MENTALISM VERSUS DUALISM: REPLIES TO COMMENTARIES

José E. Burgos
University of Guadalajara

ABSTRACT: The target paper’s main point is that mentalism and, to this extent, mentalistic (e.g., cognitive) psychology can only be materialistic and, hence, cannot be dualistic. The commentaries to the paper are insightful and stimulating. A few call for corrections, the rest for further clarification. Most criticize the mind-brain identity theory. This criticism is beside the point, as I did not intend to champion this theory (or functionalism), but only use it to illustrate how mentalism commits us to materialism. Still, all the criticisms of the theory are fallacious (ad hominem attacks against philosophers of mind, commitment to a particular ontology of causation and personhood). Other commentators criticize my focus on ontology, also fallaciously, by arguing ad populum (few scientists are interested in it) and name-calling that misrepresents ontology as anti-scientific. Overall, none of the commentaries invalidates the target paper’s main point.

Key words: mentalism, dualism, radical behaviorism

Mentalism versus Dualism: Replies to Commentaries

The commentaries to the target paper (Burgos, 2015) are insightful and stimulating. Some call for corrections to, others for further clarification of some specific claims, but none invalidates the target paper’s main point. Before my replies, it will be helpful to summarize the point: Contrary to what some radical behaviorists have claimed, mentalism (or cognitivism) is necessarily materialistic and, hence, cannot possibly be dualistic, not even “imply,” “invite,” “lead to,” or “open the door to” dualism. Nor can dualism coherently be a “form of mentalism,” or vice versa (cf. Lamal, 1993, p. 332). This point opposes radical behaviorists’

---

1 I thank all the commentators for their insightful and stimulating remarks. I also thank Peter Killeen for many illuminating conversations about the topics discussed here. Correspondence should be addressed to jburgos@cuba.udg.mx.
MENTALISM VS. DUALISM

rejection of mentalism as inevitably, or possibly, dualistic. We could reject mentalism for several reasons, but dualism is not one of them.

I use the term “dualism” in its standard philosophical sense to name the ontological view that divides reality into two fundamentally different kinds of entities (substances in Cartesian dualism, properties in property dualism): Material (or physical) versus immaterial (or nonphysical, or spiritual). In discussions of dualism thus formulated, “material” usually means to have extension in 3-dimensional space (length, breadth, and depth). On this use, only the physical-nonphysical duality is dualistic. Other dualities often regarded as necessarily “dualistic” (e.g., brain-body, objective-subjective, private-public) are not. Some (brain-body) cannot possibly be dualistic; others could be only if mind is nonphysical, but they do not commit us to this view. Mind can also be subjective and private, but still be physical. The most discussed form of dualism in this sense of the term is Cartesian dualism, after Descartes, who devised the so-called “Real Distinction Argument” to prove the division between a mind as an unextended (thinking) substance and a body as an extended (non-thinking) substance.

Mentalism too is an ontological view, or at least has a strong ontological aspect, in that some of its defining theses are about the nature of mind (e.g., causal power and internality). Hence the paper’s focus on ontological discussion: The ontological import of dualism and mentalism call for such a focus, even if the radical behaviorists’ rejections of both also have epistemological and linguistic aspects. However, these have been far more widely discussed than the ontological aspects, and can thus be safely put on hold for the purpose of thematic delimitation, for future discussion. Whatever they are, they will not invalidate the paper’s main point that mentalism cannot possibly be dualistic in the standard philosophical sense of the term. This point obtains even if mentalism suffers from the same epistemic and linguistic problems as dualism. Dualism’s epistemic and linguistic problems are not the same as, nor do they conversely entail, dualism.

The point is much stronger than some mentalists’ (e.g., Fodor, 1968; Sperry, 1980) warning that they can be a materialists, or that mentalism and materialism are not mutually exclusive. Their warning allows for the possibility that a mentalist can be a dualist, or that mentalism and dualism are compatible, which is what worries radical behaviorists. They do not deny the possibility of materialistic mentalism, but they worry that it is uncomfortably close to dualistic mentalism. Mentalists themselves are partly guilty, for not seeking a final and complete break-up with dualism. The target paper does precisely that by arguing that mentalism and dualism are mutually exclusive.

The expression “dualistic mentalism” (e.g., Moore, 2013; Schnaitter, 1984, p. 10; Terrace, 1984) thus becomes an oxymoron. So does “spiritualistic mentalism”

2 I distinguish between Descartes’ dualism and Cartesian dualism, following some authors’ (Baker & Morris, 1996) argument that the latter is a “caricature” (p. 159) of the former. Still, my focus is on the caricature, as it is the most familiar and discussed view, as distorting of Descartes’ real views as it might be. The caveat is that one must be careful not to identify Descartes’ with Cartesian dualism.
BURGOS

(e.g., Mallot & Shane, 2016, p. 396), if “spiritual” means “immaterial” or “nonphysical.” And the expression “materialistic mentalism” (e.g., Mallot & Shane, 2016, p. 396) becomes redundant. Thus, contrary to Uttal’s (2004) metaphor, dualism is not a “sin” of mentalism. Rather, to paraphrase Fodor (1975, p. 4), confusing mentalism with dualism is the behaviorists’ sin; but then again, mentalists are partly guilty of promoting this confusion by implicitly allowing for a position such as dualistic mentalism. On the main target paper’s point, such a position is logically impossible.

Not that “mentalism” is synonymous with “materialism,” as not all forms of materialism are mentalistic: Radical behaviorism is materialistic but antimentalistic (and antidualistic too). The proper labels, rather, are “mentalistic materialism” (or “mentalism,” for short) and “antimentalistic materialism” (or “antimentalism” for short; e.g., radical behaviorism).

Allowing for the possibility of dualistic mentalism has made it harder for mentalists to articulate and defend their commitment to materialism. As a result, their positions tend to be highly unintuitive, biased (e.g., Fodor’s formulation and defense applies only to a form of functionalism), and misguided (e.g., Sperry mistook reductive materialism to exclude mentalism). Exactly how a mentalist can be a materialist, then, has never been quite clear. My argument for the impossibility of dualistic mentalism clarifies the matter in a more intuitive and unbiased way that allows for quite different, in fact opposed forms of mentalism (viz., mind-brain identity theory and functionalism).

Mentalism cannot possibly be dualistic simply because two of its defining theses, mental causation and mind as internal (or internalism about the mind), are fundamentally inconsistent with dualism; hence the “versus” in the title. The other two defining theses, mind as subjective and nonbehavioral, are compatible with dualism but admit purely materialistic formulations that avoid dualism. In this summary, I focus on the first two theses.

Descartes’ contemporaries Princess Elisabeth and Pierre Gassendi first pointed out the incompatibility between mental causation and dualism in their criticism of Cartesian dualism. This criticism has become the standard reason to reject Cartesian dualism in the philosophy of mind, and is echoed by radical behaviorists, often by asking rhetorically how something physical can cause or be caused by something nonphysical. Philosophers of mind nowadays often make the criticism in terms of the working hypothesis that physical reality is causally closed (i.e., physical events can only cause and be caused by physical events).

This criticism does not apply to non-Cartesian forms of substance dualism, such as occasionalism and the theory of preestablished harmony, the precursors to today’s psychophysical parallelism (nondualistic forms of psychophysical parallelism are also possible). Although they keep a physical-nonphysical partition of substances (which defines Cartesian dualism), they reject mind-body causal interaction (also defining of Cartesian dualism). Nor does the criticism apply to another form of non-Cartesian dualism, Baruch Spinoza’s double-aspect theory, according to which the physical-nonphysical division is to be applied to aspects, not substances.
The contemporary form of Spinoza’s theory is property dualism, which divides properties (not substances) into physical and nonphysical. Property dualism has been argued for in terms of so-called “zombie argument,” which postulates the possibility of creatures that are physically (bodily, neurally, and behaviorally) identical to us but lack any phenomenal consciousness (feel nothing). This possibility has also been argued to imply epiphenomenalism, the negation of mental causation. If mental causation defines mentalism, property dualism cannot be a form of mentalism (nor can be non-Cartesian forms of substance dualism).

All of this strongly indicates that dualism, as a partition of entities (whether substances or properties) into physical and nonphysical, is logically inconsistent with mental causation. Cartesian dualism, in particular, risks becoming self-contradictory by combining substance dualism with mental causation, especially under the common view of mental causation as efficient. For those who believe in mental causation, this outcome is fortunate: Far from committing them to dualism, it commits them to materialism.

The incompatibility between dualism and internalism about the mind seems equally clear, if not more. If being inside something requires spatial extension, how can an immaterial mind, which is spatially unextended, be inside a body or a brain? If something is inside something else, then both are physical. Therefore, physical reality is closed with respect to internality as well: Only something physical can be internal to something else physical. If mind is internal, then it is physical. Therefore, just like mental causation, internalism about the mind, far from committing us to dualism, commits us to materialism. If internalism about the mind defines mentalism, mentalism cannot possibly be dualistic and can only be materialistic.

The mind-brain identity theory (mental properties are brain properties) and functionalism (mental properties are causal roles), imply the internality of the mind. If, as asserted in the mind-brain identity theory, all mental states are brain states, and all brain states are internal (to the subject that has them), then all mental states are internal. Likewise, if, as asserted in functionalism, mental states are causal mediators between the inputs and outputs of a system, they are internal to it. Such implications indicate that both theories are mentalistic and, hence, materialistic and antidualistic.

Their logical links to mental causation are no less intuitive. Both theories have been traditionally formulated and defended in ways that invite, if not imply, mental causation. Mental causation defines functionalism: Mental properties are causal roles. Functionalism thus implies mental causation. The relation between mental causation and the mind-brain identity theory is equally strong. Smart’s (1959) formulation appealed to the scientific view of organisms as “physico-chemical mechanisms” (p. 142). This formulation has a strong causal import, insofar as

---

3 These two theses, mental causation and internalism about the mind, are logically independent: Neither one commits us to the other. One can thus coherently believe in mental causation and be externalist about the mind, or be an internalist about the mind and reject mental causation.
mechanisms are causal in nature. More recently, Polger (2004) asserted that a virtue of the identity theory is that it “provides a robust account of mental causation” (p. xxiii). All of this suggests that by committing us to mental causation, both philosophies are inevitably materialistic and, hence, antidualistic, although neither one commits us to any specific ontology of causation.

