**Multiple realization and the commensurability of taxonomies**

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*Abstract*

The past two decades have witnessed a revival of interest in multiple realization and multiply realized kinds. Bechtel and Mundale’s (1999) illuminating discussion of the subject must no doubt be credited with having generated much of this renewed interest. Among other virtues, their paper expresses what seems to be an important insight about multiple realization: that unless we keep a consistent grain across realized and realizing kinds, claims alleging the multiple realization of psychological kinds are vulnerable to refutation. In this paper I argue that, intuitions notwithstanding, the terms in which their recommendation has been put make it impossible to follow, while also misleadingly insinuating that meeting their desideratum virtually guarantees mind-brain identity. Instead of a matching of grains, what multiple realization really requires is a principled method for adjudicating upon differences between tokens. Shapiro’s (2000) work on multiple realization can be understood as an attempt to adumbrate such a method.

*Multiple realization*, *neuroscience*, *autonomy* *of* *psychology*, *intertheoretic* *reduction*

**1. Introduction**

The multiple realization (“MR”) hypothesis asserts, at its baldest, that the same psychological state may be realized in neurologically distinct substrates (Polger 2009). Hilary Putnam’s (1967) ingenious suggestion that pain is likely to be a multiply realized kind (“MR kind”) rather neatly captures the thought here—while both mammals and molluscs presumably experience pain, they’re likely to instantiate it in neurological systems of a very different sort.

MR was played against a popular philosophical theory of mind in the 1960s which attempted to identify mental states with neural states. Since MR implies a many-to-one mapping from neural states to mental states, if it is in fact true that mental states are multiply realized, it follows that no clear identity relation can hold between them. As Bechtel and Mundale (1999, 176) frame the issue, “[o]ne corollary of this rejection of the identity thesis is the contention that information about the brain is of little or no relevance to understanding psychological processes.” When the MR hypothesis first came to prominence, its critics by and large accepted it as empirically correct, and merely denied its touted antireductionist implications. In recent years the debate has struck a new note, with many philosophers calling the empirical hypothesis itself into question. Bechtel and Mundale’s (1999) influential paper, followed quickly at the heels by Shapiro’s (2000) penetrating analysis of functions, perhaps did most to reignite the old controversy and drag MR back into the philosophical limelight. Bechtel and Mundale express what seems to be an important insight about multiple realization: that unless we keep a consistent grain across realized and realizing kinds, claims alleging the multiple realization of psychological kinds are vulnerable to refutation. In this paper I argue that, intuitions notwithstanding, the terms in which their recommendation has been put make it impossible to follow, while also misleadingly insinuating that meeting their desideratum virtually guarantees mind-brain identity. Instead of a matching of grains, what MR really requires is a principled method for adjudicating upon differences between tokens. Shapiro’s (2000) work on MR can be understood as an attempt to adumbrate such a method.

**2. Bechtel and Mundale’s grain requirement**

Bechtel and Mundale appeal to “neurobiological and cognitive neuroscience practice” in the hope of showing how claims that psychological states are multiply realized are unjustified. Intuitively, theirs is an argument from success: cognitive neuroscience’s method assumes MR is false, and the success of that method is evidence that MR *is* false. They argue that it is “precisely on the basis of working assumptions about commonalities in brains across individuals and species that neurobiologists and cognitive neuroscientists have discovered clues to the information processing being performed” (1999, 177).

Bechtel and Mundale examine both the “neuroanatomical and neurophysiological practice of carving up the brain.” What they believe this examination reveals is, firstly, that the principle of psychological function plays an essential role in both disciplines, and secondly, that “the cartographic project itself is frequently carried out comparatively—across species” (1999, 177), the opposite of what one would expect if MR were “a serious option.” It is the very similarity (or homology) of brain structure which permits generalization across species; and similarity in the functional characterization of homologous brain regions across species only makes sense if the claims of MR are either false or greatly exaggerated. For instance, “[e]ven with the advent of neuroimaging, permitting localization of processing areas in humans, research on brain visual areas remains fundamentally dependent on monkey research…” (1999, 195). “The clear assumption is that the neuralorganization in the macaque will provide a defeasible guide to the human brain” (1999, 183). Brodmann’s famous brain maps were based upon comparisons of altogether 55 species and 11 orders of mammals. If MR were true, “one would not expect results based on comparative neuroanatomical and neurophysiological studies to be particularly useful in developing functional accounts of human psychological processing” (1999, 178). They also argue that the ubiquity of brain mapping as a way of decomposing cognitive function points to the implausibility of the MR thesis. The understanding of psychological function is increasingly “being fostered by appeal to the brain and its organization” (1999, 191), again, the opposite of what one would expect “[i]f the taxonomies of brain states and psychological states were as independent of each other as the [MR] argument suggests” (1999, 190-91).

