Autonomy and Multiple Realization

Robert C. Richardson†

Multiple realization historically mandated the autonomy of psychology, and its principled irreducibility to neuroscience. Recently, multiple realization and its implications for the reducibility of psychology to neuroscience have been challenged. One challenge concerns the proper understanding of reduction. Another concerns whether multiple realization is as pervasive as is alleged. I focus on the latter question. I illustrate multiple realization with actual, rather than hypothetical, cases of multiple realization from within the biological sciences. Though they do support a degree of autonomy for higher levels of explanation and organization, they do not have the dire consequences critics of multiple realization fear.

1. Introduction: The Appeal of Multiple Realization. In "The Nature of Mental States," Hilary Putnam wrote: "Consider what the brain-state theorist has to do to make good his claims. He has to specify a physical-chemical state such that *any* organism (not just a mammal) is in pain if and only if (*a*) it possesses a brain of a suitable physical-chemical structure; and (*b*) its brain is in that physical-chemical state. This means that the physical-chemical state in question must be a possible state of a mammalian brain, a reptilian brain, a mollusc's brain" (1967, 436).

On this view, though mammals and mollusks differ fundamentally in the underlying neurophysiology, they nonetheless both experience pain. Putnam thought this had dire implications for the identity theory, and for reductionism. If this line of thought is compelling, we must identify *pain* as a type of psychological state, and independently identify the "underlying" neurological states as physiological states. It is then supposed to be an empirical fact that the "underlying" neurological states are distinct. Only with such independent criteria would the thesis of multiple realization have any bite. Of course, Putnam did think that both of these were possible. He was articulating and defending a functionalist analysis of mental states and arguing that it is consistent with multiple realization,

†To contact the author, please write to: Department of Philosophy, University of Cincinnati, P.O. Box 210374, Cincinnati, OH 45221-0374; e-mail: robert.richardson@uc.edu.

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while a more reductionist account that identifies mental and physical states is not.

If we consider the case of the eye, we can distinguish a wide variety of structures in a wide variety of organisms. The eye is actually *surprising* in the array of structures exhibited. Roughly one-third of all animal phyla have what we would count as eyes. Cnidaria, mollusks, annelids, onchophors, arthropods, and chordates all have representatives that have organs for image formation. In the evolution of eyes, functional constraints were crucial in limiting the range of possible "design" solutions. Light, after all, travels in straight lines; it is subject to laws of refraction; as a consequence, any image-forming device must respect those facts. Several types of eyes also evolved in water, and that constrains the range of wavelengths that can be deployed. If we consider just vertebrates (say, fish) and cephalopods (say, octopi), in both cases we have chambered eyes (as opposed to the compound eyes characteristic of insects), focusing images on two-dimensional receptive fields. The photoreceptors are quite different, the lens has a radically different shape, and the retinal structure is reversed, with the photoreceptors at the front in cephalopods and at the back in vertebrates. The eyes are also developmentally quite different. They arose independently, and are the products of convergent evolution.[1] On the face of it, this looks like a straightforward case of multiple realization. The problem is that, despite considerable and imposing functional constraints, there are not only structural, but functional, differences between the eyes. So where one might see multiple realization, others might see something less than multiple realization—perhaps multiple devices, each uniquely realized (see Polger 2004; Couch 2005).[2] In the end, there is no particularly clear way to distinguish kinds of eyes, and so no principled reason to favor one of these views over the other. We really can say little more than this. Octopus eyes are more similar to the eyes of fish than they are to the eyes of fruit flies, but in all these cases there are both

1. There are other eyes even more different in structure. Mollusks and vertebrates form images by refraction. The eyes of, say, scallops form images by refraction rather than reflection, and correspondingly, the receptor array is moved forward. Light is reflected off the back of the eye, and the curvature of the eye focuses it on a forward structure. Other eyes have no lenses at all, but operate essentially as pinhole cameras. See Fernald 2006 for an overview.

2. The fact that there are functional differences associated with structural differences won't really bear much weight. If two structures exhibit a common function, it hardly follows that they are exactly the same in all respects (Shapiro 2005). So, for example, the eye of the octopus doesn't have a blind spot as does the vertebrate eye; and they have a single type of photoreceptor, which makes them monochromats. It hardly follows they do not exhibit common functions.

