EVALUATING THE EVIDENCE FOR MULTIPLE REALIZATION

Thomas W. Polger, University of Cincinnati
thomas.polger@uc.edu

> Consider what the brain-state theorist has to do to make good his claims. He has to specify a physical-chemical state such that *any* organism (not just a mammal) is in pain if and only if (a) it possesses a brain of suitable physical-chemical structure; and (b) its brain is in that physical-chemical state. This means that the physical-chemical state in question must be a possible state of a mammalian brain, a reptilian brain, a mollusc's brain (octopuses are mollusca, and certainly feel pain), etc. At the same time, it must *not* be a possible (physically possible) state of the brain of any physically possible creature that cannot feel pain. Even if such a state can be found, it must be nomologically certain that it will also be a state of the brain of any extraterrestrial life that may be found that will be capable of feeling pain before we can even entertain the supposition that it may *be* pain.
>
> It is not altogether impossible that such a state will be found.… But this is certainly an ambitious hypothesis. (Hilary Putnam 1967/1975: 436)

1. Of Pains and Brains

Hilary Putnam was certainly not the first philosopher to suppose that there are or may be psychological beings that do not have brains just like human brains. And it is doubtful that Putnam was the first to see that the biological diversity of psychological creatures can be made into a *prima facie* objection to the mind-brain identity theory. But Putnam did succeed in giving that objection its canonical formulation and in establishing the "multiple realization" of mental states—so the phenomenon came to be called—as the most important objection to the identity theory. The belief that mental states are multiply realized is now nearly universal among philosophers concerned with psychology and neuroscience, as is the belief that this fact decisively refutes the identity (brain state) theory. Yet the empirical support for multiple realization does not justify the confidence that has been placed in it.

Multiple realization is, at root, a thesis about similarity and difference. The hypothesis is that some things that are psychologically similar are nevertheless neuroscientifically dissimilar. Of course, nobody would suppose that every thinking thing must be exactly similar to every other thinking thing, or even exactly neurologically similar. That is, nobody would believe that there can be absolutely no neuroscientific differences among thinking things. In particular, the mind-brain identity theory is a theory of kinds or types: It says that psychological kinds are identical to neuroscientific kinds. And members of a kind need not be exactly similar to one another, they need only share the properties that are characteristic of the kinds. Thus clarified, the mind-brain identity theory asserts that all instances of a psychological kind have a neurological commonality, i.e., there is a neuroscientific kind of which they are all instances. In order for multiple realization of mental states to be an objection to the identity theory, the neurological differences among pains, to take Putnam's example, must be such as to guarantee that they are of distinct neuroscientific kinds.

Putnam seems to have thought that it was an empirical fact of some sort that the brains of mammals, reptiles, and mollusks are sufficiently different that their brains are obviously of different neuroscientific kinds. And *ipso facto* that they could not be in the same neurological state. It is unclear exactly what evidence for this claim he has in mind—the gross anatomical differences among animals of various classes, variations in their cortical development, or whatnot. To be safe, Putnam adds that even if all terrestrial thinking things have brains of the same kind, it is unlikely that extraterrestrial brains would also be of this kind and so unlikely that it is a law of nature that thinking things must have brains of a certain kind.[1] But here again one might reasonably wonder what evidence or justification can be given for such a claim.

At this point we can profit by attending to the work of Ned Block and Jerry Fodor (1972), who endorse Putnam's basic argument schema and go on to explicate the evidential grounds that they suppose must support it. Specifically, they suppose that the claim that "types of psychological states are not in correspondence with types of physical states" depends on "three kinds of empirical considerations" (1972: 160). These are:

(1) "the Lashleyan doctrine of neurological equipotentiality"

(2) "the assumption that the doctrine of Darwinian convergence applies to the phylogeny of psychology as well as the phylogeny and morphology of behavior"

(3) "the conceptual possibility that psychological predicates could apply to artifacts"

Of course the brain state theorist rejects (3) as question-begging. Insofar as it is a possibility that artifacts could have psychological states, that is simply the possibility that the brain state theory is false. We brain state theorists accept that we are fallible, but we deny that it is a prior constraint on any theory of mind that it permit that artifacts can have psychological states (Polger 2004). Admittedly, the opponent of identity theories will not let the dispute rest at this retort. But in any event, *pace* Block and Fodor (3) is plainly not an empirical reason for supposing that mental states are multiply realized or

[1] It's unusual to adduce possible future evidence in support of a hypothesis. In this case it is especially odd because if the current evidence is not forthcoming then it is entirely obscure what grounds there could be for making bets as to how future evidence would turn out. See the discussion of Block and Fodor's (3), below.

realizable. So, it's not the kind of consideration that can be invoked to support Putnam's empirical hypothesis that mental states are in fact multiply realized.

The brain state theorist also sees no Darwinian reason for expecting multiple realization, as (2) suggests. Block and Fodor seem to have in mind the fact that convergent evolution may produce similar behaviors in anatomically and phylogenetically distinct creatures—flight in birds and insects, say. Applying this idea to psychological features, they argue that evolution may produce psychological similarities in morphologically distinct creatures: "Psychological similarities across species may often reflect convergent environmental selection rather than underlying physiological similarities" (1972: 161). This argument begins by comparing psychological states to behavioral states, a theory that Block and Fodor had rejected on the previous page (1972: 160). But then it descends into the plainly question-begging assertion that "we have no particular reason to suppose that the physiology of pain in man must have much in common with the physiology of pain in phylogenetically remote species" (1972: 161). It may be that psychological similarities are frequently produced by convergent evolution rather than by common ancestry. But this will only be a reason to think that psychological similarity is frequently accompanied by physiological dissimilarity if we already have an independent reason to think that the psychological similarity and neurological similarity are dissociable—that psychological convergence is not a predictor of physiological convergence.[2] But this is just the claim under dispute.

