Neuroscience and Multiple Realization: A Reply to Bechtel and Mundale

Ken Aizawa
Department of Philosophy
Centenary College of Louisiana

One theme in recent philosophical attention to neuroscience has been that closer, more serious attention to actual neuroscientific research, and its results, challenges the familiar view that psychological properties are multiply realized by neuroscientific properties. Shagrir, (1998), presents a number of diverse reasons to think that diversity in neuroscientifically identified structures and properties does not inevitably lead to multiple realization. Bechtel and Mundale, (1999), argue that neuroscientific practice extending over a century contradicts the consequences of the hypothesis that psychological functions are multiply realized. Bickle, (2003), argues that a series of animal models of the consolidation of short-term memories into long-term memories reveals that this process is uniquely realized by a single biochemical cascade involving cAMP, protein kinase A, and cAMP response element binding proteins. Shapiro, (2004), argues that experiments on neuroplasticity do not show that there are many ways in which a brain might be wired in order to achieve a given psychological function.[1]

In a series of independent and collaborative works, Carl Gillett and I have tried to develop a clear, well-articulated theory of realization and multiple realization that is simultaneously true to the metaphysics of these relations and true to actual scientific discovery and practice.[2] In addition to our positive view of the nature of realization and multiple

[1] Keeley, (2000), also looks closely at neuroscientific work, in particular research on electric fish, but not to show that multiple realization does not exist. Instead, he argues that the existence of multiple realization does not preclude theoretical interactions between neuroscience and psychology.

[2] Gillett, (2002, 2003), Aizawa, (2007), Aizawa & Gillett, (forthcoming, unpublished-a, unpublished-b).

realization in psychology and neuroscience, our work has included replies to some of these recent challenges to the multiple realization of psychological properties. Gillett, (2003), articulates the Dimensioned view of realization which explicates what appears to occur when mental properties are realized by properties of the brain and its components. Gillett, (in progress), expands the account of the realization into a full-blown theory of compositional relations in the sciences. Aizawa, (2007), relies upon an informal understanding of the realization relation and multiple realization to make the case that, Bickle's argumentation notwithstanding, the psychological process of memory consolidation is, in fact, multiply realized at the biochemical level. If there is indeed a single cognitive process of memory consolidation common to mice, *Aplysia*, and *Drosophila*, then this process is realized by distinct sets of amino acids in distinct species of organisms. Aizawa & Gillett, (unpublished-a), argues that certain purported methodological implications of the multiple realization of psychological processes are not genuine implications. Aizawa & Gillett, (unpublished-b, forthcoming), further elaborate on our theory of realization and multiple realization, drawing attention to the fact that the realization and possible multiple realization of a high-level property is implicitly indexed to some lower level property or set of properties. Thus, it is in principle possible to have a psychological property be multiply realized at one level of organization, say, the biochemical level, but also uniquely realized at some other level of organization, say, the neuronal level. That paper also presents evidence suggesting that psychological properties are realized at the biochemical and cellular levels.

This paper is one component of the broader Gillett-Aizawa agenda. It replies to several of the principal challenges Bechtel and Mundale raise in their complex treatment of neuroscience and multiple realization. Reading their paper, one can find three principal arguments against

(some form of) multiple realization of psychological functions.[3] The first of these, what I take to be the Central Argument, maintains that, if psychological functions were multiply realized, then taxonomies of the brain would have to be carried out independently of psychological function. Further, if psychological functions were multiply realized, we would not find brain mapping studies that compare the brains of diverse animal species. The purported problem, then, for the hypothesis of multiple realization of psychology is that actual brain mapping research makes essential use of psychological function and makes frequent interspecific comparisons. This is the argument that seems to be at the heart of sections 1-3, of Bechtel and Mundale, (1999). The second argument is found in section 4 of their paper, where they draw attention to the way in which it is possible to use facts about the brain to guide psychological theorizing. More specifically, it is possible to use functional localization to guide theories of the mechanisms of brain processing. Finally, in section 5 of their paper, Bechtel and Mundale claim that, for any given psychological function, one can find a grain of description of a region of the brain such that activity in that region uniquely realizes the psychological function. We might call this the "grain matching claim."

To some extent, the second argument has been addressed in a general way in Aizawa &

[3] Bechtel and Mundale are, in the first instance, concerned with the possible multiple realization of functions, where Gillett and I suppose that it is properties that are, or are not, multiply realized. In this discussion, I will write of functions, because it is Bechtel and Mundale's preference. What is said here about functions can be related to the scheme in the Gillett-Aizawa papers by construing a thing's having a function as its bearing a particular kind of property. So, the Gillett-Aizawa approach is general enough to encompass Bechtel and Mundale's focus on functions.

They also sometimes talk of the multiple realization of states, as in "If the taxonomies of brain states and psychological states were as independent of each other as the multiple realizability argument suggests, brain decomposition would be a poor guide for psychological decomposition" (Bechtel & Mundale, 1999, pp. 190-191). This focus, however, is somewhat strained by their claim that "Actually, the notion of a brain state is a philosopher's fiction; a notion closer to what neuroscientists would use is activity in the same brain part or conglomerate of parts" (ibid., p. 177).

Gillett, (unpublished-a). Central to this argument is a presupposition of the form: if the multiple realization of psychological functions were true, then there would be no methodological constraints of type M. There are methodological constraints of type M, therefore psychological functions are not multiply realized. The cornerstone of the reply in Aizawa & Gillett, (unpublished-a), is a theory of the realization relation according to which realization is a non-logical, non-casual determination relation between lower level properties and higher level properties. If lower level properties determine higher level properties, then there must be some sorts of constraints between well-confirmed theories of the lower level properties and well-confirmed theories of the higher-level properties. In other words, the major premise of their *modus tollens* is false. Contrary to what Bechtel and Mundale presuppose, realization is not a "ghostly" relation in which what happens at the psychological level floats free of what happens at the lower neuroscientific levels. Because this second argument has been addressed elsewhere, this paper will focus on the two other principal arguments found in Bechtel & Mundale, (1999).

Section 1 of this paper will constitute an extended and detailed reply to Bechtel and Mundale's Central Argument. It will include an explanation of an important respect in which Bechtel and Mundale misrepresent the nature of anatomical brain mapping. It will also explain how they misinterpret the bearing of studies of functional localization on the issue of the multiple realization psychological properties. Finally, it will argue that, although there are methodological implications of the realization and multiple realization of psychological functions, these implications are not what Bechtel and Mundale presuppose. It is not true that if psychological properties are multiply realized, then taxonomies of the brain would have to be carried out independently of psychological function would not involve comparisons of the brains of diverse animal species. Section 2 of this paper will constitute a reply to Bechtel and

Mundale's "grain argument."

1. Bechtel and Mundale's Central Argument

The Central Argument of Bechtel and Mundale's paper may be found in the following passage:

> Two aspects of these [brain mapping] practices will be especially noteworthy: (1) the appeal to function, especially psychological function, is an *essential* part of both the project and its tools, and (2) the cartographic project itself is frequently carried out comparatively–across species. For multiple realization to be a serious option, brain taxonomy would have to be carried out both independently of psychological function, and without comparative evaluation across species (Bechtel and Mundale, 1999, p. 177, italics in original).

The centrality of this argument derives from the fact that the second and third sections of their paper are dedicated to defending the two minor premises. For the sake of clarity, the foregoing argument can be more clearly stated as follows:

> 1. If psychological functions were multiple realized, then brain taxonomy would have to be carried out independently of psychological function and without comparative evaluation across species.
> 2. Brain taxonomy requires an appeal psychological function.
> 3. Brain taxonomy uses comparative evaluation across species.
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> Therefore, psychological functions are not multiply realized.