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structural and functional differences. This isn't especially surprising, given the differences in evolutionary history, and their ecological niches.

I'm going to look at several different cases in what follows. My question is simply *whether* these are plausibly cases of multiple realization. I'll spend some time exploring *why* I do see them as genuine cases of multiple realization. Some big issues need simply to be set aside for my limited purposes. Most saliently, I'll have very little to say about the implications of multiple realization for theories of reduction. David Hull (1972, 1974) and William Wimsatt (1975) raised issues arising from multiple realization in a biological context. Their conclusion, with which I'm wholly sympathetic, was that the prevalence of multiple realization told against the *analysis* classically offered for reduction, and not against reductionistic science. John Bickle (1998) has made the same point, more recently, under the rubric of "new wave" reductionism. I'll also have nothing to say about the metaphysical issues that often feature strongly in these discussions. Metaphysics will have to take care of itself once we get a clear focus on the cases. To help frame my discussion, I want to look at two challenges to multiple realization in the more recent literature.

2. The Identification of Structures in Functional Terms. Let me begin with an issue I've hinted at in the case of the eye. In an important article, "Multiple Realizability Revisited," William Bechtel and Jennifer Mundale argue that "the claim that psychological states are in fact multiply realized is unjustified" (1999, 177). We should be clear that their primary interest is with the idea, often grounded in appeals to multiple realization, that "information about the brain is of little or no relevance to understanding psychological processes" (177). They reject that conclusion. With that latter, and more central, moral I have no reservations. Likewise I have no quarrel with the idea they also defend, that discussions of multiple realization have often been insensitive to scientific details.

They reach these conclusions by pressing that, in actual practice, "functional considerations get built into developing the structural taxonomy" (1999, 201). They explain that, for example, in the famous work by Brodmann at the beginning of the twentieth century, there are at least two factors that tell against multiple realization. First, Brodmann didn't simply begin with anatomical, or structural, differences; instead, his goal was to distinguish different *functional* areas of the brain. Given this, we would expect convergence of structural (anatomical) categories and functional (behavioral) categories, rather than multiple realization. Second, his project was a comparative one, looking for common patterns across species, and identifying conserved structures. Given this, we would expect similarity of structural (anatomical) categories and functional (behavioral) categories across species, rather than multiple realization. Of course,

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there's no a priori guarantee that such a project would succeed. Yet to a considerable extent, it did. And insofar as it did, it does bring into question the assumption of multiple realization.

In more recent work, as Bechtel and Mundale notice, there has been considerable success using imaging technologies (PET and fMRI) in identifying regions which exhibit enhanced activity in the brain, under a variety of cognitive demands. It is clear that the identification of brain regions depends on the cognitive tasks subjects perform. Once again, since the brain areas implicated are identified functionally, we would again expect convergence of structural (anatomical) and functional (cognitive) categories, rather than multiple realization. And again, the studies are often comparative, at least in identifying similar brain areas across individuals. Imaging results also require a rather controversial process of "averaging" in order to achieve adequate resolution. Here is what Bechtel and Mundale conclude: "Averaging will cancel out individual differences; thus, the fact that any results at all survive averaging as well as transformation into a common brain plan indicates a great deal of commonality. This alone suggests much less variability than the multiple realizability arguments would allow" (1999, 190).

It is true that averages will not reflect individual differences. It is difficult to see how this undercuts variable realization, or even multiple realization. To take a simple parallel, averaging heights also yields a single value across a population; but that hardly means there are not significant individual differences in height. Indeed, if there were *not* significant individual differences, averaging would hardly be needed. Perhaps the thought is that if averaging and transformations work, then the differences need not be dramatic. Again, though it might be true that in many cases there are not dramatic differences, the implication doesn't follow. Averaging, in short, is genuinely insensitive to differences. It does not at all reflect a lack of differences.

Larry Shapiro turns this sort of observation into a dilemma for the advocate of multiple realization (2000, 644). It goes this way. To exhibit multiple realization, a single function must be exhibited by different structures. On the one hand, if the structures differ *only* in functionally irrelevant ways, then they are not genuinely "different realizations." On the other hand, if the structures differ in functionally relevant ways, then there is no single function the various structures realize (see Shapiro 2004, 2005). In either case, there is no multiple realization. Essentially, Bechtel and Mundale tell us that Brodmann took the second leg of the dilemma, identifying structures in functional terms. He didn't find multiple realization.