The artificial intelligence and evolutionary arguments are non-starters. Thus it seems that the weight of Block and Fodor's explication of Putnam's argument falls on the

---

[2] Shapiro (2004) argues persuasively that empirical evidence weighs against Block and Fodor's assertion, and instead supports the hypothesis that there are mental constraints on physiological realizers.

first consideration, the doctrine of Lashleyan equipotentiality. This is the reason for thinking that "the empirical likelihood that creatures of different composition and structure, which are in no interesting sense in [type] identical physical states, can nevertheless be in [type] identical psychological states" (1972: 160).

Before we consider the Lashleyan doctrine, let us review the subsequent elaboration of the multiple realizability argument against the brain state theory. The core idea is codified in Fodor's classic, "Special Sciences, or The Disunity of Science as a Working Hypothesis" (1974). The question of whether the brain state theory is true is variably cast as a question about whether the taxonomies of psychology and the brain sciences are coextensive, whether the predicates of psychology and the brain sciences are coreferring, or whether there are "bridge laws" between psychological laws and the physical laws that govern brains. Each of these is one way of talking about the "reduction" of psychology and its laws to physical or brain science and their laws. Fodor argues that "reductionism is too strong" (1974/1980: 123). The reasons he offers for thinking so are not "knock-down reasons; they couldn't be" because "the question of whether reductionism is too strong is finally an *empirical* question" (1974/1980: 124). Nevertheless, he finds the reductionist hypothesis—the brain state hypothesis, in the case of psychology—to be unlikely:

> The reason it is unlikely that every kind corresponds to a physical kind is just that (a) interesting generalizations (e.g., counterfactual supporting generalizations) can often be made about events whose physical descriptions have nothing in common; (b) it is often the case that *whether* the physical descriptions of the events subsumed by such generalizations

have anything in common is, in an obvious sense, entirely irrelevant to the truth of the generalizations, or to their interestingness, or to their degree of confirmation, or, indeed, to any of their epistemologically important properties; and (c) the special sciences are very much in the business of formulating generalizations of this kind. (1974/1980: 124)

Now (b) and (c) are methodological or epistemic claims that have no direct consequences for the question of whether taxonomies are coextensive, predicates coreferring, or bridge laws in the offing. It seems that the point of asserting (b) and (c) is to make room for a kind of "autonomy" of the special sciences even if (a) should prove false.[3]

The central reason for doubting reductionism is (a), the claim that there are special sciences events "whose physical descriptions have nothing in common", i.e., that are multiply realized. Fodor illustrates (a) by providing the example of "Gresham's Law" in economics (viz., that bad money drives out good), and concludes:

> the preceding discussion strongly suggests that economics is not reducible to physics.… There is, I suspect, nothing peculiar about economics in this respect; the reasons why economics is unlikely to reduce to physics are paralleled by those which suggest that psychology is unlikely to reduce to neurology. (1974/1980: 125)

Those parallel reasons for doubting that psychology is reducible to the brain sciences (despite the fact that "many psychologists believe this sort of thing") are the reasons for thinking that (a) above applies to psychological kinds, predicates, or laws. That reason, according to Fodor, is:

---

[3] Putnam (1974) makes the corresponding claim using the example of square pegs and round holes.

> There are no firm data for any but the grossest correspondence between types of psychological states and types of neurological states, an it is entirely possible that the nervous system of higher organisms characteristically achieves a given psychological end by a wide variety of neurological means. It is also possible that given physiological structures subserve many different psychological functions at different times, depending on the character of the activities in which the organism is engaged. In either event, the attempt to pair neurological structures with psychological functions could expect only limited success. Physiological psychologists of the stature of Karl Lashley have held this sort of view. (1974/1980: 125)

Fodor's speculations about what is "possible" in psychology have regularly been repeated as though they were observational facts about psychology. But clearly he is not entitled to the conclusion that psychological kinds are multiply realized. Rather, at best, he has observed that the evidence for the brain state theory is inconclusive. Yet the lack of evidence is not evidence of a lack. So the evidence for multiple realization must also be inconclusive unless there is independent reason to favor the multiple realization claim.

Similarly, Fodor goes on to argue that even if psychological and neurological kinds should prove to be coextensive, the coextension "cannot be lawful" because "it is increasingly likely that there are nomologically possible systems other than organisms (viz,. automata) which satisfy the kinds predicates of psychology but which satisfy no neural predicates at all." (1974/1980: 125). But this reasoning we have already seen to be question begging.

Thus it looks like Fodor provides no reasons for doubting the brain state theory other than those that favor the "Lashleyan doctrine of equipotentiality." At last we should turn out attention to that doctrine.

2. The Lashleyan Doctrine and Neural Plasticity

As we have seen, in the "Special Sciences," Fodor seems willing to appeal to the views of Karl Lashley to support the claim of psychological multiple realizability (1974). But two years earlier, writing with Block, Fodor allowed that the appeal to Lashley should be qualified:

> the Lashleyan doctrine of equipotentiality holds that any of a wide variety of psychological functions can be served by any of a wide variety of brain structures. While the generality of this doctrine may be disputed, it does seem clear that the central nervous system is highly labile and that a given type of psychological process is in fact often associated with a variety of distinct neurological structures. (For example, it is widely known that trauma can lead to the establishment of linguistic functions in the *right* hemisphere of right-handed subjects.) But physicalism, as we have been construing it, requires that organisms are in type-identical psychological states if and only if they are in type-identical physical states. Hence if equipotentiality is true, physicalism must be false. (1972: 161).[4]

Equipotentiality was Lashley's name for the hypothesis that "structural elements are relatively unimportant." He offers this hypothesis as an alternative to the reflex-arc

[4] Here, by "physicalism" Block and Fodor refer to the "brain state" or "type identity" theory.

doctrine, according to which cognitive activities are localized to the actions of particular neurons, and to other theories that posit individual neurons and synapses as the loci of psychological phenomena. For example, with respect to the ability of rat to learn its way through a maze, Lashley summarizes his conclusions by writing, "the capacity to learn the maze is dependent upon the amount of cortical tissue and not upon its anatomical specialization" (1929: 175).