Notice that the apparent conclusion of Bechtel and Mundale's central argument, captured in this reconstruction, is that multiple realization is false. There is, however, an alternative interpretation of what they take their arguments to establish. Very early in their paper, they claim that "Our primary concern, rather, is with the implication drawn from the multiple realizability argument that information about the brain is of little or no relevance to understanding psychological processes" (Bechtel & Mundale, 1999, p. 176), and later "Our efforts have been aimed at discrediting the claim that cognitive states are multiply realized in any

way that discounts the relevance of neuroscience to our understanding of cognitive processes" (ibid., p. 201). On one alternative reading, they may be taken not to be challenging the multiple realization of psychological functions *per se*. Instead, they are willing to accept it, merely denying some putative consequences that have been drawn from it. On another alternative reading, they do not mean to challenge just any kind of multiple realization, only a specific kind, namely, the kind that entails that brain science is irrelevant to psychology. There are, however, a number of reasons for responding to the strong interpretation of Bechtel and Mundale's view expressed above. In the first place, it seems to capture what they in fact argue better than do the summary statements cited above. Instead of the Central Argument, it is the second of their arguments, the argument of section 4 of their paper, that appears to aim for a weaker conclusion, such as that multiple realization does not entail that neuroscience is irrelevant to psychology. In their section 4, they claim that "If the taxonomies of brain states and psychological states were as independent of each other as the multiple realizability argument suggests, brain decomposition would be a poor guide for psychological decomposition" (Bechtel & Mundale, 1999, pp. 190-191). Second, Bechtel and Mundale give numerous other assessments of their work that support the idea one of their goals, the one in the central argument, is to discredit the hypothesis that psychological functions are multiply realized. For example, the title of the fifth section of their paper is "A Diagnosis of Why Multiple Realizability Looked Plausible". They also write, "If we have been successful [in our argument], then an interesting question arises as to why the claim that psychological states are multiply realized by different biological species has seem so compelling in the philosophy of mind" (ibid., p. 201). In other words, the question is why this false hypothesis seemed plausible.[4] Third, even if we accept the interpretation that Bechtel and Mundale are offering a

[4] Other passages that suggest that Bechtel and Mundale actually wish to challenge the multiple

critique of only a special type of multiple realization—some version of "ghostly" multiple realizability according to which neuroscience is irrelevant to psychology—we are then considering a version of multiple realization that is not *prima facie* plausible. Such ghostly multiple realization does not do justice to the multiple realization of even such mundane objects as hammers and screwdrivers. So, however we interpret the Central Argument, it does not

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realization of psychological functions are the following:

A proper examination of neurobiological and cognitive neuroscience practice will show that the claim that psychological states are in fact multiply realized is unjustified (Bechtel and Mundale, 1999, p. 177).

Two aspects of these practices will be especially noteworthy: (1) the appeal to function, especially psychological function, is an *essential* part of both the project and its tools, and (2) the cartographic project itself is frequently carried out comparatively–across species. For multiple realization to be a serious option, brain taxonomy would have to be carried out both independently of psychological function, and without comparative evaluation across species (Bechtel and Mundale, 1999, p. 177, italics in original.

If the gloomy implications of multiple realizability were to be taken seriously, one would not expect results based on comparative neuroanatomical and neurophysiological studies to be particularly useful in developing functional accounts of human psychological processing (Bechtel and Mundale, 1999, p. 178).

When a common grain size is insisted on, as it is in scientific practice, the plausibility of multiple realizability evaporates (Bechtel and Mundale, 1999, p. 178).

Nevertheless, it is important to note that in interpreting these deficits [of Broca's aphasia] researchers implicitly reject multiple realization (Bechtel and Mundale, 1999, p. 184).

Identifying brain areas through neuroimaging depends critically on the cognitive tasks subjects are asked to perform; thus, the possibility of multiple realizability is restricted at the outset (Bechtel and Mundale, 1999, p. 190).

In the previous section our focus was on how the identification of brain parts itself relied on (1) functional considerations that were frequently psychological in nature, and (2) cross-species comparisons. Both of these features undercut the possibility of finding instances of multiple realization of psychological states within terrestrial animals. If the taxonomies of brain states and psychological states were as independent of each other as the multiple realizability argument suggests, brain decomposition would be a poor guide for psychological decomposition (Bechtel and Mundale, 1999, pp. 190-191).

constitute a serious challenge to multiple realization properly understood. Let us begin to see how this goes.

1.1. A Critique of Premise 2

Stated in a plain fashion, the claim that brain taxonomy makes essential use of psychological function should seem implausible. One would expect it to be possible to identify visually distinctive, regularly occurring parts of the mammalian brain, even without understanding what functions, psychological or otherwise, those parts might have. At the grossest level, one can presumably identify the left and right hemispheres, the cerebellum, the white and gray matter, the pineal gland, and the corpus callosum, with no idea what these structures do. It is relatively easy to guess that the fibers connected the eye to the brain have something to do with vision, but this would seem to be an exceptional case. Smaller, less familiar structures of the brain, and the way in which they are named, reinforces the point. There is the globus pallidus, the substantia nigra, striate cortex, the lateral geniculate nucleus, the anterior commissure, and the posterior commissure. These names are not reflections of (psychological) function, but of visual appearance to the investigator.

Thus, one diagnosis of what has made the multiple realizability claim as plausible as it has been is that researchers have employed different grains of analysis in identifying psychological states and rain states (Bechtel and Mundale, 1999, p. 202).

Having invoked different grains, it is relatively easy to make a case for multiple realization. But if the grain is kept constant, then the claim that psychological states are in fact multiply realized looks far less plausible (Bechtel and Mundale, 1999, p. 202).

We have tried (i) to demonstrate that the claim that psychological states are multiple [sic] realized has not been demonstrated, at least within animal life form, (ii) to show how denying multiple realization allows fruitful use of neuroscience in guiding the decomposition and understanding of cognitive systems, and (iii) to diagnose why multiple realizability has been so widely accepted (Bechtel and Mundale, 1999, p. 204).

But, perhaps the foregoing considerations miss the point that Bechtel and Mundale are pursuing. Perhaps they do not mean to say that the mapping of the entire brain requires an appeal to psychological function, but only, say, the cortex. Perhaps they do not mean to say that the classification of absolutely all brain structures has to proceed by appeal to function. Since Bechtel and Mundale are not explicit about the scope of their claim that brain mapping requires appeal to psychological function, we should consider the specific brain mapping projects that Bechtel and Mundale describe in support of their view. These projects are of two sorts: neuroanatomical brain mapping and the various methods of functional localization. We can examine these in order.

1.1.1. Neuroanatomical Brain Mapping. Bechtel and Mundale chronologically order their examples of neuroanatomical brain mapping, beginning with Korbinian Brodmann's *Localization in the Cerebral Cortex*. They write,

> Significantly, Brodmann's goal in identifying different regions of the brain was, ultimately, to understand *function*; he writes, "Although my studies of localization are based on purely anatomical considerations and were initially conceived to resolve only anatomical problems, from the outset my ultimate goal was the advancement of a theory of function and its pathological deviations." (Bechtel and Mundale, 1999, pp. 179-180).