In light of such considerations, Bechtel and Mundale (1999, 178-79, 201-04) resort to grains as a way of making sense of what they perceive to be the entrenched, almost unquestioning consensus prevailing around MR. They think that it can be traced to the practice of philosophers appealing to different grain sizes in the taxonomies of psychological and brain states, “using a coarse grain in lumping together psychological states and a fine grain in splitting brain states.” When Putnam went about collecting his various specimens of pain, he ignored the many likely nuances between them. At the same time, he had few compuctions about declaring them different at a neurological level. His contention that pain is likely to be an MR kind can only command our respect if we can be sure that when he was comparing his specimens from a neurological point of view he was careful to apply no less lenient a standard of differentiation than he applied when comparing his specimens from a psychological point of view. Bechtel and Mundale maintain that when “a common grain size is insisted on, as it is in scientific practice, the plausibility of multiple realizability evaporates.” As their examples of neuroanatomical and neurophysiological practice attest, scientists in these fields typically match a coarse-grained conception of psychological states with an equally coarse-grained conception of brain states. Despite the habit of philosophers individuating brain states in accordance with physical and chemical criteria, a habit no doubt originating with Putnam, this is not how neuroscientists characterize them. The notion of a brain state is “a philosopher’s fiction” (1999, 177) given that the notion neuroscientists actually employ is much less fine-grained, namely “activity in the same brain part or conglomerate of parts.”

A not unrelated factor is that the MR hypothesis often gets presented in a “contextual vacuum.” The choice of grain is always determined by context, with “different contexts for constructing taxonomies” resulting in “different grain sizes for both psychology and neuroscience.” The development of evolutionary perspectives, for instance, in which the researcher necessarily adopts a coarse grain, contrasts with the much finer grain that will be appropriate when assessing differences among conspecifics:

One can adopt either a coarse or a fine grain, but as long as one uses a comparable grain on both the brain and mind side, the mapping between them will be correspondingly systematic. For example, one can adopt a relatively coarse grain, equating psychological states over different individuals or across species. If one employs the same grain, though, one will equate activity in brain areas across species, and one-to-one mapping is preserved (though perhaps further taxonomic refinement and/or delineation may be required). Conversely, one can adopt a very fine grain, and differentiate psychological states between individuals, or even in the same individual over time. If one similarly adopts a fine grain in analyzing the brain, then one is likely to map the psychological differences onto brain differences, and brain differences onto psychological differences. (1999, 202)

At least among some philosophers Bechtel and Mundale’s message has evidently been well received (Couch 2004; Polger 2009; Godfrey-Smith, personal communication; see also tacit approval in Aizawa and Gillett 2009, 573). Polger (2009) explains the motivation for the grain requirement in an illuminating way. Neuroplasticity has in recent times been thought to provide compelling evidence for the MR of mental states. He concludes that “contrary to philosophical consensus, the identity theory does not blatantly fly in the face of what is known about the correlations between psychological and neural processing” (2009, 470). The grains argument figures prominently in his reasoning. As he points out, it might be tempting to regard a phenomenon like cortical map plasticity—where different brain regions subserve the same function at different times in an individual’s history, say, after brain injury or trauma—as an existence proof of MR. But not if the point about grains is taken to heart. It all comes down to what we mean by “*different* brain regions” subserving “the *same* function.” Consider that recovered functions are frequently suboptimal. Genuine MR would indeed require the *same* psychological state to be underwritten by different neurological states; but suboptimality is evidence of difference underlying difference, not difference underlying sameness, as MR requires:

