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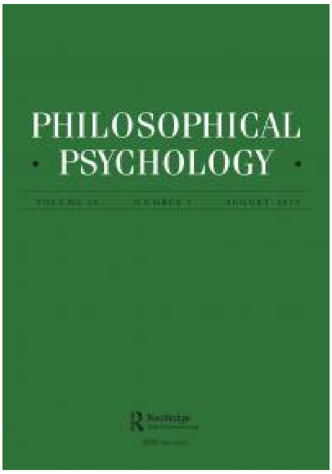

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Ahistorical homology and multiple realizability

Sergio Balari and Guillermo Lorenzo

The Mind-Brain Identity Theory lived a short life as a respectable philosophical position in the late 1950s, until Hilary Putnam developed his famous argument on the multiple realizability of mental states. The argument was, and still is, taken as the definitive demonstration of the falsity of Identity Theory and the foundation on which contemporary functionalist computational cognitive science was to be grounded. In this paper, in the wake of some contemporary philosophers, we reopen the case for Identity Theory and offer a solution to the problem of multiple realizabilty. The solution is based on the necessity, at the time of establishing identity relations, of appealing to the notions of "homology" and "analogy" developed in the nineteenth century by Richard Owen. We also suggest that these notions are useful in order to correct certain shortcomings of some recent attempts at rebutting the Multiple Realizability argument.

Keywords: Ahistorical Homology; Analogy; Historical Homology; Identity Theory; Multiple Realizability

1. Introduction

Mind-Brain Identity Theory is essentially the idea that mental properties of all types *are* nothing beyond brain states or processes, where 'are' is to be read here as expressing identity in the strongest possible sense, instead of one or another form of correlation. Mental activity is not just regularly associated with brain activity, but it is brain activity and nothing more. The former tenet (regular association) opens the door to the prospect of mental activity not being brain activity, but only correlated with it, whilst the latter (identity) completely excludes such a possibility (Kim, 2005, chapter 5). At first sight the question looks empirical, but it is not clear whether it can really be exhausted by the discovery of the relevant facts. This explains why, soon after

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the Identity Theory was developed in the mid-twentieth century (Feigl, 1958; Place, 1956; Smart, 1959), much ink has been spilled over how to conceptually prove or refute the thesis. After the publication of the "anti-identity" argument by Putnam (1975), Kripke (1972), and others, attempts to articulate and defend the Identity Theory largely vanished and most philosophers adopted a functionalist view of mind at odds with the Identity Theory. On the functionalist view, the same mental state ("type" reading intended) can be "multiply realizable" in both naturally evolved and artificially crafted structures.

More recently, a growing number of philosophers (Bechtel & Mundale, 1999; Bickle, 2010; Polger, 2004; Shapiro, 2000, 2004) have set out to demonstrate that the arguments of functionalists have not sufficiently considered when brain states or structures can be deemed the "same" (or "different"), something that is required in order to show that they multiply realize identical mental properties. In the wake of this reaction, which is rapidly constituting enough of a consensus against the functionalist view of mind, this paper contains a specific proposal as to how contemporary defenders of Identity Theory should respond to the Multiple Realizability (henceforth MR) argument. The core of this proposal is the newly invigorated version of the concept of "homology," originally due to Owen (1843, 1848, 1849), known under the tag of 'biological homology concept' (Wagner, 1989a, 1989b).[1] In a nutshell, our line of argumentation boils down to the following contentions:

(1) Discussions on the multiple realizability of a given psychological category (say, pain or vision) cannot be based on a raw notion of "variability," as variation unexceptionally applies to every single organic phenomenon (including brain states).

(2) The notion of "identity under variation" (or "homology") is thus required, to which end philosophers may benefit from well-established approaches in the field of comparative biology.

(3) The exact content of the homology concept, however, is controversial in that field. One thus faces the need to choose among competing concepts: a historical one, based on common ancestry, and a biological one, based on common underlying generative factors. It is our position that the latter is preferable in that it offers greater explanatory power than does the former and helps to unveil cases that may pass undetected by a more superficial look of history.

While approximations to points (1) and (2) have been defended previously, we think that point (3) serves to locate the debate in a conceptual ground that reinforces the conclusions of these prior efforts.

The paper is organized as follows. In section 2, we offer a detailed account about how Putnam actually framed the argument of Multiple Realizability, thus setting the stage for arguing against it on Owenian grounds in section 3. Building on earlier criticisms along similar lines by Bechtel and Mundale (1999) and Shapiro (2000, 2004), we argue in these sections that contrary to Putnam's self-assumed project, he failed both in establishing the uniformity of mental types across species and substances on empirical grounds (section 2) and in justifying the diversity of brain states correlating with these mental types on an evolutionary basis (section 3). We then turn to Identity Theory and try to correct some shortcomings of current formulations of the thesis that concern the granularity of mental types claimed to be the same. Some modern versions of the theory incorporate a concept of “species-specific” (or “local”) identity to neutralize the effect of the MR argument, but this move fails to establish the correct level of analysis at which identities must be sought in biology, including the biological study of mind. We also try to remedy this on Owenian grounds in section 4. There, we also respond to some recent efforts to apply the homological method to this question that use biological instead of historical criteria. Finally, in section 5 we expound our reasons for rejecting the contention that artificially made devices can share mental states with naturally evolved entities, with which we close our case against MR.

2. On Putnam’s Subversion of Science

The Brain-State Theory of mind claims that mental activities, such as knowing, wanting, or feeling, are “identical” to brain processes at a particular time. Hence the alternative name of this theory: the Identity Theory of mind (see Polger, 2004; Smart, 2011; and the papers in Gozzano & Hill, 2012, for a recent defense of the theory and relevant sources). To support Identity Theory thus amounts to holding that, for example, feeling pain “is” being in a certain brain state and consequently that only beings with brains or with a particular type of brain are capable of feeling pain. Having a well-functioning brain of that particular type and not feeling pain in the relevant situations, or feeling pain at some time or other without having a brain, are thus two scenarios inconsistent with the theory. The intuitive appeal of Identity Theory in connection with a currently widespread physicalist worldview seems clear. Notwithstanding, Identity Theory is considered to be a false theory by many scholars, mostly philosophers who, for conceptual or empirical reasons, contend that this particular version of physicalism is not the answer to the mind-brain problem.