On the face of it, Brodmann's claim that his studies of localization are based on purely anatomical considerations flatly contradicts any natural reading of Bechtel and Mundale's claim that the appeal to psychological function is an essential part of Brodmann's project and its tools. Brodmann might have ultimately hoped to relate neuroanatomy to psychology, but by his own account doing so was not necessary to his project. Other passages from Brodmann are even more damning for Bechtel and Mundale's second premise. Brodmann begins his Introduction writing, "The subject of the following treatise is histological localization in the cerebral cortex, that is to say localization which uses exclusively anatomical features as the basis for

investigation, in contrast to physiological or clinical aspects" (Brodmann, 1999, p. 3). Later, Brodmann strenuously rejects any appeal to physiological or psychological function in his project: "Those who find it to their taste can dress up the individual layers with terms borrowed from physiology or psychology, such as "*sensitive"* or *"perceptive"* layers, *association* or *projection* layers, *"memory"* or *"psychic"* layers, but they should not claim to be serving scientific progress in so doing. These, and all similar expressions that one encounters repeatedly today, especially in the psychiatric and neurological literature, are utterly devoid of any factual basis; they are purely arbitrary fictions and only destined to cause confusion in uncertain minds" (Brodmann, 1999, p. 9). If Brodmann says that his studies are based on purely anatomical considerations and that appeals to physiological and psychological terms are "utterly devoid of any factual basis," that is *prima facie* evidence that he, at least, thinks the appeal to psychological function is not essential to his project or his tools. This is not to say that Brodmann never mentions such things as visual or motor cortex. He does, putting the terms in scare quotes (cf. Brodmann, 1999, p. 186, p. 189). The fundamental conclusion to draw, therefore, is that Brodmann maintains that psychological functions are not essential parts of his project or his tools.

Yet, one's methodological pronouncements are one thing; one's actual practice another. Perhaps Brodmann is misguided about how he does what he does. What really matters, one might say, are the actual methods Brodmann uses; not the methods he says he uses. So, we should consider whether Brodmann aptly describes his work. It seems he does. Brodmann's technique was relatively simple. It involved fixing brain sections in formalin, embedding them in paraffin, and applying the cresyl violet stain. The method, developed by Franz Nissl, stains the mRNA in the cytoplasm of neurons, thereby enabling an observer to determine the number,

size, and form of nerve cells in the sample. Having prepared slices of brain tissue, Brodmann, then, used changes in such features as the total thickness of the cortex, the thicknesses of individual layers of the cortex, and cell densities to delimit different regions of the brain. In Figure 1, for example, we see the kind of differences Brodmann used as a basis for delimiting his numerous regions of the brain. Neither the project nor the tools relies in any obvious way on any putative psychological functions of the brain. For all that the project and the tools require, the tissue that is stained could be involved with digestion, rather than cognition. For Brodmann, at least, neuroanatomical brain mapping does not essentially involve function.

Variations on Brodmann's methods continue to this day in the form of different staining techniques that highlight different features of brain cells.[5] Bechtel and Mundale allude to Oskar and Céclie Vogt's work on myeloarchitectonics, which involved staining the myelin sheaths of various neurons. The Vogts's methods and Brodmann's methods gave somewhat different pictures of the brain, since not all nerve cells are myelinated. More recent stains for, say, acetyl choline yield still different pictures of the brain, since acetyl choline is not distributed throughout the brain in just the way that myelin and mRNA is. What the majority of these anatomical approaches to brain mapping have in common is some method for hardening the tissue (as by freezing), slicing it thinly with a microtome, and then exposing it to a series of different solutions for different periods of time. Such procedures do not require or involve any knowledge of the psychological function of the tissues. Many staining procedures could as easily be applied to brain tissue as to liver or muscle tissue. Applications of some stains designed for brain tissue might, of course, be utterly useless for revealing structures in the liver or muscle, but they could be so applied. The point, therefore, is that such methods do not involve any knowledge of the

[5] Numerous examples of high-resolution, whole-brain images of humans, macaques, cats, dogs, etc., using a number of distinct staining techniques is now freely accessible online at: www.brainmaps.org.

psychological function of the tissues.

To this point it has been argued that brain mapping techniques that involve staining brain tissue and discerning differences in structure over the volume of the brain do not make essential use of *psychological* function. But, perhaps this is not exactly what Bechtel and Mundale wish to maintain. After all, they say that "the appeal to function, especially psychological function, is an *essential* part of both the project and its tools." Perhaps we should simply drop the qualifier, "especially psychological function". In addition to avoiding the problems raised by cytoarchitectonics, this interpretation of their view might also help make better sense of what Bechtel and Mundale wish to defend on another score. Bechtel and Mundale entitle the third section of their paper "Use of Psychological Function in Brain Mapping: Deficits, Stimulation, and Neuroimaging." Were they to have the view that the appeal to psychological function is an *essential* part of the brain mapping project and its tools, this section title seems strange. If brain mapping involve essential appeal to psychological function, how can there be a sub-category of brain mapping that uses psychological function? And more important than simply doing justice to what Bechtel and Mundale claim, perhaps it is true that anatomical brain mapping techniques make essential use of (non-psychological) function. Whatever Bechtel and Mundale's view, this is alternative hypothesis merits comment.

There is no doubt that knowledge of physiological functions can be of help in the development and use of neurohistological techniques. For example, if one knows that acetyl choline is a neurotransmitter in a particular brain region, then one can use this information in deciding whether acetylcholinesterase staining is an appropriate technique. If one knows enough about the genetics of an organism, it may be come possible to insert genes into the organism that will produce fluorescent proteins in desired tissues.[6] Such observations, however, do not show

[6] See, for examples, Grutzendler, Kasthuri, & Gan, (2002), Holtmaat, Trachtenberg, Wilbrecht,

that appeal to even physiological function is necessary for brain mapping. In principle, one could develop staining techniques largely through a process of trial and error with various sorts of dyes with very little knowledge of the mechanisms underlying the staining process. Surely the best known case approaching something like this is the Golgi stain. Even today, it is often said that the mechanism underlying the Golgi method is largely unknown. Nevertheless, this did not prevent Camillo Golgi and Santiago Ramon y Cajal from making great strides in late 19th and early 20th Century neuroanatomy. What holds for psychological functions, seem to hold for functions generally: one can develop and apply procedures for such things as hardening brain tissue, slicing it with a microtome, and exposing it to a series of different solutions for different periods of time, without knowledge of the functions of the tissue so treated. It simply does not appear to be the case that knowledge of function, any kind of function, is essential to brain mapping.

1.1.2 Functional Localization. The third section of Bechtel and Mundale's paper is likely the most problematic. To begin with, a more apt name for the section might have been "Functional Localization" or "Functional Localization: Deficits, Stimulation, and Neuroimaging," since what ties together the appeal to studies of brain lesion, electrical stimulation of the brain, and neuroimaging for Bechtel and Mundale is the fact that these are methods for localizing cognitive functions. They are methods for trying to determine where in the brain particular psychological functions are taking place.

Yet, once we notice that the section is about functional localization, we can see how Bechtel and Mundale could have come to the erroneous second premise, that all brain mapping makes essential use of psychological function. Conflate, for a moment, the task of mapping

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Shepherd, Zhang, Knott, and Svoboda, (2005), Trachtenberg, Chen, Knott, Feng, Sanes, Welker, & Svoboda, K. (2002), and Zuo, Lin, Chang, & Gan, (2005), use this and related techniques.

brain regions and the task of localizing the regions of the brain where particular functions are carried out. Clearly, psychological functions are essential to the project of functional localization. So, with the conflation in place, psychological functions must be essential to the project of brain mapping.

It is important to notice that the third section of the paper is concerned with functional localization, since this helps make it clear how functional localization is orthogonal to the issue of multiple realization.[7] The attempt to localize a psychological function, such as vision, is the attempt to find a particular portion of the brain that more or less regularly carries out that function. Over the past two hundred years or so, a variety of methods have been dedicated to discerning such possible localizations. Phrenology, of course, attempted to use protuberances on the skull as an indicator of enlarged underlying brain regions, which were in turn indicators of more developed cognitive abilities, which in turn indicated the locus of certain cognitive mechanisms or processes. Studies of naturally occurring brain injuries, such as those suffered by Phinneas Gage or Paul Broca's patients, attempted to correlate damage to particular regions of the brain with loss of particular cognitive functions. Wilder Penfield, among others, stimulated the exposed surface of the brain in an attempt to correlate the locations of the stimulations with particular psychological effects. Much more recently, positron emission tomography (PET) and functional magnetic resonance imaging (fMRI) have used biological indicators of activity in an attempt to ascertain which regions of the brain are active when specific psychological function are being carried out.