3. The Problem with Disjunctions. The conclusion drawn from multiple

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realization in the psychological case is that it thwarts the explanation of psychological phenomena in terms of physical phenomena; more generally, the phenomena identified within the "special sciences" will invariably escape capture in terms of lower-level theories (see Fodor 1974, 1997; and, for a dissenter, Bickle 1998). In Fodor's hands, the problem was that the "disjunctive laws" identification would require are suspect. Fodor thought the *fact* of multiple realization was beyond serious doubt. He also thought that disjunctive types at a lower level would thwart interesting theoretical identifications of the type that would warrant reduction. So the autonomy of psychology was assured.

Elliott Sober objects to "Fodor's Horror of Disjunctions" (1999, 552). His objection is not to the fact of disjunction so much as to its supposed implications. He poses the problem formally, as one of derivations from laws with disjunctive antecedents. He asks, rhetorically, "Are we really prepared to say that the truth and lawfulness of the higher-level generalization is *inexplicable*, just because the . . . derivation is peppered with the word 'or'? I confess that I feel my sense of incomprehension and mystery palpably subside when I contemplate this derivation. Where am I going wrong?" (1999, 552–553).

Kenneth Schaffner made exactly the same move in a paper entitled "Reduction in Biology" (1974), though geared toward the case of genetics that previously had been raised by Hull and Wimsatt. These discussions raise a variety of issues which I'll have to pass on.

The problem, however, is not with disjunctions per se. Sober and Schaffner are right that it could not be *merely* the fact that there are disjunctions that enforces the antireductionist conclusion. Fodor's central thought, like Hull's, is that we have lower-level properties that are of disparate kinds, while the higher-level property is supposedly homogeneous. It is, despite Fodor's suggestion, not fundamentally that the disjunction "enumerating the possible realizations" is "open-ended." It is that it is disjoint and disparate. I'll try to illustrate this thought in what follows, though with cases rather different from those Fodor favored. I do think that what I say is sympathetic with what drove Fodor.

Here is what is fundamental. In the end, lower-level properties are not the same *kinds*, construed in terms of theories pitched toward the lower level. However, assuming there is an articulated and defensible higher-level theory, the corresponding higher-level properties are of a unified—perhaps homogeneous—kind, in terms articulated by the appropriate higher-level theory. So it would not be, for example, just that there is variation in the structure of eyes that would create a problem for reduction, but the fact that the eyes themselves differ fundamentally in structure. Much depends here on talk of levels of organization and on talk of levels of theory, and I do assume that some good sense can be made of it. I

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obviously would love to do that here, but won't. Take this as the moral I want to draw. The problem is not with disjunctions. Some disjunctions will be problematic. Others will not. It is also not simply with *diversity* among the "realizers." All our eyes are modestly different. If multiple realization is a problem, it lies with the *disparity* among the "realizers" together with the unity of what is "realized."[3]

4. Multiple Examples of Multiple Realization. So let me shift to some better-defined examples. Within philosophy of biology, when multiple realization was initially explored by David Hull (1972) and William Wimsatt (1975), the key example was the redundancy of the genetic code. I also made use of this as the key example in my paper on that topic (Richardson 1979). The general line is relatively well understood. Since the code is a triplet, with four nucleotides, this allows for 64 possible nucleotide triplets, though there are but 20 amino acids. Some triplets are *start* or *stop* codes, but in any case there is a tremendous amount of redundancy in the code. (In many cases, the redundancy is in the third position, which has a number of interesting consequences for evolutionary biology, but variation is not limited to the third position.) This is, prima facie, multiple realization. At a higher level, when we're interested in genetic transcription, we want to know the sequence of amino acids that are coded for in producing a protein. That's what protein synthesis is about. At a modestly lower level, when we're instead interested in, say, genetic replication, we want to know what the specific nucleotide sequences are. There is no question that these mappings from genetic sequence to amino acid sequences are well-articulated functions—ones that would please the hearts of logicians. However, there are two different projects. One is focused on protein identity and protein synthesis. What matters is the sequence of amino acids integrated into the protein and in what order; correspondingly, two triplets that code for the same amino acid are equivalent functionally. The appropriate "grain" must reflect amino acid sequences, and genes can be regarded as sequences of codons. The second project can be seen as focused on replication. What matters is the specificity of the code. Two sequences that differ in even a single nucleotide count as different sequences. In any case, the consequence seems inevitable: what are certainly different nucleotide sequences can, and sometimes do, serve to code for the same protein.