It’s true that this kind of representational plasticity involves the “same” function being mediated by “different” cortical areas. But here one faces the challenge leveled by Bechtel and Mundale’s charge that defenses of [MR] employ a mismatch in the granularity of psychological and neuroscientific kinds. If we individuate psychological processes quite coarsely—by gross function, say—then we can say that functions or psychological states are of the same kind through plastic change over time. And if we individuate neuroscientific kinds quite finely—by precise cortical location, or particular neurons—then we can say that cortical map plasticity involves different neuronal kinds. But this is clearly a mug’s game. What we want to know is not whether there is some way or other of counting mental states and brain states that can be used to distinguish them—no doubt there are many. The question is whether the sciences of psychology and neuroscience give us any way of *registering the two taxonomic systems*. (2009, 467, my emphasis)

**3. Problems with the grain requirement: imprecise, impracticable, and misleading**

But now the question is this: what, precisely, can it mean to use a “comparable” grain, or to keep a grain size “constant,” across both psychological and neurophysiological taxonomies? Polger’s motivation makes a lot of sense, to be sure, but talk of “registering” taxonomies (as of *aligning* classificatory regimes, or rendering distinct scientific descriptions *commensurable*, or however else one might care to put it) doesn’t shed any light on how the desideratum for consistent grains can actually be met. Since it is intended to serve in part as a methodological prescription, it’s important to know what to make of this requirement—metaphors won’t help us here. How, in *concrete* terms, is an investigator meant to satisfy such a condition as *this* on their research?

Perhaps it means this. Suppose you have two tokens of fruit. The science of botany (say) could deliver descriptions under which the two are classified the same (e.g. from the point of view of *species*), but also descriptions under which they come out different (e.g. from the point of view of *varieties*). The first description could be said to apply a coarser grain than the second. Now imagine economics coming into the picture. The science of economics can likewise deliver descriptions under which both tokens are classified the same (e.g. both are forms of tradable fresh produce) or different (e.g. one, being typically the crunchier and sweeter variety, has a lower elasticity of demand than the other). Once again, the first description could be said to apply a coarser grain than the second. Perhaps, then, we could take it that botany and economics deliver descriptions at the same grain of analysis when their judgments of sameness or difference cohere in a given case. In the example, botanical descriptions via species classification would be furnished at the same grain as economic descriptions via commodity classification, so that species descriptions in botany are “at the same grain” as commodity descriptions in economics. By the same logic, *variety* descriptions in botany would be comparable to *elasticity* descriptions in economics. Fine. But if that is all that “maintain a comparable grain” amounts to, it really does beg the question, for this is simply type-type identity by fiat. *Of course* such a recommendation will ensure that the mapping between psychology and neuroscience will be “systematic” (to use Bechtel and Mundale’s term), because on this account yielding concordant judgments of similarity or difference across taxonomies is what it *means* to apply the same grain. So we haven’t solved the problem: *this* version of the grain requirement makes type-type identity a fait accompli, effectively obliterating all MR kinds from the natural order.

It’s just as well that I don’t think this is what Bechtel and Mundale had in mind when they made their move to grains; supposing otherwise would serve only to trivialize an important aspect of their analysis. Still the construal is by no means far-fetched: “[o]ne can adopt either a coarse or a fine grain,” they tell us, “but as long as one uses a comparable grain on both the brain and mind side, the mapping between them will be correspondingly systematic” (note that—it *will* be!). This sounds like someone with the utmost confidence in the grain requirement, which is of course what one *would* have if one thought grains could be legitimately matched in just this way. My guess is that, while they do have something important to tell us about MR, a beguiling metaphor has led them to suppose that MR is easier to refute than it actually is. (I’ll support this contention with a few examples in a moment.)

Of course matters aren’t much helped by the reasonable suspicion that MR is the result of pairing *inconsistent* grains. For what is neuroscience if not a fine-grained description of psychology, and psychology if not a coarse-grained description of neuroscience? It is surely plausible that the neural and psychological sciences line up in something like this way, given that talk about the mind is really talk about the brain from a somewhat more abstract point of view.

