science (though that derivation may require nuance and philosophical sensibilities).

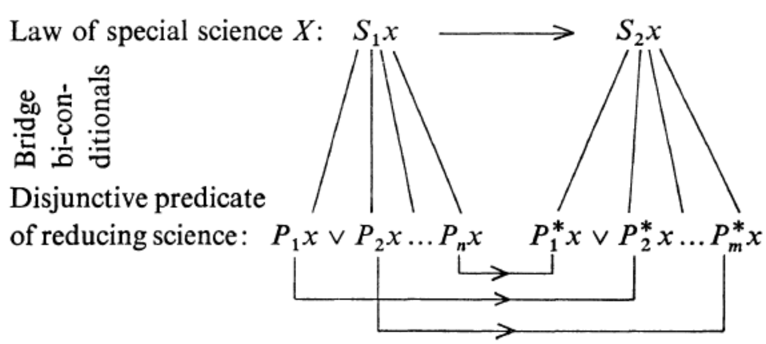
Admittedly, that's all fairly sketchy and programmatic. Here's the concrete plan. Section II frames MR as a claim about relations between levels in a layered picture of the sciences (and, correspondingly, of the world), with some sciences operating at a higher level and some at a lower and with physics typically treated as the basic level. On this view, any given scientific kind (property, process – I'll run these three together hereafter) appears at the level appropriate to the scientific discipline that the kind is distinctively part of; and, if it's a nonfundamental kind, its realizers are at a lower level than the kind itself. Section III surveys reasons many philosophers of science have rejected levels-based views of the sciences (and their domains) and develops additional reasons for such skepticism. Taken together, Sections II and III suggest strongly that, as a component of our best analysis of the structure of the scientific enterprise and the world it reveals, MR must go; for without levels, it's difficult to articulate a coherent theory of MR that assigns to that relation the properties historically associated with MR and that is likely to be applicable to the actual world. Moreover, in light of the picture of scientific modeling that emerges from these sections, it seems that little substantive work remains for a theory of MR to do, which renders the difficulty of trying to rejigger our account of MR a waste of effort. Section IV proposes that, instead of treating what have appeared to be higher-level MR properties as such, we should treat them as "multiply explainable" (ME) and explores the relation between MR and ME.

II. Fodor's Layered Picture and Its Discontents

The connection between MR and a layered picture of the sciences entered the discourse early on, and it has permeated the literature since then, particularly in philosophy of mind. Jerry Fodor's classic paper "Special Sciences" (1974) makes great hay of MR and of the anti-reductionism Fodor takes it to support, while also presenting a physicalist vision (relying on token identities – *ibid.,* 110) of the resulting "disunified" scientific enterprise. Fodor treats realization as a relation between the states of a higher-level science and a lower-level one. On Fodor's view (expressed in the material mode, though he's just as likely to put it in the formal mode), many nonfundamental sciences discover reliable relations among properties or kinds, between, for example, rates of inflation and unemployment (here Fodor is inclined to talk in terms of laws).

Each instance of any such relation is identical with an instance of a relation between token physical properties or kinds, which latter relation is covered by a law of physics. Nevertheless, the variety of token physical states that plays the role of any given special science kind or property is heterogeneous and, in at least some cases, open-ended – just about anything could count as a society's currency, under the right circumstances, Fodor argues. Such open-ended, heterogenous collections of property instances are not, taken collectively, legitimate physical kinds; the heterogenous categories in question are heterogeneous in precisely the sense that they are not of interest to physicists when they're doing their own work; physicists do not devise experiments in order to investigate them and these collections are not referred to in the confirmed general statements that constitute the subject matter of physics. Thus, the special sciences investigate and discover their own proprietary properties or kinds and laws that relate them to each other.

Here's Fodor's illustration (1974, 109):



The Fodorian picture raises a host of questions. For my purposes, however, I'd like to focus on a central presupposition of Fodor's view and the way it's been incorporated into discussions of MR: that the sciences trade in disjoint vocabularies that are, in some important sense, layered, with a pairwise relation between many such vocabularies (and the properties and kinds they pick out) being "higher than" and, conversely, "lower than." These may be two distinct ideas, one concerning disjoint vocabularies (or clusters of properties and kinds) and one concerning layering or levels.[3] I'll focus primarily on the idea that scientific domains, including their explanatory resources, are layered, with successive levels built up, one on top of the other. The problems to be identified with that view are sweeping and existential in nature; the vision behind the layered view seems to be fundamentally out of touch with large swaths of contemporary scientific work. The nature and depth of the problems also undermine the claim to proprietary scientific vocabularies (and proprietary clusters of corresponding properties and kinds – take this as read hereafter) insofar as that might provide a natural framework in which theories of MR might be cast.

This view of realization and MR as a relation between levels is widely accepted. Ned Block says,

> ...think of realization this way. Suppose we have a family of interconnected macro-properties (e.g., mental properties or economic properties) of a given system (say a person). Suppose that corresponding to each of these macro properties there is a micro property in this system, and that the family of interconnected micro properties provides a mechanism for explaining the connections among members of the macro family. Then the micro properties realize the macro properties. (Of course, this talk of macro and micro is relative; properties that are micro relative to one set of properties can be macro relative to another.) (Block 1997, 108)

On Block's view, realizer-realized relations hold between members of families of properties at different levels, here articulated with regard to scale (macro and micro). Dan Weiskopf paints a similar picture:

> Two taxonomies are aligned when there is a systematic, one-to-one mapping from higher level taxa onto lower level taxa, and unaligned otherwise...Unaligned taxonomies correspond to the classic picture of MR, on which a single upper-level category may be mapped onto many lower level ones. So put, what is needed [in

[3] In the latter case alone, one might distinguish between a strong notion (of layering) that requires a linear ordering of all scientific domains and a weaker notion (of levels) that requires only a partial ordering and thus does not admit pairwise comparisons in some cases.

