METHODOLOGICAL BEHAVIORISM AS A RADICAL BEHAVIORIST VIEWS IT

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ABSTRACT: Methodological behaviorism is a stance on verbal processes and the meaning of “psychological” terms and concepts that are deployed in theories and explanations of behavior. According to this stance, all such terms and concepts should be based on observable stimuli and behavior. Over the years, psychologists have interpreted the phrase “based on” in at least three different ways. One interpretation was that psychologists should remain formally silent on causal mental terms, and not speak at all about unobservables. A second interpretation allowed psychologists to appeal indirectly to mediating mental terms, provided the psychologists could logically connect the terms to observables through operational definitions, where those definitions were exhaustive. A third interpretation again allowed psychologists to appeal indirectly to mediating mental terms, provided the psychologists could logically connect the terms to observables through operational definitions. This time, however, the definitions need be only partial instead of exhaustive. We argue the interpretations lead to an incomplete psychology, if not also an institutionalized mentalism, because they fail to recognize private behavioral events. None of the interpretations are consistent with the radical behaviorism of B. F. Skinner.

Key words: methodological behaviorism, radical behaviorism, operationism, logical positivism, theoretical terms, psychological terms

Methodological Behaviorism as a Radical Behaviorist Views It

Skinner (1964) opened one of his canonical articles with the following paragraph:

Behaviorism, with an accent on the last syllable, is not the scientific study of behavior but a philosophy of science concerned with the subject matter and methods of psychology. If psychology is a science of mental life—of the mind, of conscious experience—then it must develop and defend a special methodology, which it has not yet done successfully. If it is, on the other hand, a science of the behavior of organisms, human or otherwise, then it is part of biology, a natural science for which tested and highly successful methods are available. The basic issue is not the nature of the stuff of which the world is

AUTHOR’S NOTE: Professor Julian Leslie served heroically as action editor for this article. The article draws on themes in other work by the author, and includes revised portions of that work. Please address correspondence to J. Moore, PhD, Department of Psychology, University of Wisconsin–Milwaukee, Milwaukee, WI 53201; Tel: (414) 229-4746; Email: jcm@uwm.edu
made or whether it is made of one stuff or two but rather the dimensions of the things studied by psychology and the methods relevant to them. (p. 79)

Over the years, the literature of psychology has seen numerous discussions and analyses of the subject matter and methods of psychology, starting with Watson’s (1913) classic article on S-R behaviorism and extending to the present (see Moore, 2008). Many of these discussions and analyses distinguish behaviorism from other viewpoints in psychology such as humanism, psychoanalysis, and especially cognitive science, as if behaviorism was a single homogeneous point of view. Moore (2008) has suggested otherwise: There are different forms of behaviorism, including some forms to which applying the label behaviorism is questionable.

The aim of the present review is to critically examine one of these forms—methodological behaviorism—from the standpoint of another: radical behaviorism. In so doing, we hope to gain a greater understanding of the differences between them, and in a broader sense a greater understanding of scientific epistemology. Radical behaviorists argue that methodological behaviorism began in the first quarter of the 20th century and has been the modal, orthodox orientation that underlies nearly all of psychology since the middle of the 20th century. For instance, in an influential article more than 50 years ago, Bergmann (1956), perhaps the archetypical methodological behaviorist, argued that “Virtually every American psychologist, whether he knows it or not, is nowadays a methodological behaviorist” (p. 270). In later statements reminiscent of Bergmann, George Mandler, a prominent cognitive psychologist, explicitly advocated some form of methodological behaviorism in the following passages:

We [cognitive psychologists] have not returned to the methodologically confused position of the late nineteenth century, which cavalierly confused introspection with theoretical processes and theoretical processes with conscious experience. Rather, many of us have become methodological behaviorists in order to become good cognitive psychologists. (Mandler, 1979, p. 281)

[N]o cognitive psychologist worth his salt today thinks of subjective experience as a datum. It’s a construct. . . . Your private experience is a theoretical construct to me. I have no direct access to your private experience. I do have direct access to your behavior. In that sense, I’m a behaviorist. In that sense, everybody is a behaviorist today. (Mandler in Baars, 1986, p. 256)