What Bechtel and Mundale are ultimately trying to convey through their discussion of grains is the thought that claims of MR cannot be advanced willy-nilly—that there is an objective and standard way to go about verifying the existence of MR kinds and arbitrating disputes involving them. For the reasons just canvassed, however, it strikes me that talk of grains doesn’t serve their purposes at all well. In fact they would have been nearer the mark had they said that what MR requires is some sort of principled *mismatching* of grains.

So far I’ve tried to indicate in what respects Bechtel and Mundale’s grain requirement is imprecise and impracticable. Before I can show that the grains strategy is also misleading, and actually often gets things wrong, I need to set it against an account which demonstrably gets things right, indeed an account which even its detractors concede gets something very important right (e.g. see Gillett 2003, 591, 592, 596, 597, 599, 600). Shapiro (2000) expresses with enviable lucidity what I think is the crucial insight towards which Bechtel and Mundale were uneasily groping. Interestingly, some philosophers—e.g. Polger (2009)—write as if the grain requirement and Shapiro’s own formula for MR were effectively interchangeable. This is a mistake: the two approaches deliver different judgments in nontrivial cases (as I’ll illustrate in a moment).

As Shapiro reminds us:

Before it is possible to evaluate the force of [the MR thesis] in arguments against reductionism, we must be in a position to say with assurance what the satisfaction conditions for [the MR thesis] actually are. (2000, 636)

For him, “[t]he general lesson is this. Showing that a kind is multiply realizable, or that two realizations of a kind are in fact distinct, requires some work” (2000, 645). Furthermore, “[t]o establish [the MR thesis], one must show that the differences among purported realizations are causally relevant differences” (2000, 646). Shapiro’s concerns revolve around what motivates ascriptions of difference, and therefore sameness. The issue is important because the classic intuition pump that asks us to conceive a mind in which every neuron has been replaced by a silicon chip depends on our ascription of an interesting difference between neurons and silicon chips, apparently even where silicon chips can be made that contribute to psychological capacity by one and the same process of electrical transmission. His answer too, like Bechtel and Mundale’s, depends ultimately on context—in particular, the context set by the very inquiry into MR itself.

Shapiro (2000, 643-44) argues that “the things for which [the MR thesis] has a chance of being true” are all “defined by reference to their purpose or capacity or contribution to some end.” This is the reason why carburetors, mousetraps, computers and minds are standard fare in the literature of MR. They are defined “in virtue of what they do,” unlike, say, water, which is typically defined by what it is, i.e. its constitution or molecular structure, and accordingly *not* an MR kind. Genuine MR requires that there be “*different* ways to bring about the function that defines the kind.” Truly distinct (indeed *multiple*) realizations are those that “differ in causally relevant properties—in properties that make a difference to how [the realizations] contribute to the capacity under investigation.” Two corkscrews differing only in color are not distinct realizations of a corkscrew, because color “makes no difference to their performance as a corkscrew.” Similarly, the difference between steel and aluminium is not enough to make two corkscrews that are alike in all other respects two different realizations of a corkscrew “because, relative to the properties that make them suitable for removing corks, they are identical.” In this instance, differences of composition can be “screened off.” Naturally there may be cases where differences of composition *will* be causally relevant (and it turns out that this will be important to the broader point I make below about where the grains strategy goes wrong). Perhaps rigidity is the allegedly MR kind in question. In that event, compositional differences will necessarily speak to how aluminium and steel achieve this disposition. The crucial thing to note here is that MR *is* the context, and MR makes *function* the relevant consideration, i.e. the specific point of view from which we will compare a set of tokens in the first instance (not phenomenology, not behavioral ecology, or anything else for that matter). Explanatory considerations may of course fine-tune the *sort* of function that captures our attention (cork-removal, rigidity, vision, camera vision, etc.). But function here is our key preoccupation, and having settled on a specific function which a set of tokens can be said to perform, the all-important question on Shapiro’s analysis is *how* the two tokens bring that function about. Each case must be judged on its own merits. Thus unlike the two corkscrews identical in all respects save color, which do not count as distinct realizations, waiter’s corkscrews and winged corkscrews are enabled to perform the same task in virtue of *different* causally relevant properties, and therefore *do* count as genuinely distinct realizations of a corkscrew, one based on the principle of simple leverage, the other relying on a rack and pinions (Fig. 1).