The argument against Identity Theory that we are directly interested in here is Putnam’s (1975) MR thesis. In Polger’s opinion, it is “the most widely mobilized argument against identity theory and is often thought to be decisive” (2004, p. 38).[2] So no Identity Theorist can avoid responding to it.

The main thesis that we want to put forward in this paper is that the key to overcoming these threats is to anchor the concept of identity in criteria routinely used by comparative biologists since at least the mid-nineteenth century. Our point could be summarized by saying that if one argues for MR by assuming a criterion of identity that imposes strict equivalence of form or shape of biological structures, then, given the fact that inter- and intraspecific variation are the norm rather than the exception, multiple realization of any property will be trivially (and vacuously) true. There are, however, biological criteria for determining when two structures are identical or not that allow for the natural patterns of variation. We argue that when these criteria are used, most putative examples of multiple realization turn out to be false.

We understand the Identity Thesis as a *scientific hypothesis* about how the mental fits into a more or less general picture of the natural world (Armstrong, 1970; Borst, 1970, pp. 28–29; Place, 1970). So it is an issue that ultimately has to be settled empirically and by scientific means and not by an a priori conceptual analysis, for example. Our stance is appropriate since Putnam himself took the question as one to be solved empirically (1975, p. 433). The question is whether he actually honored this or not. It is our contention that he did not and that his arguments for MR against Identity Theory and for functionalism are not based on empirical evidence, but rather on a number of hidden premises and presumptions about the neurobiology of pain and other mental states as well as about the criteria appropriate to determine identity. This line of argumentation is not completely new, as it is advanced in Bechtel and Mundale (1999) and further developed in Bechtel (2012), Bickle (2003), Couch (2004, 2005, 2009), Polger (2004, p. 22), and Shapiro (2000, p. 637; 2004, chapters 1 & 2). But we deem it important to strengthen this response by showing that Putnam's intuitions were unwarranted even at the time when he constructed his argument.

Putnam never called into question that the words 'pain' or 'hunger' and their corresponding psychological predicates are robust in the sense that they refer to natural kinds. His approach is a top-down one in which the burden of proof is on the Identity Theorist to show that there exist brain states that are identical in different creatures and to which the appropriate psychological predicate can be applied (Putnam, 1975, pp. 436–437). A crucial assumption in Putnam's MR argument is that brain states of the same type must be identical to one another (i.e., they cannot be different), given the (hidden) assumption that mental states of the same type are identical to each other. This point already puts much stronger constraints on the empirical side of the issue by imposing a notion of sameness that makes little biological sense (Shapiro & Polger, 2012). It will be central in later sections when homology enters the debate, but here it unveils limitations in Putnam's claim to defend MR as an empirical thesis.

Putnam's claimed commitment to empirical sciences notwithstanding, the enterprise was never really carried out as a collaborative one, and there always remained something whose truth was not called into question, a point previously raised in Bickle (2008), Couch (2004), and Shapiro (2000). Only some 20 years later did Putnam (1988, p. xii) avow that this was so. The net effect of Putnam's argumentative strategy is a purportedly empirical argument against the reducibility of the mental where the irreducibility of the mental is (tacitly) assumed. Moreover, since Putnam also assumed, without further justification, that function is more important than matter, he succeeded in turning his MR argument into an indefeasible metaphysical argument for the autonomy of functions: functions are there to be realized by whatever material structures we can think of, be they naturally, artificially or divinely manufactured.

Once these hidden assumptions are revealed, a new avenue to respond to the putative threats of MR opens, one that challenges the kind of "transcendental functionalism" taken for granted by Putnam (and many other philosophers), as well as the "folk neurobiology" that is often appealed to in these kinds of discussions. Let us concentrate here on the second and turn to the neurobiology of pain.

More or less around the same time Putnam was developing his arguments for functionalism, Melzack and Wall (1965) sketched a neurophysiological model for pain, which was described in detail later by Melzack and Casey (1968). This latter work (see especially p. 434) provides a characterization of what pain is which does not describe it as a sensation, or a variety of perceptual state (Pitcher, 1970), but as a process in which a number of areas of the central nervous system participate (along with the peripheral pathways leading to them). No specific signal or area is identified as the "pain signal" or "pain center" since pain is the complex integration of all these plus a number of additional cognitive factors viewed as establishing the intensity and the quality of pain. Melzack and Casey (1968) give us the following list: anticipation of pain; anxiety and attention; suggestion and placebos; cultural background; evaluation of the meaning of the pain-producing situation; hypnosis; early experience; and prior conditioning. All this supports their contention that 'pain' is a label standing for "a multitude of different, unique experiences" that nonetheless exhibit a number of sufficiently relevant regularities in terms of types of signals, areas involved, etc., in humans and non-humans. So we think that it is fair to conclude that empirical research has long placed us in an epistemic position from which a variegated typology of pain experiences suggests itself instead of a monolithic or unvarying form of pain, in correspondence with a diversified organic background that does not preclude the existence of a commonality of differently overlapping nervous resources. Such a conclusion looks very much like the mirror image of the inferences that Putnam derived based on his own intuitions (Hardcastle, 1997).