In sum, mentalism is not and cannot possibly be dualistic. Therefore, mentalistic, in particular cognitive psychology cannot be dualistic either. Antidualism thus gives no special advantage to radical behaviorism, as mentalism is equally antidualistic. As Fodor (1968) put it,

... once it has been made clear that the choice between dualism and behaviorism is not exhaustive, a major motivation for the defense of behaviorism is removed: we are not required to be behaviorists simply in order to avoid being dualists (pp. 58-59).

Radical behaviorists have overlooked this crucial point (or distorted it beyond recognition), to the detriment of radical behaviorism. If radical behaviorism is a better philosophy than mentalism, it is not because of antidualism. Radical behaviorists will have to look for other reasons to repudiate mentalism and advance radical behaviorism as the better philosophy. Now I proceed with the replies, in alphabetical order by the commentators’ last name.

**Baum**

**Baum** begins his commentary as follows: “someone could support dualism and not any interaction between the physical and the nonphysical.” Indeed, this is precisely what non-Cartesian substance dualists do to avoid mental causation (contemporary property dualism seems to entail epiphenomenalism, the negation of mental causation). I also agree with **Baum** that rejecting mental causation “seems of limited use in understanding behavior.” But I must also insist that mental causation, contrary to what some radical behaviorists have said (**Baum** included), championing mental causation (which I do in the target paper, although without adopting any particular ontology of causation) cannot possibly commit us to any form of dualism. Mental causation is at least logically independent of, perhaps even fundamentally incompatible with, both forms of dualism usually discussed in philosophy of mind (substance dualism and property dualism).

I also agree with **Baum** that “Dualism cannot serve any science, and certainly not a science of behavior—behavior analysis.” The elaboration is unnecessary, even misleading, as it suggests that behavior analysis is unique in being less served by dualism than, say, cognitive science. Dualism is equally a disservice to both, cognitive science and behavior analysis.

**Baum** then makes the following interrelated claims about dualism, folk psychology, and ordinary language, all of which seem very debatable to me:

- Descartes put forward a version of folk psychology;
- Folk psychology is dualistic;
MENTALISM VS. DUALISM

- Descartes has been widely influential;
- Ordinary or everyday language, in particular French and English, are dualistic.

Descartes could not have proposed a version of “folk psychology,” as there was no such notion in his time. Folk (or commonsense) psychology is not a fact or phenomenon, as Baum and others treat it, but a 20th century hypothesis about how we explain and predict others’ behaviors. The notion of folk psychology became standard in the philosophy of mind during the 1980s. Nowadays, there are two different (not necessarily opposed) forms of this hypothesis, folk psychology as a theory (the so-called “theory-theory”) and folk psychology as simulation. Neither one views folk psychology as dualistic (cf. Bloom, 2004).

Also contrary to what Baum and others say, Descartes’ dualism was not that influential. To begin with, all of the objections made by his contemporaries and published with his Meditations were very critical of his entire philosophical project, his dualism included. I mentioned Gassendi, but there were other luminaries, like Hobbes, Mersenne, and Arnauld, all of whom expressed major concerns. Arnauld’s objection, in particular, is traditionally viewed as having demolished Descartes’ entire project (cf. Christofidou’s commentary). Princess Elisabeth was another critic, but her objections were published much later. This is not to say that Descartes had no influence at all. He had some influence, but it was not as widespread as Baum and others claim. Nowadays, the influence of Cartesian dualism in philosophy and psychology is (and has long been) minimal.

As for the claim that ordinary language is dualistic, there are at least two arguments against it. One, Strawson (1959, pp. 90ff) argued that Cartesian dualism is inconsistent with the way pronouns work in ordinary language. For example, we say “I believe that it is going to rain” and “I have a headache,” which are attributions of mentality to the self, and could be viewed as presupposing some sort of dualism (although this view requires more support than it has received). However, we also say “I am six foot tall,” “I weigh 200 lb” “I have two arms and two legs,” “I am in the living room,” and so on, which attributes bodily features to the self that are clearly incompatible with its alleged immaterial character.

The other argument against the dualistic character of ordinary language is Ryle’s famous (1949) category-mistake argument. Ryle’s main claim, that Cartesian dualism is a category mistake, means that Cartesian dualism violates the way psychological ordinary language actually works. The implication is that psychological ordinary language does not work according to Cartesian dualism. Hence, Cartesian dualism is incompatible with ordinary language, for which ordinary language cannot be dualistic.

Furthermore, Cartesian dualism is the result of a highly sophisticated piece of technical philosophical argumentation (and argumentation is another thing philosophers do, not just definition) that by far exceeds the nontechnical character of ordinary language. To begin with, “substance” is a technical term in philosophy that has no counterpart in ordinary language (the closest is “thing,” but even uses of it in ordinary language lack many technical details of philosophical uses of
substance"). To claim that ordinary language is substance-dualistic is to ascribe it a technical character that it does not really have.

Baum also asserts, contrary to the target paper’s main point, that “mentalism inevitably entails dualism.” To this, I can only repeat the target paper’s main point: If mentalism asserts that mind is causal and internal, then mentalism excludes dualism (and idealism), as an immaterial mind cannot possibly be causal and internal. If mind is causal and internal, as mentalism asserts, it must be physical. Far from entailing dualism, then, mentalism entails materialism. Thus, I did not claim that the problem of mentalism was dualism. Quite the opposite: I argued that mentalism excludes dualism. Nor did I claim that the problem of mentalism was mental causation. How could I, if I argued that mental causation is a very good, perhaps the best available, way to avoid dualism? Internalism about the mind is an equally good way.

What I claimed was that the problem of Cartesian dualism was the incoherence of combining substance dualism with mental causation, which is very different. Again, there are two ways to eliminate the incoherence: Either keep substance dualism and reject mental causation (as in epiphenomenalism and psychophysical parallelism), or reject substance dualism and keep mental causation. The latter is what mentalists do. Mentalism thus hardly entails dualism.

Baum also questions the validity of my argument for materialism (p. 11). However, he does this by adding the premise that mental events are nonphysical. Obviously, this makes the argument incoherent. Such assumption, however, is nowhere to be found in my argument. Here is my argument again:

1. All mental particulars are events (no mental substances)
2. All events are causes or effects (causal determinism)
3. All mental particulars are causes or effects (deduced from 1, 2)
4. All causes and effects are physical (causal closure of nature)
5. All mental causes or effects are physical (deduced from 3, 4).

Here is Baum’s rendition of the argument, with his added premise numbered as (5) in bold and italics and the conclusion renumbered as (6):

1. All mental particulars are events (no mental substances)
2. All events are causes or effects (causal determinism)
3. All mental particulars are causes or effects (deduced from 1, 2)
4. All causes and effects are physical (causal closure of nature)
5. All mental events are nonphysical (Baum’s added assumption)
6. All mental causes or effects are physical (deduced from 3, 4).

Clearly, (5) contradicts (6), but then again, adding (5) is a gross distortion of my argument. Nowhere do I assume (5). Nor do I assume that mental events are physical; otherwise, I would have begged the question. It is Baum who adds (5), but why does he? Only a dualist would want to do that, but he is no dualist. Nor am I. I was thus mystified by this part of his commentary.
Baum then rehearses his extended-activities account and how it allows us to either dispense with (i.e., endorse eliminativism about) the mind, or view the mind as behavioral in nature (I am unsure which is it). His account, he claims, calls for a different mode of causation (final, rather than efficient), whereas my focus is on efficient causation. It would take me too long to discuss this claim here in the detail it deserves. I will only say that my focus only follows philosophical tradition: Most if not all philosophical discussions on mental causation in particular and causation in general focus on efficient causes, to the point that the term “efficient” has fallen in disuse and discussions are just about “causation.”

A major reason for such focus is the widely adopted metaphysical conjecture that events are the relevant ontological category for causation. That is to say, causal relations are assumed to obtain between events. Causes as events are not the kinds of entities of which it makes sense to say are material, formal, or final causes (the latter due to the temporal asymmetry of causal relations, where causes are supposed to occur before their effects). Discussions on mental causation in the philosophy of mind are no exception to this tradition: It is standard to assume that mental particulars (i.e., concrete, specific, individual entities) are events. I adopt this conjecture in my argument for materialism (Line 1) in the target paper (p. 11), to axiomatically deny mental substances and, with it, any form of substance dualism (whether or not Cartesian). Exactly how substances differ from events remains a subject of much discussion in metaphysics, but the distinction is standard and I have no reason to reject it. Nor do I have any reason to reject the assumption that mental particulars are events, and I have a very good reason to adopt it (as a working hypothesis): It allows us to reject the existence of mental substances.

Of course, much hinges on the ontological nature of events and causation, about which there is much philosophical literature, too extensive to do it justice here. I will only say that embracing mental causation does not commit us to any particular ontology of events or their causal relations. Mental causation can be fleshed out with any ontology of events and their causal relations. Regarding events, specifically, and according to all available ontologies of events, all events are temporally extended and can have any temporal extension. A soccer match is as much an event as an electron’s spin change, and a single barpress is as much an event as a sequence of barpresses. Hence, viewing mental particulars as events does not commit us to molarism or molecularism about the mind, contrary to what Baum and Rachlin might believe.

Baum ends his commentary with the claim that “mind-brain identity theory or functionalism ... seem nonsensical to anyone other than philosophers.” To this, I must clarify that I did not intend to champion either theory in the target paper. I only used them to illustrate how their links to mental causation and internalism about the mind committed us to materialism and, hence, avoided dualism. Baum’s criticism of these theories, then, is beside the target paper’s main point and invalidates nothing of what I said in it. Still, I must say that the criticism is fallacious, little more than an ad hominem attack against philosophers of mind.
BURGOS

(they are wrong because of what they are, philosophers of mind). The criticism thus provides no valid argument against either theory.

Christofidou

In contrast to many, perhaps most, behavior analysts, I have much respect for philosophy of mind and metaphysics. Thus, although I am very fortunate to have received commentaries from some behavior analysts, I am equally fortunate to have received commentaries also from Descartes scholar Christofidou. Like me, she also has much respect for metaphysical discussion as integral to the philosophy of mind. Her commentaries thus introduce a much-welcome balance amidst extensive contempt towards philosophy of mind and metaphysics among the behavior analysts.

Christofidou organized her commentaries into three parts. In the first part, she points out that the standard argument for Cartesian dualism cited in the target paper (p. 6) is a misinterpretation of Descartes’ argument. In this misinterpretation, the “Real Distinction Argument” is identified with the “Argument from Doubt,” whereas the two are not the same, Christofidou argues. The Argument from Doubt is “a misrepresentation by Antoine Arnauld,” a contemporary of Descartes’ who critically commented on his Meditations.