 

**(a) (b)**

Figure 1. A waiter’s corkscrew (a) and a winged corkscrew (b). Each contributes to the capacity of cork-removal in different ways.

Notice that to the extent Shapiro’s causal relevance criterion envisages certain realizing properties being “screened off” from consideration in the course of inquiry, there is a sense in which the taxonomies of realized and realizing kinds may be said to be “commensurable” or “registrable” (no doubt explaining why some philosophers have simply confused commensurability with causal relevance). Thus when comparing the cork-removing properties of two waiter’s corkscrews, compositional differences will not feature in the realizing taxonomy (if we accept Shapiro’s characterization of the problem). So we have *cork-removal*, which features in what we may regard as a coarse-grained taxonomy, realized by two objects described by a “science” of cork-removal in which microstructural variations do not matter, hence which might also be regarded as a coarse-grained taxonomy. If on the other hand we were comparing the same corkscrews for rigidity, where one was made of steel and the other of aluminium, compositional differences *would* feature in the realizing taxonomy. Here we would have *rigidity*, which features in what we could well regard as a more fine-grained taxonomy than that encompassing cork-removal, realized by two objects described by a science in which microstructural variations really *do* matter (namely metallurgy), and which might also be regarded as a fine-grained taxonomy, at least more fine-grained than the fictitious science of cork-removal. But my point is this: commensurability nowhere appears as an independent criterion of validity in Shapiro’s account of MR, for it is an artifact of the causal relevance criterion, not a self-standing principle. Taxonomic commensurability is in fact an *implicit* requirement of the causal relevance criterion in the sense that it’s taken care of once the proper question is posed. As an explicit constraint it is a will-o’-the-wisp.

Armed with this analysis, let’s examine how Bechtel and Mundale attempt to refute the status of hunger as an MR kind. Putnam (1967) had compared hunger across species as diverse as humans and octopuses to illustrate the likelihood that some psychological predicates are multiply realizable. On the basis of their grains critique, however, Bechtel and Mundale suggest that hunger will not do the work Putnam had cut out for it; for “at anything less than a very abstract level,” hunger is different in octopuses and humans (1999, 202). The thought is that a finer individuation of hunger refutes the existence of a *single* psychological kind, hunger, which can be said to cross-classify humans and octopuses. Thus they essay to challenge the cognitive uniformity which MR requires at the level of psychology.

Perhaps we might first note that when identifying a *single* psychological state to establish the necessary conditions for MR, nothing Bechtel and Mundale say actually *precludes* the choice to go abstract. If context is what fixes the choice of grain (as they are surely right to point out), who’s to say that context couldn’t fix the sort of grain that makes hunger relevant in an abstract sense? It may be tempting to think that a more detailed description of something is somehow more *real*. But there is of course nothing intrinsically more or less real about a chosen schema relative to others that might have been chosen. There is no reason to suspect, for instance, that a determinate has any more reality than a determinable.