> order to support MR] is a principled case for adopting different higher and lower taxonomic schemes in some particular contexts. (Weiskopf 2011, 242)

And operating within the framework of levels, Ken Aizawa fine-tunes his (and Carl Gillet's) view in the following way, to avoid an absurd implication of a less subtle version of it: "To refine the account, one can add that the two distinct realizers that multiply realize G must be at the same level" (2017, 217; also see Aizawa and Gillett 2009, 188). Here, then, is evidence that at least two generations of philosophers of mind – Fodor, Block, Weiskopf, and Aizawa – treat realization and MR as a relation between families of properties appearing at different levels.

The relation of supervenience is often cast in the similar terms, as a relation between layered families of properties, where, typically, the ultimate interest is in the dependence of all properties on the privileged set of those used in physics. With regard to scientific (and metaphysical) levels, though, one of the most influential theorists of supervenience, Jaegwon Kim, expresses deep doubts. He does so against the backdrop of Oppenheim and Putnam's famous compositional, or mereological, hierarchy, which consists of six levels, all of the objects properly at a given (nonbasic) level being exhaustively composed of parts from the next lower level (the six levels being social groups, multicellular living things, cells, molecules, atoms, and elementary particles – see Oppenheim and Putnam 1958). Kim says:

> At any rate, if there are biological organisms with free molecules, they fail to decompose fully into cells, violating the Oppenheim-Putnam requirement. Similarly, I don't think that real-life social groups...could be considered as composed merely of individual human beings (or other organisms). Surely, the natural physical environment (e.g., climate, terrain, the availability of food and other resources) must be considered a constitutive element, if not a "member", of a social group – as belonging to the domain of discourse of social theory – if we are to have a comprehensive explanatory theory... (Kim 2002, 14)

More generally, "All the domains of interest are subdomains of the universal domain U; but some subdomains may intersect one another without any of them wholly including any other, and there are likely to be subdomains that do not intersect with each other at all (e.g., plants and animals)" (*ibid.* 17) (also see Wimsatt 1994). Thus we confront the messiness of real-world composition, in contrast to Oppenheim and Putnam's tidy compositional view.

For present purposes, the question about compositional hierarchy takes a back seat to causal-explanatory interests. The former might bear on naturalistic concerns about levels and realization, but, in the first instance, we should focus on matters related more directly to science as practiced, that is, to causal-explanatory-predictive concerns. At least to some extent, Kim has the causal-explanatory-predictive question in mind: "Concerning the choice of levels in the Oppenheim-Putnam hierarchy, we noted that these particular levels are selected because objects belonging to each are thought to constitute a significant nomic kind. Nomic kinds are kinds defined by a cluster of laws with significant explanatory and predictive powers" (*ibid.*, 18). In this way, Kim's concerns about real-life social groups and their surroundings and about organisms made of both cells and free molecules can be recast as points about scientific modeling,[4] a transition that naturally follows from Kim's remarks, quoted above, about "comprehensive explanatory theory." Here is the gist of such a translation, which has been fleshed-out and elaborated upon in various ways in the two intervening decades: The causal-explanatory apparatuses – models, typically – used in the sciences often include properties at what might appear, intuitively, to be at many supposed levels, as part of individual modeling

[4] To be clear, Kim does not always have in his sights the kind of activity typically associated with scientific modeling. Kim (*op. cit.*, 18–19) sometimes takes as scientific *explananda* the behavior of a whole, which is to be accounted for by invoking the behavior of the parts. In many (perhaps most) cases, though, scientific modelers aim to explain something else, for instance, the results of measurements (accumulation of a certain amount of snow, for instance), and do so via the construction and application of a model of the various forces, quantities, etc. that contribute to the outcome in question (that is, the amount of snow on the ground). Similar remarks apply to the explanation of reaction times in experimental psychology: the button was pressed at a certain time, and what gets modeled is the collection of stimuli, processes, and forces that led to the button's being pressed at that time, which is not, on its face, anyway, a compositional explanation.

endeavors, and this is so regardless of how we attempt to identify scientific levels – e.g., by indexing them to compositional relations or to scales.

What follows enumerates reasons to reject scientific levels, summarizing concerns extant in the literature and adding further concerns.

A. Individual models draw from mixed-scale resources within a single discipline.

Various authors have made this point in the context of individual disciplines – from physics (Thalos 2006, M. Wilson 2010,[5] J. Wilson 2010) to neuroscience (Bechtel 2016) to cognitive science (Rupert 2018).[6] The idea here is that, given some data, models of them can and often do include materials drawn from what might be thought of as different scales that are part of the same general enterprise. Bechtel provides a clear case:

> This problem has been approached by computational modeling efforts in which a model for the intracellular oscillator is supplemented by terms and equations capturing a hypothesized pattern of interaction of neurons. There is an intuitive sense in which interactions between neurons is at a higher level of organization than operations within individual neurons, so that if the interaction is modulating the activity of individual neurons, one is dealing with top-down effects of a higher-level system on its components. But interestingly a differentiation of levels did not figure in these accounts. Rather, the modelers simply extended the scale of their model by adding equations to capture effects on a neuron's behavior from operations outside it. (Bechtel 2016, 725)

The important point, relative to matters at hand, is Bechtel's final one: that neuroscientists feel free to incorporate into their models properties of both individual neurons and patterns of interactions between neurons, without feeling constrained by differences in scale or perceived

[5] "In an allied vein, the usual philosopher's talk of the 'kind predicates' or 'basic quantities' of a discipline like physics strikes me as ill founded as well. The more that a subject has developed a wide variety of mixed-level tools of the sort under survey [that is, mixed-scale tools], the harder it can become to confidently ascertain what its basic quantities comprise" (M. Wilson 2010, 944).