I find this part of Christofidou’s commentary very illuminating and deserving of much attention, with wide implications for how Descartes is discussed in and outside of philosophy. The bottom line is that Descartes has been grossly misrepresented and this calls for some amendments and further clarifications to my discussions of Descartes in the target paper. I must begin by recalling (as Christofidou does) that, at the time of writing the target paper, I was well aware of how misrepresenting Cartesian dualism was of Descartes’ dualism. In Note 6 (p. 7) of the target paper, I acknowledged opposition to the official story about Descartes’ dualism, and even cited some of Christofidou’s work on this.

I also clarified that I was going to stick to the official story, as nothing I was going to say depended on Descartes’ actually having said it. In the summary to these replies, I further clarified that I stick to the official story, as misrepresenting of Descartes’ views as it is, because it is the most familiar and widely discussed version of dualism. All of this calls for a more explicit separation between Descartes’ dualism and Cartesian dualism, clarifying that the two should not be confused.

But, as Christofidou rightly asks, why bring Descartes in at all? Well, I brought him in not to attribute Cartesian dualism to him, but only to acknowledge a widespread and old historical practice that has become standard in philosophical discussion about the mind. It is a very unfortunate practice, to be sure, and I am putting my two cents in here to oppose it. To do this, however, I have to bring Descartes back in, if only to insist that this thing called “Cartesian dualism,” although a misrepresentation of Descartes’ dualism, still is the most familiar and discussed form of dualism. Perhaps, we should ask not why bring Descartes in, but why call “Cartesian dualism” “Cartesian.” Indeed, it may well be a misnomer.
Unfortunately, it stuck, and I am not inclined to seek a new name. But, yes, perhaps we should begin making it up to Descartes by replacing “Cartesian” in “Cartesian dualism” for another, more accurate label.

Still, Christofidou’s points on this are well taken. The relevant correction in this regard to the target article is to separate more explicitly Descartes’ Real Distinction Argument from Arnauld’s misinterpretation of it as an argument from doubt. In the version of the argument cited in the target paper (p. 6), this separation can be achieved by eliminating all the lines that depend on doubting, and on clear and distinctive perceptions, to leave the purely metaphysical parts. As Christofidou (2013) clarifies, “Descartes is concerned with matters of metaphysics, with things or substances, their nature and individuation. Concern with epistemology [which underlies an argument from doubt and clear and distinctive ideas] is secondary to this” (p. 1).

There also is Christofidou’s claim that Cartesian dualism is a straw man. However, I am not so convinced. Although a misinterpretation of one individual’s views (Descartes’), it does not seem to be as bereft of followers as Christofidou claims. Although Descartes was not a Cartesian dualist, some of his followers might have been. For example, as Schmaltz (2004) discusses, French philosophers like Robert Desgabets (1610–1678) and Pierre-Sylvain Régis (1632–1707) seem to have been Cartesian dualists. They seem to have propounded modified versions of Descartes’ dualism that echo Arnauld’s interpretation of Descartes’ dualism as an argument from doubt and the thesis of mind-body causal interaction as efficient. There also are contemporary defenses of Cartesian dualism that do the same (e.g., Dilley, 2004; Foster, 1991; Hart, 1988).

None of this changes my key claim that Cartesian (not Descartes’) dualism, despite having an epistemic component (mistakenly attributed to Descartes), remains a metaphysical thesis. As such, it calls for a metaphysical assessment that does not admit a smooth reduction to epistemic or linguistic considerations. Thus, contrary to what Leigland and Marr claim, Cartesian dualism cannot be rejected on purely epistemic or linguistic grounds. There is an irreducible metaphysical aspect to dualism that cannot be explained away linguistically or methodologically. Such aspect is the focus of the target paper.

Christofidou also seems to agree with my assertion that Descartes never said the mind was internal to the body. She argues, correctly in my view, that the very few passages where Descartes appears to do this (see target paper, p. 14) are taken out of context and just speculations about how the mind could relate to the brain, more than a systematic internalism about mind. On this argument, then, internalism about the mind, so often ascribed to Descartes, is yet another gross misinterpretation of his dualism. That this is the case seems obvious from Descartes’ key proposal, of which Christofidou kindly reminds us, that mind and body form a substantial “union” or “fusion,” which is clearly incompatible with the notion of mind as internal. Even if Descartes had really proposed that being internal to the body was essential to mind (a big “if”), it would, as I have argued, make his dualism patently incoherent.
All of this supports my conclusion that internalism about the mind is fundamentally incompatible with Descartes’ dualism. As for the caricature, Cartesian dualism, my focus in the target paper, it is quite often associated with internalism about the mind. However, my key claim in the target paper is that such association makes Cartesian dualism incoherent, regardless of whether Cartesian dualists really maintain it. If they do not, they are being misrepresented. If they do, they are being incoherent. Either way, the radical behaviorists’ accusation of dualism against internalism about the mind misses the mark. The true accusation is that internalism about the mind makes Cartesian dualism incoherent, not that internalism is dualistic.

Relatedly, Christofidou rejects my assertion that Descartes viewed the soul’s continued existence after bodily death as a matter of fact, which I made to further support the conclusion that he could not have viewed being inside the brain as essential to the mind. Alas, my assertion was too strong. As Christofidou clarifies, Descartes only argued for the logical possibility of such existence, which only allowed for the “hope” for an afterlife (albeit one bereft of any feelings, emotions, and bodily sensations, as Descartes believed all of this required a body). Descartes admitted that it was equally logically possible that the soul ceased to exist with the physical body’s demise.

None of this, however, invalidates my conclusion that Descartes could not have viewed being inside the brain as essential to the mind, for if he did, he would have precluded even the logical possibility of an afterlife. Again, in his view, only thought, which is to say inextension (lack of “length, breadth, and depth”) was essential to mind. In Descartes’ view, nothing else (being internal to or causally interacting with the body, surviving corporeal death, being subjective) was.

In the second part of her commentary, Christofidou gets into the metaphysics of causality. Most directly related to my discussion of this topic in the target paper is her claim that the Elisabeth-Gassendi criticism, which alleges the incoherence of physical-nonphysical causal interaction, is yet another misinterpretation of Descartes’ dualism, and thus leaves it unscathed. She leaves a defense of this claim to a forthcoming paper, but I am sure it will be compelling. This claim seems to go against my key claim in the target paper that mental causation is inconsistent with dualism.

However, once again, my focus is on Cartesian dualism as the official, most familiar and discussed interpretation of Descartes’ dualism, as mistaken as this interpretation, and as much of a misnomer the qualifier “Cartesian,” are. The key, I insist, is not to identify the two. My reply to this part of Christofidou’s commentary is that it does not invalidate my point that mental causation in Cartesian dualism is efficient causation and, as the Elisabeth-Gassendi criticism points out, it is inconsistent with mind as nonphysical. In short, the criticism may not apply to Descartes’ dualism but it applies to Cartesian dualism. All of this supports my key point that Cartesian dualism, as the conjunction of substance dualism and mind-body causal interaction, is incoherent (cf. Foster, 1991, pp. 159–163).
MENTALISM VS. DUALISM

On this same topic, Christofidou also questions the principle of causal closure of the physical, which I use in my argument for materialism, as “unscientific and unphilosophical.” However, much hinges on what she means by “unscientific” and “unphilosophical,” which would require a specification of the nature of science and philosophy, no less. If by “unscientific” she means “unsupported by empirical evidence,” such meaning is nowhere in the philosophy of science, although much also hinges on what is evidential support.

Under a standard sense, there has never been any evidential support for the phlogiston and ether conjectures. Yet, under an influential demarcation criteria in classical philosophy of science, falsifiability, both conjectures qualify as scientific, even today (when scientists reject both), because both have implications that are empirically testable. Let us not confuse scientific with empirically supported. Under falsificationism, all empirically falsified scientific theories retain their scientific status.

Similar considerations apply to the causal closure of the physical: If suitably augmented with methodological assumptions about the observability of the physical, it could be taken as a working hypothesis with empirical implications. They could be, for example, observed correlations between certain kinds of events under certain conditions (e.g., orderly changes observed in a dependent variable after systematic manipulations of an independent variable under controlled experimental conditions). Such correlations could be taken as indicators of causal relations. If one could formulate the causal-closure hypothesis in a way that entails non-circularly such indicators, it would be scientific, at least by traditional standards in the philosophy of science, regardless of how much evidential support it has. It remains to be seen whether such formulation is possible, but I would not dismiss the possibility without giving it some thought.

As for the “unphilosophical” character of the principle, much hinges on the differences and relations between science and philosophy. Under a widespread stereotype, science and philosophy are mutually exclusive. Under this stereotype, if the causal-closure principle is scientific, as I have argued, then it indeed is “unphilosophical,” as Christofidou claims. If we do not want to perpetuate this stereotype, I think the principle lends itself to some fleshing-out that has been traditionally viewed as philosophical, without losing its scientific status. For example, it could include an explicit metaphysics of causation, which traditionally is more of philosophical (in the sense of a priori) than scientific.

Granted, all of this is interpretative, but so is Christofidou’s claim. I am compelled to state the obvious here: There are multiple views of the nature of science and philosophy, and how they differ and relate, all of them subject to debate. Christofidou certainly is entitled to her views, which others might share. But I am equally sure that her views on such matters are not the only ones, and are far from perfect. Alas, she does not state them, so I cannot discuss them here. My point is that all of this calls for a more thoughtful and thorough analysis, very different from the kind of hasty, single-stroke kind of rejection Christofidou seems to advise.
I also disagree with her claim that the principle “begs the question against ... the reality of mind or consciousness.” Rather, the principle (qua working hypothesis) serves as a logical step toward a deductive demonstration of the existence of mind or consciousness as material. Additionally, as I understand its philosophical technical sense, begging the question is a kind of fallacious argument also known as circular, whereas the principle in and of itself is a statement, not an argument. Only arguments, not their constituting statements, can beg the question in that technical sense. To beg the question in this sense, at least two statements are necessary, one as a premise, the other as a conclusion derived from that premise. For example, the following argument quite obviously begs the question: “Some humans are psychologists. Therefore, some humans are psychologists.” However, the assumption “Some humans are psychologists” in and of itself begs no question, any more than does the assumption “All humans are mortal.” Similarly, there is no circularity in the statement “only physical events cause, or are caused by, physical events,” or, perhaps more clearly, “For all events, if an event causes another, then both are physical.” On this latter formulation, the principle expresses an ontological correlation between being a cause or effect, and being physical. There is no circularity here.