As readers might infer from the comments above, methodological behaviorism currently underlies mainstream research programs in psychology as well as professional socialization in that discipline. It underlies courses in research methods, experimental design, and statistics in most psychology departments at colleges and universities. It underlies such standardized tests in the discipline as the Graduate Record Examination. Research and psychological explanations that are not consistent with these features are given less weight, if any weight at all, in the scientific community, for example, as reflected in the editorial practices of journals and research support from granting agencies.
METHODOLOGICAL BEHAVIORISM

Radical behaviorists argue that in the final analysis methodological behaviorism is quite complex and, indeed, quite problematic. Because of the complexity of the topic we take a historical–critical approach and also engage a wide variety of correlated matters to provide some historical and conceptual background for the review. In some cases we address one matter in one section and then return to it in a later section to see it from a different perspective or to complete the picture. We conclude that methodological behaviorism is more closely tied to mentalism than to Skinner’s radical behaviorism. Let us begin.

What is Methodological Behaviorism?

Interestingly, writers have defined methodological behaviorism somewhat differently over the years, depending on their purposes. We look at three such definitions. The first is from Skinner (1945). Here, Skinner wrote critically about “methodological behaviorism” in connection with the “operationism of Boring and Stevens”:

The operational attitude, in spite of its shortcomings, is a good thing in any science but especially in psychology because of the presence there of a vast vocabulary of ancient and non-scientific origin. . . .What happened instead was the operationism of Boring and Stevens. . . .It was an attempt to acknowledge some of the more powerful claims of behaviorism (which could no longer be denied) but at the same time to preserve the old explanatory fictions unharmed. . . .A concession is made in accepting the claim that the data of psychology [sic] must be behavioral rather than mental if psychology is to be a member of the United Sciences, but the position taken is merely that of ‘methodological’ behaviorism. According to this doctrine the world is divided into public and private events, and psychology, in order to meet the requirements of a science, must confine itself to the former. This was never good behaviorism, but it was an easy position to expound and defend and was often resorted to by the behaviorists themselves. It is least objectionable to the subjectivist because it permits him to retain ‘experience’ for purposes of self-enjoyment and ‘non-physicalistic’ self-knowledge. . . .The position is not genuinely operational because it shows an unwillingness to abandon fictions. . . .The distinction between public and private is by no means the same as that between physical and mental. That is why methodological behaviorism. . . .is very different from radical behaviorism. . . .(pp. 271, 292-294)

Worth noting is that in this article Skinner was making the case for a particular approach to psychology, which he called radical behaviorism. An important feature of radical behaviorism was that it did allow psychologists to talk meaningfully of private behavioral events. Indeed, Skinner was arguing in favor of a particular conception of verbal behavior as an important element of his approach to psychology. As Skinner indicated, radical behaviorism is to be contrasted with the traditional approach to psychology, which he called methodological behaviorism. Methodological behaviorism argued that psychologists should talk directly only of things that were publicly observable. Skinner argued that
methodological behaviorism led to a variety of problems, definitional as well as procedural, and an entirely different approach to psychology as well as to verbal behavior was warranted.

The second definition is from Bergmann (1956). In this well-known article on the contributions of John B. Watson, Bergmann wrote approvingly rather than critically on methodological behaviorism:

Second only to Freud, though at a rather great distance, John B. Watson is, in my judgment, the most important figure in the history of psychological thought during the first half of the century. . . . [His] contribution was not, as probably Watson thought, his materialism or metaphysical behaviorism—i.e., the thesis, which is merely silly, that there are no minds—but, rather, his methodological behaviorism. . . . What follows is the thesis of methodological behaviorism. It must in principle be possible to predict future behavior, including verbal behavior, from a sufficiency of information about present (and past) behavioral, physiological, and environmental variables. (Bergmann, 1956, pp. 265, 269, 270)

Here, Bergmann downplays Watson’s anti-mentalism in favor of a methodological prescription. Although Bergmann does not use the phrase “publicly observable” in his description of methodological behaviorism, the implication is that when he is talking about the predictive and explanatory sufficiency of present and past behavioral, physiological, and environmental variables, those variables must be publicly observable in order to gain agreement.