Suppose, now, simply for the sake of argument, that a study of the functional localization

[7] In *challenging* the multiple realization of psychological states and processes, Shagrir, recognizes that the multiple localization does not support multiple realization in the following: "Thus, *psychological states and processes could be realized in different areas of the brain without being multiply realizable*" (Shagrir, 1998, p. 448). The point to be made here in reply to Bechtel and Mundale is that unique localization does not support unique realization.

of some cognitive function is "successful" in locating a portion of the brain where a specific cognitive function always occurs in a given species. That would show that the cognitive function in question is "uniquely localized" in that region of the brain. But, what does unique or multiple localization of a psychological function have to do with unique or multiple realization? Very roughly speaking, unique localization means something like always occurring in the same place, unique realization means something like always constructed in the same manner. They are entirely separate distinctions. To see this, consider a very simple example. Suppose that automobile engines are multiply realized by having some that use fuel injection where others use carburetors, some have pistons where others have a rotary valve, some are water cooled where others are air cooled, some have variable valve timing where others do not. Such multiply realized engines might be found only in the front of the car, giving rise to the unique localization of multiply realized engines. Of course, in the actual world, automobile engines are multiply realized and multiply localized. Some cars have engines in the front, others have them in the back. And, of course, a world in which automobile engines are uniquely localized and uniquely realized might be one in which there is only one automobile manufacturer who makes only one type of engine and always puts it in the front of the car.

The simple example of automobile engines reinforces our understanding of how Bechtel and Mundale could be right in maintaining that psychological functions are uniquely localized, but nevertheless wrong to deny that they are multiply realized. Suppose, contrary to fact and merely for the sake of argument, that all human brains are of exactly the same size, down to the last neuron and glial cell, if necessary, and that every human brain has a region of primary visual cortex that is exactly the same size and is found in exactly the same position within the brain. This unique localization of a given psychological function within a single region of the brain

does not show that within that region, neurons or glial cells or some other structures are not configured in a diversity of ways that lead to many distinct ways to realize the given psychological function. In other words, unique localization of a given psychological function does not entail the unique realization of that function.

The objection that Bechtel and Mundale are conflating uniqueness of localization and uniqueness of realization is indicated, in part, by the fact that much of the work they describe is frequently thought of as trying to determine functional localization. It is also, however, indicated by the following compressed argument concerning neuroscientific practice: "Nevertheless, it is important to note that in interpreting these deficits, researchers implicitly reject multiple realization among human brains and assume that damage to a brain area in anyone will result in a deficit to a particular cognitive function that is performed by that area in undamaged brains." (Bechtel & Mundale, 1999, p. 184). What is the argument here? In the first part of the sentence, we are told that researchers implicitly reject multiple realization. Why do they reject it? Because researchers assume, roughly, that if damage to a region leads to an impairment of cognitive function F in one person, then that same region in another person is responsible for F. Thus, Bechtel and Mundale attribute to neuroscientists an inference from the conclusion that particular cognitive functions are uniquely localized to the further conclusion that they are uniquely realized. Bechtel and Mundale, thus, tacitly attribute to neuroscientists the principle that all cognitive functions that are uniquely localized are uniquely realized. This tacit assumption, however, is false. Uniqueness of localization does not entail uniqueness of realization. So, Bechtel and Mundale attribute a fallacious argument to neuroscientists. So, even if Bechtel and Mundale are correct in their description of the sociology of neuroscience and that neuroscientists are indeed drawing a fallacious inference, there is no need for us to follow the

fallacious neuroscientific reasoning.

The core of the foregoing objection to the move from unique localization to unique realization is based on the idea that uniqueness of localization does not logically entail uniqueness of realization. It might be suggested, however, that Bechtel and Mundale do not have to assume that there is a logical entailment relation between uniqueness of localization and uniqueness of realization. Instead, it could be enough if there were a well-confirmed empirical generalization to the effect that any psychological function F that is uniquely localized is very likely to be uniquely realized. On this reading, Bechtel and Mundale would not run afoul of an objection from the philosopher's armchair.[8] Nevertheless, there would remain the problem of the confirmation of this empirical generalization. What reason is there to believe that many or most uniquely localized psychological functions are also uniquely realized? Bechtel and Mundale provide none. Moreover, trying to confirm this principle, say, by enumerative induction of cases of psychological functions that are both uniquely localized and uniquely realized would seem to require independent determination of the unique realization of given psychological functions. But, in that case, one would hardly need the argument from univocal localization to unique realization. One could simply display the uniquely realized psychological functions. So, even if we revise our interpretation of Bechtel and Mundale's tacit premise regarding the relationship between unique localization and unique realization, this still leaves the premise without empirical support.

To conclude this discussion of functional localization, we need to consider some comments that Bechtel and Mundale make regarding neuroimaging.[9] Bechtel and Mundale, first, note that because individual brains differ in size and shape, it is common to map subject brains

[8] Alex Morgan proposed something like this in correspondence.
[9] See, Bechtel & Mundale, 1999, p. 190.

onto a single standard brain defined in the Talairach and Tournoux, (1988), atlas. The basic idea of this normalization method is to select various points on a subject brain, such as the anterior commissure, posterior commissure, the highest point of the brain, the lowest point of the brain, etc, then map these points onto the corresponding points on the standard brain. The remaining intermediate portions of the subject's brain are then aligned so to fit on the remaining intermediate points of the standard brain. In other words, an image of a subject's brain is stretched or squeezed so as to fit on an image of the standard brain. Although Bechtel and Mundale do not explicitly state how they take the facts regarding normalization to bear on the issue of multiple realization, we can see that this data manipulation technique merely throws away information about the differences among individual subjects. It merely discards information that may, or may not, be relevant to distinct realizations of psychological functions. It does not, therefore, show that there are no differences among the subjects. It does not show that there is no multiple realization of psychological functions. In other words, while Bechtel and Mundale seem to think that this feature of the practice of neuroimaging challenges the *existence* of multiple realization of psychological functions, it seems perfectly plausible to suppose that this feature of neuroscientific practice merely undermines the chances of *finding* multiple realization.

Indeed, the second feature of neuroimaging to which Bechtel and Mundale draw our attention has exactly the same shortcoming. They write, "Second, in PET especially, but also in [f]MRI, the signal to noise ratio is relatively low, requiring averaging across subjects. Averaging will cancel out individual differences" Bechtel & Mundale, 1999, p. 190). Again we have a data manipulation technique that discards information about individual differences. The technique, therefore, may only mask differences among realizations, rather than show that there are no

differences in realizations. Thus, in both normalizing and averaging, we have techniques of which we might say that the possibility of *finding* multiple realization is restricted at the outset, but not necessarily techniques in which the possibility of multiple realization is restricted at the outset.

Following the two foregoing observations, Bechtel and Mundale claim that "thus, the fact that any results at all survive averaging as well as transformation onto a common brain plan indicate a great deal of commonality. This alone suggests much less variability than the multiple realizability arguments would allow" (op. cit.). This comment is odd. If one applies techniques that mask differences in support the discovery of commonality, it should not be all that surprising that some commonalities survive. More importantly, we need to consider more closely what constitutes this "great deal of commonality". For simplicity, let us consider just blood-oxygen level dependent (BOLD) fMRI at first. What we find in this case will carry over in relatively straightforward ways to other neuroimaging techniques. In this form of fMRI, one supposes that during a particular cognitive task, certain neurons fire and that they consume oxygen in virtue of this firing. This local decrease in oxygen is then thought to trigger a hemodynamic response in which more blood is carried to the region. When hemoglobin releases oxygen to tissues, it shifts from its oxygenated to its deoxygenated form and changes some of its magnetic properties. It is these changes in magnetic properties that are detected by fMRI. Thus, the short of the matter is that BOLD fMRI is thought to measure local changes in blood oxygenation. So, fMRI shows that distinct brains regions have something in common, namely, sameness in changes in blood oxygenation.