Equally debatable is Christofidou’s depiction of Kant as a dualist, which she presents in the third part of her commentary. Her depiction only perpetuates the kinds of abuses I pointed out in the target paper (p. 18) of the term “dualism,” where all dualities are treated as dualistic. Kant’s analytic-synthetic, a priori-a posteriori, noumena-phenomena, scheme-content, and pure-practical distinctions certainly are dualities in the sense of being dichotomies or two-category distinctions. However, they are not dualistic in the standard philosophical sense of a physical-nonphysical partition of reality in and of itself. None of Kant’s distinctions implies or presupposes such partition. Hence, Kant is not a dualist in this sense of the term.

Nor can we correctly see him as a dualist about the mind or the self either. Christofidou is right in that there is no suggestion that Kant viewed the mind as material or physical, but I did not say the contrary. Nor did I say that Kant viewed the mind as nonphysical either. As I said in the target paper, following some interpreters of Kant, he is no dualist or materialist (he regarded himself as a sui generis idealist), if being any of this means to view things in themselves (noumena) as inherently material or immaterial in nature. Remember, he viewed space as a purely subjective form of intuition. Again, dualism and materialism (and Berkeleyan idealism) are views about how things are in themselves, inherently, objectively. Such views are the very negation of Kant’s transcendental idealism that noumena are neither physical nor nonphysical. I thus stand by what I said

---

4 This argument is valid, as the conclusion can be justified by $p \implies p$ (the material conditional is reflexive), which is a tautology. Strictly speaking, then, it is not fallacious, if this means to be invalid. Rather, its circularity prevents us from making any progress in the matter at hand.
about Kant in the target paper: Kant is not a materialist, dualist, or even idealist (despite the name he himself chose for his view) in the standard, objectivist, realist ways these views have been formulated in philosophy.

Finally, a few reactions to Christofidou’s commentaries on behavior. She clearly opposes the mind-brain and mind-behavior identity theories, as well as functionalism. I cannot launch a full critical analysis of this here. I will only say that her criticisms of the mind-brain identity theory, like those of Baum, Marr, and Rachlin, are beside the point, as I did not intend to champion it, but only use it as an example of how mentalism committed us to materialism, and how Cartesian dualism could not possibly be a form of mentalism. The same applies to functionalism. Nor did I mean to champion a mind-behavior identity theory. I discussed it only as an example of a thesis that radical behaviorists might wish to develop if they want to have a behavioristic metaphysics of the mind.

Still, I disagree with Christofidou that the mind-brain identity theory is “question-begging,” as I find no circularity in the statement “mental properties are brain properties.” I also disagree there is no empirical reason to give it some consideration, which is not to say that it should be accepted as a true. In fact, I agree with Christofidou that there is no a priori reason to accept the theory. However, this is by design. The original identity theorists proposed their theory not as a philosophical theory but as a working scientific hypothesis, pending empirical testing. Obviously, the fact that it has been proposed and defended by philosophers does not mean they intended it as true a priori. As a scientific working hypothesis, it is not supposed to be assessed a priori. Thus, strictly speaking, Christofidou’s claim that we have no a priori reason to accept the mind-brain identity theory does not apply, because no such reason is part of the formulation of the theory (perhaps except for parsimony). In sharp contrast, functionalists have always formulated functionalism as a philosophical theory to be accepted for a priori reasons. Christofidou’s claim thus applies only to functionalism.

As for empirical support, again, much hinges on what we mean by this, but I would say that the mind-brain identity theory presently has far more scientific support than the other theories, especially Descartes’ dualism. I wonder what kind of empirical reasons anyone could have to champion Descartes’ dualism. Whatever the reasons, it is far from obvious to me that they are more compelling than those for the mind-brain identity theory (viz., evidence from cognitive neuroscience). If Descartes viewed the mind as immaterial, I cannot envisage what kind of empirical evidence would support such a view more than a view where mind has spatial location, given the key importance of space in all scientific evidence. But then again, perhaps Descartes did not propose that, in which case, his dualism is something far more different than anyone has ever thought.

Leigland

Leigland begins his commentary by wondering “…whether a cumulative, progressive, and useful scientific field needs metaphysics or to address ontological issues” (emphasis mine). He seems to answer in the negative by claiming “…what
scientists working in a cumulative scientific field need are clearer empirical
questions, better scientific methods, more effective research technologies, and
explanatory practices that engage new directions, discoveries, and applications”
(emphases mine). This answer implies that scientists do not really need ontological
discussion.

As a scientist myself, I am for all that Leigland claims scientists need. However, it is more complicated. To begin with, much hinges on what is all that. Leigland does not say, so I do not know what he means by a “clearer empirical question,” “better scientific method,” and “more effective explanatory practice.” More generally, I do not even know his views on what is an empirical question, scientific method, or explanation. Of course, I have my views and they probably differ from Leigland’s, but this is beside the point. The point is that as soon as we start discussing our views, ontological issues arise rather naturally and quickly.

For example, according to a standard view, an empirical question is one that we can answer based only on some experience. But what is an experience? This question raises the ontological issue of the nature of consciousness. In addition, according to a standard view, a scientific method is one that is conducive to truth by correspondence with an objectively existing reality. This view raises the ontological issue of the nature of such reality. And at least one kind of explanation, quite common in science, propounds causal mechanisms to explain phenomena, which raises the ontological issue of the nature of causal mechanisms.

Clearly, then, asking what is all that which Leigland claims scientists need raises ontological issues. Does this mean that we “need” to address them and thus engage in ontological discussion? Not at all. Sometimes, one is forced to address issues under duress (e.g., thesis advisors requiring their students to address some issues in their dissertations, reviewers requiring authors to address some issues in their submitted manuscripts, all on pain of being rejected). But many times there is no such duress, so one is not forced to address any issues. Quite often, someone raises an issue and then anyone can address it, should they wish to do it. If no one does, so be it. No one is going to be executed or go to jail for it.

Nor am I one to tell others what they do or do not “need.” In particular, I have no business telling astronomers, physicists, chemists, and biologists that they “need” “clearer empirical questions,” “better scientific methods,” or “more effective explanatory practices,” whatever these are. To tell them all this implies that their questions are not sufficiently clear, their methods not sufficiently good, and their explanatory practices not sufficiently effective. I have no authority whatsoever to say any of this, much less how to improve their questions, methods, and explanatory practices. Perhaps Leigland does, but then again, he does not say what he means by any of that, so I cannot assess his claim.

What I am getting at here is that “need” is too strong a word. I, for one, do science and philosophy not because I “need,” or anyone else needs me, but because I want to do them, for fun, personal interest, and a sense that it is something important, larger than myself. Needs are also very complex: They can be very specific and depend heavily on myriad personal goals, tastes, and interests that vary widely across moments, individuals, groups, and circumstances. Because of
such complexity, it is very difficult to formulate a universal, reliable, valid criterion to assess needs effectively, objectively, and fairly.

A related danger of casting the matter in terms of needs is that, because they are relative to goals, moments, individuals, groups, and circumstances, they can very easily be used to dismiss, even ridicule, anything that differs from one’s needs: Quite often, someone’s needs are someone else’s repulsions. This situation quickly leads to a dangerous “who-needs?” game, which anyone can play with anything, not just ontological discussion, but also mathematics, science, art, college education, economics, ethics, Skinnerian operant analysis, experimentation, neuroscience, and quantum mechanics, just to mention a few examples. Leigland would be rightly offended by the question “Who needs that?” about his own work, and I would be the first to repudiate such a question as improper, at best.

To drive the point closer to home, I do not know whether physicists “need” a Skinnerian interpretation of physics, but it would not matter if they did not vis-à-vis its validity, as this would not invalidate the interpretation. Nor would it matter if they did, as this would not validate the interpretation either. Many, perhaps most behavior analysts do not need neuroscience, and this is fine: To each their own. I am no one to tell them they do or do not. However, it would be very egotistical of them to dismiss neuroscience as invalid or intellectually worthless just because they do not need it. It would be equally egotistical of them to dismiss quantum mechanics as invalid or intellectually worthless just because they do not need it. I do not need volcanology or speleology, but it would be very egotistical of me to dismiss them as invalid or intellectually worthless merely because they do not interest me.

Perhaps, then, it is better not to discuss the matter in terms of “needs.” Instead, let us see what issues have been raised, whether and how they have been addressed, whether we want to address them, and, if we do, where it takes us, without asking whether we “need” to do it. I do not know whether physicists “need” ontological discussion, even those who have engaged in it. Nor am I one to tell them they do or do not. All I know is that some have engaged in it and I want to learn why and how, and what they have achieved, because it might be helpful to address certain issues, or lead to interesting results, not only in quantum mechanics but also in my own research field. To ask whether we need it, or worse, others needed it, is pointless, even dangerous, as it promotes an academic prejudice that only hinders progress.

For example, Bohm and Hiley (1993) proposed what they called an “ontological interpretation of quantum theory,” in order to solve some of its “mysteries” (e.g., nonlocality, the wave-particle duality, etc.). In this interpretation, they adopt a realist view about a standard elementary distinction in metaphysics (concrete particulars vs. their properties) to address the ontological issue of the nature of the electron. The ontology in this book is very elementary, but it goes a very long way, which attests to the heuristic value of ontology. It is erroneous to restrict ontological discussion to technical academic ontology. Any discussion about the nature of anything is ontological, even if it does not use the academic ontologists’ technical terms and theories.
It is useful to distinguish between pure ontology and applied ontology. Pure ontology is the discussion of being in the most general sense, independently of science. Applied ontology is the reflection on and articulation of our views of the nature of specific parts of reality, such as those studied in science. Applied ontology may but need not use the technical results of pure ontology. However, applied ontology can be interpreted in terms of such results. Scientists’ ontological discussions fit more clearly in applied than pure ontology. It is a mistake to identify the two, but applied ontology is still ontology.

The full impact of this attempt on quantum theory remains to be determined but the book received much editorial praise:

“This book will, I believe, change the way quantum theory is taught.”

“I believe that this is a brilliant book, of great depth and originality. Clearly written, it provides an unusually incisive account of quantum phenomena. ... Every physicist and physics student who wants to understand quantum mechanics should read this book.” Sheldon Goldstein, *Physics Today*.

“Bohm and Hiley ... suggest a specific way of accommodating both relativity and quantum theory at the level of experiment. This is just one example of several provocative and clearly presented theses that make this book worth a careful reading.” James Cushing, *Foundations of Physics*.