Actually, as research continued and the model was refined (Melzack, 1999; Melzack & Wall, 2008), a much richer and intricate picture of the experience has emerged, but only to corroborate Melzack and Wall's original contention that "pain … does not consist of a single ring of the appropriate central bell, but is an ongoing process" (1965, p. 976). We have learned, for example, that different nociceptive units exist, the legendary C-fibers, but also the A-δ-fibers, and that some of these units are polymodal, while others are more sensitive to thermal, mechanical, or chemical stimuli. We have also learned that these peripheral units send their signals to the spinal cord, which sends them to the thalamus. From there, they are spread to different cortical areas, where they are processed and integrated with other data to produce the full-blown sensation most of us refer to as 'pain' (see Melzack & Wall, 2008, part 2 for an overview). We have learned that pain may still be felt and identified by subjects whose brains, for different reasons, are incapable of performing all the usual processes. For example, those suffering from pain asymbolia "feel pain," but for them it is deprived of all its emotional and affective components. We could say that these patients feel some crude or raw form of pain or, to use Grahek's (2007) felicitous formulation, that they "feel pain without being in pain." But things may turn out to be much more complex than that, if we accept the alternative two-systems model developed by Hardcastle (1997, 1999). This model splits Melzack and Wall's gate-control model into two subsystems, a bottom-up nociceptor-driven pain sensory system and a top-down pain inhibitory system, with the latter capable of suppressing or enhancing the activity of the former to the extent that "our sensations of pain are almost independent of nociception" (Hardcastle, 1999, p. 146). Whatever turns out to be the final picture of pain, it is clear that pain is something quite different from what our intuitions dictate. But at least the commonsense meaning of the word 'pain' is not eliminated; humans will not refrain from using the word, not even those, like Melzack and Casey, for whom pain involves a collection of brain processes. This point is at the heart of many conceptual misunderstandings concerning the issues at stake here.

Through standard scientific practice, a new concept PAIN has emerged (or, perhaps, a collection of closely related concepts), one that does not replace the commonsense concept that was our point of departure but complements it.[3] Both concepts belong to different domains, one to, say, "folk physiology" and the other to a specific neurobiological theory trying to determine what pain really is.[4] Interestingly, Owen already identified this phenomenon 150 years ago when discussing the terminological problems faced by a unified science of comparative anatomy. Owen's point was clear: if there is a commonsense term available and it is useful—say, 'calomel' (mercury chloride, a common component of reference electrodes in electrochemistry and administered as a purgative throughout the eighteenth and nineteenth centuries)—let us use it. The chemical fact that calomel is Hg_2Cl_2 (dimercury dichloride) does not make it less useful or improper as a theoretical concept (Owen, 1848, p. 2). Indeed, such practices are entirely unproblematic for the scientist.

Consequently, it is perfectly legitimate to apply the terms 'coracoid' (a paired bone which is part of the shoulder) or 'vomer' (an unpaired bone of the facial skull) to humans, crocodiles, and whales, for example, even though the bone does not look like the beak of a crow or a ploughshare in all cases. There are scientific criteria—i.e., the criteria to establish homologies—for assuming that the bone in question is the same in humans, crocodiles, and whales, and accordingly, for using the same word to designate it. But this is precisely the kind of situation Putnam characterizes as the one that would make Identity Theory collapse, the situation in which we apply—illegitimately, we must presume—the word 'hungry' to brain states that are not identical in humans and octopuses, for example. Consider what would have happened to comparative anatomy if researchers had been restricted from applying the term 'vomer' to a bone in whales that, contrary to the case of humans, does not have the shape of a ploughshare. The enterprise would have collapsed. Perhaps scientifically inspired philosophers—to use Bickle's (2003) expression—should try to find out what there is in Putnam's claim concerning 'hunger' that does not make it sound as preposterous to them.

3. Defeating Putnam's Evolutionary Argument on Interspecific Realizability

As suggested in the Introduction, Owen's overall comparative program contains the conceptual safeguards necessary to save Identity Theory from the threats of MR. Let us remember these threats first:

(1) Brains as different as those of mammals, reptiles, mollusks, and even extraterrestrials, can share identical mental properties.
(2) Both natural brains and artificial brainless devices can share identical mental properties.

On strictly Owenian grounds, (2) must be rejected, in contradiction with metaphysical functionalism, while (1) must be supported, but within limits that run against the contentions of functionalism and serve as a corrective for current variants of Identity Theory. Such conclusions smoothly derive from an accurate use of the Owenian notion of "homology" in the case of claim (1), and the alternative Owenian concept of "analogy" in the case of claim (2). Bechtel and Mundale (1999) and Couch (2009) have appealed to this conceptual apparatus previously in the context of the realizabilty debate, but in a way that we think is not entirely consistent or clear. We urge to reintroduce it to this debate along the lines originally established in Owen's own writings (1843, 1848, 1849). Owen's approach has received renewed interest within the young and flourishing discipline known as evolutionary developmental biology—or "evo-devo" (Hall, 1999), especially with the development of the "biological homology concept" (Wagner, 1989a, 1989b).

Our Owenian contention concerning claim (1) is very clear: brains as different as those of mammals and reptiles, leaving aside for the moment mollusks and extraterrestrials, can share identical mental properties. So we partially agree with Putnam on this, but only insofar as they share the relevant physical structures—"organs," in Owen's own words—the states and processes of which *are* the properties of concern. Accordingly, we reject Putnam's "what/how" duality that inspires his method for identifications. Contrary to Putnam's own contention, what we are going to argue for in this section is that there exist evolutionary reasons that lead us to conclude that both the duality and the method are completely unwarranted.

Putnam's contention concerning this aspect of MR—i.e., "interspecific MR"—is that we should not expect such different organic systems as the brains of reptiles and the brains of mammals to adhere to a similar structural plan, because this would represent an astonishing case of "parallel evolution" (Putnam, 1975, p. 436). So according to Putnam's reasoning, we have no reasons for doubting that reptiles and mammals share mental states to a great extent. They all think and feel pain, so we have here an initially reasonable, though not unproblematic, case of multiple realization of the same mental properties in very differently structured organs.