There is no doubt that Lashley's methods are comparatively crude, relying on a coarse grained oblation technique. His surgical interventions were rather non-specific, leaving the animals with "many other disturbances of behavior, which cannot be stated quantitatively but which give a picture of general inadequacy of adaptive behavior" (1929: 176). And, as general doctrines about the brain go, the Lashleyan doctrine was a dismal failure: the competing hypothesis that brain structures are psychologically salient and that there is anatomical specialization in the cortex underlie most or all current neuroscientific research.[5] And all of this was known to Fodor in 1972. So it is difficult to take seriously the suggestion that Lashley's doctrine should be thought of as evidence for multiple realization.

Moreover, even if Lashley were correct, equipotentiality is no evidence at all for the kind of variation that constitutes multiple realization. Multiple realization requires that a single psychological state kind be grounded by different neuroscientific state kinds. But according to Lashley, there is only one neurological state kind in the cortex. Equipotentiality is the claim that there is one neurological type for all psychological types, not the claim that many neurological types can underlie any one psychological

---

[5] For a methodological explanation of why this result is all but inevitable, see Bechtel and Mundale 1999.

type. So whatever the vindication of equipotentiality might show, it would not show that psychological kinds are multiply realized.[6]

The only evidence that Block and Fodor recruit to support the thesis of multiple realization is the evidence that they collect under the heading of equipotentiality. Because equipotentiality has not fared well, one is tempted to conclude that the troubles with the Lasleyan argument undermine Block and Fodor's case altogether. But I think the appeals to Lashley are misguided. If we look closely at the reasoning supplied, it seems that Block and Fodor don't in fact rely on Lashleyan equipotentiality so much as they do on the widely accepted claim that the brain is plastic and "highly labile." If it is true that "a given type of psychological process is in fact often associated with a variety of distinct neurological structures" then that would be evidence of multiple realizability. The cited example is the migration of linguistic functions from left to right hemisphere following trauma. But Block and Fodor could just as easily have cited the more mundane fact that lateralization of functions differs between right and left-handed subjects. The trigger of trauma and subsequent migration of function only serves to lend drama to the example.

Philosophers have long been impressed by plastic recovery from trauma as in the case of language cross-lateralization, and especially with the well known plasticity of sensory and motor maps in response to either use or trauma. The question is: To what extent is evidence of neural plasticity evidence for multiple realization? This question requires a two part answer. First, we must be clear about what kinds of evidence would

---

[6] Equipotentiality might be bad news for the identity theory, but it's a different kind of bad news than multiple realization.

count as evidence for multiple realization. Second, we must determine whether there is such evidence in the case of psychological states and brain states.

In recent years there has been a great deal more care about the answer to the first question. Here I will follow the "recipe" set forth by Larry Shapiro (2000, 2004, forthcoming-a, forthcoming-b, unpublished). The simple form of the recipe is that a psychological state is multiply realized when its tokens are "the same but different"—when they are of the same psychological kind but different neuroscientific kinds.[7] However not every neuroscientific difference between two psychological states will be evidence of multiple realization. The differences must be "relevant" differences, and in particular they must be neuroscientific differences in the mechanisms that are relevant to the psychological classification of the states in question. And they must be differences in type, not just variation among tokens.

Shapiro illustrates the idea by pointing out that two waiter's corkscrews that differ only in color—one is green and the other yellow, say—do not count as different realizations of *corkscrew*. This is because the differences in the corkscrews are not relevant to their being corkscrews of a certain sort—color is not one of the factors that one considers when classifying things as corkscrews. It is not one of the features that makes something a corkscrew. What factors are relevant? This is determined by the sciences in question, in this case the imaginary science of corkscrews. But the same point applies *mutatis mutandis* to psychological states. Two psychological states, such as two pains, that differ neurally only in that one is 20°C and the other is 22°C are not

[7] It is convenient to make use of the pretense that neuroscience is one monolithic theory that makes use of a single taxonomy of scientific kinds. We know this is wrong, but it simplifies matters and the pretense is harmless as long as we remember that it is a device.

multiple realizations of *pain* because temperature is not a relevant taxonomic factor for psychological states, according to the taxonomic practices of psychology as we know it.

Furthermore, as Shapiro has argued, to count as multiple realizations, two tokens must differ in ways that contribute to their similarities, not only in ways that contribute to their differences (forthcoming-a). More than being "the same but different," multiple realization only occurs when two things are "the same in different ways." Consider again the example of two corkscrews. If two corkscrews differ physically in ways that are relevant to their classification as corkscrews but their differences directly correspond to their degree of dissimilarity as corkscrews, then we do not have a case of multiple realization. If two corkscrews differ physically in ways that are relevant to their being corkscrews—in their rigidity, say—but those differences correspond to differences in their *corkscrewness*—one is a better corkscrew than the other, say, or one can open more kinds of bottles than the other—then this will not be a case of multiple realization. We don't have a case of "same but different" but instead, as Shapiro puts it, "kind of the same, but kind of different" (forthcoming-a).

Another way of understanding this idea of two things being "the same in different ways" is in terms of individual differences. Tokens of a kind do not have to be identical to one another, they only have to be the same or similar with respect to the features constitutive of the kind. So there will be individual differences among kind members, both with respect to their accidental features (relative to the kind) and with respect to their kind-essential features. If I have before me an array of corkscrews, they may differ with respect to the kind-irrelevant feature of color and (within some range) with respect to the kind-relevant feature of their ability to remove corks that resist with such-and-such force.