“You will be very impressed by this wise and deep book that will certainly broaden your horizons and start you thinking about many things you thought you were sure of.” *Science*.

Such praise aside, my point here is that these two physicists took ontological discussion seriously enough to try it, and they are not the only ones. Other contemporary physicists who have engaged in ontological discussion in physics are Barbour (1999), Carroll (2016), ’t Hooft (1997), Rovelli (2014), Smolin (1997), Shimony (1993), and Tegmark (2014). Admittedly, they are relatively few. I thus agree with Leigland’s claim that “physicists are generally uninterested in such issues,” if by this he means that most physicists do not engage in this kind of discussion. But does this make it any less intellectually valuable to physics? Leigland’s claim suggests that he would give an affirmative answer.

The obvious problem with such an answer is that it would be a form of the *ad populum* fallacy, where intellectual value is treated as a popularity contest. Popularity, even among scientists, has repeatedly been shown to be a very unreliable predictor of intellectual value. The geocentric, luminiferous aether, and phlogiston theories enjoyed much popularity among scientists for a long time, but were eventually rejected. Newtonian particle mechanics too was very popular for over two centuries, but it was replaced by Einsteinian relativity. There also have been scientific theories that were very unpopular among scientists for a long time, but were eventually widely accepted. Heliocentrism was very unpopular among astronomers for almost two centuries after Copernicus proposed it, but eventually became widely accepted.

As an example closer to home, physicists are generally uninterested in a Skinnerian interpretation of science (an internet search for physicists who discuss,
MENTALISM VS. DUALISM

let alone accept, this interpretation returns zero hits). Hence, by Leigland’s popularity criterion, this interpretation is irrelevant to physics. Surely, this is preposterous. The value of the interpretation to physics is not to be judged by how popular or interesting it is among physicists, but by its conceptual, methodological, empirical, and theoretical credentials, and whether and how it helps us better understand, perhaps even improve, physics in particular and science in general.

Of course, sometimes, high popularity coincides with high intellectual value, and low popularity with low intellectual value. Nowadays, relativity and quantum mechanics are widely popular among physicists, and the theory of evolution by natural selection (with genetic inheritance) is widely popular among biologists. All these theories have been for a long time, and still are, intellectually very valuable, not for being popular, but for having improved our understanding of reality. Geocentric, phlogiston, and luminiferous aether theories used to be very popular, but have been, and remain, very unpopular among scientists, and were ultimately shown not to improve our understanding of reality. All of this supports my claim that popularity is a very bad predictor of intellectual value.

So much, then, for Leigland’s popularity criterion against the intellectual value of ontological discussion to physics. Its low popularity does not entail that it is not intellectually valuable to physics. As suggested, ontological discussion can help physicists systematically identify and address conceptual issues about the nature of certain parts of reality, such as electrons. Ontological discussion among physicists has also revolved around the nature of space and time. These discussions, again, are ontological not because they involve technically advanced academic ontological terminology or theories, but because they are about the nature of certain parts of reality.

Ontological discussion is also common in other natural sciences. The discussion over the nature of species among evolutionary biologists (e.g., Ghiselin, 1997; Mayr, 1988) is a clear example of a major ontological discussion in biology. This discussion led to an ontology of species as individuals (an ontological category), which biologists view as being more consistent with the theory of evolution by selection.

Another example, less known but no less instructive, is the discussion over the nature of command neurons in behavioral (“integrative”) neuroscience. In a seminal paper, Kupfermann and Weiss (1978) raised the issue and proposed their influential definition of a command neuron as a neuron that was “necessary and sufficient” for certain behaviors (unconditioned escape reflexes in marine invertebrates and vertebrates). This discussion, little known outside of neuroscience, even by philosophers of neuroscience, took center stage in behavioral neuroscience for decades. It inspired a trove of empirical research that eventually showed that command neurons thus conceived did not exist (not much unlike the Michelson-Morley experiments that suggested that the luminiferous aether did not exist, which paved the way to the special theory of relativity). This evidence led to a different view of the nature neural command (see DiDomenico & Eaton, 1988). This debate illustrates very well how ontological discussion can directly inspire empirical research.
In sum, ontological discussion is not as irrelevant or alien to natural science as Leigland seems to believe. Granted, it is relatively uncommon, but then again, this does not make it any less relevant. To argue otherwise would be to commit the ad populum fallacy.

Ontological discussion is also present in psychology, not just cognitive, but also behavior-analytic. The issue of the nature of the proper subject matter of scientific psychology is a major ontological issue widely discussed by psychologists. In particular, the behavior analysts’ thesis that everything psychological, from emotion to thinking, problem-solving, and concepts, is behavioral in nature, is a way to address that ontological issue, and a deep ontological stance as well.

In Keller and Schoenfeld’s (1950) seminal book, we find this statement:

The Nature of Drives

To the question What is drive?, we must now answer that drive is the name for a fact the fact that certain operations can be performed on an organism (for example, depriving it of food) that have an effect upon behavior which is different from that of other operations. Drive is not a thing, but simply a word we use to show our recognition that behavioral functions which may depend on reinforcement are also modifiable by another influence, one exerted by occurrences which do not involve reinforcement (p. 265).

The question is ontological in that it asks for the nature of a drive. The authors’ matter-of-factly answer is equally ontological in that it propounds the true nature of drives. More generally, questions of the form “What is X?” are typical ontological questions, and matter-of-factly answers to them too are ontological. Behavior analysts use the expression “the nature of” quite often. An internet search for it in the website of JEAB (all issues and fields) returned 947 results (individual articles). A search in JABA returns 398 results.

The evidential character of such uses does not make them any less ontological. Let us dispel yet another myth about ontological discussion, that it is necessarily non-empirical or a priori. Much ontological discussion certainly is non-empirical, although, again, much hinges on what is empirical, an ontological question itself. However, it is quite common, especially in natural science, behavior analysis included, to ask and answer ontological questions empirically, based on evidence. Such empirical character does not make them any less ontological. Berkeleyan idealism is an ontological thesis, despite being rooted in empiricism, an epistemological thesis.

Other expressions for “the nature of” are “the essence of,” and “is essential to.” These expressions indicate the presence of essentialistic thinking, or something near enough, which dates back to Aristotelian philosophy (Aristotelian metaphysics is explicitly and entirely essentialistic) and is widespread in philosophy. In this way of thinking, intrinsic properties are divided into essential and accidental. Both are supposed to be possessed objectively, independently of our attempts to know them. An essential property is an intrinsic property that
MENTALISM VS. DUALISM

makes something (typically, but not always, a substance, e.g., an electron, hydrogen atom, water molecule, planet, star, rock, mountain, tree, dog, person, etc.) the kind of entity it is. There thus is a close relation between essentialistic thinking and belief in natural kinds. Essentialistic thinking and discussions is present in physics, chemistry, and cellular/molecular biology. Such presence further supports my point that ontological discussion runs deep in natural science.

Behavior analysis is no exception to this. In Keller and Schoenfeld’s (1950) book, we also find this claim: “... a contiguity of stimuli, or of stimulus and response, is essential to Type S conditioning or operant discrimination” (p. 124); and this claim: “Generalization within classes and discrimination between classes this is the essence of concepts” (p. 155). Such essentialist claims are ontological and often found in behavior analysis in general (for “the essence of,” 60 results in JEAB and 22 in JABA; for “is essential to,” 98 occurrences in JEAB and 56 results in JABA). Such talk suggests that behavior analysis is as essentialistic as is mentalistic psychology (cf. Palmer & Donahoe, 1992).

In the last chapter of his book, Keller (1973) discussed “the problem of definition.” He admitted that the problem was key, and I agree, although it is unclear to me exactly what he thought the problem was. In philosophy, the oldest and most general problem is quite clear, or at least explicit: What is a definition? Much of what I have discussed in these replies comes down to this problem.

Traditionally, two kinds of definitions have been discussed in philosophy, real and nominal. A real definition is a statement about the essence of the entity being defined, and it is commonly found in the kind of ontological discussion that I have claimed is present in natural sciences. As such, real definitions can be true or false. Real definitions are central to essentialism, and ontological discussion over the nature of things. To ask and provide real definitions is to accept essentialism in some form or other; and, to hold essentialism is to seek out real definitions. Many ontological discussions are disagreements over real definitions.

Nominal definitions, in sharp contrast, are arbitrary linguistic conventions that only seek to abbreviate and, thus, save time and space. They are the kinds of

---

5 Essentialism in these sciences is largely unproblematic, and works well (cf. Popper, 1957). I would say this about even an extreme form of essentialism where essences, to put it more modernly, are necessary and sufficient properties (not conditions) for something to be the kind of thing it is. Essentialism, especially in such extreme form, becomes more problematic with respect to the nature of species in evolutionary biology, although this does not mean it is completely out of the question (species may not be essences, but they, as individuals, could still have essences). Essentialism is equally if not more problematic in psychology, so it is better pursued cautiously and with moderation, viewing properties as more likely necessary than sufficient, and hypothetically so, based on evidence. This is no small task, given the great complexity of psychological phenomena. Some might argue that such complexity hinders the search for psychological essences. I agree with this, if it means that the quest for psychological essences may never end. However, this does not mean that we should not pursue such a quest at all: We can still do our best to get as close as possible to finding psychological essences, even if we may never find them.
definitions that we find in logic (e.g., the definition of material conditional in terms of conjunction and negation) and mathematics (e.g., the definitions of “prime number,” “odd number,” “divisibility,” “sine,” “addition,” “triangle,” etc.). As such, they are not statements that can be true or false. Nominal definition can only be clear or unclear, qualitative or quantitative, useful or useless, but never true or false, or factually correct or incorrect.

Nominal definitions are also commonly used in natural science to introduce new technical terms. For example, Heinrich Wilhelm Gottfried von Waldeyer-Hartz coined the term “neurone” to abbreviate the somewhat longer expressions “nerve-element” and “nerve-unit.” Later, English-speaking neuroscientists shortened it to “neuron” and defined it with the longer and more detailed expression “cell that has at least an axon and fires action potentials under certain conditions.” Sir Charles Sherrington coined the term “synapse” to abbreviate the longer expression “the mode of nexus between neurone and neurone.” James Watson and Francis Crick coined the term “deoxyribonucleic acid” (further abbreviated as DNA) to abbreviate the much longer expression “molecule that consists of a chain of nucleotides linked through covalent bonds in a double-helix manner, which carries the genetic instructions for the growth, development, functioning and reproduction of living organisms and many viruses,” and so on. These examples illustrate the obvious role of nominal definitions: To save space and time.

