COVERT BEHAVIOR AND MENTAL TERMS: A REPLY TO MOORE

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In a recent paper, Moore (2001) compares the behavior analytic treatment of so-called mental phenomena with other behaviorist approaches, in particular, with logical behaviorism and conceptual analysis (see also Moore, 1980, 1981, 1995). Moore concludes that behavior analysis gives a more adequate account of the phenomena than the other positions. In this note, I will argue that the behavior analytic treatment has itself some fundamental shortcomings. These problems are avoided if we adopt a behaviorist theory that has the following features: (i) it only accepts the existence of entities (factors, events, states, etc.) if this existence is supported by empirical evidence, (ii) it is based on Quine’s materialist repudiation theory which assigns no role at all to mental entities,¹ (iii) it acknowledges the well documented fact that organisms can acquire new behavior not only by operant conditioning but also by being exposed to the pairing of two (salient) stimuli. Since this theory assumes fewer entities than the other behaviorist theories while accounting for the same evidential data, I will call it parsimonious behaviorism (PB).

There exists another materialist theory that gives an adequate account of mental phenomena, the so-called identity theory. This theory, which has been pioneered by Ullin Place (1956), identifies mental entities with physiological ones. But since the identity theory admits mental entities, questions can be raised about the status of these entities, about the nature of their relation with behavioral phenomena, with physiological phenomena, about the location of these entities, etc. By basing PB on Quine’s repudiation theory, all these problems are avoided.

Covert Behavior

Let me now turn to the shortcomings of behavior analysis. The first is its introduction of covert-behavior events. These events are supposed to be inner events that have behavioral features. But although the events play a central role in behavior analysis, they have not been clearly defined. Rather, they are usually introduced with the help of examples. Thus, Moore (2001) gives as an illustration

of covert-behavior events the process of “thinking,” which may be defined as “acting in a way that produces stimuli that have some effect on the thinker and that are associated with subsequent reinforcement” (p. 170). For example, covert behavior may occur “when individuals attempt to solve a difficult problem” (p. 172), as when they might try to privately solve the problem “in their heads.”

But these informal characterizations of covert behavior can hardly be called a definition. They don’t give us the necessary and sufficient conditions for calling an event “covert behavior.” Nor do they give us the tools for determining the physical dimension of the behavior—to determine, for example, whether there are four instances of the letter “t” in the covert behavior that occurs when John tries to solve “in the head” the problem of how much is 24 times 26. The demand for such tools is legitimate, since the inner events are supposed to have behavioral features.

Further, and this is even more important, behavior analysts have not cited clear empirical evidence that confirms the existence of covert-behavior events, and the evidence they did discuss is far from being satisfactory. Moore, for example, thinks that the events are the ones “whose occurrence the individual can report and to a limited extent describe” (2001, p. 169). But a verbal report is merely a series of sounds that are produced by the subject, a verbal response. And according to behavior analysis, the response is ultimately evoked by external stimuli, just as a rat’s lever press is evoked by the antecedent stimulus that exerts discriminative control. Even if we admit that the verbal response is evoked by an inner event, we need much more data in order to conclude from the “semantic content” of the report that the event to which it “refers” or “reports” or “describes” has behavioral features.

Skinner (1969) relies on introspection for attributing behavioral features to the inner events: “Some of the objects of introspection are private (covert) responses” (1969, p. 242). But Skinner does not cite empirical data that support the conclusion that the “objects of introspection” (whatever that means) have behavioral features. Skinner even adds the conjecture that “the responses are executed with the same organs as observable responses but on a smaller scale” (p. 242). But again, no empirical evidence is mentioned that supports this conjecture, and as far as I understand the notion of covert response, not even the use of instruments can confirm the conjecture, because as soon as the covert behavior has been observed, it has become accessible to third persons, and this prevents it from being covert. Note again that even if someone gives us a verbal report of an introspection, we merely receive a set of sounds, with a “semantic content” that has to be given a very biased interpretation in order to allow us to conclude that the inner events that evoke the responses have a behavioral character.