And yet there is a deeper problem with Bechtel and Mundale’s deployment of the grains strategy here. To repeat their complaint: “at anything less than a very abstract level,” hunger is different in octopuses and humans. But now why should *this* be relevant? Who would deny it? They themselves seem to be oblivious to the context which the very inquiry into MR makes paramount. They are not right to allege, as they do, that “the assertion that what we broadly call ‘hunger’ is the same psychological state when instanced in humans and octopi has apparently been widely and easily accepted without specifying the context for judging sameness” (1999, 203). The reason why hunger, pain, vision and so on were all taken for granted—assumed to be uniform at the cognitive level—is because MR made *function* the point of view from which tokens were to be compared. As Shapiro reminds us, “the things for which [the MR thesis] has a chance of being true” are all “defined by reference to their purpose or capacity or contribution to some end.” It was understood that, say in the case of pain, regardless of phenomenal, ecological or behavioral differences between human and octopus pain (I doubt any of which were lost on Putnam), all instances of pain in these creatures had something like *detection and avoidance* in common. This might be to cast pain at “a very abstract level,” but this just happens to be the context which the inquiry into MR itself sets. A similarly abstract feature is what unites all instances of hunger: let’s call it *nutrition-induction*. It is not that decades of philosophers had simply forgotten to specify the point of view from which these psychological predicates were being considered: it is rather that they simply didn’t need to, since all of them had read enough of Putnam and the early functionalists to know what they were about. Phenomenal and other differences that one might care to enumerate between these predicates come a dime a dozen. But the whole point of functionalism was to abjure the inquiry into essences and focus instead on the causal role of a mental state within the life of an organism. Yes, this is to compare tokens from an “abstract level,” but that’s what made functionalism intriguing to begin with. And if Shapiro’s analysis is any guide, it is really the *next* step in the endeavor to verify the existence of an MR kind that is the crucial one. Genuine MR requires that there be “*different* ways to bring about the function that defines the kind.” So the follow-up question concerns *how* the relevant organisms achieve their detection and avoidance function, or nutrition-induction function, or whatever the case may be. It is in fact only by asking this next question that we can appreciate just how badly the grains strategy fares. The attempt to individuate hunger more finely does *not* refute the multiple realizability of hunger as between humans and octopuses, for it is extremely likely that humans and octopuses achieve their nutrition-induction capacities in different ways. The attempt to individuate pain more finely would likewise *not* refute the multiple realizability of pain as between humans and octopuses, for it is extremely likely that humans and octopuses achieve their detection and avoidance capacities in different ways. So we see that the grains strategy, to the extent that it involves fine-graining psychological states in order to undermine the cognitive uniformity required by MR, sets itself a very easy job indeed, and mischaracterizes the nature of MR by its neglect of function. Moreover Shapiro’s causal relevance criterion—which honors the core concerns motivating Bechtel and Mundale’s resort to grains—does *not* demonstrate that hunger (or pain) is type-reducible.

A good illustration of the grains strategy in action is provided by Couch’s (2004) attempt to refute the claim that the human eye and the octopus eye are distinct realizations of the kind *eye*. Conceding differences at a neurobiological level, the strategy again involves challenging the alleged uniformity at the cognitive level. As he explains, “[e]stablishing [MR] requires showing that…the physical state types in question are distinct [and] that the relevant functional properties are type identical. Claims about [MR] can be challenged at either step” (2004, 202). Reminding us that psychological states “are often only superficially similar,” and that “at a detailed level the neural differences make for functional differences” (2004, 203), he states:

Psychologists sometimes talk about humans and species like octopi sharing the same psychological states. However, they also recognize that there are important differences involved depending on howfinely one identifies the relevant features...Establishing multiple realization requires showing that the same psychological state has diverse realizations. But we can always disagree with the functional taxonomy, and claim there are psychological differences at another level of description. (2004, 203)

Thus he relates that while the two types of eyes have similar structure in certain respects, both consisting of a spherical shell, lens and retina, they use different kinds of visual pigments in their photoreceptors, as well as having different numbers of them, the octopus having one in contrast to the human eye which has four. They also have different retinas. The human retina, with rods and cones, focuses light by bending the lens and so changing its shape. The octopus eye, with rhabdomeres instead of rods and cones, focuses light by moving the lens backwards and forwards within the shell. All these factors show up as differences in output, not just structure. The octopus, having only a single pigment, is colorblind, while its receptor’s unique structure allows it to perceive the plane of polarized light. Retinal differences likewise make for functional differences, with very little information processing occurring on the octopus’s retina, unlike the case of the human retina. This produces differences in stimuli and reaction times. So the two eyes might be similar, but when described with a suitably fine grain, he contends, they come out type distinct. In the result they are both physically *and* cognitively diverse, and so not genuine examples of MR.