The third definition is from Day (1983). Here, Day wrote about Skinner’s understanding of methodological behaviorism as follows:

Skinner looks at methodological behaviorism as something that happened in the history of behaviorism. For him, it was an outlook often associated with behaviorism in the 1940s, and it is linked by him with the logical positivism and operationism of that time, which he speaks of as involving a philosophy of truth by agreement. It was a view which at times appeared to Skinner to want to substitute the verbal report of experience for the experience itself, or on the other hand to want to rule experience out of bounds for science. . . . Skinner’s conception of methodological behaviorism is so narrow that for him simply to make a distinction between methodological and radical behaviorism is for him not to engage at all the complete set of professional practices and beliefs that are now orthodox in most psychology departments. . . . Simply in the distinction between radical and methodological behaviorism nothing is said about strategies of research. No objection is raised at all to the notion of experimental science itself as it is generally understood in the profession. I am thinking here, for example, of such things as the concept of the controlled experiment and its aims. (p. 97)

In these comments Day suggests that methodological behaviorism has two features: (a) a position on how terms relating to the subject matter of psychology should be understood if psychology is to be considered as a science, and (b) a position on how research should then be carried out, in light of that understanding.
Feature (a) is largely consistent with Skinner (1945) and Bergmann (1956), whereas feature (b) elaborates on (a), suggesting procedural and methodological matters beyond the definitional.

Following from such treatments as Skinner (1945), Bergmann (1956), and Day (1983), we suggest that methodological behaviorism is the name for a prescriptive orientation to psychological science. In its current form, its principal feature concerns verbal processes and meaning. According to this feature, all psychological terms and concepts in theories and explanations should be based on observable stimuli and behavior. Observable here means roughly that the stimuli and behavior are measurable according to the instruments commonly used in the laboratory, such as meters, dials, pointers, counters, scales, visual displays, and so on. These measures can be agreed upon by two observers. A common synonym is “intersubjectively verifiable.” Direct appeals to literally mental terms and concepts such as consciousness and the classic introspective processes of early forms of psychology are not allowed; those mental terms and concepts are taken to refer to phenomena that are unobservable and that cannot be agreed upon.

**Methodological Behaviorism: Verbal Processes and Meaning**

We stated above that according to methodological behaviorism, psychological terms should be based on observable stimuli and behavior. We suggest that the phrase “based on” and the attendant views of verbal processes and the meaning of psychological terms have been interpreted in three different ways over the years.

*Interpretation 1*

Interpretation 1 of “based on” is that psychologists should remain formally silent on the mental. In other words, psychologists should not appeal at all to unobservable mental phenomena in their theories and explanations—they should certainly ignore, if not deny, things that aren’t observable. This interpretation arose in the first quarter of the 20th century. Max Meyer’s often-cited book, *The Psychology of the Other-one* (Meyer, 1922), represents one instance of early methodological behaviorism that took this stance. Here Meyer presented the case in an introductory-level book for psychology as an objective, positivistic science of behavior, concerned with its measurable properties. Meyer attempted to call attention to the pragmatic issue that science was primarily concerned with phenomena that could be touched and measured. Conscious experience could not be measured as such. Accordingly, psychology needed to deal with what it could touch, point to, and measure: observable behavior. Psychology had to rule consciousness and mind out of bounds, not so much because they didn’t exist, but rather because they could not be reached by methods whose products could be touched, measured, and agreed upon. Introspection was regarded as largely irrelevant to psychology as a scientific method. A representative passage from early in Meyer’s book provides the flavor of his approach:
In times past one used to turn to psychology books when he wanted to learn something about his Self—his Soul... Modern science owes its triumphs to the fact that it has learned to restrict itself to describing merely that which one can measure. The psychology of the Other-one follows the same road. Why should Robinson Crusoe, wanting information [on Friday], use the antiquated, the sterile method? ... Crusoe's desire to know as much... as possible about his man Friday cannot be satisfied by the psychology of Selves. He needs the psychology of the Other-one. He needs the psychology which applies sense organs to the object of study, compares what the sense organs perceive, counts and leaves the question whether Friday has a Self, a Soul, a Mind, a Consciousness to the single being whom it might concern, to Friday. (Meyer, 1922, pp. 3-4)