If Shapiro is correct, then in order to have evidence of multiple realization we need evidence that two entities A and B are (i) classified commonly by system S1, and (ii) classified distinctly by system S2. In addition, (iii) the facts about A and B that lead them to be differentially classified by S2 must be among those that lead them to be commonly classified by S1, and (iv) the relevant S2-variation between A and B must be greater than the S1 individual differences between A and B.[8] Criteria (i) and (ii) capture the "same but different" requirement—A and B are S1-ly the same and S2-ly different. Criteria (iii) and (iv) capture the "same in different ways" requirement. The facts that contribute to the S2-differences are relevant to S1-sameness, and they contribute to S1-ness in ways other than merely determining the S1-ly individual differences between A and B. If these criteria are met, then we have evidence that A and B are members of an S1-kind that is multiply realized relative to S2-kinds.

The remaining question is whether examples of neural plasticity give us evidence of this sort. Do examples of neural plasticity provide evidence that there are neurological differences among psychological states that satisfy criteria (i)-(iv)?

3. Plasticity Reconsidered

Thus far I have spoken generically of "plasticity" or "neural plasticity." But now we must drop this convenient simplification. There are many kinds of plasticity in the brain, underlying numerous normal and pathological or contrapathological responses.

[8] This quantitative criterion might leave room for coincidental satisfaction. Because we are formulating criteria for evidence rather than necessary and sufficient conditions, we can accept this risk. The evidence is therefore defeasible, as evidence is wont to be.

The main variety of plasticity that has sparked the imaginations of philosophers is called "cortical map plasticity" or "cortical representational plasticity" in the case of sensory and motor systems (Buonomano and Merzenich 1998). We can generalize the category to "cortical functional plasticity," to cover the language lateralization example as well. These are cases in which some psychological function (such as visual or tactile sensory detection, motor response, and language comprehension or production) is "mediated" by a certain anatomically or geometrically specified brain area at time t_1, and at a different brain area at time t_2. It is well known that the sensory cortexes are typically organized topographically, with adjacent cortical areas responding to adjacent sensory receptors. For example, in primates the sensory cortical areas with receptive fields corresponding to the fingers are organized in such a way. It is also known that changes to the sensory input—such as increased input due to use, or reduced input due to transection or amputation—can change the size and shape of the topographically organized cortical areas. In particular, with increased use of a digit, the corresponding cortical area may expend. And with decreased or deprived input, the cortical area will become inactive until it is invaded and co-opted by the expanding adjacent map areas (Figure 1).

INSERT FIGURE 1 HERE

Another well-studied variety of plasticity is "synaptic plasticity." This is the facilitation or inhibition of particular synaptic connections between individual neurons, as in response to their past coordination or lack thereof. That is, connections are enhanced between neurons that fire together, and inhibited between neurons that do not fire together (Figure 2). Synaptic plasticity is widely believed to be the underlying mechanism for numerous varieties of learning and memory, but significant questions remain, particularly as to the presence of the phenomena outside of laboratory conditions (Buonomano and Merzenich 1998).

INSERT FIGURE 2 HERE

There are other varieties of plasticity in the brain, but let us restrict our attention to these two for the time being. It is natural to think that cortical synaptic plasticity is the underlying mechanism for cortical representational plasticity, but in fact that explanatory link remains an open question. The issue is not whether cortical representational plasticity is the result of plastic changes in neurons and synapses, but whether it is solely plastic changes in neurons in the cortical map that mediate cortical map plasticity, or whether changes elsewhere in the brain—outside the cortical maps area in question—are also implicated (Buonomano and Merzenich 1998). Our interests here are in the more generic question. We're concerned not just about synaptic plasticity in the cortex, but more generally in the mechanisms responsible for cortical map plasticity, wherever they may be located. Each link in the chain provides an opportunity to look for evidence of multiple realization: Are psychological functions multiply realized by different cortical maps? And, are cortical maps multiply realized by different synaptic mechanisms?

It might be tempting to think that plasticity is, as it were, *by definition* a kind of multiple realization. This is plainly false in the case of synaptic plasticity—the biochemical changes in the synapses are hypothesized to mediate representation and functional changes. That is, we have a case of difference underlying difference rather than difference underlying sameness, as multiple realization requires. Moreover, although there are many ways of initiating plastic changes in synapses, "there are few data suggesting that the mechanistic underpinnings of the LTP [changes] produced by these different protocols are any different" (Buonomano and Merzenich 1998: 177).

The "by definition" interpretation is slightly more plausible in the case of cortical map plasticity, but only slightly. 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 multiple realization employ a mismatch in the granularity of psychological and neuroscientific kinds (Bechtel and Mundale 1999). 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. The identity theory—the brain state hypothesis—says that there is some such way. The advocate of multiple realization holds that there is not. And now we want to know which side is better supported by the available evidence.

Yet the evidence so far gives little solace to the advocates of multiplicity. Contrary to the "Lashleyan doctrine" even highly plastic functions do not move about the cortex in a "free range" manner. Under normal conditions, the range of plastic change is quite constrained. And even under traumatic and laboratory conditions, there are constraints on plastic changes. Digital receptive fields expand or contract to recruit adjacent map space left unused following transection, but they do not "jump" to recruit unused but non-adjacent cortical areas. Moreover, quite often the preserved function is not maintained at the same level of performance as prior to plastic change. Contrary to what is implied by philosophical appeal to "Lasleyan equipotentiality," even Lashley's conclusions were much more narrow: that "within a functional area, the efficiency of performance is conditioned by the quantity of nervous tissue available and independent of any particular area of association tracts" (1929: 88), and that the lesioned animals suffered "general deterioration" in behavior (1929: 135).[9] This suggests that there are substantial constraints on plastic changes. Thus it is reasonable to conclude that cortical map plasticity typically involves differences among tokens that do not amount to differences in kind.