That is a thumbnail account of the relevant science, so what is its philosophical import? This sameness in changes in blood oxygenation is far from showing that these brain regions

constitute a unique type of realization for a given psychological function. Commonality of metabolic activity is far from showing commonality of realization. In principle, many distinct realizations could make the same demands on the oxygenation of the brain. It could be, for example, that there are two distinct neural circuits—collections of neurons synapsing on each other in radically distinct patterns—that constitute distinct realizations of a given psychological function, but that contain very nearly the same number of neurons. Such circuits could make the same demands on blood oxygen levels. Or, they could make demands that are so close to each other that current fMRI technology cannot discriminate them. Alternatively, it could be that there are two neural circuits that differ in number and wiring pattern, but such that the larger circuit does not require as much activity in its entirety as does the smaller circuit, so that again they make comparable metabolic demands on the brain's blood supply. Consider once again an analogy involving cars. Suppose one had an automobile in which one could interchange engines A and B. Next suppose that one finds that both engines consumed the same amount of fuel in powering a vehicle over a number of different courses. Would the finding that these two engines had the same fuel economy rating show that they are univocal realizations of the function of being an automobile engine? Hardly. Two distinct engine designs, two distinct kinds of realizations of the function of powering an automobile, could easily have the same fuel economy ratings.

What holds for neuroimaging by fMRI holds *mutatis mutandis* for PET. Stated very simply, in the case of PET, what is common to the active brain regions across subjects is a high level of positron emission. These positron emissions result from the radioactive decay of various sorts of tracer materials. So, does the fact that the respective portions of the averaged, normalized brain in two distinct individual humans have the same levels of positron emission

under some set of test conditions show that the regions in the averaged subjects constitute but a single realization of the function under test? No. It is possible that the various brain regions in the two individuals have distinct mechanisms for realizing the psychological function under test, but nonetheless display the same levels of positron emission.

Notice that this is a problem for the use of neuroimaging to detect multiple realizations, even apart from the confounding influences normalizing and averaging. Even if all human brains were the same size and shape and responded identically under a given test protocol, neuroimaging would still not provide decisive evidence for univocal realization. To speak simply, sameness of BOLD signal and sameness of positron emission is not sufficient to establish sameness of realization. To make the point more generally, the idea is that a given test method may fail to find certain differences in possible realizations. When a given test method fails to find a different realizations, one moral to draw is that there is only one realization to be found. Another, however, is that the test method is not discriminating enough to find certain kinds of distinct realizations, even if they are there. At this point in time, the conclusion that Bechtel and Mundale wish us to draw on the basis of neuroimaging results is premature.

So, to recount, we have found three principal problems in Bechtel and Mundale's use of research on functional localization to challenge the putative multiple realization of psychological functions. In the first place, they apparently conflate brain mapping with the task of functional localization. If this is what is going on, we can see why they hold the erroneous view, earlier rejected in section 1.1.1, that an appeal to psychological function is essential to brain mapping. If they are thinking of functional localization as brain mapping, then since functional localization does make essential use of psychological function, then of course brain mapping makes essential use of psychological function. Second, they apparently conflate unique localization of a

psychological function with unique realization, but, to put the matter informally, unique localization is mere sameness of positioning, where unique realization is sameness of design or construction method. As yet, they have no viable account to connect unique localization, even where it may exist, to unique realization. Third, Bechtel and Mundale seem to misinterpret the implications of normalization and averaging of neuroimaging data for the hypothesis of multiple realization. These techniques do not challenge the existence of multiple realization; they are evidently not designed to support its discovery. Finally, the great deal of commonality found in many neuroimaging experiments is insufficient as of yet, to establish unique realizations. Simply producing the same BOLD signal or same pattern of positron emission in a given experiment does not provide very weighty evidence for the hypothesis that a given psychological function is uniquely realized.

1.2 A Critique of Premise 1

Turn, finally, to the major premise in Bechtel and Mundale's Central Argument. This is the claim that if psychological functions were multiply realized, then brain mapping would have to be carried out independently of psychological function and without comparative evaluation across species. In contrast to the other two premises, Bechtel and Mundale provide no reason to accept the major premise nor any explanation of why such a premise might represent a consequence of the multiple realization of psychological functions. They apparently assume that it is so obvious that it merits neither expository comments nor any justification. In truth, however, there is good reason to think it is false.

To begin with the problem at an intuitive level, think about how such a major premise might be applied to screwdrivers:

> If screwdrivers were multiply realized, then taxonomy of screwdriver realizations would have to be carried out independently of function and without comparative evaluation across types.

Surely, screwdrivers are multiply realized, yet this does not prevent our categorizing them by their function. Nor does this prevent our identifying their parts by their function. Screwdrivers are regularly classified by the kinds of screws they drive, as in slot head, Phillips head, and Robertson drive. It is also trivial to identify a part of a screwdriver as its handle. The major premise formulated for screwdrivers seems downright bizarre. In addition, the fact that screwdrivers are multiply realized does not block our comparative evaluation of their realizations across types. Screwdriver realizations have standard sizes, but comparisons of their weight, density, color, and material composition would seem not to be precluded by their being multiply realized. And, of course, similar considerations apply to hammers. Tack hammers, finishing hammers, ball pein hammers, brick hammers, and dry wall hammers are functional classifications of hammer realizations. It is also trivial to delimit certain portions of hammers as handles, other parts as heads, and other parts as nail pullers. Hammers can also be classified in terms of weight and materials. Similar considerations seem to hold for Shapiro's, (2004), favorite example, corkscrews. In short, simple functional artifacts appear to confound Bechtel and Mundale's major premise.

Nor does the major premise seem plausible when applied to neuroscience and psychology. Suppose, just for the sake of argument, that the brain has a single multiply realizable cognitive function of thinking. Why would this prevent the development of a neuroanatomy independent of psychological functions? Consider the photograph of a medial sagittal view of a human brain shown in Figure 2. Is it really plausible to suppose that if

psychological functions were multiply realized, then we would be unable to map the these structures without psychology? If psychological functions were multiply realized and we had no psychology, would we really be unable to discover the sulci, the gyri, the cerebellum, the corpus callosum, the globus pallidus, the white matter of the brain, the grey matter, the superior colliculus, or the striate cortex? Of course, there are cases where knowledge of psychological function influences neuroanatomy. Neuroscientists wouldn't call the optic nerve the "optic nerve" or the optic chiasm the "optic chiasm," if they didn't know the function of the eyes and those nerves. But, even in this case, they surely would have noticed the existence of these anatomical structures without the psychological theory. Why would multiple realization, in the absence of any theory of psychological function, prevent anyone from noticing and labeling anatomical features of the brain?

Further, why would multiple realization preclude an inspection and comparison of the neuroanatomy of, say, chimpanzees and humans? Surely neuroscientists could discover that there are some neuroanatomical similarities between human brains and chimpanzee brains and that there are some neuroanatomical differences between human brains and chimpanzee brains. Surely multiple realization cannot preclude comparisons of the sulci and gyri, comparisons of the cerebellum and corpus callosum, the superior colliculus and the striate cortex. Surely, multiple realization should allow comparisons of brain weights, cortical thicknesses, neuronal cell counts, glial cells counts, neuronal cell densities, and the existence of conduction pathways.