Notice again that, contrary to what is claimed, it has not been demonstrated that type-type identity prevails here after at all (on the understanding that the kind camera eyehuman reduces to *its* distinct neural type, and the kind camera eyemollusc in turn reduces to *its* distinct neural type).[[1]](#footnote-1) If anything what this foray into mollusc visual physiology succeeds in showing is that, relative to the kind camera eye, human camera eyes and octopus camera eyes count as distinct realizations(!), for, assuming Shapiro’s causal relevance criterion applies, human camera eyes achieve the function of *camera vision* differently to the way octopus camera eyes achieve this function. Were we to attend to the original inquiry, which concerned whether human eyes and octopus eyes count as distinct realizations of the kind eye, Shapiro’s own response, for what it’s worth, is clear (2000, 645-46): here we do seem to confront a genuine case of type-type identity, as Putnam himself assumed, because, relative to the function of *vision* (not *camera* *vision*), both humans and molluscs achieve the function the same way (namely, by camera vision!). Differences that would be relevant at the neural level between humans and molluscs when asking how camera vision is achieved can be conveniently screened off when the question is how vision, as distinct from camera vision, is achieved. Again if pain or hunger were the kind in question, it seems more likely than not that we *would* confront a case of MR (unlike with vision), as we conjectured earlier. Explanatory context dictates the function of interest, and the function is one that we have to assume is common to the tokens in question in order to get the inquiry into MR off the ground. Indeed if Shapiro’s analysis is correct, with MR we’re always asking how some common function is achieved by different tokens that *do that thing*. Where there is no common function the question of MR cannot so much as arise. The fact that the question *does* arise in all the cases we’ve considered is a powerful indication that we’re dealing with functions which all the relevant tokens actually share. The grains strategy confuses matters by suggesting that in many cases involving putative MR kinds, psychological states can be individuated using a finer grain of description. But if what I have been saying is right, this is not the proper way to refute a putative case of MR.

That mine is the correct assessment of the situation is not only attested to by Shapiro’s analysis of MR, but also by the fact that it avoids the very mug’s game Polger sought to eschew by embracing the grains strategy in the first place. If for any putative MR kind I am free to cavil with the choice of your size of grain (“oh, that’s far too coarse for psychology,” or “now that’s really not coarse enough for neuroscience”), how is the resulting game any less of a mug’s game than the one we were trapped in at the start? I myself have played a few of these games with philosophers. No one wins. Couch’s remarks are telling: “we can always disagree with the functional taxonomy, and claim there are psychological differences at another level of description.” So the game goes on.

**4. Conclusion**

In sum, I think there’s a genuine problem with the grain requirement. The central difficulty is that in the terms in which it’s been put it is largely unworkable, and at best no more than a loose metaphor. For a recommendation intended to serve at least in part as a methodological reform, this is clearly unsatisfactory. I don’t deny that Bechtel and Mundale were onto something. But whatever value their insight into MR might have has been obscured by their unfortunate formulation of the issue. Moreover, as I have tried to show, the formulation is unfortunate not *just* because it happens to be unworkable. More worryingly, the argument from grains distorts the truth about MR by encouraging the view that mind-brain identity comes for free once we invoke the “same grain” of description across both realized and realizing kinds. But when the insight to which this locution seems to point is expressed in terms that are intelligible and empirically tractable (namely, Shapiro’s causal relevance criterion), mind-brain identity seems anything but a fait accompli. Grains talk makes it tempting to think MR is easier to refute than it in fact is. It is certainly true, as Bechtel and Mundale acknowledge, that context fixes the choice of grain (where by “grain” we mean the respect under which we seek to compare a set of tokens); but we are not ipso facto obliged to employ a consistent grain across realized and realizing kinds (since this is just about meaningless as far as a researcher into these matters would be concerned and raises a host of difficulties beside). Rather than matching grains, what MR really behooves us to do is to apply a principled method for adjudicating upon differences between tokens of a functional kind. Shapiro’s work on MR shows us how to approach this important task.

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1. More precisely, this sort of type reduction is always (trivially) true, since at the limit everything is a special case. Of course there are occasions when “kind splitting” like this may actually be *mandated* by the sciences concerned, such that the higher level kind has no taxonomic relevance, and genuine type reduction succeeds; but this is by no means a given (see Polger and Shapiro 2016, 103, 104-105, 110-111). [↑](#footnote-ref-1)