How does PB deal with phenomena such as thinking and introspection? Consider the person who at time $t$ hears the question, “How much is 24 times 26?” and who after a while gives the answer “Six hundred twenty four.” PB first assumes that previous experience, such as memorizing the multiplication tables, have introduced specific changes in the person’s physiological system—they left physiological traces. (For empirical evidence of such physiological effects, see e.g., Byrne, 1987; Hawkins & Kandel, 1984.) Hearing afterwards the above
question initiates a series of physiological events in the person’s modified physiological system, and these inner physiological events then evoke different types of verbal and nonverbal responses, including the verbal response “Six hundred twenty four.” Admittedly, we only have limited knowledge of the physico-chemical nature of the inner physiological events. But future empirical research will surely increase our knowledge of this nature. On the other hand, it is not clear whether something similar can be done for covert behavior.

Actually, there is no fundamental difference between the account of PB and of behavior analysis, because the latter also admits that “neurophysiology participates in the private behavioral event” (Moore, 2001, p. 174). The only difference is the issue of covert behavior. Behavior analysis attributes not only physiological but also behavioral features to the inner events, whereas PB makes a more modest claim. It only attributes physiological features to the events. PB’s account is therefore more parsimonious than the one given by behavior analysis. It accounts for the same evidence, including the same behavioral evidence, while assuming the existence of fewer entities. (On deciding ontological issues on the basis of parsimony considerations—in the present case, deciding that covert-behavior entities do not exist—see e.g., Smart, 1963, pp. 8-13; Stemmer, 2001, p. 199.)

But Moore thinks that PB does not account for all evidential data. Moore does not explicitly discuss PB’s approach, but his argument against the identity theory, which likewise attributes so-called mental phenomena to physiological factors, also affects PB:

Consider the rat’s lever press. Neurophysiology certainly participates in the motor activity of pressing the lever, but the rat’s physiology does not identify the antecedent stimulus that exerts discriminative control or the schedule of reinforcement according to which the response is reinforced. So is it with private behavioral events. Neurophysiology participates in the private behavioral event, as it does in a public behavioral event, but an appeal to physiology does not explain how the private behavioral event develops or how it comes to exert discriminative control. (Moore, 2001, p. 174; my italics)

But it is not difficult to see that Moore’s criticism is based on an unwarranted premise. It presumes that there are inner events that have behavioral features. However, as I have argued above, no evidence has been adduced that confirms the existence of such events. It follows that physiological factors indeed account for the relevant data. Consequently, Moore’s criticism affects neither Place’s identity theory nor PB.

Moore’s argument illustrates a rather questionable attitude of most behavior analysts. They usually take it for granted that covert-behavior events exist, and they think that everyone must admit this “obvious” fact. They therefore immediately object to any account that does not include such events. But not only is there no empirical evidence that confirms the existence of the events, what is even more ironic is that there is actually no need at all for introducing such events. For we have seen that PB accounts for the relevant data without assuming a “special kind of mind stuff” (Skinner, 1974, p. 220). Moreover, PB satisfies
another desideratum of behavior analysis, since it attributes to inner and outer
events “the same kinds of physical dimensions” (Skinner, 1964, p. 84).

PB also avoids opening “the door to the dualism of the self as an initiating
agent” (Moore, 2001, p. 171). Since PB explicitly mentions the modifications
introduced by historical events in the organism’s physiological system, it
acknowledges the causal role of these events. Nor does PB ignore the initiating
role of the environment, because it explicitly mentions the environmental factors
that on time $t$ operate on the modified organism—the factors that generate the
physiological events in the organism. In particular, Quine’s notion of physiological
trace (1974, pp. 24-27) is extremely helpful here, because it not only directs our
attention to the historical events that left the trace but also to the environment that
“activates” this trace, the environmental elements that at time $t$ stimulate a
modified organism.