Although Interpretation 1 began in the first quarter of the 20th century, its influence has continued in many respects. For example, we can look to the views of the philosopher May Brodbeck (as cited in Natsoulas, 1984):

According to... [Brodbeck’s 1966] interpretation, mental states have no place in scientific explanation. In support of this as an implication of methodological behaviorism, Brodbeck (1966... ) gave two reasons: (a) The psychologists need not mention mental states in any explanation of behavior, and (b) “by the usual scientific criteria of objectivity and reliability, mental terms cannot occur in science” (p. 291). That is, other people’s mental states are not open to the scientist’s scrutiny: “Science is concerned with what is publicly observable and verifiable. The psychologists cannot inspect anyone else’s mind. It follows that no mental terms can literally occur in scientific description and explanation” (Brodbeck, 1971-1972, p. 50). (Natsoulas, 1984, p. 52)

Even on the contemporary scene, a version of Interpretation 1 that argues we need to remain formally silent on unobservables appears from time to time. For example, consider the following passage from Uttal (2008):

The cognitive processes underlying behavior are inaccessible to standard laboratory techniques of measurement and quantification. The properties of psychological time and space are not the same as those of the physical world. We do not have the advantage, therefore, of saying that the laws of the mind are the same as those of overt behavior. Therefore, it is not possible to infer from behavior what are the underlying mental processes and mechanisms. Therefore, the laws of mental causality are extremely difficult if not impossible to identify. Only descriptive correlations are possible... 

Another caveat that has to be expressed before I continue this discussion is that by no means am I rejecting the reality of mental processes. Although it is difficult to define the mind, and standard measures of mental activity are always open to judgments of validity, there seems to be little argument that our consciousness or self-awareness is a real and natural result of neural activity. No one yet knows how the mind is generated by the brain, but there is hardly any disagreement that mental activity is a process of this particular material...
To reject the reality of mental processes would make all of human activities meaningless. (pp. 39-40)

Similarly, consider the following three even more recent passages:

I am not saying they don’t exist. Many different types of private events occur within the skin: . . . subvocal speech (i.e., thinking), and so on. . . . I will argue that private events are not useful in a science of behavior, and . . . constitute an unnecessary distraction. (Baum, 2011a, p. 185)

Thinking and feeling are included. . . not as private events, but as patterns of public behavior. (Baum, 2011b, p. 123)

[A]ll mental states (including sensations, perceptions, beliefs, knowledge, even pain) are rather patterns of overt behavior. [C]onsciousness is . . . the organization of behavioral complexity in which overt behavior is distributed widely over time. (Rachlin, 2012, pp. 3-4)

Interpretation 1 looms large in such passages.

For present purposes, we note that two early forms of psychology were structuralism and functionalism. These forms tended to be popular toward the end of the 19th century and early in the 20th. Although these two forms differed from each other, both took consciousness to be the principal subject matter for psychology, rather than observable behavior. Further, both took introspection to be an appropriate method for revealing facts about consciousness. However, both structuralism and functionalism were decidedly controversial. For instance, many so-called experimental findings often couldn’t be replicated across laboratories. In addition, many individuals in the lay community had trouble understanding what psychologists meant when they said they were investigating whether the “texture” of an image differed from that of a sensation. Consequently, any practical implications of structuralism and functionalism in the broader culture were obscure at best.

Interpretation 1 grew, in part, out of the reaction to the ambiguities of structuralism and functionalism during the first quarter of the 20th century, and it is frequently associated with the rise of classical S-R behaviorism (e.g., Watson, 1913). One assumption commonly (though not universally) associated with Interpretation 1 and hence with classical S-R behaviorism is that of classical positivism: A science should only deal with a subject matter that is observable, measurable, and capable of generating agreement. If psychology was to be a science of mental life, then it was unclear how such a science could be realized. Accordingly, the key element of Interpretation 1 is that psychologists should be formally silent on the mental.

