

Radical Cognitivism? Distinguishing Behavior from Thought[☆]

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De Houwer, Hughes, and Barnes-Holmes offer up what appears to be a simple recommendation for the field: that we should distinguish—and even separate—our descriptions of the phenomena that we investigate from our theoretical accounts of those phenomena. They characterize this as a distinction between functional explanation and cognitive explanation, respectively. And they maintain that more assiduous application of this distinction would be of value not only for basic research but also for applied research, the focus of this article. In this commentary, we consider several aspects of this proposal, including its emphasis on the stimulus-environment-response level of description, the implications for the labeling of phenomena, the rationale for separating description from explanation, and the potential consequences for the application to basic research. On some of these, we find ourselves in agreement; on others, we respectfully disagree.

The Functional Level of Explanation

De Houwer et al. (p. 2) hold that “Within the functional level of explanation, behavior is explained in terms of the (current or past) environment and the way organisms interact with the environment.” This echoes an earlier characterization by De Houwer (2011, p. 204), that “A cornerstone of the functional approach in psychology is the practice of defining behavioral effects exclusively in terms of elements in the environment.” Interestingly, there is also a more distant echo: “The psychology which I should attempt to build up would take as a starting point, first, the observable fact that organisms, man and animal alike, do adjust themselves to their environment” (Watson, 1913/1994, p. 250). In this sense, the functional approach that De Houwer et al. champion is more aligned with behaviorism than with classical

functionalism. As Watson (p. 25) puts it “*behaviorism is the only consistent and logical functionalism.*” In contrast, classical functionalism was a reaction to structuralism, which defined the proper domain of psychology as the study of consciousness and the mental structures that composed conscious experience, and which rested heavily on the method of introspection (Titchener, 1929, p. 43). Functionalism sought to discard the method—introspection—and instead emphasized dynamic (and physiological) adaptation to the environment for both behavior and “mental life,” but still held the study of consciousness to be central to the mission. It thus appears that the functionalism of De Houwer et al. is more akin to the behaviorism of Watson, keeping in mind, of course, that the cognitive level of explanation is superimposed on the functional explanation. De Houwer et al., of course, are clear on this commitment noting that the “functional approach... can be linked to behaviorism, at least certain forms of behaviorism such as radical behaviorism” (p. 1). This commitment is important as it suggests a deeper incompatibility between the functional and cognitive perspectives than is maybe reflected in De Houwer et al.’s characterization of it as an “illusory barrier” (p. 9). This particular issue with the functional-cognitive approach has been cogently argued in a separate comment on the approach (see Proctor & Urcuioli, 2016).

We are entirely in agreement with De Houwer et al. that our first job as psychologists should be to carefully and thoroughly analyze and describe the phenomena that we study. Indeed, without doing so, we would argue that we would have nothing to explain. And there is no question that it is misguided to equate behavioral effects with mental processes in a kind of one-to-one mapping, a point that has been made in the past (e.g., Jacoby, 1991). It is a virtual certainty that phenomena, or tasks, can never be process-pure: “problems interpreting task

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dissociations have arisen from equating particular processes with particular tasks and then treating those tasks as if they provide pure measures of those processes” (Jacoby, 1991, p. 513). It is not surprising, therefore, that a great deal of the creativity and effort in research on cognitive phenomena is leveled at trying to “clean up” tasks to eliminate as many processes as possible other than the one that is the target of primary interest. Still, we know that we never reach the “holy grail” of isolating one and only one process. Thus, De Houwer et al.’s reminder that we need to remain vigilant against equating behavioral effects with mental processes is certainly welcome.

One of the two illustrations that De Houwer (2011) uses to show why functional and cognitive levels should be separated is the case of negative priming. Negative priming is the situation where a target item on a current trial was an ignored item on a prior trial (e.g., Fox, 1995; Tipper, 2001) and the result is slowed processing on the current trial, relative to an unrepeated control. The cost evident in the phenomenon has predominantly been explained by inhibition—that to be ignored, the prior trial item must have been suppressed, with that suppression carrying over to slow the processing on the current trial. This was an appealing and intuitive account, such that negative priming came to be treated as a measure of inhibition: The theory became grafted to the phenomenon. But over time, another theory—the episodic retrieval account—gained in currency, explaining the same cost as due to the response appropriate for the current trial conflicting with that retrieved from the prior trial. It was not easy for the retrieval account to gain traction given the prevalence of the inhibition account, although the inhibition account is probably the less-supported account now (see MacLeod, Dodd, Sheard, Wilson, & Bibi, 2003; Mayr & Buchner, 2015). Not keeping the phenomenon and the theory distinct from each other may well have hampered progress, consistent with the argument of De Houwer et al.