At this point, one might object that we are examining too strong an interpretation of Bechtel and Mundale's premise. Recall Bechtel and Mundale's claim that "Our primary concern, rather, is with the implication drawn from the multiple realizability argument that information about the brain is of little or no relevance to understanding psychological

processes" (Bechtel & Mundale, 1999, p. 176), and "Our efforts have been aimed at discrediting the claim that cognitive states are multiply realized in any way that discounts the relevance of neuroscience to our understanding of cognitive processes" (ibid., p. 201). The way to understand the first premise is as containing an implicit restriction to only a special kind of multiple realization, call it "Ghostly Multiple Realization".[10] If psychological properties were multiply realized *in a ghostly way*, then brain mapping would have to be carried out independently of psychological function and without comparative evaluation across species. One could then take the major premise to be a tautology. This special kind of ghostly multiple realization would just be defined as the kind of multiple realization that would require that brain mapping would have to be carried out independently of psychological function and without comparative evaluation across species. Maybe one could fault Bechtel and Mundale for not being more explicit in their exposition of this kind of multiple realization, but one could easily see why they would not feel the need to justify the premise. The premise is, again, just a tautology, hence not in need of justification.

Here we have a strategy for saving the truth of the premise, but there is a cost. The truth appears to be purchased at the price of irrelevance. As we noted above, this kind of Ghostly Multiple Realization does not even apply to ordinary functional artifacts such as hand tools. Even screwdrivers, hammers, and corkscrews are not multiply realized in this ghostly way. Further, there appears to be little textual evidence for supposing that those who believe in the multiple realization of psychological functions, philosophers such as Putnam, Fodor, and Block, believe in any kind of ghostly multiple realization, much less the specific form of ghostly multiple realization identified in this premise. Bechtel and Mundale claim that "Some advocates

[10] See Aizawa & Gillett, (Unpublished-a), for further explanation of this terminology and criticism of the view.

of multiple realizability, such as Fodor, (1975), have explicitly contended that, in fact, neural studies have not and will not enhance psychological understanding" (op. cit, p. 178). Yet, it is not clear what in Fodor, (1975), they think constitutes this contention.[11] Moreover, Fodor, (1968), pp. 107-11, appears to flatly contradict ghostly multiple realization in favor of something like what Patricia Churchland has called a "co-evolutionary research strategy." At one point, he writes,

> On the one hand, it is clear that a psychological theory that attributes to an organism a state or process that the organism has no physiological mechanisms capable of realizing is ipso facto incorrect. If memory is a matter of forming traces, then there must be subsystems of the nervous system that are capable of going from one steady state to another and that are capable of remaining in the relevant state for periods that are at least comparable to known retention periods. If no such mechanisms exist, then the trace is the wrong model for the functional organization of memory.
>
> On the other hand, the relevant notion of a neurological subsystem is that of a biochemical mechanism who operations can correspond to some state or process that is postulated by a satisfactory psychological theory (Fodor, 1968, p. 110).

In addition, in describing the computer model of the mind, Block, (1990), also appears to reject ghostly multiple realization:

> As far as the computer model goes, it does not matter whether our [computer logic] gates are realized in gray matter (which is actually gray only when preserved in a bottle), switches, or cats and mice.
>
> Of course, this is not to say that the computer model is in any way incompatible with a biological approach. Indeed, cooperation between the biological and computational approach is vital to *discovering* the program of the brain. Suppose one were presented with a compute of alien design and set the problem of ascertaining its program by any means possible. Only a fool would choose to ignore information to be gained by opening the computer up to see how its circuits work.[12]

Here Block is apparently offering a gloss on Putnam's famous dictum, "We could be made of

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[11] Keeley, (2000), has a comparably vague attribution to Fodor, (1974): "In his 1974 paper… Fodor argues from the multiple realizability of functionally defined generalizations to the theoretical independence of those sciences that make use of functionalist theories… For the sake of brevity, I will refer to this argument as the multiply realization thesis..." (Keeley (2000), p. 446). It is unclear exactly what Keeley is referring to here.

[12] Thanks to Carl Gillett for bringing the Block passage to my attention, and in fact for bringing the Fodor passage to my attention in an earlier paper we worked on.

Swiss cheese and it wouldn't matter" (Putnam 1975a, p.291), and that gloss is not an endorsement of ghostly multiple realization.

The upshot, therefore, is that if we interpret Bechtel and Mundale to be putting forth a challenge to multiple realization *simpliciter*, then it appears that the major premise is simply false. If, instead, we interpret Bechtel and Mundale to be challenging ghostly multiple realization, then they are missing what appears to be a more important target.

2. Bechtel and Mundale's "Grain Matching Claim"

In section 5 of their paper, having concluded that the multiple realization of psychological functions is false, Bechtel and Mundale offer a diagnosis of why the hypothesis that psychological states *are* multiply realized nonetheless looked plausible.[13] They propose that multiple realization looked plausible because philosophers had played fast and loose in the individuation of psychological and neuroscientific states. Philosophers supporting multiple realization individuated psychological states, such as hunger, very coarsely, but neuroscientific states very finely. By doing this, it should be no surprise to find that there are many neuroscientific states underlying any given coarsely individuated psychological state. So, multiple realization is the natural outcome.

Bechtel and Mundale's diagnosis may, or may not, be the correct intellectual history of the plausibility of multiple realization, but it does not provide an argument against the multiple realization of psychological states. So, we shall set that point aside here. There are, however, other comments in this section that constitute what we might call the "grain matching claim" for the univocal realization of psychological states. The claim is that for every psychological state,

[13] In the final section of their paper, Bechtel and Mundale switch to an ontology of states, so we will follow them in this.

there exists a unique neuroscientific state that realizes it. Given a psychological state of a given grain, finding a neuroscientific state that uniquely realizes that psychological state is merely a matter of directing one's attention. This is a bold claim. And it is a claim, since no argument is given in its support. Indeed, it is so bold that one might naturally doubt that this could be what Bechtel and Mundale wish to assert. Yet, a careful reading of the following passage supports this view:

> One can adopt either a coarse or a fine grain, but as long as one uses a comparable grain on both the brain side 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 (Bechtel & Mundale, 1999, p. 202).

To begin to substantiate the foregoing interpretation, consider the state of being hungry. The picture seems to be that we have something like the following set of grains for hunger: Hunger, human hunger, Jerry Fodor hunger, and Jerry Fodor on May 5, 2007 at 12:00 p.m. EST hunger. What commits Bechtel and Mundale to these properties? To begin with, they note that "Yet, at anything less than a very abstract level, hunger is different in octopi and in humans" (ibid.). So, the grain corresponding simply to hunger would apparently be this very abstract level. What about "human hunger"? Given that there is a difference between octopus hunger and human hunger, we need a grain for that. The category "human hunger" fits this bill.[14] Do they mean that there is a grain as fine as Fodor's hunger? Do they mean that there is a grain as fine as

[14] This choice is not, strictly speaking, forced. It is possible that Bechtel and Mundale have in mind instead (or in addition) something like a "class grain" at which octopus and human hunger differ, so that there would be cephalopod hunger and mammalian hunger. Or they could have in mind instead (or in addition) an "order grain" at which octopus and human hunger differ, so that there would be octopoda hunger and primate hunger. Nothing of philosophical significance, however, seems to turn on this detail of their picture.

Fodor at time t hunger? Apparently so. They say "one can adopt a very fine grain, and differentiate between individuals." This seems to commit them to Fodor's (or at least some individual person's) type of hunger. And, in fact, they write, "one can adopt a very fine grain, and differentiate between individuals, or even in the same individual over time." This seems to commit them to Jerry Fodor on May 5, 2007 at 12:00 p.m. EST hunger.