A conflict exists between those who, like us, grant reptiles and mammals the same mental properties *because* they share the relevant physical structures—to the extent that they do share them—and those who, like Putnam, agree with the main observation *despite* the fact that they fail to share the relevant structures. The Putnamian line of reasoning is (1) that identity at the structural level is not a requirement here because mental states are not identical to brain structures, and (2) that the reasonable evolutionary expectation of strong variability at brain level is not in contradiction with a parallel expectation of strong uniformity at the level of the mind. One may wonder what a mind is then, if it is something that escapes the expectations of naturally evolved phenomena, but this is not a problem for the Putnamians, as they think that whatever a mind is, it happens to be a property of both organic and inorganic stuffs and thus beyond the strictures of natural evolution.[5]

Since Putnam himself situated the interspecific debate in the evolutionary arena, it is appropriate to consider how an Owenian evolutionist would respond. The main problem with Putnam's "evolutionary argument" on interspecific realizability is that it is not accepted by biologists. It is true, as Putnam suggests, that parallel evolution (i.e., independent evolutionary processes leading to similar outcomes) is a relatively exceptional phenomenon. However, a well-known occurrence that also mimics the effects that Putnam presumably wants to avoid in order to make his idea acceptable is the fact that evolution is extremely conservative. Moreover, structures and processes tend to be canalized and follow certain developmental inertias at all levels of organization (see Minelli, 2011 and Waddington, 1957 for two representative samples of more than 50 years of lively research in this area). And this happens to be the norm, instead of the exception, in the workings of natural evolution. To be sure, the net effect of evolutionary phenomena is diversification—remember Bateson's motto: "Variation … is the essential phenomenon of Evolution. Variation in fact is Evolution" (1894, p. 6). Yet, diversification arises against such a background of massive organic conservation that the more diversified biological phenomena can be safely deemed the "same" despite appearances. This was exactly the idea for which Owen coined the concept of homology (see Minelli & Fusco, 2013 for an overview of the different concepts of homology, and Brigandt & Griffiths, 2007, and references therein, where its philosophical significance is also emphasized):

> Homology—the same organ in different animals under every variety of form and function. (1843, p. 379)

In current philosophical jargon, this definition amounts to saying that biological objects constitute "natural kinds," where diversified members of the same evolutionary units are reunited because they share a common developmental background (see below for some qualifications). This is what allows us to refer to such apparently different structures as the forelimbs not only of bats, moles, horses, or humans, but also of reptiles and birds, as the "same" structure.

How does all this relate to brain structures and mental properties? It shows that Putnam is wrong in both his main two assumptions: (1) that there is extreme uniformity at the level of mental states; and (2) that there is a pattern of unrelated organic structures at the level of their physical realizers. However, a still intuitive but now biologically informed view on this matter invites us to think that pain, for example, may not be such a uniform experience across species after all, yet it may be the same experience case after case, as we have good reasons to believe that varieties of the same experience can exist as a result of the diversification of their common physical basis—whatever that happens to be in different animals. Note that if we take Melzack and Casey's characterization to heart, there exists significant "elbow room" for mental properties like pain to vary while still being the same property. Indeed, as Grahek (2007) points out, neither a fully subjective nor a fully objective approach to pain is accurate because both dimensions participate in the experience. Thus, given its emotional and affective components, no pain experience will ever be identical to another, although both will share an important collection of objective components (Melzack & Wall, 2008, part 1). This is a kind of pattern for which we'd rather use the label 'multiple variability'—naturally diversified variants of the same mind/brain states or processes, in order to avoid the implicit commitment of MR to the unwarranted thesis that completely different types of brains can evolve to instantiate the same mental states and processes.

For us, this is the right combination of intuition and biology, which implies putting the technical concept of homology as the footing of our empathetic insights concerning the experiences of other minds. This is a strong foundation for our intuitions and also the guiding principle of much current research in comparative neuroanatomical studies (Bechtel & Mundale, 1999; Bickle, 1998; Kim, 1992), according to which the extreme degree of conservation in brain structure across vertebrates is but an inescapable conclusion (Striedter, 2005). Note that the homology concept is but a shortcut to refer to the extreme conservativeness of the developmental resources underlying organic structures and, in the end, of structures themselves—we turn to this topic in the next section. Thus, the reptilian/mammalian divide does not seem too big a gap after all, which encourages us to locate not just the same mental properties on both sides of the gulf, in agreement with Putnam's intuitions, but also the same brain states as their putative sites, now in contradiction to Putnam's evolutionary statement.

Nothing said so far implies that for whatever mental property you select, there will exist the corresponding homologue in whatever vertebrate you choose. Actually, "eccentric" mental properties are known to exist (Polger, 2004, pp. 33–35), e.g., echolocation in microchiropteran bats or electro-sensitivity in torpediniformes, to mention two proverbial examples of eccentricity among vertebrates. But the fact that eccentric properties will exist inasmuch as the correspondingly exceptional brain structures exist, with mental properties specifically existing at the same taxonomical level (species, genus, and so on), is a welcome finding insofar as the corresponding structures exist and are shared at those levels. This actually happens to be the case in the two above-mentioned examples (Striedter, 2005, pp. 56 & 96). This means that the application of the homological method to this question promises to turn the question into an empirically debatable one, as we try to justify in section 4.

The method also leads us to expect that mollusks, besides their own eccentric mental properties, also share some properties with mammals and reptiles (perhaps including pain), but only inasmuch as the relevant physical bases are confirmed to exist among these organisms. In this respect, it is important to note that current applications of the comparative method to the brain show that homologies can be unearthed at different levels of analysis, from macroscopic regions to neuron-typical molecules (Striedter, 2005, chapter 3). A further reasonable expectation, explored and amply documented by Bickle (2003), is that the highest degrees of conservation are found at lower levels of analysis such as the cellular and molecular ones.[6] The most relevant conclusion regarding this question, however, is that the homological method locates all these considerations in a territory relatively well safeguarded against too much conjecture.

In concluding this section, we cannot but introduce a short comment on extraterrestrials hypothesized by Putnam. Again, we are able to take our response almost directly from Owen's own pages, as he touched on the issue in the beautiful closing section of *On the nature of limbs* (1849). There he expounded the idea that if extraterrestrial creatures were someday found on, say, Jupiter, they would most probably be assembled from the same "organic mechanisms" as earthly creatures, at least if natural laws were the same on both planets. So the mental life of these creatures, we dare add, would not be so different from that of the creatures that populate the Earth, i.e., a mixture of homologous and eccentric experiences relative to our own. This is not bare speculation, but a prediction (perhaps never to be confirmed) of the identity model put forward in this section (see Conway Morris, 2011 for discussion).