Consider the fantastic case discussed by Shapiro, of the "rewired ferrets" whose visual systems project into what would in unmolested ferrets be the location of auditory cortex (Shapiro 2004: 61-64, discussing Sharma, Angelucci, and Sur 2000). After the manipulation, these ferrets are able to process and respond to some visual stimuli by

---

[9] Indeed Lashley (1929) spends significant effort in arguing that the effects he observes are specific to the functional capacities of interest (e.g., memory) and not just artifacts of the general "retardation" caused by the experimental procedure. Lashley also thought that equipotentiality, so far as it was true, was true of "association" areas of the brain, and not so much of sensory areas; he was aware of the phenomenon of sensory and motor maps.

means of the “audtory” cortex. Surely this is evidence of multiple realization if anything is?

Not so fast, says Shapiro. One sensory system is “wired” into another—so arguably we do not have systems of distinct kinds to begin with. Moreover, Shapiro concludes that this case is not an example of multiple realization, because it is not a case of “same but different.” In particular, it fails to satisfy criterion (ii) because gross neural location is not an essential feature of neuroscientific kinds whereas neural connectivity and receptive field response is. The “auditory cortex” in a rewired ferret develops the columnar organization and orientation-sensitive cells that are typical of visual cortex. I use the term “develops” self-consciously here, for the “plastic” changes in the ferrets are not induced in adults, but rather in neonatal ferrets whose visual systems are still forming. A fair description of these cases is that visual cortex has simply been “grown” in a geometrically different part of the brain than that in which it would usually develop. So the rewired ferrets are not an example of one psychological kind, vision, being implemented by tokens of two type-distinct neurological kinds, visual cortex and auditory cortex.

How does this case measure up on the criteria articulated earlier? Because the rewired ferrets have significant visual deficits vis-à-vis normal ferrets, criterion (i) is not satisfied.[10] Because the case is better described as one in which visual cortex is “grown” in or “moved” to the location normally occupied by auditory cortex, criterion (ii) is not

[10] It is too easy to think of the ferrets as either seeing or not seeing using their “auditory” cortexes. This is to take a very coarse-grained view of the matter. Fined-grained detail such as level of performance is too often ignored in philosophical discussions, but it is evidentially salient. For example, many philosophers mistakenly believe that blindsight patients can sometimes perform as well as normal visual perceivers, but this is false. Actual performance of blindsight patients is nowhere near that imagined by Ned Block when he describes “superblindsighters” (1995). This is another case where philosophers have confused hypothetical evidence for real evidence, with disastrous effects.

satisfied. Because difference in gross brain location is irrelevant to the function of the system in vision, criterion (iii) is not satisfied. And because the normal and rewired visual cortexes do their jobs in much the same way (insofar as they do the "same" thing at all), criterion (iv) is not satisfied. So the rewired ferrets are not good evidence of multiple realization.

The cross-modal aspect of the rewired ferrets lends drama to the example. But the more important feature of the case is that the functional studies suggest that the rewired ferrets developed processing structures—in particular, columnar organization—that is typical of visual processing. This is important because it suggests, contra Block, Fodor, and Lashley, that there is anatomical specialization within processing areas. The same observation can be made more clearly, if less dramatically, by examining research on rodent barrel cortex.

Barrel cortex is the sensory area in rodents that processes input from facial whiskers. The whiskers map onto individual cortical barrel structures, which are analogous to visual processing columns; and the barrels are arranged spatially, like other tactile sensory areas (Figure 3). Barrel number, size, and organization can be manipulated by genetic modification, as well as by neonatal removal of whisker input (Das 1997). In this sense, the barrels are quite like the tactile sensory areas dedicated to hands, fingers, lips, and so forth, as illustrated in Figure 1. What makes the barrel cortex a striking example for the evaluation of multiple realization is that the barrels are not only functionally measured, but are anatomical structures that might as well be gross anatomy from the point of view of the neurosciences. The barrels can be identified cytoarchitecturally, and are readily visible under cellular stains (Figure 4). Compare this to the previous example. Though there are reasons to favor one answer over another, the rewired ferrets nevertheless present us with difficulties about whether to say that their visual systems feed in to "auditory" or "visual" cortex. In contrast, the whisker barrels make for a much stronger case that change in function is accompanied by change in structure. This suggests that, at least with respect to these well-studied varieties of sensory representational plasticity, neurobiological specialization rather than multiple realization is the rule. If so, then evidence that criteria (ii) and (iv) are satisfied will be hard to find.

4. Ambitious Hypotheses

That psychological states are multiply realizable is by far the most widely cited reason for thinking that the mind-brain identity theory must be false. It is speculation but not too risky to guess that the acceptance of this line of reasoning would have been significantly diminished if it were grounded only on contentious assertions about the possibility of alien or artificial minds. (The Darwinian argument suggested by Block and Fodor has had relatively little influence.) Clearly the widespread acceptance of the multiple realizability thesis is attributable, in large part, to the belief that examples of psycho-neural multiple realization are actual if not plentiful.

It seems that the advocates of multiple realization took their best evidence to come from examples of neural plasticity.[11] But in the preceding pages I have argued

[11] As is clear from Block and Fodor's articulation of the evidence, merely pointing out (as Putnam did originally) that different terrestrial creatures appear to have sensations like pains is not going to do the job. Though here I have argued that the evidence is not as clear as advocates of multiple realization suppose, plasticity still has much better potential as a source of evidence. Elsewhere I have discussed the problems with direct comparative arguments between humans and other terrestrial species. See my 2000, 2004, and forthcoming-b.

that neural plasticity does not provide definitive evidence for multiple realization. The trouble is that, once we unpack Shapiro's recipe of "same but different, and differently the same," it is evident that common forms of individual and interspecies variation will not be enough to establish multiple realization. It is premature to score the available evidence as favoring mind-brain identity theories. But at least it seems 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.