The interpretation of what Bechtel and Mundale have in mind regarding the psychological grains is relatively straightforward, but the supposed "brain grains" are less clear. How are we to explicate the metaphor of "grains" when it comes to properties of the brain? Polger, (2004), implicitly identifies Bechtel and Mundale's concept of brain grains with levels.[15] But, levels do not appear to be the same things as grains. All of our psychological functions mentioned above appear to be at a single level, a psychological level. They appear to be related as determinable-determinate or perhaps genus and species. Human hunger may, thus, be interpreted as a species or determinate of hunger. Fodor hunger might, then, be a further species or determinate of human hunger. Levels, by contrast, may informally be aligned in terms of part-whole relations or the sizes of the individuals at those levels. So, neurons are at a lower level than the brain, since brains have neurons as parts and neurons are smaller than brains. Protein molecules are at a lower level than neurons, since neurons have protein molecules as parts and protein molecules are smaller than neurons.

Rather than understanding coarser and finer "brain grains" in terms of a familiar notion of higher and lower levels, Bechtel and Mundale mention "activity in brain areas." But, how exactly are we to come up with different brain activities corresponding to hunger, human hunger,

[15] Polger, 2004, p. 69, p. 188. In the following passage, however, Polger might be taken to be treating Bechtel and Mundale's psychological grains as related by determinable-determinate or genus-species: "The taste of chocolate is presumably a finer-grained sensation than the taste of sweetness, for example" (Polger, 2004, p. 22).

Jerry Fodor hunger, and Jerry Fodor on May 5, 2007 at 12:00 p.m. EST hunger? What are these activities and how do we know that they uniquely realize these psychological states? Bechtel and Mundale do not explain this.

Setting aside the sketchiness of the claim, it sits poorly with the rest of their paper. Part of the rhetoric of Bechtel and Mundale's paper is that if we look more closely at the actual scientific practices of neuroscientists, we will find that they are committed to the univocal realization of psychological functions. But, actual neuroscientific practice does not seem to involve explorations of psychological states such as Fodor's hunger or Fodor's hunger at time t. Neuroscientists do not index or relativize brain activities to individual organisms or individual organisms at a particular time. Such notions are the work of philosophers. Moreover, the sweeping claim that for every psychological state, there is a corresponding neuroscientific state of an appropriate grain that realizes it, sounds like it is a purely logical, *a priori* assertion. Yet, one might have thought that it is an empirical question whether or not a given psychological state is uniquely or multiply realized. If each of these grains were to have scientifically established realizers, then one might expect references to journal articles detailing what those realizers are for specific psychological states. By contrast, the assertion seems to be that finding a univocal realization is as easy as turning one's attention to the appropriate thing. This kind of approach, however, is entirely contrary to the spirit of the rest of their paper that is aimed at developing a scientifically informed assessment of the hypothesis that psychological properties are multiply realized.[16]

When the "grain matching" claim is spelled out in detail, one wonders how Bechtel and Mundale could hold this view. But, there is, I think, an answer. There seem to me to be some

[16] Some will surely doubt that the foregoing is a correct interpretation of Bechtel and Mundale's claim, but if the foregoing is not the correct interpretation, then what is? What *are* they claiming here?

enabling factors. In the first place, they do not articulate a theory of realization. This enables them to talk rather loosely of there being a "systematic mapping" between psychological states and neuroscientific states. But, realization is not just any systematic mapping between these properties. Instead, it appears to be something like a non-causal, non-logical determination relation of some sort. Gillett, (2002), and Shapiro, (2004), provide theories of realization along these lines, but defending or maintaining either of these views is not essential to recognizing the omission on Bechtel and Mundale's part. In the second place, recall that Bechtel and Mundale contend that brain regions are essentially delimited by psychological functions.[17] Now, suppose that a given portion of the brain X is defined as the region that performs function F. This gives rise, then, to an obvious systematic, and roughly *a priori*, "realization" relation between the psychological function F and brain region X. What realizes F? Activity in brain region X. How do we now this? Brain region X is simply defined as the region of the brain that performs function F. It is simply defined as the region whose activity brings about F. So, this apparently boils down to the idea that F is uniquely realized by the activity in the brain region that performs F. If realization is just any systematic mapping between something identified in psychology and something identified in neuroscience, then one can be sure to have this systematic mapping. But, this purely logical statement of unique realization, that F is uniquely realized by the activity that brings it about, is mere logical trickery. So, it appears that a lack of a theory of realization, along with an overly strong assumption about brain taxonomy, has lead Bechtel and Mundale to an implausible method of finding univocal realizations.[18]

But, suppose we reject the idea of "made to order" unique lower level realizers. Suppose we also reject the bold claim that for any psychological function, there is a unique brain state or

[17] Bechtel and Mundale, (1999), p. 177.
[18] For further criticism of the "grain matching claim," see Aizawa & Gillett, (forthcoming).

activity that realizes that function. One might think that there is still some kernel of truth to which Bechtel and Mundale are pointing. It may, for example, seem intuitively plausible to suppose that it is easier to find multiple realization if one begins with a coarse-grained higher level property, rather than with a relatively finer-grained higher level property. What of this possibility?[19] In the first place, one might concede that finer psychological states or functions are less likely to be multiply realized than are coarser states or functions, without thereby conceding that narrow psychological states or functions are uniquely realized. How often the finer or finest functions or properties are uniquely realized is a question this weaker principle leaves open. So, the weaker principle buys its plausibility at the price of not challenging multiple realization. In addition to this purely logical point, there is a reason we should not be hasty in accepting this intuition or assumption. Consider a relatively coarse-grained psychological function, such as making normal human color discriminations on all the Ishihara color plates, and a relatively finer grained psychological function, such as making normal human color discriminations on some proper subset of the Ishihara color plates.[20] The capacity to make the correct discriminations on all the plates requires normal human trichromacy, where the capacity to make the normal discriminations on certain proper subsets of the plates can be accomplished with normal human trichromacy or some forms of dichromacy.[21] Insofar as trichromacy and certain forms of dichromacy are two ways of realizing the function of making certain visual color discriminations, we have more distinct realizations with a finer category than with a coarser category. Presented more generally, the intuition or assumption under consideration may have

[19] Larry Shapiro seems to have some such idea in mind in an unpublished manuscript, "Flat and Happy."

[20] The argument being made here does not depend on an appeal to the details of the Ishihara test for color deficiencies. It would be run in terms of, say, normal human color discrimination versus discrimination of a subset of colors, such as some blues.

[21] Appealing to trichromacy versus some form of dichromacy is not necessary to make the foregoing point. One can instead appeal to the various combinations of cone opsins.

this problem going against it. It could be that a more coarse-grained function or property may impose more or tighter constraints than does a more fine-grained function or property. If so, then it can turn out that there are more ways to meet the fewer or weaker constraints than there are to meet the more numerous or tighter constraints. So, appealing to coarse-grained functions need not make multiple realization more likely. The example of the Ishihara plates is meant to illustrate this possibility.

In personal correspondence, Shapiro has offered the following kind of reply to the foregoing. Consider the function of keeping time with an accuracy of one hundredth of a second per day versus the function of keeping time with an accuracy of one thousandth of a second per day. The former function is a coarser function than the latter function. Should we not expect that make a watch having the latter function would require greater precision—lower tolerances—in the machining of its parts? In such a case, we do apparently find that finer categories do impose tighter constraints on realizations. Perhaps this is correct. Perhaps there are cases in which finer functions have tighter constraints. And perhaps those cases are those in which the dimension of coarseness versus fineness in measured in terms of the precision with which the function is to be carried out. Nevertheless, Bechtel and Mundale, and Shapiro following them, suppose that memory and hunger are coarser categories than human memory and human hunger, and this kind of coarseness does not seem to be captured by the idea of coarseness versus fineness scaled along a dimension of precision. It is not as though memory and human memory differ only insofar as human memory is a more precise form of memory. So, the original point seems to stand. We cannot simply assume that speaking of memory, rather than human memory, facilitates the discovery of multiple realizations. This claim would appear to be an empirical claim on which the evidence is currently out. More generally, we cannot simply assume that

more coarsely individuated functions are more likely to be multiply realized than are more finely individuated functions. Further empirical, and perhaps conceptual, work needs to be done on this topic.