The question thus arises: Which definitions in behavior analysis are real? Is Keller and Schoenfeld’s (1950) definition of drive and concepts intended as real or as nominal? What of Keller’s definition of psychology? And the definitions of operants as response classes versus individuals? There are no obvious answers, but if debate is an indicator, definitions that are the subjects of debate are likely to be intended as real. Otherwise, why debate over them? If they are, debates over them are ontological. The discussion over the nature of operants (response classes vs. individuals), for example, is an intensely ontological discussion within behavior analysis (see Baum, 2002).

Leigland also comments on my assertion “After all, they do not reject causation per se, only causation conceived in a certain way, as functional relation” (p. 31). However, I must correct this assertion. What I meant to say was that radical behaviorists (following Ernst Mach) do not reject causation per se, but only causation conceived in the traditional metaphysical way, as a necessary connection. They admit causation in another sense, as functional relation. My suggestion in the discussion in the target paper was that they could use this view of causation to articulate a behavioristic approach to mental causation, based on a mind-behavior identity theory.

For example, suppose, as stated in the mind-behavior identity theory, that pain is pain behavior (which too is an essentialist and, hence, ontological claim), and pain behavior is functionally related to certain environmental contingencies. From this, a behavioral view of mental causation obtains where pain behavior causes certain environmental changes (e.g., make someone help us). In this view, some effects of the mind would be environmental. That is, mental causation,
behaviorally viewed, would be a species of behavior-environment causation. Other effects could be behavioral (e.g., pain could control certain behaviors that eliminate or reduce the environmental cause of the pain).

Still, Leigland’s commentaries on this serve to further clarify the particular way in which behavior analysts use the term “functional” and how this use could relate to the issue of mental causation. Despite the use of the same word, “functional,” behavior analysts clearly do not view mental causation in the functionalists’ way, which, again, involves inner causation. Of course, behavior analysts do not own the term “functional,” so they cannot validly restrict its use to theirs, unless they intend their definition of “functional” as a real definition that specifies the essence of what functional really is, in which case this would be yet another example of essentialist thinking that prompts ontological discussion in behavior analysis.

Marr

The part of Marr’s commentary most directly relevant to the target paper revolves around my emphasis on ontological matters (beyond what he sees as a few basic undiscussed assumptions), and the lack of emphasis on linguistic and epistemological discussion (following standard practice in philosophy, I treat empirical and evidential matters as epistemological). I will thus spend most of this reply on this issue.

Like Leigland, Marr questions why the natural scientists “need” an ontology. My reply to Leigland’s commentary in this regard applies here as well: The word “need” is too strong and lends itself to a dangerous “who-needs” game anyone can play with anything and only promotes academic prejudice. Do behavior analysts “need” quantum mechanics or neuroscience? Probably not, but it would be egotistical of them to dismiss either one as intellectually worthless for that. Do quantum physicists need behavior analysis? Probably not, but it would be equally egotistical of them to dismiss behavior analysis as intellectually worthless for that. I am sure that Marr would be offended, and rightly so, by the question “Who needs that?” about his own work. More importantly, as I argued in my reply to Leigland, ontological discussion is present in all natural sciences, behavior analysis included, not just in the form of a few general undiscussed assumptions, but of essentialistic thinking that has prompted various debates, even empirical research.

Marr also finds my focus on ontology “strange,” which does not surprise me. Strangeness and familiarity, like needs, are largely in the eye of the beholder. My focus is far from strange in academic philosophy, where metaphysics qua ontology is one of the largest and oldest areas of specialization in philosophical investigation. Ontological discussion is quite common in philosophy, even if a few philosophers and many scientists, like Marr, dislike it. Not everyone likes everything.

As I said in the target paper (and Marr acknowledges), I do not mean my focus on ontological matters to diminish in any way the importance of epistemological or linguistic matters. My focus just follows the venerable, tried-and-true strategy of thematic delimitation, figuratively known as “divide and
BURGOS

conquer,” a strategy I am sure Marr appreciates. The ontological character of
dualism, mentalism, and their rejection and association by radical behaviorists was
what drove such delimitation.

I thus agree with Marr that the radical behaviorists’ accusations of dualism
against mentalism are more than just ontology. I said nothing to the contrary in the
target paper. Their rejections of dualism and mentalism certainly have
epistemological and linguistic aspects that deserve serious attention in further
discussion. However, epistemological and linguistic matters are not more or less
important than ontological matters. The three kinds of matters are equally
important, but one cannot discuss them all in a paper-length essay. Delimitation is
thus inevitable. Ignoring most other things does not mean to dismiss them as
unimportant. It only means putting them on hold for future discussion, although
they have been far more discussed than ontological matters.

I thus oppose the outdated logico-positivistic view, which Marr seems to
hold, that ontology is eliminable for reducible to linguistics and epistemology. I
also reject the extreme Wittgensteinian view that all philosophical issues are to be
dissolved via analysis of meaning qua ordinary linguistic use. I equally oppose
attempts to reduce all philosophical to ontological matters. In sum, I am a pluralist
about philosophical matters, someone who gives equal importance to the
ontological, epistemological, and linguistic. I oppose any form of philosophical
reductionism.

Marr differs, dismissing ontological discussion as unnecessary at best,
meaningless at worst. Here is where we part ways. He admits a substantive
ontological aspect to dualism, mentalism, and the radical behaviorists’ associations
of the two. However, he swiftly disparages all of it with the single stroke of anti-
ontology. He even scolds radical behaviorists for playing along the, to him,
pointless ontology game.

If we reject ontology as nonsense (as logical positivists did), as Marr
recommend, his claim that “the “epistemological” and “linguistic” ... cannot
simply be put aside” becomes trivially true. With such rejection, obviously
epistemological and linguistic considerations are the only options left for
philosophical discussion. Of course they cannot be put aside or even on hold! They
become the only philosophical matters to be discussed always. In such
philosophical reductionism, metaphysical matters deserve no focus whatsoever.

Marr also asserts that “all the discussion of what is or is not “physical, or
“material,” and the like is not relevant to radical behaviorists.” I know it is not
relevant to him, maybe others, but I am not sure how representative this is of
radical behaviorism. In the target paper, I provided what I thought was sufficient
textual evidence that such a discussion was central to rejections of dualism and its
association to mentalism by radical behaviorists. Apparently, it was not enough. I
thus feel compelled to repeat two quotations from Skinner that I included in the
introduction to the target paper (the second one echoes the Elisabeth-Gassendi
criticism of Cartesian dualism) and have become standard in radical behaviorists’
rejections of dualism and its association with mentalism:
MENTALISM VS. DUALISM

It is usually held that one does not see the physical world at all, but only a nonphysical copy of it... Sensations, images, and their congeries are characteristically regarded as psychic or mental events, occurring in a special world of “consciousness” where... they occupy no space... We cannot now say with any certainty why this troublesome distinction was first made (Skinner, 1953, p. 276).

But where are these feelings and states of mind? Of what stuff are they made? The traditional answer is that they are located in a world of nonphysical dimensions called the mind and that they are mental. But another question arises: How can a mental event cause or be caused by a physical one? (Skinner, 1974, p. 10).

I cannot but read these quotations as good indicators that the physical-nonphysical distinction was quite relevant to Skinner’s rejection of dualism. The same applies to other radical behaviorists’ rejections of dualism quoted in the target paper. Of course, this does not mean that the distinction is the only grounds on which radical behaviorists reject dualism and associate it with mentalism. There also are epistemological and linguistic grounds, but then again, they are not any more or less important than the ontological grounds.

None of it matters to Marr, though, as he dismisses it all by rejecting ontology. In principle, I have no problem with this rejection, as long as it is valid. Marr’s, however, is not, as it amounts to little more than name-calling (“blind faith,” “esoteric,” “ideological,” “magic”) that misrepresents metaphysics as anti-scientific. Admittedly, between the 18th and 19th centuries, academic metaphysics by philosophers like Christian Wolff, Henri Bergson, Martin Heidegger, and others, became too detached from everyday-life experience and science, for which logical positivists rejected it as nonsense. But at least logical positivists offered a rationale for it (metaphysical assertions did not meet the verifiability criterion), which is more than I can say for Marr’s fallacious name-calling rejection.

Still, we all know how that worked out for logical positivists. With the demise of logical positivism, metaphysical discussion made a vigorous return to the philosophy of science. Today, it is stronger than ever. Much of it remains pure, analytic, general ontology that goes beyond the specific sciences, but it is far more driven by science than the metaphysics logical positivists repudiated. The tendency has also been towards applied ontology in special philosophies of science, mostly philosophy of physics, philosophy of biology, philosophy of psychology, and philosophy of neuroscience. All of these philosophies involve extensive ontological discussion closely guided by science. There also are recent attempts to develop a naturalized metaphysics that opposes analytic (pure) metaphysics in favor of a metaphysics that is even more strongly, if not completely, driven by science (e.g., Ladyman & Ross, 2004).

Contemporary metaphysics, then, is not as anti-scientific as Marr seems to believe. And, once again, as I said in my reply to Leigland, ontological discussion is not exclusive to philosophy but has always run deep in all natural sciences as
well, past and present, behavior analysis included, well above and beyond a few basic undiscussed assumptions.

In sum, Marr gives no valid argument against ontological discussion, in philosophy or natural science. I suggest to him and other ontology naysayers to get used to it: Ontological discussion is here to stay and they cannot do much about it. Ontology is not as evil as Marr and others portray it, and can be a lot of fun, even beneficial to natural science. At least, as I have shown, some natural scientists take ontological discussion seriously, and much scientific discourse is essentialistic and, to this extent, ontological.

Marr also criticizes the mind-brain identity theory. As others’, Marr’s criticism is beside the point, as I intended my discussion of this theory and functionalism in the target paper not to champion them but only to illustrate how mentalism commits us to materialism. Still, to be fair to the theory, Marr’s criticism, albeit different from others’, also misses the mark, and grossly underestimates mind-brain identity theorists. They are much smarter than making such obvious mistakes. If we are going to reject the mind-brain identity theory, let us do it for the right reasons.

It would take me too long to refute all Marr’s, so I will focus on the two strongest ones. One is that if beliefs are brain states, then a person is his brain states. In this line of reasoning, Marr implicitly assumes that the mind-brain identity theory commits us to the thesis that a person is his beliefs. If a person is his beliefs, and beliefs are brain states, then a person is his brain states. This implication is counterintuitive, although this per se is not a valid reason to reject it (the theories of relativity and quantum mechanics are very counterintuitive, but this per se is not a valid reason to reject them).