Despite its prescriptive nature, this interpretation allowed psychologists to remain silent for any of several reasons. For example, psychologists might remain silent because they denied the existence of anything that wasn’t observable and measurable, including the mental. Consequently, that psychologists should remain silent on the mental is not especially surprising. Alternatively, psychologists might
acknowledge that causal mental phenomena do exist, but they could be ignored on
the grounds that if psychology is to meet the requirements of science it should only
deal with observables, which mental phenomena manifestly were not. A critical
examination of these phenomena can only be undertaken by another discipline,
such as philosophy or theology, rather than by a science like psychology. In any
case, the theories and explanations expressed in terms of observables were argued
to be just as satisfactory as those expressed in mental terms.

As time passed, slightly different interpretations of “based on” emerged. These interpretations led to different prescriptions for accommodating mental terms in psychological theories and explanations. We may now consider a second interpretation, derived from a particular view of operationism.

**Interpretation 2**

Interpretation 2 of “based on” was that psychologists could appeal to psychological terms ostensibly referring to unobservables after all, but the terms should be “logically connected” to observable stimuli and behavior through an “operational definition.” An operational definition expressed the meaning of a concept in terms of the operations entailed in its measurement. For example, if a psychologist assumed that a participant’s choice behavior in a two-alternative task was mediated by “demand,” the concept of demand needed to be operationally analyzed. Presumably, the concept could be related to the magnitude of the goal object or other consequence of the behavior (“purpose”), the hours of deprivation that were imposed on the subject (“drive”), and so on. By so doing, psychologists could rigorously anchor their explanatory concepts to the public world in a way that could be agreed upon. After all, psychologists did not want to be accused of investigating pseudo-problems or spouting patently meaningless nonsense, and operationism was thought to provide the necessary guarantees. Importantly, in Interpretation 2 the operational definition should be “exhaustive” in the sense that the total meaning of the term was imparted through the particular observable circumstances that constituted its operational definition.

Thus, Interpretation 2 argues for a different way of proceeding than does Interpretation 1. Interpretation 2 reflects an attempt to deal scientifically with unobservable mental phenomena, instead of a requirement for psychologists to remain formally silent on the mental. Operationism made the appeals to such terms scientifically and empirically respectable. Nevertheless, operationism admits such unobservables as mental variables only indirectly and implicitly, rather than directly and explicitly, as it attempts to guarantee that scientific terms and concepts are rigorously based on observables (Smith, 1986).

Interpretation 2 arose early in the second quarter of the 20th century. Despite the seeming advantages of the earlier S-R formulation based strictly on observables, by the 1930s three problems with the formulation were apparent. One problem was the variability of behavior. Even when a characteristic stimulus did precede a response, the topography and frequency of the response often varied significantly over time. A second problem was the seeming spontaneity of behavior.
Some responses seemed to develop even though a characteristic stimulus did not apparently evoke them. A third problem was that the culture had an abundance of “psychological” and “subjective” terms that seemed to appeal to unobservable mental phenomena. Are we not able to talk meaningfully about our aches and pains, beliefs and intentions, and other ostensibly mental phenomena? In light of such concerns, how could a science gain credibility in the culture at large if it disregarded talk about these ostensibly mental phenomena, as an S-R approach based strictly on observables (e.g., Interpretation 1) seemed to do?

How, then, were seemingly mental phenomena to be accommodated? According to a common practice, the phenomena were inserted as organismic variables to mediate the relation between stimulus and response, thereby creating an S-O-R formulation. By mediating we mean that observable external stimuli activate or trigger some unobservable intervening or mediating phenomena that are causally connected in some complex but systematic way to an ensuing observable response. The approach may be called mediational S-O-R neobehaviorism to distinguish it from the classical S-R behaviorism that preceded it. Further psychologists should designate the psychological terms as “theoretical terms,” in contrast to such “observational terms” as stimuli and responses, which could be directly measured. The learning theories of E. C. Tolman, C. L. Hull, and K. W. Spence all contained such mediating theoretical terms (e.g., demands, expectancies, vicarious trial and error, inhibition, afferent neural interaction, stimulus intensity dynamism). In light of the professional influence of these learning theorists, the emerging fields of social psychology, personality theory, abnormal and clinical psychology, sensation and perception, and so on adopted a similar mediational approach.