[6] In Rupert's case, part of the point is that we can easily be misled by intuitive or *a priori* judgements about levels in such a "scale-nebulous" area as psychology.

level or by a worry about "top-down" causation (where the thought that causes coming from the 'top' have a worrisome status would seem to presuppose layering and the assigning of metaphysical, or at least methodological, importance to such structure).

B. Individual models (and the aggregate of models within a discipline) draw from (sometimes mixed-scale) resources that are not plausibly part of the "home" discipline.

Kim's two examples reviewed above fall under into this category, when translated to the context of modeling. Along similar lines, Potochnik and McGill say, "Tissues are only partly composed of cells; also crucial are the macromolecules that hold the cells together. Polymer molecules are composed of monomers, but individual ions are also an important ingredient" (2012, 127). Potochnik and McGill also flag common approaches in ecology and population dynamics: "whether or not bird species live in an area is controlled by random chance of dispersing to that area at scales of a few meters, by species interactions at a slightly larger scale, by habitat preferences (e.g., tall trees vs. grass) at scales of tens to hundreds of kilometers, and finally by matches between climate and the species' physiological adaptations at scales approaching those of a continent" (*ibid.* 134). The collection of elements that enters into a model of the shifting distributions of plants or changes in the prevalence of members of a given species includes everything from weather systems to microscopic pathogens. And this is not merely a matter of mixed scale. It's obvious that the components of such models have natural or primary homes (if we were to do our best to assign such things) in different disciplines – microbiology, geology, meteorology, and more.[7]

[7] One might attempt to preserve the levels-based view by proposing that a given property has a home in multiple disciplines or at multiple levels, but where the relevant home-level in any particular case is unique and determined by context. This is almost certain to give rise to the problems detailed by Eronen (2015) in connection with the strategy of "going local about levels," which is adopted by some of the most prominent proponents of the new mechanist approach in philosophy of the life sciences.

C. Phenomena associated with a given discipline do not necessarily appear at a single level, compositional or otherwise, in individual cases or in the aggregate.

The phenomena proprietary to a given scientific discipline or field of study, i.e., the data themselves, often fall at mixed levels. For instance, the *explananda* of a given scientific endeavor often concern relations between different measurements made at different scales or relations between properties that one might naturally assign to different disciplines. Consider, for example, correlations between galvanic skin response and verbal output (as is of interest in the science of pain). In these cases, the *explanandum* itself comprises elements of different scales and from different regions of the compositional hierarchy and comprises elements some of which find a natural home in a different discipline than some of the others do. Similarly, consider the correlation, commonly explored in the literature on metacognition, between a subject's degree of

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It is worth commenting on the relation between the present discussion and what Polger and Shapiro call 'anchored kinds' (2016, 33–35), which are a sort of cross-level kind, cross-level because the kind is specified partly functionally but also in a way that requires a particular structure or element from a "lower-level" science to be at least partly responsible for the execution of the functional role in question. (A corkscrew is defined partly functionally – by its capacity to remove corks – but is an anchored kind because it must remove corks via the contribution of a rigid, screw-shaped part.) The existence of anchored kind helps to undermine the layered view, by mixing (supposed) layers within the identity of single kinds. (Also see Paul Churchland's discussion of what would seem to be an "anchored-kind" functionalist strategy for dealing with objections from consciousness and qualia – 1984, 40–41.) Polger and Shapiro themselves sometimes acknowledge problems with the layered view (*op. cit.* 170, 217–218), but in discussing the implications of levels-skepticism, they focus primarily on the observation that levels-elimination clears a path for the identification of psychological and neural kinds, by combining cognitive psychology and cognitive neuroscience into one science, where it becomes natural to identify some kinds picked out by different terms in the vocabularies of the two (now merged) sciences. This point differs from those made in the main text in support a view of scientific modeling as opportunistic; in many of the sorts of cases I have in mind – say, in which the model of neglect patient's performance in drawing tasks includes both the lesion and the internal state created by the experimenter's verbal instructions – the question of identifying the two kinds is not at issue (whatever physical structure one might associate with the subject's belief that the experimenter wants her to draw is not the neural structure at site of the subject's lesion). It's not clear to me whether Polger and Shapiro wish to take on the fully mixed-and-opportunistic message about modeling at the heart of the present section. If they do, however, I'm not sure how they understand the use of 'taxonomic system' in their official statement of the recipe for MR (*ibid.*, 67). The full extent of successful science's "sharing" of modeling resources across contexts and disciplines suggests a genuine challenge to anyone committed, as Polger and Shapiro seem to be (when they allow for some MR), to the idea that distinct taxonomies provide the framework for realized-realizer relations.

confidence expressed verbally (in her response on a given trial) and her reaction times, measured in milliseconds, on the task itself.