Let's explore some fairly straightforward consequences. First, there's

3. This is not a trivial point. Each person's eyes differ both structurally and functionally from every other person's. Otherwise, one prescription would fit all. Insisting on the lack of multiple realization would typically require that each eye is unique, as they are, both structurally and functionally. We need principled standards for individuation.

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no useful thought here about the disjunctions being "open-ended." There are only 64 possible triplets, and 20 amino acids, after all. There's nothing open-ended about that. With proteins having hundreds or thousands of amino acids, and with the myriad problems with protein synthesis such as splicing and alternative coding (see Burian 2005), there are very large numbers involved in mapping from nucleotide sequence to proteins. Still, it's a well-defined function, even if computationally daunting. The morals for our purposes are two. First, it's not the simple fact that different sequences code for the same proteins that is crucial, or the fact that we could list this as a disjunction that matters. Second, the focus should be on the problem of disparity. We certainly can determine the mapping from sequences to amino acids. We know there are many triplets for each amino acid. The next question is whether the triplets that code, say, for arginine are of disparate kinds or the same kind, conceived at the lower level. Of course, all six triplets code for arginine. So at the higher level, they count as the same. That's not sufficient to settle the question we started with. (And, of course, when the code was worked out in the 1960s, the project was to work out how transcription worked. In this respect, that is very much like Brodmann's project.) The answer is that the code is *not* chemically determined by the sequence. It could have been otherwise, so far as the chemical identity of nucleotides matters. Indeed, it *is* otherwise in some isolated cases. It seems, then, that *disparate* triplets code for the *same* amino acids. That looks like multiple realization.

Try another example. A cell includes an enormous variety of enzymes. These are the products of gene action. In modeling cell behavior, we can sometimes treat the cell as simply a bag of enzymes, mixed randomly. The cell takes in some substances (say, a sugar) and outputs others as waste. In between, there are truly astronomical numbers of interactions, converting the various substrates into other substrates, with existing enzymes. The key explanatory unit in this case is *enzymes*. These are functional units within cell physiology. Their job is to convert one substrate into another; in the process, energy is either released or stored. At a somewhat lower level, we can wonder about the constitution of the enzymes that the cell synthesizes. As it turns out, it is often possible to substitute one amino acid for another, without changing the catalytic properties of the enzyme; that is, we sometimes can substitute different amino acids without changing the active sites for the enzymes. The reason is that enzymes are three-dimensional structures. Their catalytic properties depend on the active sites that are exposed. That defines them as enzymes. What defines protein structure, though, is amino acid sequence rather than catalytic properties. Again, this is a case of multiple realization. There are distinct proteins that have the same catalytic properties. Of course, it could be argued by the metaphysically minded that this is not truly mul-

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tiple realization, given the similarity of function. Perhaps we could count these different proteins as a unique kind. Again, though, the best accounts belie the commitment. It seems to be true that the folding of sequences of amino acids depends critically on cellular "chaperones," and not merely on the amino acid sequence. If so, amino acid sequence does not determine the three-dimensional structure of the protein, and so does not *determine* its catalytic properties. Quite different proteins sometimes do have the same enzymatic properties. Disparity rules.