Settling the question of whether we have evidence of psycho-neural multiple realization will await further evidence. Philosophers are not usually at their best when they succumb to the temptation to speculate about what evidence might or might not be forthcoming. The purpose of drawing attention to rewired ferrets and reconfigured whisker barrels is not to settle the matter, but rather to come to a better understanding of how this sort of evidence should be evaluated as we examine it. In closing I want to draw attention to the dialectical and evidential burdens shouldered by advocates and critics of multiple realization.

Putnam's rhetoric, in posing the challenge of multiple realization, emphasizes the strength of the claim made by the brain state theorist: "Even if such a state can be found, it must be nomologically certain that it will also be a state of the brain of any extraterrestrial life that may be found that will be capable of feeling pain before we can even entertain the supposition that it may *be* pain. …this is certainly an ambitious hypothesis" (Putnam 1967/1975: 436). In this way, Putnam poses the question as between two competing hypotheses:

(A) Every actual and nomologically possible instance of mental state kind M is nomologically guaranteed to be an instance of some physical state kind P.

(B) Some actual or nomologically possible instance of mental state kind M is not actually or not nomologically guaranteed to be an instance of any physical state kind P.

Put in this way, the options are (A) a universal generalization and (B) a logically weaker existential claim. Putnam then urges that the weaker hypothesis is more plausible.

But the tables can be turned on Putnam's rhetoric. For the brain state theorist may say that the relevant competing hypotheses are not (A) and (B) but rather:

(C) There is some neuroscientific kind P such that is nomologically coextensive with some mental state kind M.

(D) Every neuroscientific kind P is such that is nomologically certain that it is not coextensive with any mental state kind M.

If (C) and (D) are the options, then it is the brain state theorist who advances the weaker existential claim and the advocate of multiple realizability who must defend the stronger universal generalization.

It would be a shallow victory for the identity theory to show that there is just one psychological state—the sensation of indigestion, perhaps—that can be identified with a neuroscientific kind. Clearly what we want to know is whether such identifications are the exception or the rule, and whether there are classes of psychological states that are more or less likely to be identified with brain states. Answering this question will require examining more than a few favorite cases, and it will require a different kind of argument than has typically been employed in philosophical discussions of multiple realization.[12]

5. Acknowledgements

I am grateful for ongoing discussion of these matters with Fred Adams, Ken Aizawa, John Bickle, Bob Richardson, Larry Shapiro, Jackie Sullivan, and many other friends, colleagues, and students. Thanks to Centenary College for its support for a 2007 workshop on neuroscience and philosophy, as well as to the Charles P. Taft Research Center at the University of Cincinnati.

[12] Progress at updating the terms of the discussion can be found in Bechtel and Mundale (1999) and Shapiro (2004).

6. References

Bechtel, W. and J. Mundale. 1999. Multiple realizability revisited: Linking cognitive and neural states. *Philosophy of Science*, 66: 175-207.

Block, N. (ed.) 1980. *Readings in Philosophy of Psychology, Volume One*. Cambridge, MA: Harvard University Press.

Block, N. and J. Fodor. 1972. What psychological states are not, *Philosophical Review* 81: 159-181.

Block, N. 1995. On a confusion about the function of consciousness. *Behavioral and Brain Sciences*, 18: 227-247.

Buonomano, D. and M. Merzenich. 1998. Cortical plasticity: From synapses to maps. *Annual Review of Neuroscience*, 21: 149–86.

Das, A. 1997. Plasticity in adult sensory cortex: a review. *Network: Computation in Neural Systems*, 8: R33–R76.

Fodor, J. 1974. Special sciences, or the disunity of science as a working hypothesis. *Synthese* 28: 97-115. Reprinted in Block 1980.

Moore, C,. S. Nelson and M. Sur. 1999. *Trends in Neuroscience*s, 22 (11): 513-520.

Ohsaki, K., N. Osumi, and S. Nakamura. 2002. *Developmental Brain Research* 137: 159-170.

Polger, T. 2002. Putnam's intuition, *Philosophical Studies*, 109, 2: 143-170.

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

Polger, T. forthcoming-a. Two confusions concerning multiple realizability. *Philosophy of Science*.

Polger, T. forthcoming-b. Realization and multiple realization, chicken and egg. In J. Yoo and B. McLaughlin (eds.), *Mind, Body and Realization*.

Putnam, H. 1967/1975. The Nature of mental states, in H. Putnam (Ed.) *Mind, Language and Reality: Philosophical Papers, Volume 2* (New York: Cambridge University Press, 1975).

Putnam, H. 1974. Philosophy and our mental life. In H. Putnam (Ed.) *Mind, Language and Reality: Philosophical Papers, Volume 2*. (New York: Cambridge University Press, 1975).

Shapiro, L. 2000. Multiple realizations, *The Journal of Philosophy*, 97, 635-654.

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

Shapiro, L. forthcoming-a. How to test for multiple realization. *Philosophy of Science*.

Shapiro, L. forthcoming-b. Multiple realizability, seriously. In J. Yoo and B. McLaughlin (eds.), *Mind, Body and Realization*.

Shapiro, L. unpublished. The Science behind multiple realization.

Sharma, J., A. Angelucci, and M. Sur. 2000. Induction of visual orientation modules in auditory cortex. *Nature*, 404: 841-847.

Figure 1. Changes in the cortical sensory mapping for fingers, following transection.

(From Buonomano and Merzenich 1998: 164.)