D. It's often unclear what level the data would appear on, if one were attempting to divide the world into levels

Consider key-pressing, as a measurable behavior in a psychological experiment. Does the whole organism press the key or does only the finger press the key? Similarly, is a galvanic skin response a measurement of a property of the entire organism or of a proper part of the organism? We do know that if we index such responses to the whole organism (to which the finger or area of dermis in question is attached), we can identify correlations useful for predicting the results of further measurements, the results of which are themselves indexed to the same organisms; but that hardly decides the issue of whether the data are at a determinate level. Similarly, consider the question of correlations between verbal reports and reaction times. Are these two measures on the same scale or level?[8] There's a difference in temporal scale, to be sure. But when it comes to the physical scale, it's simply not clear. (Moreover, it's not clear what's at stake in the science if we don't place them at determinate levels.) Neither of these is at an obviously comparable physical scale. In fact, these complications appear even when we ask about the model only of the button-pressing: it includes a motor command, the state of the dominant network in the brain at

[8] Bermudez (2000, 65) categorizes reaction times as personal-level behaviors, which I find somewhat baffling. What would make it determinately the case that such data appear at the level of, say, the whole organism, as opposed to the level of some proper part of the organism, perhaps just the collection of the parts involved in processing the stimuli and causing the button to be pushed (which would surely exclude some of the parts of the organism – the toenails, for instance, or the ankle)? Of course, Bermudez may have something more conceptual in mind, e.g., that because we conceive of button-pushing as an intentional action, performed by an agent, reaction times are personal-level data; but that introduces a scientific level by conceptual fiat, not as something derived from successful practices of collecting and modeling data. Bermudez might argue that subjects, *qua* persons, must understand the instructions they were given by the experimenter and that that alone reaction times at the personal level or the level of organism. But such a claim would simply move the bump in the rug. Why should our models of, say, sentence processing be placed at one level (whole agent, whole organism) as opposed to another (of just the portions of the organism that process and respond to verbal input)?

the time of the motor command, the activation of visual processing, and more. These are at different temporal and physical scales. Which of these, if any, determines at what level button-pressing properly resides?

Perhaps there's no cause for concern. We should wonder what scientific problem the assigning of data to a determinate level is supposed to solve. What are scientists missing out on when they fail to place determinately a process or property at a level or to assign it to one and only one discipline? (Klein 2008, 166; see also Eronon 2015, 46, for data on the apparent lack of interest in neuroscience in the germane notion of levels – where less germane notions focus on differing degrees of abstraction[9] or refer to stages in a stepwise causal chain.)

E. The inclusive nature of models.

On one commonly employed concept of a model, a model includes the data together with what one is inclined to think of as the guts of the model, that is, the machinery that explains, accounts for, or best fits the data. Now consider that the use of such machinery is itself partly justified by measurements other than those that produced the data at hand. On an overly simplistic view, one begins with data, formulates competing models, and the model that best fits the data is winner (and further commitments are made on the basis of its being winner, commitments perhaps driven by inference to the best explanation). In contrast, a more comprehensive and nuanced

[9] To be clear, abstraction plays a role in scientific modeling in the special sciences, but ascending to a higher level of abstraction is not, in and of itself, a matter of shifting disciplines or shifting from one to another of what have historically been viewed as levels. Consider the following common sort of situation. Sometimes variation in condition doesn't produce statistically significant differences in behavioral outcome, and so data from the different conditions is pooled. This provides one kind of motivation for including a more abstract or generic property in a model instead of more specific ones. We might break down our data by gender, but then, upon statistical analysis, see that there's no effect of gender on the outcome, and then pool the data of the genders together (revealing only a main effect, for instance), with the model including only the element "person" or "subject," not including any elements corresponding to specific genders. This sort of abstraction appears to have nothing to do with shifting scales or levels.

picture acknowledges that which models get formulated and which are taken seriously is affected significantly by professional judgments concerning which resources are worth considering for inclusion in a model. Imagine one wants to model the process that produces tornadoes. One might formulate a model that includes God's vengeful breath and then pit that model against models that include high differentials in humidity. In fact, models of the latter kind are automatically given preference by meteorologists, because difference in humidity and various effects of it have been measured and modeled successfully in other contexts.

This complicates the attempt to place a model at a determinate level, because the choice and the success of the (local) model in question derives from other contexts; in our example, contexts in which the effects of differences in humidity levels have been measured are likely themselves to be contexts in which either the *explananda* or the explanatory parts of models of the data mix levels, mix scales, or mix predicates associated with various disciplines. The expansive nature of this entire "epistemic object" (as one might say) increases the probability that, when one looks more extensively at the set of resources being used to model a given set of data (and including the data themselves), the entire ball of epistemic wax includes various compositional levels or scales, or properties drawn from different disciplinary homes.

This is especially clear in such interdisciplinary, applied sciences as biomedical engineering. The model of a given set of data – say, concerning proliferation rates of cardiomyocytes that have been differentiated from pluripotent stem cells – might include concepts drawn from materials science, the study of cellular adhesion, the physics of diffusion, the chemistry of nutrient-cell interactions, and more (Rodriguez et al. 2014); and each component of such a mix of causal-explanatory modeling resources itself comes with its own history of confirmation that involves the application of opportunistically mixed models.

F. On the prominence of measurement.

Frequently (perhaps even typically) scientific data are the results of a process of measurement. For this reason, a theory or model of the apparatus being used also contributes to the epistemic status of the data collected, which introduces further questions about the level at which to place the fruits of some bit of scientific enquiry and modeling. It might strike some readers as too ambitious to include such a theory or model (that is, of the experimental apparatus) among the set of factors or properties that determine at what level given bit of scientific output or activity should be placed. If our goal, however, is to understand the structure of scientific enquiry and the relations between scientific domains or disciplines, then it's worth considering all of the major components of the activity of the relevant scientific practitioners. If biologists spend any significant amount of time applying models from biophysics in their efforts to devise the instruments that measure cell function (contractile force in cardiomyocytes, for example), then the contribution of biophysics is part of cellular biology.