The Nominal Fallacy

Philosophers refer to the *nominal fallacy* as the belief that something has been explained when it has been given a name. This, certainly, is a seductive error, one that likely is invited by integrating the description (functional) with the explanation (cognitive). Negative priming borders on this, with “negative” suggesting “below-zero suppression,” but there are better examples. A perfect illustration is the phenomenon known as “inhibition of return,” in which, after a short delay, people are slower to return to a recently cued item or location even though the cue has no validity (Posner & Cohen, 1984; Posner, Rafal, Choate, & Vaughan, 1985; see Lupiáñez, Klein, & Bartolomeo, 2006). Why? The longstanding answer is because when nothing was initially found at the cued location, attention prefers not to go back there, inhibiting the location, much as a well-adapted bird would not return to a cache site that has already been emptied. This inhibition account may eventually be proven correct, but that is not a given and there are certainly alternative accounts (see, e.g., Berlucchi, 2006; Martín-Arévalo, Kingstone, & Lupiáñez, 2013). Thus, Martín-Arévalo et al. argue that instead of “inhibition of return” the phenomenon should be referred to

by its (more functional) description, that is, a detection cost at the location of the previous cue. Such a characterization leaves potential deeper cognitive accounts more open; of course, it is also more a description than a name. Yet, as Berlucchi laments, the theory-driven name of the phenomenon lives on.

Carefully considering the organism–environment interaction in the De Houwer et al. version of a functional approach would, we believe, help to preclude naming a phenomenon using the initial cognitive theoretical explanation for that phenomenon. Others have made this same argument which led MacLeod et al. (2003) to suggest that we adopt the language of cost and benefit to avoid slipping from the outset into more cognitively loaded terms such as inhibition and facilitation. Clearly, separate terminology for phenomena and their cognitive explanations would, as De Houwer et al. maintain, help to avoid this trap. We certainly agree that “different researchers use the same concepts at multiple levels of explanation” (p. 9), and that this can be very confusing and can lead subsequent researchers astray.

Separating Description from Explanation

“Defining behavioral effects in purely functional terms not only maximizes theoretical freedom but also promotes cumulative science” (De Houwer et al., p. 4). This sounds like an unassailable truth . . . but is it? Most scientists would quickly note that research should test theories, not just explore phenomena, so if phenomena must take precedence, that represents a problem. We are of the view that the stringent testing of theory should most often directly motivate empirical research. But there must also be room for discovery, in the sense of finding a new phenomenon and pursuing it empirically for a while until its boundaries provide a basis for theorizing. Relatedly, there should be room in psychological science for rich descriptions of human behavior, particularly as it occurs in relatively natural conditions (Kingstone, Smilek, & Eastwood, 2008; Neisser, 1978; Risko, Richardson, & Kingstone, 2016; Tunnell, 1977). This is not, however, coincident with simply exploring a phenomenon for its own sake and never stopping to link its explanation to broader theory. So yes, theoretical freedom is greater if not pinned down from the outset, but science ultimately hinges on the development of good theories.

There is another side to this claim, that of promoting cumulative science, and it is here that a problem can arise. A scientist can become too enmeshed in a phenomenon, leading to the “what hasn’t been done yet?” mentality. Dustbowl empiricism—collecting data for their own sake without regard to theory—will not in the end advance science to the same extent as theory-driven research will (although mining the same strip repeatedly may advance a prolific scientist’s career). Too much focus on the functional side would seem to enhance the likelihood of this problem, as has certainly happened in the history of psychology. An example might be the verbal learning tradition (see Hall, 1971) that predated the modern study of memory, wherein simply relating stimulus and response conditions seemed like the goal in itself, but there was a serious shortcoming in the development of understanding. We would

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