Figure 1

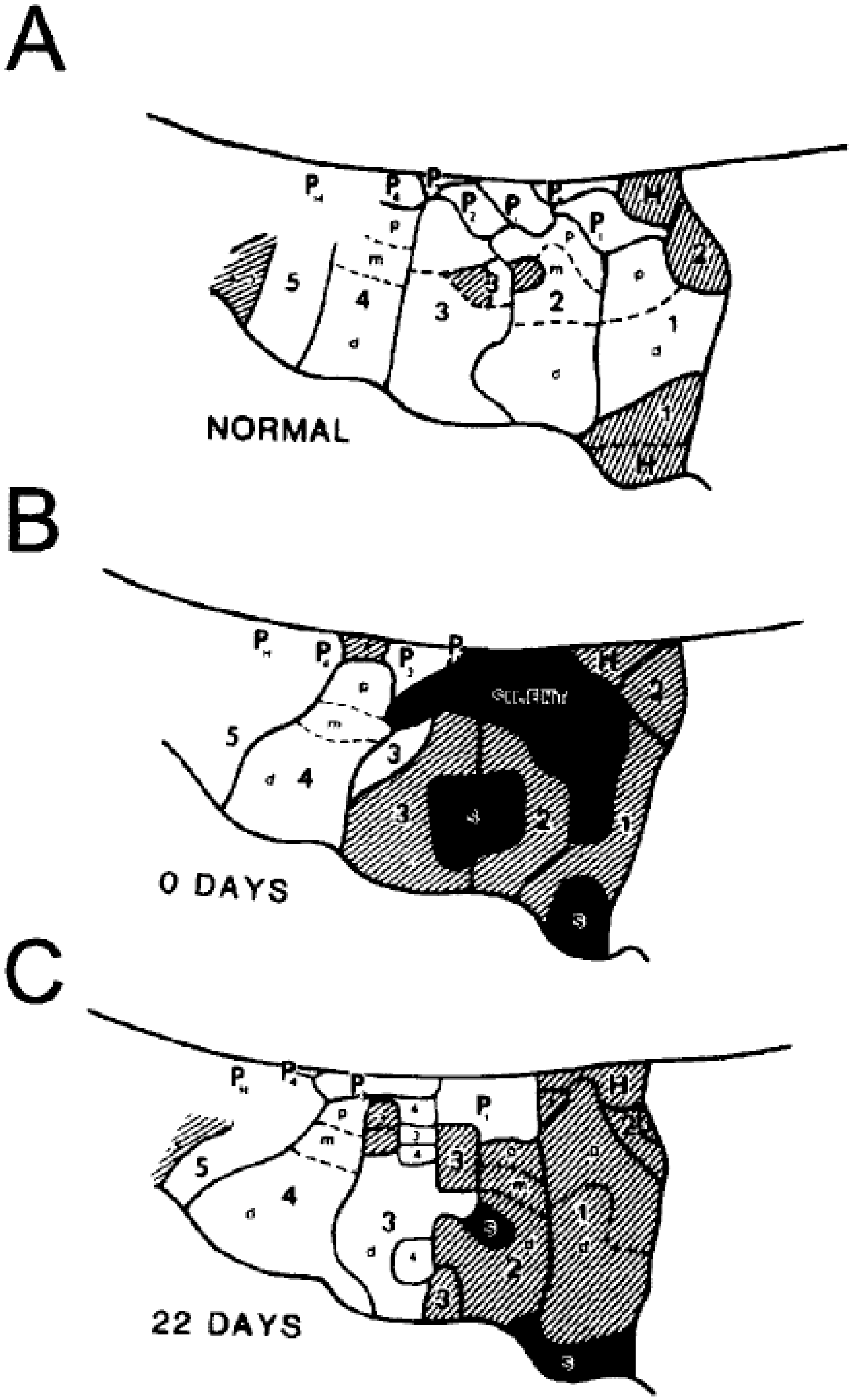


Figure 2. Activation-dependent changes in synaptic connection strength. (From Das 1997: R70.)