In summary, then, the response I advocate is not to be upset or confused, but to let levels go, and MR with them. It simply doesn't matter whether there is a determinate level on which some data, the measurement process, an experiment as a whole, a model, a field of study, etc. can be placed. What matters is how best to account for the data, drawing on whatever resources do the trick.

Fodor's vision is dead. Let us abandon the idea of an autonomous computational psychology, occupying its own level, trafficking only in its proprietary vocabulary (except when *ceteris paribus* clauses are sprung), the terms in which designate properties realized by properties at the next lowest methodologically autonomous scientific level, at which appears a different proprietary (that is, level- and discipline-specific) set of resources that do the causal-explanatory work *vis-à-vis* their own levels-proprietary *explananda*. In place of the layered Fodorean landscape, we should acknowledge a countryside cluttered with a wide array of cross-cutting epistemic artifacts, a host of models constructed opportunistically, exploiting resources from whatever quarter and in whatever manner is scientifically useful. *This is not to claim that all scientific modeling is pluralistic, opportunistic, interdisciplinary, or multi-scaled.* Not at all. But there is enough such work to sink the layered view; "mixed" models are not merely outliers or oddball exceptions to the rule.

IV. MR and ME

Perhaps the reader is, at this point, convinced that scientific modeling, particularly in the nonfundamental sciences, does not respect boundaries between (supposed) levels or between scales, or between proprietary disciplinary vocabularies. The reader may also be willing, at least for the sake of argument, to consider what might take the place of realization as the glue in our unified naturalistic picture of the scientific enterprise (and the world it investigates). I suggest that scientific explanation provides that adhesive. In other words, we should set aside our interest in realization and MR and focus instead on explanation, 'multiple explainability', and properties that are 'multiply explainable' (with apologies to lexicographers and grammarians).[10]

[10] A Google search identifies only one occurrence of either of these terms in a context related to realization or reduction. At the end of a discussion of the MR-based argument against reduction, Christian Sachse says, "Consequently, for each of the tokens of one type of the special sciences there is a reductive explanation but not the same reductive explanation for all of them. This is why 'multiple realization' is taken to be multiple description, or multiple explanation. There are multiple physical descriptions (explanations) of property tokens that are homogeneously described (explained) by a special science" (2007, 120). Sachse's essential idea seems to be closely related to what I'm proposing, though the motivation and theoretical framework (of intertheoretic reduction) differs significantly. Sachse introduces the explanation-based approach as a refinement of standard MR-based reduction-blocking arguments. I do not appeal to ME as a way to block reduction, though, partly because reduction is a slippery notion, and I am more interested in unification than reduction. As I see things, models in sciences other than physics are frequently successful and although that success can be explained by physics – well enough, anyway, to

What are sometimes thought of as higher-level, MR states sometimes play causal-explanatory roles in mixed (or unmixed) models, and in many contexts, the question of their contribution ends there.[11] But, if one asks how such properties make their contribution – for instance, if one asks further for a model of the way such properties contribute as part of the model of initial interest – then one might be given a further model, a meta-model, if you will, of how the model of initial interest succeeds. This meta-explanation needn't involve modeling, though. It could be more qualitative in nature. More generally, one might offer different sorts of explanations in different cases and the standards for success might vary. In this approach's most noncommittal form, one would not necessarily explain why the so-called higher-level property P plays a productive role in the target model; rather, one might merely explain why the system that produced the measurement results that the model including P is being used to account for are such that their interaction with the measurement apparatus yielded a value that can be used to predict the results of some other measurement of that system (or region). When, relative to a given property, there are more than one such explanations (different ones for different cases), then the property in question is multiply explainable relative.

quell possible concerns about the metaphysical spookiness of nonfundamental scientific kinds – such explanations often do not rest on logically tidy premises, involving identities or biconditionals flanked by properties from different scientific domains or the results of strict mathematical derivations of special science kinds or laws from those of physics. Thus, to my mind, reduction, as it's often conceived of, is blocked, but not by the 'multiple' in ME, but rather by the nature of the explanations themselves (even in cases in which there is only one relevant explanation, not multiples). To plumb a bit deeper, I'm inclined toward a view of special-science properties that would, by many, be seen as metaphysically eliminativist. I resist the eliminativist label, though, in sympathy with the kind of view expressed by Ladyman and Ross (2007). Physics can explain why special-science measurements yield the results they do and why those results are usefully correlated with the results of other special-science measurements.

Note, too, that Colin Klein (2013) mentions "intertheoretic explanation and reduction" (694), though without elaborating on what he takes to be the relationship between the two. From his compressed remarks, however, I gather he holds that the project of giving multiple intertheoretic explanations is best divorced from claims about MR and holds that ME is consistent with reduction, which doesn't cohere with my perspective (which is both unificationist and either anti-reductionist or eliminativist – see the remarks of the preceding paragraph).

[11] And for good reason, because the scientific work done by such models is sufficient for our purposes or answers the questions at hand (Campbell 2010, Weiskopf 2017).

I contend that ME is all that remains of MR. And, unlike MR, ME, as I conceive of it, does not follow a single, straightforward recipe.[12] In particular, what counts as an acceptable naturalistic explanation (of the role of a ME property in a particular model or family of models) can vary depending on context and purpose and the state of current knowledge. In what follows, I explore some ways such model-relations and explanations might go.