4. How Intuitive Can an Identity Theorist Be?

Another welcome consequence of adopting an Owenian perspective on the mind/brain problem is that it allows us to correct certain intuitive appeals by current Identity Theorists that we believe to be in need of biological support. For example, Polger says that "we do not typically ascribe empathetically the same mental states to creatures that are quite different from us in their biology" (2004, p. 155), contrary to Putnam's intuitions. As a first person testimony, we do not have reasons for doubting its truthfulness. But, just as with Putnam's intuitions, the Identity Theorist's intuitions need to be scientifically supported before we allow them to play a theoretical role.

Let us explore Polger's intuitions. As presented, they are as problematic as Putnam's. Polger presents them as scientifically motivated, but he fails to employ the proper level of analysis to do so. Polger's personal strategy for dealing with his intuitions follows previous suggestions by, among others, Churchland (1979), Enç (1986), Kim (1972), and Lewis (1969) by framing them as a so-called "species-specificity identity," which is the claim that mental properties are identical to their physical bases only inasmuch as we locate ourselves at the classificatory level of the "species." We will not enter into a detailed discussion of this particular variant of Identity Theory. Let us simply note here that it has the virtue of referring identity claims to an independently warranted biological category. Yet there is a crucial limitation: no current concept of species incorporates a criterion of organic identity or similarity (Mallet, 2006). But species share organs ("type" reading intended, of course) and, we would promptly add, the corresponding activities and properties, including mental ones. This reinforces the conclusion in our previous section: identities are framed in terms of homologues that traverse interspecific frontiers.

At this point readers may be thinking that independent theoretical support for our own homological approach is also still wanting, and that just referring it to the authority of a nineteenth-century biologist, and, for many, a rather obscure one, is not enough. To address this, we will expand on the criteria for judging a collection of organic structures, activities, or properties to be homologues. In our opinion, the main shortcoming of previous homological approaches to the threats of MR—mainly Bickle (2003), and Couch (2004, 2005, 2009)—is that they do not rely on a clear concept of homology or that they rely on the Darwinian or historical homology concept, which is completely helpless in establishing bona fide natural kinds (see Griffiths, 2007, for discussion). This is why we suggest adopting the biological homology concept instead, as it is capable of grounding natural classes on more objective and informative grounds than the historical contingency of ancestry.

According to Padian's (2007) historical analysis, Owen's original concept of homology was a very restrictive one, based on three different, yet complementary criteria:

(1) a *positional* criterion, which referred to the possession of a distinctive pattern of structural organization;
(2) a *histological* criterion, based on the possession of identical or very similar kinds of tissues or cell types (today we should add molecules);
(3) an *ontogenetic* criterion, pointing to the existence of a characteristic developmental pathway, typically starting in identical embryonic precursors.

Criterion (1) was prevalent in Owen's time (Amundson, 2007, p. xxiii; also Amundson, 2005 and Rupke, 1994), in part due to the fact that it could be grounded directly in observations, but also in part because it was deeply rooted in the application of Geoffroy's (1818, 1830) well-established "*principe de corrélation des formes*." But even Owen was aware that it was no more than an expedient shortcut of the homological method, and he reacted against certain extensions of the criterion that resulted in unrealistic natural associations, such as Geoffroy's (1820) suggestion that vertebrate and invertebrate limbs were variants of the "same" organ, as they share a common basic structural schema (Owen, 1849).

Nowadays the prevalent concept of homology, known as the "biological homology concept" (Wagner, 1989a, 1989b), is mostly centered around criterion (3) and boils down to the idea that organic identities ("homologies") can be safely established inasmuch as common factors and constraints can be identified underlying the development of the relevant structure, from which positional and histological properties can be seen as derivative. The biological homology concept is proposed as apt to overcome some important shortcomings of the alternative historical concept:

(1) First, it is compatible with the well-established fact that traits (i.e., individual parts of phenotypes) are not the units of inheritance. What is subject to intergenerational transmission are the means (i.e., the developmental mechanisms) to build traits anew in each generation. Thus, homological correspondences do not make sense when understood as an identity relation between traits inherited from a common ancestor, as there are no such things as "inherited traits."
(2) Secondly, it is compatible with the well-known phenomenon of "serial homology" (or "homonomy"), i.e., repetitions of a trait (for example, thoracic segments in insects, vertebrae, teeth, and so on) within the same organism in a different number from that of the ancestor. In most cases, it is impossible to establish correspondences between the ancestor's and the descendants' traits, while at the same time is clear that they all are built from the same mechanistic resources.

Such difficulties disappear as soon as the developmental perspective is adopted and homological relations are sanctioned based on the existence of a common (even if diversified) background of developmental factors constraining the individuality of the resulting traits. Here, 'developmental factors' is a term for referring to a semi-autonomous system of generative influences (genetic, epigenetic, and environmental) acting in an auto-regulatory and robust fashion to bring about differentiated parts of a phenotype. Note that in this way homological relations can be traced back to the conservation of the relevant developmental mechanisms, which is always "a matter of degree rather than an all-or-nothing relation" (Wagner, 1989a, p. 55). This paves the way for accommodating the complementary fact of variation (in terms of types of material, developmental tempo, inductive influences, and so on) also registered at this level and thus the evolvability of the traits of concern (Brigandt, 2007).

It is straightforward to apply this concept to establish identities at the mind/brain level (see Ereshefsky, 2007 and the papers in Moore & Moore, 2013 for discussion). As organic entities, brains develop following characteristic paths that lead to correspondingly characteristic steady properties—neuron-typical molecules, kinds of tissue and tissue concentration, differentiated macroscopic regions and patterns of reentering connectivity among them, etc. On the one hand, shared (homologous) mental properties are expected to exist across species to the extent that they share developmental pathways at the nervous system level, according to the standards of developmental biology. On the other hand, species-specific (eccentric) properties are expected to arise where developmental novelties enter into the picture (Hall, 1999; Reid, 2007). Because every species, as we noted before, is a peculiar combination of shared and novel traits at every level of organization, this is a good framework to tame our intuitions and develop a scientifically grounded view of mind.