4. Conclusion

The aim of this paper was not to make a positive case for the multiple realization of psychological properties. Instead, it was to clear away some of Bechtel and Mundale's principal objections to such multiple realization. To this end, we have seen that Bechtel and Mundale make an unsupported and, indeed, implausible assumption regarding the consequences of the hypothesis that psychological properties are multiply realized. They suppose that if psychological states, properties, and processes were multiply realized, then brain taxonomy would have to be carried out independently of psychological function and without comparative evaluation across species. Second, Bechtel and Mundale do not notice a distinction between claims about (multiple/unique) realization and claims about (multiple/unique) localization. Roughly speaking, realization concerns how things are put together; localization concerns where things are placed. Thus, they mistake evidence in support of unique localization for evidence of unique realization. Third, they misrepresent the nature of brain mapping. Neuroanatomical approaches, such as those relying on cytoarchitectonics, do not make essential use of an appeal to psychological function. Fourth, they are mistaken about the implications of normalization and averaging techniques in neuroimaging. Fifth, they overrate the significance of the commonalities found in brain imaging. While one can concede that neuroimaging discovers certain identities in brain regions, identities such as sameness of blood deoxygenation, such identities fall short of establishing identities of realization bases. Sixth, and finally, Bechtel and Mundale's path to

unique realization by way of "grain matching" fails to do justice to the nature of the realization relation. Realization is not just some one-to-one mapping between lower level states and higher level states. Nor is it a mere logical relation between lower level states and higher level states. What realizes what must be discovered through empirical investigation. And, if the Gillett-Aizawa project is on the right track, it is a non-logical, non-causal determination relation that is to be determined by empirical investigation. The state of play on that investigation, however, is the topic of other of our works.

References

Aizawa, K. 2007: "The Biochemistry of Memory Consolidation: A Model System for the Philosophy of Mind." *Synthese,* Vol. 1155, pp. 65-98.

Aizawa, K. and Gillett, C. (Unpublished-a): "Multiple Realization and Methodology in Neuroscience and Psychology."

Aizawa, K. and Gillett, C. (Unpublished-b): "The (Multiple) Realization of Psychological and Other Properties in the Sciences."

Aizawa, K. and Gillett, C. (Forthcoming). "Levels, Individual Variation and Massive Multiple Realization in Neurobiology." In Bickle, J. (ed.) *Oxford Handbook of Philosophy and Neuroscience*.

Allen, J. S., Damasio, H., and Grabowski, T. J. 2002: "Normal Neuroanatomical Variation in the Human Brain: An MRI-Volumetric Study." *American Journal of Physical Anthropology*, Vol. 118, pp. 341-358.

Bechtel, W. and Mundale, J. 1999: "Revisiting Multiple Realization." *Philosophy of Science*, vol. 66, pp.175-205.

Bickle, J. 2003: *Philosophy and Neuroscience: A Ruthlessly Reductive Account.* Boston: Kluwer.

Block, N. 1990: "The Computer Model of the Mind." In Osherson, D. & Smith, E. E. *An Invitation to Cognitive Science: Thinking*. Vol. 3. Cambridge, MA: MIT Press.

Fodor, J. 1968: *Psychological Explanation.* New York: Random House.

Fodor, J. 1974: "Special Sciences." *Synthese*, Vol. 28, pp. 97-115. Reprinted in Fodor (1981). (All references are to the reprint).

Fodor, J. 1975: *The Language of Thought*. Cambridge, MA: Harvard University Press.

Fodor, J. 1981: *Representations*. Cambridge, MA: MIT Press.

Gillett, C. 2002: "The Dimensions of Realization: A Critique of the Standard View." *Analysis*, Vol. 62, pp. 316-23.

Gillett, C. 2003: "The Metaphysics of Realization, Multiple Realizability and the Special Sciences". *Journal of Philosophy*, Vol. 100, pp.591-603.

Grutzendler, J., Kasthuri, N., & Gan, W. 2002: "Long-term dendritic spine stability in the adult cortex." *Nature,* vol. 420, pp. 812-816.

Holtmaat, A., Trachtenberg, J., Wilbrecht, L., Shepherd, G., Zhang, X., Knott, G., and Svoboda,

K. 2005: "Transient and Persistent Dendritic Spines in the Neocortex In Vivo." *Neuron,* vol. 45, pp. 279-291.

Keeley, B. 2000: "Shocking Lessons from Electric Fish: The Theory and Practice of Multiple Realization." *Philosophy of Science*, vol.67, pp.444-465.

Polger, T. 2004: *Natural Minds*. Cambridge, MA: MIT Press.

Putnam, H. 1975: "Philosophy and Our Mental Life." In Putnam, H. (ed.) *Mind, Language, and Reality: Philosophical Papers, Vol. 2*. Cambridge University Press.

Shagrir, O. 1998: "Multiple Realization, Computation and the Taxonomy of Psychological States." *Synthese*, Vol. 114, 445-461.

Shapiro, L. 2004: *The Mind Incarnate*. Cambridge, MA: MIT Press.

Stensaas, S. S., Eddington, D. K., & Dobelle, W. H. 1974: The Topography and Variability of the Primary Visual Cortex in Man. *Journal of Neurosurgery*, Vol. 40, pp. 747-755.

Tailarach, J., & Tournoux, P. 1988: *Co-Planar Stereotaxic Atlas of the Human Brain.* New York: Thieme Medical Publishers, Inc.

Trachtenberg, J., Chen, B., Knott, G., Feng, G., Sanes, J., Welker, E., & Svoboda, K. 2002: Long-Term*In Vivo* Imaging of Experience-Dependent Synaptic Plasticity in Adult Cortex. *Nature*, vol. 420, pp. 788-794.

Zuo, Y., Lin, A., Chang, P., & Gan, W. 2005: "Development of Long-Term Dendritic Spine Stability in Diverse Regions of Cerebral Cortex." *Neuron*, Vol. 46, pp. 181-189.

Figure 1. Human Fetal Brain at 8 Months. (Based on Brodmann, 1909, Fig. 20).

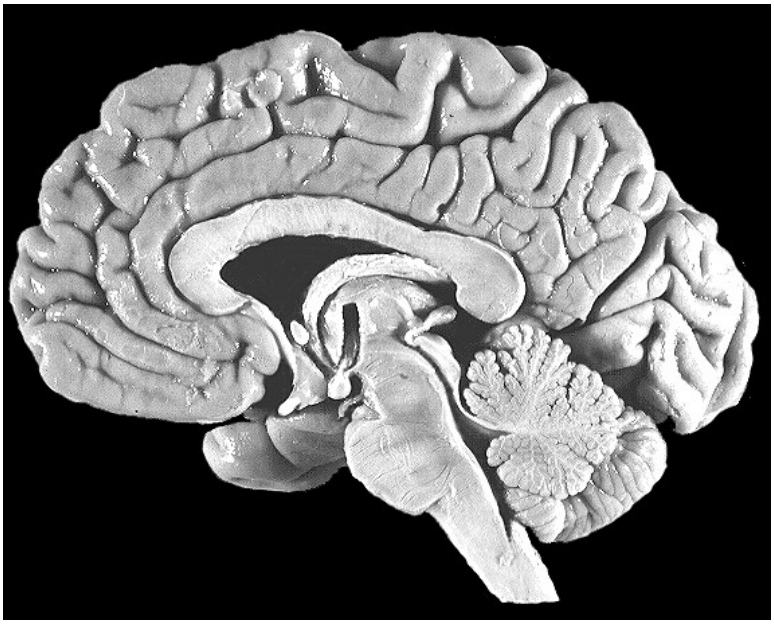

Figure 2. Saggital Section of the Human Brain.