Imagine we want to know how a particular special-science property contributes to a given model's success. The explanation might go like this. A family of related data sets are usefully treated by two models that bear the following relations to each other. Model 1 captures a certain (not unimpressive) amount of the variance in the data sets. Model 2 captures significantly more of that variance. Also Model 2 is framed entirely in terms of entities and forces the contributions of which to the parade models in which they appear (the models that account for the scientific community's inclination to continue to include such elements in the formulation of new models or in a broader range of explanations) are better explained by fundamental physics than are the entities and forces appearing in Model 1. The latter condition is a way of trying to capture what's left of the idea that Model 2 is part of a lower-level science than is Model 1 (though there's much more to be said about this in light of the arguments of Section III). Assume that there is an identifiable component *a* of Model 1 and an identifiable component *b* of Model 2 such that *a* and *b* play a structurally similar role in the two models, and multifactorial analysis reveals that *b* accounts for more variance in the data than does *a*. Here we find something that's as close as I think we'll find to a realized-realizer relation.[13] If the situation is reduplicated in the relation

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[12] In a sense, ME does for general patterns of the intertheoretic relation what John Norton's material theory of induction does for general principles of inductive inference (Norton 2021).

[13] And for this to look most like the realization relation, we might add that model components *a* and *b* find their most natural institutional homes in scientific disciplines such that, within those disciplines, there is widespread agreement that *b*'s discipline is, as a whole, more closely related explanatorily to fundamental physics than is *a*'s home discipline.

between Model 1 and Model 3 (including an element *c*, distinct from *b*), then the situation meets appears to manifest ME, where the role of *a* in Model 1 is multiply explainable, and in a way that allows us to see something analogous to the multiple-realizer relation.

I don't actually think the situation I've described, whether it involves a single explanation (two models) or multiple explanation (three or more models), is likely to count as realization or as MR, for reasons that go beyond the concerns already raised about levels and proprietary vocabularies. There's something missing from the preceding sketch of model-relations: the explanatory bit. Accounting for variance, for instance, is a purely statistical, correlational fact. Parallel to the motivation for moving from supervenience to superdupervenience (Horgan 1984), we may demand something more than statistical bookkeeping in the explanations in question. When the behavior of model component *b* accounts for more of the variance in the data than *a* does, the question might arise whether it's the *same* variance. One might think it would have to be, on standard views of realization – if element *b* realizes *a*, then element *b* must have causal powers that include *a*'s causal powers (Wilson 1999, Shoemaker 2001) or *b* must play a structural-causal role in which *a*'s role can be embedded (within, for instance, a Ramsey sentence – Lewis 1972). But, those are tall orders. It's one thing to say that the model in which *b* plays a part fits the data better than some other model. It's another thing to say that it does so by having a component that roughly matches the contributions of a component in the model to which it's being compared, and that the former component outperforms the paired component in the model to which it's being compared. And, it's yet another thing to trace such outperformance to specific data points. It seems that only the last would satisfy the standard necessary conditions for realization.[14]

[14] In some powerful examples of unifying explanation, the model components in question fail to map onto the same aspects of the target system. The data might be recovery rates from an illness. One model may track self-reported

But, I contend that all three of these suffice (at least in some contexts) for naturalistic "cross-level" unification via explanation. Consider blindsight patients. They report a lack of conscious visual experience yet perform above chance on various visual tasks. The "high-level" hypothesis is that their performance is guided by subconscious perception. A neural account of blindsight can be somewhat sketchy and still do the explanatory trick. It can provide a qualitative sense of how a neural system might produce the behavior exhibited by blindsight patients, where (obviously) the neural components invoked are explanatorily related more closely to physics than is the posit of subconscious perception. And, this can be an effective unifying explanation (removing sufficient mystery as to deflate any spooky speculation about magical powers of a third eye, or what have you) absent a precise neural model that has a component that can be paired up with subconscious percepts and the contribution of which accounts for more variance than subconscious percepts. Because these are acceptable and commonly found forms of unifying explanation, we should not demand, of the explanations in question, that they reveal subsets of causal powers or sameness of causal role across the model elements in question. So, it seems unlikely that we'll be able to reconstruct something that looks like MR, in general and as it's normally conceived of, within the framework I'm advocating for.

Three paragraphs back, I embarked on an attempt to find an image of MR in the proposed framework of ME. I arrive here empty-handed. Setting MR aside, I want to emphasize now the single most important explanatory dimension in play, that is, whether Model 2 explains why the

pain of a certain sort over time, as a predictor of recovery. A second model, which fits the data better, might track certain hormone levels. It would be satisfying if a further model of the situation would explain why the hormone level is correlated with the verbal reports in question, but depending on the context, that further model may not need to meet very high standards in order to satisfy the demands of unification; these might be standards too low to support the pairing up of causal powers in the fashion normally associated with MR. But this isn't just a matter of low standards. Facts about hormone levels, together with other bits of the model in which hormone levels appear as contributors, may explain quite well, relative to demanding standards of explanation, why subjects would produce verbal reports that are predictively useful, without providing anything that, strictly speaking, has a superset of the causal powers of the (supposed) states that produce the verbal reports.