Marr’s assumption, however, is mistaken: The mind-brain identity theory does not commit us to that or any other particular view of personhood. Although the theory does not preclude that view, it does not preclude others where a person is more than his beliefs (e.g., his actions, consciousness, genotype, phenotype, etc.). One can thus coherently adopt the mind-brain identity theory and a theory of personhood according to which a person is more than his beliefs. If one does this, Marr’s criticism does not follow. He even admits this possibility by viewing a person’s beliefs as part of a person. Nothing in the mind-brain identity theory precludes this view.

Another reason Marr gives to reject the mind-brain identity theory is that its reductionist character supposedly forces us to go all the way down to the sub-subatomic realm of vibrating strings. He asks rhetorically “Why stop” at brain function and not seek a “mind-string identity theory”? Because, as he says, seeking such a theory “would simply be ridiculous.” However, any mind-brain identity theorist would give the same answer, so I do not understand the criticism. I would understand it if Marr’s view of reductionism is one that forces us to go the whole hog, as it were, and carry out all reductions down to string theory (or whatever theory available about the ultimate constituents of reality). However, I do not know of any formulation or pursuit of reductionism that includes such a prescription. In particular, nothing in the mind-brain identity theory dictates that we should seek a
MENTALISM VS. DUALISM

mind-string (or even a mind-atom or mind-cell) identity theory. Of course, reductionism does not forbid us to do it either: Any reductionist is free to try a reduction to the subatomic realm (e.g., see Penrose, 1994), but this is a choice, not an obligation imposed by reductionism. Reductionism has its problems, but this is not one of them.

Two more clarifications before the next set of replies. One, Marr seems to suggest (in his extended self-quotation) that I propound a “brain-behavior identity.” Nowhere in the target paper (or anywhere else) did I do that, nor is it implied by the mind-brain identity theory. I take brain and behavior to be different kinds of physical entities (events, states, processes), even if closely related. The exact ontology of their relation remains to be investigated (see below for a preliminary and tentative proposal). Perhaps he meant to say in that article a “mind-brain” identity, but if he did, as I have repeatedly said, I do not mean to champion this identity either.

Two, contrary to what Marr says, I did not recommend radical behaviorists to adopt the mind-brain identity theory. I know better than this. How could I, if I used the theory as an example of mentalism, and opposed mentalism to radical behaviorism? Rather, I recommended radical behaviorists to adopt an explicitly behavioristic metaphysics of mind, and said that Rachlin’s mind-behavior identity seemed like a good starting point. This theory, again, is just a first proposal that needs some more metaphysical fleshing-out. I find it curious why Marr and others associate me with the mind-identity theory if I also discussed functionalism, and even more extensively.

For the record, however, I do find the mind-brain identity theory far more appealing (parsimonious and biologically plausible) than functionalism. To me, despite its many flaws, the mind-brain identity theory has more empirical support than functionalism, which is not to say that the former does not need more support. The theory’s virtues, I think, outweigh its flaws. None of this means that I accept it as true. It would be premature to do this with any theory of mind. Rather, I take it as identity theorists proposed it, a working empirical hypothesis. I cannot do the same with a mind-behavior identity theory, as it remains too underdeveloped (conceptually and empirically) to allow for proper assessment.

Despite this, I also find intuitively appealing the idea that behavior too is constitutive of the mind. I thus have entertained for some time now a richer view where mental properties are compound or structured brain-behavioral properties. Mental states are thus neural and behavioral in nature. This conjecture could be combined with another one I have also been entertaining for some time now about the brain-behavior relation as a part-whole relation. On this conjecture, brain states are proper parts of behavioral states.

On this basis, a mental state could be viewed as a behavioral state constituted, at least partly, by certain brain states. For example, a pain could be viewed as a behavioral state of moaning, wincing, and writhing partly constituted by C-fiber and dorsal posterior insula firing. If environmental conditions are constitutive of behavior, then they also are constitutive of mental states thus conceived. This
conjecture is similar to the one Rockwell (2004) propounds, to whose commentary I reply later on.

All of this, of course, needs more elaboration and empirical support, and faces difficult hurdles. One is the issue of whether composition is identity: Is a whole the same as the collection of its parts? Another is the role of mental causation, if any (my conjecture may well make it dispensable). It also remains to be seen whether this conjecture, the mind-brain, and the mind-behavior identity theories have different empirical implications. If not, the three theories would be empirically underdetermined and would have to be assessed analytically, in terms of coherence, clarity, precision, and parsimony.

**Rachlin**

Rachlin begins his commentary with a summary of the main target paper’s point. His summary is, for the most part, correct. I would only repeat my clarification in relation to his assertion that “you can be a neural identity theorist (a kind of mentalist) ... but not a dualist” (emphasis mine). To say “can” allows that a neural identity theorist (or a functionalist) can be a dualist. The main point of the target paper is that a mentalist, whether a mind-brain identity theorist or a functionalist, not just “can” but must be a materialist. If one is a mind-brain identity theorist or a functionalist, one cannot coherently be a dualist.

Rachlin continues by asserting that I identify him as a radical behaviorist, without calling his position “teleological behaviorism.” Although his concern in this regard is understandable, I did not mean to label him strictly as a “radical behaviorist,” nor did I overlook the label “teleological behaviorism.” As I clarified in Note 1 of the target paper (p. 2), the criticism is not just against radical behaviorism but also other philosophies that are alike in sharing a rejection of mentalism as dualistic, among which I mention “teleological, molar, and interbehaviorism.” Admittedly, in that note, I did not mention Rachlin, and said I was going to focus on radical behaviorism, only to quote him later on. The implication is that he is a radical behaviorist, but then again, I did not mean to say this strictly. I am (and was when I wrote the target paper) well aware that he is a teleological behaviorist, and regret the seeming conflation.

The relevant clarification is that my criticism focuses on rejections of mentalism as dualistic, which teleological and radical behaviorism, as well as interbehaviorism, share. Rachlin, it seems to me, agrees with Skinnerians and Kantorians on such rejection. Of course, their positions differ in other important respects, but then again my focus is on this rejection. The target paper’s main point is that, although their rejection of dualism is fine, their rejection of mentalism as dualistic misses the mark.

A further clarification is that I used the label “radical behaviorism” liberally (perhaps abusively) as a more familiar label to name a family of behavioristic positions (teleological, interbehaviorism, theoretical, methodological) that differ in several important respects but are crucially similar in their rejection of mentalism as dualistic.
MENTALISM VS. DUALISM

One respect that Rachlin discusses where being a teleological behaviorist makes a difference is zombies. He argues that in his teleological behaviorism zombies are logically impossible: If having consciousness is to behave in a certain way (e.g., if pain is pain behavior), then, he concludes, zombies are impossible. I am totally fine with this outcome, as nothing I said in the target paper hinges on the possibility of zombies. I intended my discussion of zombies not to endorse them (I do not) but only to argue that mental causation is incompatible not just with Cartesian dualism but also with a contemporary form of non-Cartesian dualism (property dualism). Denying the possibility of zombies does not invalidate this argument in the least. Quite the contrary, denying the possibility of zombies allows for mental causation, which only reinforces my argument. Conversely, mental causation denies property dualism by denying the possibility of zombies. Therefore, adopting Rachlin’s proposed mind-behavior identity theory is not the only way to repudiate zombies.

Still, I take issue with his point that the notion of zombies is “morally repugnant.” The notion certainly raises the kinds of questions he asks, in particular how do I know others are not zombies. As he correctly points out, I cannot know if others are zombies and, hence, feel pain like me. Again, I am no fan of zombies, but if we are going to reject them, let us do it for the right reasons.

One problem with Rachlin’s argument here is that negative answers to those questions do not entail morally questionable positions. In particular, the fact that we cannot know if others feel pain does not entail that we can or should harm others. More generally, a negative solution to the problem of other minds, that we cannot know with certainty if other minds exist, does not entail any moral position whatsoever, whether or not reprehensible. To be sure, it is possible to use this fact to justify morally reprehensible positions, although the logic of it would be unclear; presumably, part of the point of some morally reprehensible actions such as torture is to make others feel pain, which assumes that others are not zombies. In any case, the fact does not force anyone logically into morally questionable views or actions.

Another problem is that the possibility of using zombies to justify morally questionable views and actions is a very weak reason to reject the possibility of zombies, but not because these positions and actions are not questionable (they certainly are). Rather, the possibility of using zombies to justify questionable morals does not entail that zombies are impossible. Rachlin’s argument here is not much different from the argument that we should reject the existence of atoms because it can be (in fact, has been) used to build atomic bombs, at the cost of much human suffering.

Besides, anyone could also use behavior analysis principles as a rationale for morally questionable actions. Drug dealers could use the hyperbolic discount function to design strategies for better drug dealing. Although this would be morally repugnant, it does not mean that the function is false or invalid. Another well-established principle of behavior analysis is that positive punishment decreases the punished behavior, and punishment has been widely used as a way to modify behavior. In particular, oppressive governments have used it to deter
behaviors that oppose the regime. However, this does not mean that the principle is false. Likewise, the notion of inheritance can be (and has been) used as a rationale for morally repugnant actions (e.g., genetic cleansing via genocide), but this does not mean that we should reject the notion either, and so on. Clearly, Rachlin’s moral argument against zombies also applies to scientific principles that I do not think he would reject.

Another respect in which Rachlin’s teleological behaviorism makes a difference is mental causation. He correctly points out, like Baum, that my discussion in the target paper focuses on efficient causation. But then again, as I said in my reply to Baum, I just follow the trend in philosophical discussions about causation in general, where the focus has been on efficient causation, largely due to the metaphysical conjecture that causal relations obtain between events. Discussions on mental causation are not the exception to this: Mental particulars are assumed to be events, which favors a view of mental causation as efficient. As I said, I have no reason to reject any of this. Events are not the kinds of entities that makes sense to view as material, formal, or final causes. Therefore, if we wish to view mental causation as something other than efficient, we would have adopt a view where mental particulars are not events. However, it is far from clear what would they be.

Rachlin, like Baum, also criticizes the mind-brain identity theory, but then again, this criticism is beside the target paper’s main point. At least, I must give credit to Rachlin as I do to Marr, for not relying on the sort of ad hominem fallacy that Baum relied upon in his rejection of the theory. Still, to be fair to the theory, Rachlin’s criticism fails for three reasons. First, it hinges on a view of efficient causes as necessary and sufficient conditions, whereas the theory does not commit us to this or any other specific ontology of efficient causes. This view of efficient causation has been widely discredited in philosophy for a very long time now and, hence, is philosophically outdated (see Psillos, 2002). No mind-brain identity theorist, or functionalist, has ever been committed to such a view.