Figure 2

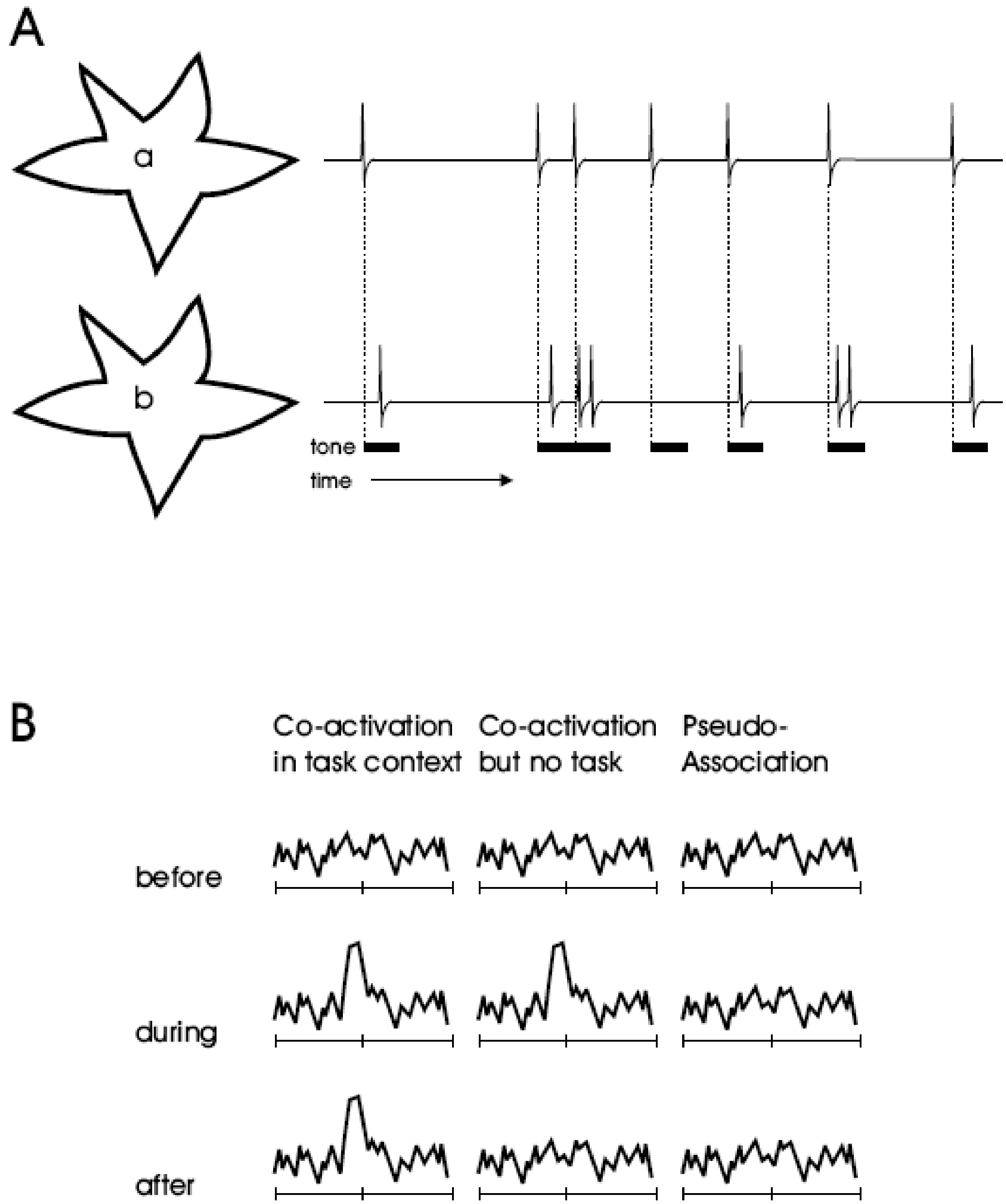

A
a
b
tone
time
B
Co-activation in task context
Co-activation but no task
Pseudo-Association
before
during
after

Figure 3. Rat barrel cortex organization in relation to whisker layout. (From Moore, Nelson, and Sur 1999: 513.)

Figure 3

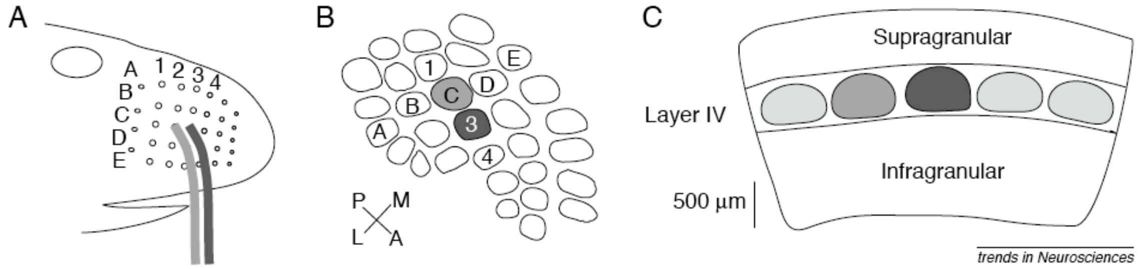
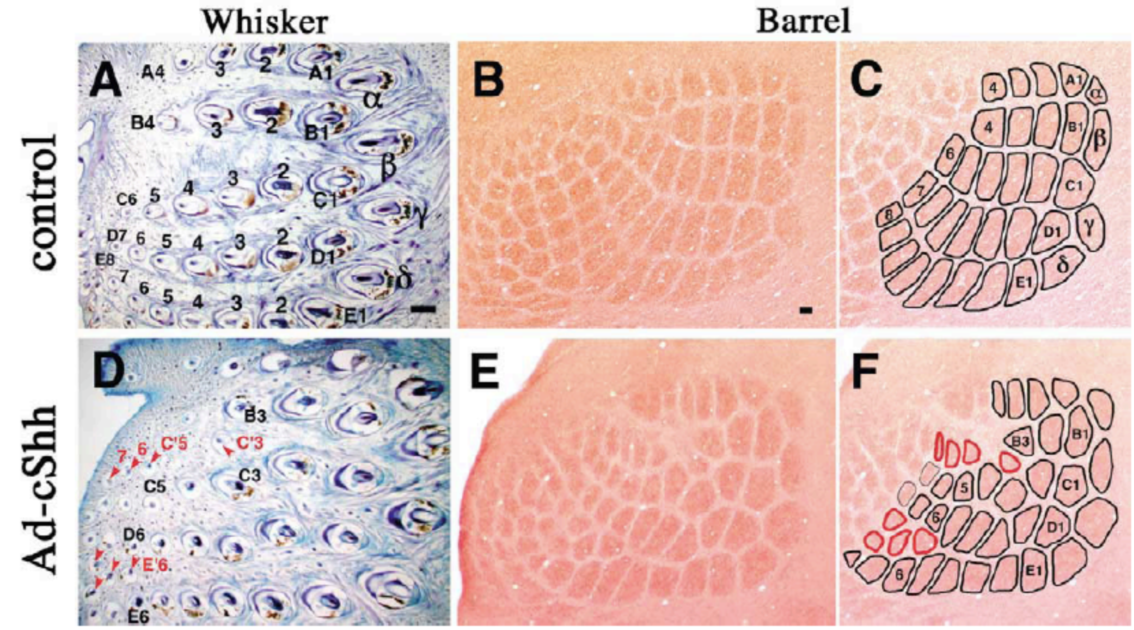

A
A
1
2
3
4
B
C
D
E
B
1
E
C
D
B
A
3
4
P
M
L
A
C
Supragranular
Layer IV
Infragranular
500 μm
trends in Neurosciences

Figure 4. “Alternation of the whisker positions are represented topographically in the barrels.” (From Ohsaki, Osumi, and Nakamura 2002: 166.)

Figure 4


Whisker
Barrel
control
Ad-cShh
A
B
C
D
E
F
A4
3
2
A1
α
B4
3
2
B1
β
C6
5
4
3
2
C1
γ
D7
6
5
4
3
2
D1
E8
7
6
5
4
3
2
E1
δ
4
A1
α
4
B1
β
6
C1
7
8
D1
γ
E1
δ
B3
7
6
C'5
C'3
C5
C3
D6
E'6
E6
B3
B1
5
C1
6
D1
8
E1