As noted before, the biological homology concept has the virtue of unearthing natural kinds where the alternative historical (or Darwinian) concept fails to discover them. As is well known, to sanction homological relations depends, according to the latter, on the existence of a common ancestor from whose organic constitution variants of a particular organic structure or system can be safely said to derive. So the concept obscures the fact that similar structures and systems (at all levels of analysis) are shared by species with a common ancestry deeply embedded in the past from which we do not know the patterns of descent. In cases like these, the weight of the historical criterion cancels out all evidence coming from the organization, composition, and, crucially, basic generative factors underlying the development of these organs. For purely conceptual, non-empirical reasons, these structures are not treated as homologous. Alternative concepts need then to be proposed to rescue these cases before concluding that they are the result of haphazardly convergent evolution—which they clearly are not. One such concept is that of "parallelism," which boils down to the idea that latent generative factors in a common ancestor pave the way for the intermittent emergence of shared traits in the absence of relevant common ancestry.[7] Once we can appeal to contemporary evidence with the biological homology concept, we need not to be dependent anymore on historical notions and (potentially unknowable) entities like common ancestors (see Balari & Lorenzo, 2013, pp. 81–88 for discussion and references).

We are therefore sympathetic with Couch's (2004, 2005, 2009) overall project of incorporating concrete criteria of biological classification as a basis for addressing the puzzles of MR. But our position goes further, eliminating the need to appeal to historical considerations when the task is that of establishing bona fide natural kinds, since there are good reasons to think that true kinds exist that can be identified even among distantly related species. Let's take the case of vision, specifically raised by Couch (2009) and widely invoked, from Putnam (1975) on, in the multiple realizabilty debate. For Couch, eyes were not a well-chosen illustration of multiple realization, as the different types of eyes (from the eye of the octopus to the eye of the mammal, at least) probably belong to one of those exceptional cases where "identical structures" (physically speaking) have arisen from completely independent evolutionary processes. A different position, however, was defended in Block and Fodor (1972), where the contention was made that eyes illustrate how the evolution of physically unrelated structures (thus, different realizers) may concur in bringing about identical psychological capacities. Like Shapiro (2000, 2004), we believe that opinions like these cannot be entertained without considering the relevant empirical evidence. As for the case at hand, common wisdom according to which visual systems are in most cases the result of different independent evolutionary processes has been recently challenged by evo-devo considerations. The new image points to a background of massive conservation at the level of the developmental resources leading to all classes of eyes (camera-type eyes, compound eyes, mirror eyes), which can be found across any taxonomic bifurcation, from humans to sponges (Gerhing & Kazuko, 1999; Gehring, 2005). In all cases, photosensitive cells containing rhodopsin as a light receptor are associated with a developmental circuitry of varying complexity, but sharing high-level transcription (regulatory) factors and targets (*Pax6*, *so*, *six*, etc.) that similarly constrain the individuation of the resulting structures. The most elementary instances of this conserved developmental mechanism build rather simple assemblies of a varying number of photoreceptor and pigment cells; more sophisticated eye types add additional developmental resources to the underlying common mechanism (in some cases, just duplications or fusions of its most basic components), capable of introducing further architectural complexity and functionality (ommatidiae, lenses, retinae, brain and cortical components, and so on).[8] So all visual systems are instances of the same organ of vision developing under different constraints. Note, finally, that Couch's implicit commitment to the historical homology concept is the only clear rationale for his maintaining the form/function split—and thus an autonomous level of functional analysis—when confronting the (historically) problematic categories of vision in flies versus vision in humans. No corresponding (biologically) problematic category arises when applying the homological method using the biological homology concept (remember: "a matter of degree rather than an all-or-nothing relation"; Wagner, 1989a, p. 55), which means that the perspective prevents us from adopting such a split. We think that this is a good result according to every standard of proper philosophical and scientific practice.

Coming back to pain, we discover a situation comparable to the case of vision: homology is the norm rather than the exception. First, it is important to distinguish two different processes, each with a slightly different neurobiological grounding: nociception and pain. The first is the mere identification of noxious stimuli capable of tissue damage and the second is the sensory and emotional experience associated with actual or potential tissue damage (Crook & Walters, 2011). Nociception is a quasi-universal trait (Smith & Lewin, 2009) observed in such disparate organisms as lower vertebrates (Sneddon, 2004; Sneddon, Braithwaite, & Gentle, 2003), gastropods (Illich & Walters, 1997) and mollusks in general (Crook & Walters, 2011), leeches (Pastor, Soria, & Belmonte, 1996), worms (Wittenburg & Baumeister, 1999), and fruit flies (Tracey, Wilson, Laurent, & Benzer, 2003), in addition of course to higher vertebrates. As for pain, it is generally associated with the presence of complex brain structures, such that, as stated by Crook and Walters, "several considerations suggest that pain may be absent in at least some invertebrates," although, as the same authors are quick to add, "even if the critical mechanisms turn out not to be equivalent, it is not possible to be certain that an animal does not feel pain" (2011, p. 187).

What all this boils down to is, first, that the "species-specific identity" advocated by some Identity Theorists is too conservative a stance, since, as already pointed out, homology is more the norm than the exception and it can help us tidy up our conceptual space of scientific terms (e.g., nociception versus pain). Second, the historical homology concept implicitly adopted by some authors is not the best that current biology offers for uncovering natural kinds relevant to addressing MR. Third, the very idea of MR, in biology at least, may just be an artifact derived from the top-down methodological principle according to which biological structures realize preexisting functions, a point we elaborate further in the following section.

5. On the Mental Life of Brainless Machines

A refutation of the second and more general threat of MR is still wanting. So let us begin by remembering what the threat amounts to:

(2) Both natural brains and artificial brainless devices can share identical mental properties.

Functionalists think that there is no frontier between organic and inorganic stuffs that prevents them from sharing properties, notably mental properties, very much against what Identity Theorists would contend. Our reaction to the functionalist position is that it relies on an unwarrantedly wild concept of identity, but we want to elaborate on this, as our Owenian framework again offers the tools for clearing up the question.