systems or regions of space-time measurements on which produced the data for which Model 1 accounts are such that those measurement correlate with or allow effective prediction of the results of some other measurements (whether of the same region or object or a different one). Is there a so-called lower-level explanation of why the relevant portion of the physical world that produces these data is such that it will also produce, directly or indirectly, correlated data?[15] When an acceptable explanation of that fact is given, in a way that can be explanatorily traced to physics-based roots, naturalistic unification has been achieved. When there is more than one such explanation, ME appears. This is not to say we don't find more robust kinds of explanations. It might be that Model 2 does the explanatory work just described in "modular" fashion, in the sense that the explanation of the measurements in question is componential, where component *b*'s contribution to the explanation of the measurement helps to make specific and quantitatively precise sense of the contribution that component *a* makes to measurements. That will, in some sense, be a more satisfying explanation of Model 1's success; in particular, it will satisfy some of the intuitions that motivate the discussion of MR. Nevertheless, the typical case will not involve a matching of causal powers. This arises partly from the messy nature of the probabilistic processes that account for the measurement outcomes of the special sciences and the correlations between them.[16]

Physics is comprehensive. That is one of the grounding assumptions of the current discussion. The combination of forces in physics, at different scales, yield a wide variety of useful measurements, given the measurement apparatuses at our disposal – including special

[15] And note that the measurements in question are typically not geared toward fundamental quantities or other quantities that play a role in physics proper, even though the measurements can be modeled as interactions of fundamental quantities in the measurement device and the system being measured – as can be done, for example, in the hearing of a subject's verbal report. The lack of orientation toward fundamental quantities tends to be, I think, what makes these measurements parts of a special science domain or endeavor.

[16] One might think that appeal to idealization (to a competence-performance distinction, perhaps) or appeal to a noise term could yield better matching causal roles, but such a move would have to be independently motivated.

science apparatuses not aimed at detecting values of fundamental quantities. When ME looks most like MR, we see a certain tidiness, a near-matching of the cross-model causal roles of components with regard to the outcomes of measurements and with regard to correlations between outcomes of different measurements. High degrees of matching may be most likely to occur in engineered contexts, in computer science, for example (Polger and Shapiro 2016, 73).

Note an advantage of thinking in terms of ME-based naturalistic unification. There's no need to answer the question, "When are two explanations sufficiently different as to qualify the element whose contribution is being explained as ME, emphasis on the 'M'?" MR blocks Nagel reduction and sends philosophers in search of an account of naturalistic unification that accommodates anti-reductionism. But, the physicalist doesn't need anything so strong as Nagel reduction for unification. As an account of the structure of science and of naturalistic unification, what matters is whether the success of the models that are successful can be explained in a way that makes sufficient contact with physics. Whether that explanation is singular or multiple, relative to a given model element, doesn't affect the question of unification.[17]

Let us turn to unanswered metaphysical questions. Should we be realists about special science properties? Yes, but with a substantive qualification. The special-science measurements in question are genuine measurements. A physicalist should want to explain why, if all there ultimately is, is physics, these should be productive measurements (that is, why they should measure something that is itself likely to evolve into – or otherwise be systematically related to –

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[17] This picture of naturalistic unification is inspired partly by the one developed by Lindley Darden and Nancy Maull (1977). They say, "Provided with a new analysis of the relations between fields, it becomes natural to view the unity of science, not as a hierarchical succession of reductions between theories, but rather as the bridging of fields by interfield theories. The unity of science analyzed as a hierarchical classification scheme of scientific theories, graded according to generality, is precisely the picture provided by Oppenheim and Putnam...as a 'working hypothesis.' Our preliminary analysis suggests another, new working hypothesis: unity in science is a complex network of relationships between fields effected by interfield theories." In place of "interfield theories," I would put "interfield explanations," including explanations of why certain families of models have the success they have, by appeal to the resources recognized to be from a different field, explanatorily closer to physics.

a kind of state measurements of which are correlated with the outcomes of the first set of measurements in question). Such explanations establish the reality of special-science kinds or properties, but largely because such compound physical properties are so cheap. The causes of special-science measurement results and of the correlations between them are real. At the same time, they typically aren't of any inherent interest in physics. Combinations of physical properties give rise to all sorts of states, interacting in all sorts of ways, with other combinations of physical properties. Such states are perfectly legitimate, from the standpoint of physics – and thus, with regard to the reality of properties, the world contains multitudes, including multitudes of which humans will never take interest in or measure. Physics, however, has no reason to reify them or care much about them; they're not referred to in physicists' fundamental conceptual tool kit or the phenomena investigation of which determines what appears in the physicist's fundamental conceptual took kit. So, yes, the properties are real. But, they stand alongside millions or billions (or more) of further potential special-science properties; any physical system or region that can be measured in a way that provides information about any other measurement, about it or another system or region, thereby has a real property (Ladyman and Ross 2007). So, special science properties are real, but in a way that makes them decidedly not special or privileged.

But if the underlying arrangements of physical states or quantities are legitimate states from the standpoint of physics, why not identify them as the realizers of the special-science kinds, after all? In the end, the answer may be partly stipulative, but I am advocating eliminativism about realization, on a presumptively physicalist view. The essential idea is that the explanations (and multiple explanations) in question are typically too untidy to count as descriptions or discoveries of realizers. If we were to Ramsify our leading account of realization, the best real-world satisfier of the resulting Ramsey sentence would not satisfy the Ramsey very well at all (or if it did, it would be rare, as in the case of engineered realizers); it would satisfy so few of the properties of realization specified by such Ramsification that we should favor eliminativism about realization (as an intertheoretic relation unifying the sciences of the natural world). The relation in question isn't a relation between levels, it isn't a relation between elements in proprietary vocabularies distinctive of various disciplines; and the explanations of why an element in a special-science model contributes to the success of its model often does not appeal to any identifiable structure or substructure that has the causal powers of the "higher-level" model element in question – either because of a lack of structural matching, because the relationship is noisy and probabilistic, or because the explanation is merely qualitative.