One more case might be useful, slightly higher up on the scale. When we're concerned with understanding cell physiology, we're typically interested in what the cell synthesizes, and at what rates. For some purposes, we can think of the cell as constituted by a set of pathways whose trajectory is determined by the constitutive enzymes. We have a reasonable account of what pathways amounts to: they take in some substance, and output another substance, in some relatively determinate rates. There will be a need for various intermediates, produced at specific rates, and those in turn will be variously transformed. (I'm being a bit vague here, I know, but this can be made very exact. For those who are not faint of heart, see Boogerd et al. 2005.) Enzymes, as I've said, are reasonably well defined. They have an array of catalytic capacities, which is what defines the enzyme. (These capacities are properly quantitative, so we want to know, specifically, for an enzyme that converts X to Y what its maximum rate of conversion is, and what its realized rate of conversion is. That is roughly its K value in the scientific literature.) So the first question we should ask is, can we, could we, substitute one enzyme for another without changing the pathways? The answer is that we often could. Nature often does. Most enzymes are not rate limiting. So substituting a different enzyme, with quantitatively different catalytic properties, but which can catalyze the substrates at the required rate, will yield the same pathway. It's a free substitution. The next question is whether these are sufficiently disparate types at the lower level. Again, at the lower level, these proteins have divergent catalytic capacities, which just may not be realized within the cells at hand. They are functionally equivalent, within that cellular context, but they finally differ in their "capacities." So we have different enzymes, determined in terms of catalytic properties, involved in the same pathways. Once again there is both diversity and disparity.

5. Conclusion. So here's a resolution to Shapiro's dilemma, or perhaps Bechtel's bludgeon. Is it typically true that we individuate the lower-level entities in terms of their higher-level functional properties? No. Of course, we can. And we sometimes do. But we often don't. Notice that nothing in what I say depends on any appeal to our "intuitions." Intuitions simply don't matter. We have well-defined scientific work which determines what

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counts as the same pathway, the same enzyme, the same protein, or the same gene. It turns out there is diversity, and so it turns out that there is indeed multiple realization in these cases. It also turns out that there is considerable disparity, so the differences matter.

In closing, let me return to the neurophysiology. I began by noticing that there do seem to be fundamentally different mechanisms underlying very similar functions. So, I noted that the mechanisms involved in the fixation of visual images differ substantially between, say, the octopus and fish. When Bechtel and Mundale press against multiple realization, they lean heavily on visual processing (1999, 190ff.). Their principal point is to emphasize that the understanding of psychological *function* is facilitated by appeal to the *structure* of the brain and its organization. As they explain, it is also facilitated by comparative studies. That is, structure is supposed to determine function. Visual processing is *their* example. They notice that in mammals one important projection from the retina is mediated by the geniculate nucleus of the thalamus. As they also note in a footnote, in other vertebrates the primary pathway from the retina runs through the optic tectum. This should give us some pause. The tectum is a relatively ancient structure; the optic tectum is integral to visual processing in modern reptiles and birds. The thalamus and the tectum are very different structures, by any anatomical standard. As we'd expect, there are fundamental similarities in the brains of all these organisms, who after all did descend from a common ancestor. The similarities should not blind us to the differences.[4] It certainly seems that, despite similarities among mammals, there are distinct anatomical structures serving the same function in other vertebrates.

In the case Bechtel and Mundale emphasize, Brodmann compared *homologous* structures in other species with the human case. Not surprisingly, we find similarities among, say, humans and other primates. Our eyes and their eyes are homologous structures. They are anatomically the same structure with some modifications. When we look at more divergent creatures, we find more divergence of structure, even if there has been considerable convergence under environmental pressures. The convergence yields similarity (not identity) of function *despite* divergence in structure. What we would expect, from an evolutionary standpoint, is for multiple realization to show up when we look to more distant forms. Homology will obscure the range of possibilities. Our nearer relatives, with homologous structures, would be expected to be more similar in structure, or

4. Another fundamentally different structure relevant to vision is the dorsal ventricular ridge, a structure unique to reptiles and birds. This is importantly involved in avian and reptilian visual processing, assuming functions characteristic of the mammalian cortex.

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form. This would be true *even if they differ functionally*. For our more distant relatives, without homologous structures, we would expect to find more difference in structure or form. This would be true *even if they are similar functionally*. And this seems to be what we find. The example with which I began—the octopus and the fish—exemplifies this pattern. So do the visual processing systems of birds and mammals.

I've looked at a variety of cases. I think there are many more we could look at. Notice the structure of the cases. We begin with reasonably well-defined scientific disciplines, with well-motivated standards for defining their domain. It turns out that, given their different standards, they partition the world differently. It is not so different that it compromises crossover between the disciplines, of the sort that Bechtel and Mundale want to defend. Nonetheless, the actual cases from the biology seem to sustain multiple realization, like it or not. The problem is what to make of them.

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