What functionalists find so threatening for Identity Theory is the existence of artificial (including virtual) devices capable of bringing about activities that also qualify as mental. It is worth remembering here that, according to Putnam's Functional Organization Theory, positing a mental property *m* is justified when the system implicitly satisfies a description in which *m* is a particular state leading to (1) other similar states or to (2) some motor-sensory state. This means, among other things, that possessing properties, mental or otherwise, is a matter of description assignment, not of the material (e.g., developmental) basis of the systems of concern. And this is the crux of the matter, as well as the main breakdown in functionalist thinking, because description assignment (contrary to, for example, the assignment of developmental pathways) is not objective (Polger, 2004, chapter 5). Rather, it is entirely dependent on the interests and value systems of the describer, as argued at length by Kalke (1969) and Searle (1992, pp. 237–240).

The same argument was also advanced, curiously enough, by Owen, particularly in his (1849). There, Owen offered an abridged presentation of his homological method, starting with a clarification of the distinction between the concepts of homology and analogy that he had introduced some years before. Of these categories, as Owen explains at length, it is just the analogy concept that amounts to a functionally centered notion, in that it comprises organs to which the same purpose can be assigned, irrespective of whether they belong to the same natural kind or not. So clear was this idea for Owen that he was even ready to accept artificial devices on his list of analogues of natural organs (1849, pp. 9–10), obviously insofar as they could all be reasonably described as function-sharing phenomena. Thus, for example, nothing in the analogy concept prevented it from being applied to both the forelimbs of a mole and a domestic shovel, but Owen was perfectly aware that such associations were of little use for explanatory purposes, and that natural philosophers were not required to grant them any certificate of naturalization; they simply exist in the eye of the beholder and are extremely sensitive to considerations foreign to the scientist's practice. For example, the analogy between the mole's forelimbs and the shovel may readily be expanded to also apply to the digestive/excretory system of earthworms, as an alternative "instrument for burrowing," but if we do not feel particularly tempted to do so it probably has to do with the fact that it is too alien to our own means (i.e., the describer's means) to satisfy the same purpose. Thus, analogies, i.e., purpose ascriptions or functional descriptions, are completely dependent on the describer's point of view and thus are naturally unwarranted. Actually, Owen also argued in the opening pages of *On the nature of limbs* that true natural kinds, i.e., those established on the basis of the homology concept, are normally described by many functionally centered categories (think, for example, of the very different functions of the forelimbs of moles, whales, and horses), and they even include members to which no particular purpose can be granted, as is the case for the human hand. So his idea was that functional classes (analogues) do not disrupt natural kinds (homologues) in any theoretically relevant way and do not comprise natural categories in themselves.

Similarly, the functional mind crosscuts the "natural/artificial" divide. But, what is so problematic about the argument leading to this generalized notion of mind? Why should we not accept it and, if necessary, simply correct our intuitions concerning what a mind is?

To begin with, the logic of functionalism amounts to granting causal powers to disembodied descriptions, too extreme a conclusion to take on easily. Functionalists (some functionalists, at least) may respond that this is only part of their logic, as descriptions ultimately materialize in one or another (organic or inorganic) kind of stuff, the causal powers of which are unproblematically open to observation. But what this proves, if anything, is that the causal power of the materials to which functional descriptions are ascribed is not that of the descriptions themselves (see Shapiro, 2000,

p. 638; 2004, pp. 18–19, where the point is similarly raised). The functionalists' argument, if it is one, is completely helpless regarding the putative agency of disembodied abstract descriptions. And regarding inorganic compounds in particular, the statement boils down to the idea of admitting their causal efficacy, but clearly is insufficient for granting mental activity to them, which, obviously, is not the same thing. Readers interested in going deeper into this kind of criticism are referred to Kim (1998), a revisionist project, and Polger (2004), an overtly anti-functionalist take on the matter, where it is thoroughly disentangled.

Granting causal efficacy to artificial devices adds up to the platitude that they are amenable to being ascribed purposeful behaviors, brought about by their own means, e.g., a robot, or with external assistance, e.g., a shovel or a backhoe. But purpose ascription is not an objective practice, one that can ultimately be solved by scientific discussion and consensus. It is no more objective, for example, to say that the mole "scraps and throws back the soil" than to say that it "swims through the earth," as Owen (1849, p. 7) himself wrote. No explanatory advantage obtains from one or another *façon de parler*, which just reflect the observer's familiarity with similar enterprises. They make no difference when our goal is understanding things. So when we ascribe the same purpose, for example, to the digging activity of moles and to the activity of robots designed for burrowing in rescue or research contexts, what we are actually doing is simply establishing a functional class of "analogues" with no substantial content whatsoever, as Owen so clearly observed. This would be an entirely innocuous practice were it not for the fact that it serves as a subtle gateway to further ascribing the same implicit description to the internal workings of the corresponding entities considered responsible for causing their shared purposeful behaviors, and thus treating them as equally "mindful." But doing so, however: (1) does not reinforce their identity, beyond the weak and theoretically insubstantial analogy; and (2) does not reveal that they are truly alike in causal terms. By relying on descriptions rather than substances, functionalism does not independently settle the ultimate source of mindful behaviors.