Third, Rachlin assumes that, according to the mind-brain identity theory, C-fiber firing causes pain. However, this is a deep misinterpretation of the theory. Mind-brain identity theorists have explicitly rejected this, based on a rejection of self-causation, as precluded by the assumption, widely accepted among philosophers, that cause and effect are different events. If pain is C-fiber firing, as mind-brain identity theorists hypothesize, C-fiber firing cannot be said to “cause” (or be a “correlate” or “substrate” of) pain without violating that assumption.

Under this assumption, nothing can be meaningfully said to cause (or correlate with or be a substrate of) itself. Talk of C-fiber firing causing pain makes sense only if pain is different from C-fiber firing, which is precisely what mind-brain identity theorists deny. This consideration has been integral to their formulations of the theory. Under the theory, rather, pain qua C-fiber firing can be said to cause pain behavior (which is not pain, according to the theory). Hence, Rachlin’s conclusion that stimulating a set of C-fibers in vitro “would cause them pain” does not follow.
MENTALISM VS. DUALISM

I also want to qualify Rachlin’s assertion that I “approvingly” cite his theory. Rather, as I have said repeatedly, I see the theory only as a candidate for a behavioristic metaphysics of the mind, which is different. For the record, again, I do not subscribe to a purely behavioral or purely neural identity theory. Rather, as I said in my reply to Marr, I now tend to see both as being partly correct, in favor a richer ontology of mental states as having constitutive brain and behavioral properties. In any case, it remains to be seen whether and how Rachlin’s theory can become a strong contender in the materialistic arena where the mind-brain identity theory and functionalism have been battling for a long time now.

Rockwell

Rockwell begins his commentary by responding to my criticisms of his take on Cartesian materialism in the target paper, as depicted in his 2005 book. One criticism was that he invalidly claimed that the brain-body distinction was an “essential corollary of the [Cartesian] mind-body distinction.” He clarifies that he did not claim this, but rather that Descartes did. More precisely, he clarifies that he was “only saying that (Descartes’) mind-body distinction implies a brain-body distinction,” but disagreed with this implication. I thus stand corrected on this, but as I argue below, it still is unwarranted to give the brain-body distinction such a central role in Cartesian (or even Descartes’) dualism.

Relatedly, Rockwell also pointed out that there was an invalid claim about this in the target paper (p. 14): If the Cartesian mind-body distinction implies the brain-body distinction, then it would be contradictory to make the latter without the former. I stand corrected on this as well: Contrary to what I suggested in the target paper, if the Cartesian mind-body distinction implies the brain-body distinction, it is coherent to make the latter without the former. However, also contrary to Rockwell’s claim, if the Cartesian mind-body distinction implies the brain-body distinction, it is equally coherent to make the former without the latter. The two distinctions, then, are logically independent of one another. Contrary to what Rockwell claims, such logical independence means that the Cartesian mind-body distinction does not entail the brain-body distinction (or vice versa)\(^6\). The main outcome about this in the target paper thus stands. Overall, then, there is little support for claiming some deep connection between Cartesian dualism and the brain-body distinction.

\(^6\) Formally, for singular statements, let \(a\) denote my mind, \(b\) my body, and \(c\) my brain. The mind-body distinction asserts that \(a \neq b\), and the brain-body distinction asserts that \(c \neq b\). In first-order predicate logic (with identity), \(a \neq b\) does not entail \(c \neq b\), and vice versa (a brain-body distinction does not entail a mind-body distinction). Comparable results obtain with quantified statements, existential as well as universal. Thus, \(x \ y(Mx \& By \& x \neq y)\), or “at least one \(x\) and one \(y\) exist ( ) such that \(x\) is a mind (\(M\)), \(y\) is a body (\(B\)), and \(x\) is different from \(y\),” does not entail \(z \ y(Cz \& By \& z \neq y)\), or “at least one \(z\) and one \(y\) exist such that \(z\) is a brain, \(y\) is a body, and \(z \neq y\),” or vice versa. The same obtains with universal statements: \(x \ y((Mx \& By) \& x \neq y)\), or “for all ( ) \(x\) and \(y\), if \(x\) is \(M\) and \(y\) is \(B\), then \(x\) is different from \(y\),” does not entail \(z \ y((Cz \& By) \& z \neq y)\), or vice versa.
I also criticized Rockwell in the target paper for redefining Cartesian dualism as Cartesian materialism, but he clarified that he did not do this and viewed the two as “completely different and entirely separable” proposals, and that his criticism was against Cartesian materialism, not Cartesian dualism (which has already received plenty of criticism). According to Rockwell, the main difference is that Cartesian dualism is a philosophical, whereas Cartesian materialism is a scientific proposal (although a failed one). I also stand corrected on this.

Still, why call them both “Cartesian”? Because, according to Rockwell, “Descartes created both.” However, as I argued in the target paper (pp. 18–19), the brain-body distinction was part of an ancient tradition that antedated Descartes by centuries. Descartes, then, contrary to Rockwell’s claim, did not “create” this distinction: It was well in place well before him. Descartes just used it in his speculations about how the mind related to the body. The brain-body distinction is no more “Cartesian” than the heart-body or stomach-body distinctions.

Rockwell further claims that Descartes needed to make the brain-body distinction because it was the only way for him to deal with Elisabeth’s criticism. However, this is mistaken. Descartes’ reply to Elisabeth’s letter where she pointed out the causal interaction issue does not appeal at all to the brain-body distinction. There is no mention of this distinction anywhere in Descartes’ reply (see Shapiro, 2007, pp. 63–68). Rather, Descartes replied that Elisabeth was illicitly generalizing primitive notions about the body to the soul. The brain-body distinction is nowhere to be found in Descartes’ reply to Her Highness or the rest of his critics.

It thus is conceptually and historically unwarranted to give the brain-body distinction such a central role in Cartesian (or even Descartes’) dualism, qualify the distinction as “Cartesian” and, hence, to speak of “Cartesian materialism” in Rockwell’s sense. There already is a good, clear and precise name for the thesis that Rockwell criticizes: Mind-brain identity theory. Calling it “Cartesian materialism” is not only unwarranted and unnecessary, but promotes confusion and guilt by name association. As I mentioned in the target paper, some behavior analysts have confused Rockwell’s Cartesian materialism with Cartesian dualism. Such confusion has enabled the patent absurdity that current neuroscience (perhaps even the mind-brain identity theory) is dualistic. Rockwell’s clarification supports this assessment.

Rockwell also questions my criticism of Dewey’s abuse of the term “dualism” to refer to the dualities other than a physical-nonphysical duality, such as brain-body and stimulus response. Although the word “weasel” is strong (perhaps too strong), I attributed no ill intention to Dewey: I did not mean to say that he was purposefully equivocating on the term “dualism” to get his way. It is reassuring that Rockwell believes Dewey would not have had any problem with my criticism.

However, I must give two related replies to Rockwell’s claim that “Dewey acknowledges that he is talking about two different kinds of dualism when he says that one dualism replaces the other.” One, I found no explicit acknowledgement from Dewey about this in the two works of him that I cite in the target paper. Perhaps he is more explicit about it in other writings, or perhaps Rockwell is interpreting Dewey more charitably than I am. But I would still criticize Dewey for
MENTALISM VS. DUALISM

abusing the term “dualism,” especially if he knew the term did not strictly apply to some of the distinctions to which he applied it.

Two, Rockwell persists in calling the brain-body distinction “a kind of dualism,” after acknowledging that it really is not. Such talk only indicates the strong grip the dualism-duality equivocation has on the literature (we saw another example of it in Christofidou’s commentaries about Kant’s alleged “dualism”). My criticism of it, then, is more than just a “semantic refinement.”

Another part of Rockwell’s commentary relates to his rejection of the mind-brain identity theory, which he discusses at length in his book. I do not want to make these replies a review of the book. I must say, however, as I have repeatedly said throughout these replies, that such rejection is beside the point, because I did not intend to champion the theory. Again, I used the theory (and functionalism) in the target paper only to illustrate how mentalism committed us to materialism.

Still, as with other commentaries, I feel compelled to reply to Rockwell’s criticisms of the theory in his book as misinterpretations of it, much like Marr’s and Rachlin’s. A major misinterpretation is that the theory commits us to a certain view of causation that is scientifically (or philosophically) inadequate. This misinterpretation leads Rockwell to dedicate considerable space to causality in his book. However, once again, there is no textual evidence whatsoever in the literature about the theory that supports such an interpretation. No proponent of the theory has formulated it in a way that commits us to a specific ontology of causation. To be sure, some identity theorists have expressed their views, but this does not mean that the theory commits us to them. Some do not even express any view. For example, Polger (2004) said from the outset “I do not offer any philosophical accounts of either material composition or causation” (p. xx). Such neutrality would be hard to justify if the theory really committed us to a specific ontology of causation.

Still, I agree with Rockwell that the mind-brain identity theory is not the only empirical possibility. His sort of extended-mind account is another possibility worth investigating scientifically. I also find it interesting, not only because it includes neural network modeling as integral, which is appealing to me as a neural-network modeler myself. The theory is also similar to the one I suggested in my reply to Marr (mental states as behavioral and neural in nature).

I also agree that the empirical evidence in cognitive neuroscience “does not prove it is a metaphysical fact that the mind is the brain” (2005, p. 54). However, it never will: Logically, no evidence can “prove” any theory that implies it, if by “prove” Rockwell means “deductively demonstrate.” Arguing otherwise is to commit the fallacy of the converse, which asserts the assumptions of a formal implication by asserting its logical consequences. A mind-brain identity theory implies a high correlation between mental and brain states, but such correlation (widely observed) does not conversely entail the theory. At best, we can only say, as in falsificationism, that evidence entailed by the theory corroborates it.

The same applies to Rockwell’s theory: No evidence can “prove” but only corroborate or falsify it. This point raises yet another issue. It is far from obvious whether Rockwell’s theory (and similar ones like Rachlin’s mind-behavior
identity theory, and my own conjecture of mental states as behavioral and neural in nature), the mind-brain identity theory, and functionalism have implications that allow for a differential empirical assessment. If not, the theories will be empirically underdetermined, would have to be assessed on grounds other than evidence.

References

MENTALISM VS. DUALISM