Works Cited

Aizawa, K. 2017. "Multiple Realization, Autonomy, and Integration." In D. M. Kaplan (ed.), *Explanation and Integration in Mind and Brain Science* (Oxford: OUP), pp. 215–234.

Aizawa, K., and C. Gillett. 2009. "The (Multiple) Realization of Psychological and other Properties in the Sciences," *Mind & Language,* 24 (2): 181–208.

Bechtel, W. 2016. "Mechanists Must be Holists Too! Perspectives from Circadian Biology," *Journal of the History of Biology* 49: 705–731.

Bermudez, J.-L. 2000. "Personal and Sub-personal; A Difference without a Distinction," *Philosophical Explorations* 1: 63-82.

Block, N. 1997. "Anti-Reductionism Slaps Back." In J. E. Tomberlin (ed.) *Philosophical Perspectives,* Vol. 11, *Mind, Causation, and World*, pp. 107–132.

Campbell, J. 2010. "Control Variables and Mental Causation," *Proceedings of the Aristotelian Society* 110, Part 1: 15–30.

Churchland, P. M. 1984. *Matter and Consciousness*. Cambridge, MA: MIT Press.

Darden, L., and N. Maull. 1977. "Interfield Theories," *Philosophy of Science* 44 (1): 43–64.

Eronen, M. 2015. “Levels of Organization: A Deflationary Account,” *Biology and Philosophy* 30: 39–58.

Fodor, J. A. 1974. “Special Sciences (Or: The Disunity of Science as a Working Hypothesis),” *Synthese* 28: 77–115.

Horgan, T. 1984. “Supervenience and Cosmic Hermeneutics,” *Southern Journal of Philosophy* 22, Supplement Issue on Supervenience: 19–38.

Kim, J. 2002. “The Layered Model: Metaphysical Considerations,” *Philosophical Explorations* 5 (1): 2–20.

Klein, C. 2008. “An Ideal Solution to Disputes about Multiply Realized Kinds,” *Philosophical Studies* 140: 161–177.

Klein, C. 2013. “Multiple Realizability and the Semantic View of Theories,” *Philosophical Studies* 163: 683–695.

Ladyman, J., and D. Ross (with D. Spurrett and J. Collier). 2007. *Every Thing Must Go: Metaphysics Naturalized.* Oxford: OUP.

Lewis, D. K. 1972. “Psychophysical and Theoretical Identifications,” *Australasian Journal of Philosophy*. 50 (3): 249–258.

Melnyk, A. 2006. “Realization and the Formulation of Physicalism,” *Philosophical Studies* 131:127–155

Norton, J. 2021. *The Material Theory of Induction*. (BSPS*Open*/University of Calgary Press).

Oppenheim, P., and H. Putnam. 1958. “Unity of Science as a Working Hypothesis.” In H. Feigl, M. Scriven, & G. Maxwell (eds.), *Minnesota Studies in the Philosophy of Science* Vol II (Minneapolis: University of Minnesota Press), pp. 3–36.

Polger, T. W., and L. A. Shapiro. 2016. *The Multiple Realization Book*. Oxford: OUP.

Potochnik, A., and B. McGill. 2012. “The Limitations of Hierarchical Organization,” *Philosophy of Science*, 79: 120–140.

Putnam, Hilary. 1967. “Psychological Predicates,” in W. H. Capitan and D. D. Merrill (eds.), *Art, Mind, and Religion*. (Pittsburgh: University of Pittsburgh Press), pp. 37–48.

Rodriguez, M. L., B. T. Graham, L. M. Pabon, S. J. Han, C. E. Murry, N. J. Sniadecki. 2014. “Measuring the Contractile Forces of Human Induced Pluripotent Stem Cell-Derived Cardiomyocytes With Arrays of Microposts,” *Journal of Biomechanical Engineering*

Rupert, R. D. 2018. "The Self in the Age of Cognitive Science: Decoupling the Self from the Personal Level," *Philosophic Exchange* 47, 1–36.

Sachse, C. 2007. *Reductionism in the Philosophy of Science*. Berlin: De Gruyter.

Shoemaker, S. 2001. "Realization and Mental Causation." In C. Gillett and B. Loewer (eds.) *Physicalism and Its Discontents* (Cambridge: Cambridge University Press), pp. 74–98.

Thalos, M. 2006. "Nonreductive Physics," *Synthese* 149: 133–178.

Weiskopf, D. A. 2011. "The Functional Unity of Special Science Kinds," *British Journal for the Philosophy of Science* 62: 233–258.

Weiskopf, D. A. 2017. "The Explanatory Autonomy of Cognitive Models." In D. M. Kaplan (ed.), *Explanation and Integration in Mind and Brain Science* (Oxford: OUP), pp. 44–69.

Wilson, J. 1999. "How Superduper Does a Physicalist Supervenience Need to Be?" *Philosophical Quarterly* 49 (194): 33–52.

Wilson, J. 2010. "Non-reductive Physicalism and Degrees of Freedom," *British Journal for the Philosophy of Science* 61: 279–311.

Wilson, M. 2010. "Mixed-Level Explanation," *Philosophy of Science* 77: 933–946.

Wimsatt, W. C. 1994. "The Ontology of Complex Systems: Levels of Organization, Perspectives, and Causal Thickets," *Canadian Journal of Philosophy* 20 (Supplementary): 207–274.