Thus, functions, teleological ones, are not multiply realizable, *because they are not even realizable*. As those morphologists like Owen taught us already in pre-*Origin* times, functions are perhaps ascribable, but this is a purely subjective act with little explanatory force. Biological kinds can only be individuated through the homological method with its structural and developmental criteria. The argument of MR could perhaps be saved if we appealed to a different type of function that has come to be known under the label of 'activity' (Love, 2007; Wouters, 2003). Unlike teleological functions that answer the question 'what is it for?', activities respond to the query 'how does it work?' and emphasize the dynamic aspects of biological structures instead of solely focusing on static formal aspects like shape. The notion is nowadays known for its use in Machamer, Darden, and Craver's (2000) account of mechanisms, where activities are assumed to be correlatives of entities and, therefore, realizable by them and, hence, potentially multiply realizable. The concept of activity, however, although not referred to with this term, was systematized in the field of functional anatomy by Bock and von Wahlert (1965), where the dualism entity/activity of Machamer et al. (2000) is not present. For Bock and von Wahlert, form and activity (i.e., function) are two essential and indissociable aspects of an entity (what they call a form-function complex). Activity in this sense is, as already pointed out, a strictly formal notion that just emphasizes the dynamic dimension of an entity, whereas form focuses on its static dimension (see Nuño de la Rosa, 2012 for a detailed analysis). There exist obvious parallels between Bock and von Wahlert's project and Cummins' (1975) non-teleological account of function, directed at singling out "dispositions" or "capacities" as the real flesh of the concept of function. Note that by defining function as a capacity of an entity that makes a partial contribution to a general capacity of a system containing it, Cummins paved the way to identify functions with how entities are/act and how they connect with neighboring entities. Thus general laws of organic form may eventually subsume functions (Cummins, 1975, pp. 758–759), a very different position from the assumption that they are "correlatives."[9]

From any of these perspectives, activities are perhaps *performed or executed*, not realized or implemented, because they are not independent from the entities that perform them. And the independence of function is essential for any argument in favor of MR to work. Perhaps the very idea of realization was not a very good one in the first place.

6. Conclusion

There is probably a strong and natural penchant for finding similarities among unequals, so strong that we easily take as the same what seems alike to us. It is this, we think, what ultimately explains the sort of claims raised in recent times by functionalist thinkers, according to which an identity class extends from organic brains to disembodied souls, through digital computers and virtual automata, insofar as their workings can likewise be described by charts of transitions among internal states. Such a stance allows one not just to think about brains as if they were computers or about computers as if they were brains, but to affirm that brains are computers or that computers are brains, depending on one's particular object of interest or preferred linguistic convention. But this is a view on the mental that relies on an extremely problematic position about the nature of things, according to which being the same or being different, i.e., complying with the same description or different ones, is simply in the eye of the beholder.

In this paper we have argued that, fortunately enough, it is easy to avoid this by taking note of Owen's ideas. Philosophers, cognitive and computer scientists, and anyone intrigued by or concerned with the mind should take advantage of Owen's insights, which mostly boil down to the idea that in order to make solid identity claims we need to look deep into the nature of things, instead of simply being confident in our own ingenuity for fitting them under the same descriptions. This is not to deny that descriptions have an important role in guiding empirical enterprises—of course they do—but empirical matters have ultimately to do with the nature of things, not with the nature of descriptions.

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Notes

[1] Our focus is thus narrower than most current discussions of MR in that it is restricted to the case against MR of mental states, while it remains agnostic with respect to other putative cases of MR in the natural sciences.

[2] MR actually comprises a family of arguments, of which Putnam's argument for functionalism remains the most powerful and influential one. For a complete account of these arguments, see Bickle (2008).

[3] Hardcastle's (1999, chapter 7) position with respect to this issue is much more radical.

[4] Chomsky has made a similar point with respect to the concept LANGUAGE in, for example, Chomsky (1975, chapter 1; 1995, p. 22). This is implicit in his distinction between the science-forming capacity and the capacity of common sense as elements of human nature in which the operations of different faculties converge. See McGilvray (1999, chapter 2) for an overview.

[5] According to Polger's (2004, chapter 5) comment, when viewed from a biological perspective, Putnamian minds are abstract things, which makes them very dubious entities from the point of view of causation. This is a peril to which, for example, Kim (1998, 2005) drew attention in his discussion of the principles of causal exclusion and of causal closure of the physical domain, and which has motivated his functional reductionism.

[6] Bickle's contentions have been challenged by Aizawa (2007), but on the basis of a concept of identity no molecular biologist would accept. Indeed, molecular biologists, Aizawa's recommendations notwithstanding, continue to talk about homologous proteins, orthologous and paralogous genes, and so on, because they have developed *their own biological criteria* to establish such relations; see also Bickle (2010) for a similar reply. This is not to say that homology at the molecular level is not a debated issue (e.g., Abouheif, 1997; Hillis, 1994; Holland, 1999; Patterson, 1988), but just that whatever homology is assumed to be by molecular biologists, it is certainly quite far from Aizawa's understanding of the notion.

[7] A good example is Shapiro's (2004) appeal to universal and historical constraints to justify the idea that convergent evolution is a more widespread phenomenon than traditionally assumed and, consequently, that the likelihood of the MR thesis is severely undermined.

[8] Considering Matthen's (2007) reflections, it might seem that a similar conclusion can be achieved by adopting the alternative historical view, at least from the vertebrate vision system downward. Matthen defends the claim that the different visual systems of vertebrates are specializations (thus, "homologues") relative to an ancestral counterpart from which they have departed for adaptive reasons. He also notes that this perspective serves to explain that specific types of visual systems (for example, color vision) may exist at a certain stage of evolution (the diurnal vertebrate ancestor of both birds and mammals), disappear at a

subsequent stage (ancestral mammals), and reemerge again within the same lineage (primates), given the specific challenges confronted by the corresponding species in their original environments. But contrary to Matthen's view, we think that such evolutionary intermittences are better explained by appealing to the biological perspective, as what they most probably illustrate is the silencing or activation of underlying developmental potentials. An interesting case to illustrate this position is the case of species that retain their generative potential for developing eyes (as demonstrated by the capacity of inducing ectopic eyes in other species) in spite of their having completely lost them (like *C. elegans*; see Gehring, 2005). Matthen's point of view leads to the position that the eyes of species that are descendants of eyeless organisms would have no homologues in other species, as they are not specializations of the eyes of an ancestor. Similarly, what for Shapiro (2004, p. 94) are mere convergent traits due to the action of universal constraints can be comfortably classified as homologies under the biological perspective defended here.

[9] Shapiro (2004) also appeals to Cummins-functions, but as the building blocks of his definition of realization. On our view, appeal to Cummins-functions or activities, at least as far as biological structures are concerned, makes it possible for the very idea of realization to be altogether eliminated.

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