A Dialogue With Other Psychologists

Eventually, behavior analysts intend to establish a dialogue with other
psychologists. This will probably not be an easy task, mainly, because most
psychologists, even those who consider themselves monist materialists, are
influenced by mentalist ideas. But one obstacle to such a dialogue can be removed,
namely, the use by behavior analysts of the notion of covert behavior. This notion
is so strange, so bizarre, for people who have not been educated in a behavior
analytic environment, that they immediately turn away from any position that
admits such behavior. But this reaction can be avoided because, as I have just
made clear, there is no need at all for behavior analysts to introduce the notion. All
relevant phenomena can be accounted for by assuming only physiological factors,
events, or processes. I would therefore strongly recommend behavior analysts to
eliminate the unnecessary notion of covert behavior from their analysis.

Exposure to the Pairing of Two Salient Stimuli

A central issue that concerns the treatment of mental entities is to explain how
so-called mental language is learned. Several behavior analysts have investigated
these learning processes, including Skinner. But I will concentrate on Moore’s
(1980) account of the learning of the word “pain,” which is based on Skinner’s
conclusions. This account will help me to illustrate the behavior analytic
inexcusable neglect of one of the most important components of the learning
process of mental terms as well as of many other terms.

Moore examines the learning of the word “pain,” and he assumes it takes
place in a context of the three-term contingency, which is “the fundamental unit of
analysis for radical behaviorism” (1980, p. 461). In this learning process, a person
reinforces a child’s utterance of “pain” on correct occasions. Moore points out the
problems with this process since the reinforcing person does not have direct access
to the correct occasion: the private painful stimulation. He suggests that the person
will base the reinforcement on public features that are associated in a reasonable
regular way with painful stimulations. This conclusion is indeed plausible. But Moore’s account ignores a learning process that must occur previously, for the person can start reinforcing the response “pain” on correct occasions only if the child produces the word on such an occasion. But the probability that a child, who has never heard the word “pain,” will produce this English word on a correct occasion is practically zero. And even if the child has already heard this word, for example, when she heard her caregiver saying “I have a pain in my neck,” the probability that she will produce the word when she herself is painfully stimulated is still practically zero.

So what is missing here? What is missing is that before the reinforcement can start, the child must have heard at least once an utterance of “pain” while she was in pain; that is, she must have been exposed at least once to the pairing of the word “pain” with a painful stimulation. Such an exposure, or a couple of such exposures, transforms a probability that is practically zero into one that is sufficiently high for the reinforcement to start (see also Stemmer, 1973, pp. 36-40; 2001, pp. 190-192).

The importance of the exposure to such pairing situations in language learning has been noted by scholars such as Russell (e.g., 1927), Osgood (1953), Staats (1968), Tonneau (2001), and others, including Skinner himself (1957), who mentions ostensive learning (p. 360) and also alludes to Pavlovian conditioning (p. 357). The rather strange fact is that Skinner’s followers have concentrated on the three-term contingency while neglecting other learning processes. But we have just seen that if we ignore the role of the pairing events, we will never be able to give a correct account of the learning of mental terms. Moreover, as I have argued in several publications, the events also play a decisive role in the learning of many other terms (see, e.g., Stemmer, 1989, 1992, 1996.) I therefore strongly recommend behavior analysts to seriously consider these learning events.

Conclusions

The introduction of the notion of covert behavior has caused several problems in behavior analysis, including preventing a fruitful dialogue with other psychologists. But these problems are unnecessary because PB, which pursues the same scientific goals of behavior analysis, is able to account for the relevant phenomena without relying on the notion.

The second shortcoming I have discussed, namely, the absence of a serious consideration to the role of pairing events in establishing new behavior, especially new verbal behavior, can also be easily solved. Behavior analysts just have to be more open-minded and admit that not everything begins and ends with the three-term contingency.

There are other shortcomings of behavior analysis, such as the absence of a detailed account of the structure-dependence of grammatical behavior (see, e.g., Stemmer, 1990, 1994). But a discussion of these issues would exceed the scope of this note.
References


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