We may now consider a third interpretation of “based on” derived from a different view of the principle of operationism.

**Interpretation 3**

Interpretation 3 of “based on” was that psychologists could continue to appeal to psychological terms ostensibly referring to unobservables, again with the stipulation that the unobservables should be logically connected to observable stimuli and behavior. As with Interpretation 2, Interpretation 3 allowed psychologists to indirectly and implicitly admit the unobservables as operationally defined theoretical terms, commonly as mediating organismic terms in a neobehaviorist S-O-R model. However, this time the operational definition need only be “partial” and could admit surplus meaning.

Interpretation 3 shows the influence of further developments in philosophy of science and psychology later in the second quarter of the 20th century. These developments permitted definitions of theoretical terms to be partial instead of exhaustive. Philosophers of science departed from exhaustive definitions and embraced partial definitions of theoretical terms because the philosophers wanted to resolve technical considerations related to formal logic. For example, a common tactic was to render psychological terms as “dispositions,” or observable behavior
in specific antecedent circumstances. However, what was the existential status of a disposition if the test operation that verified the disposition was not being carried out at literally that moment in time? Partial rather than exhaustive definitions allowed philosophers to circumvent this question. Psychologists departed from exhaustive definitions and embraced partial definitions of theoretical terms because the psychologists wanted to promote theories that had an agreeably broad range of application, which they could not do with exhaustive definitions. In a landmark article, the psychologists MacCorquodale and Meehl (1948) proposed a formal linguistic convention concerning unobservable theoretical terms. More specifically, they suggested that one type of theoretical term be called an “intervening variable” and another type a “hypothetical construct.” An intervening variable did not refer to an entity that was assumed to actually exist. Rather, it was simply a summary term that was exhaustively defined according to the processes and operations by which it was formulated. For example, it might be the product of a defined mathematical function. In contrast, a hypothetical construct was assumed to have some existential status. As an entity that was assumed to actually exist, it was not defined by a single referent or process. Rather, it had multiple referents, none of which was all inclusive. Given its existential status, it was assumed to have properties and implications and extrapolations that hadn’t yet been demonstrated. Therefore, it could have an explanatory application in more than one case. The hypothetical construct interpretation came to predominate in the discipline. Slightly later, Miller (1959) continued in this same vein by advocating a “liberalized” approach to the meaning of psychological explanatory concepts, consistent with the spirit of the times. Overall, it was not so much that prior to MacCorquodale and Meehl’s article all theoretical terms were intervening variables, and afterwards all were hypothetical constructs. Recall intervening variables were not things that were assumed to exist. Rather, it was that earlier theoretical terms were assumed to be things that existed but had to be exhaustively defined, and psychologists couldn’t develop general principles or build general systems under these restrictions. The advent of hypothetical constructs gave the necessary degrees of freedom that permitted new flexibility, particularly as the hypothetical constructs were construed as independent variables.

Readers may notice that according to Interpretation 1, psychologists can’t talk at all about mental events, whereas according to Interpretations 2 and 3, psychologists can. How, then, do the three interpretations count as instances of the same point of view? The important word here is direct. We can say that all are instances of the same point of view because all three interpretations prohibit direct and explicit talk about mental events. Interpretation 1 explicitly prohibits any kind of talk about mental events. Interpretations 2 and 3 do allow talk, but that talk must be indirect and implicit, by virtue of operationism, rather than direct and explicit.

In any event, in one version or another Interpretation 3 is currently the orthodox position in the discipline in areas ranging from learning theory to social psychology to various flavors of cognitive science, and it has been since the middle of the 20th century.
Radical Behaviorism and Private Behavioral Events

The passage from Skinner’s (1945) contribution to the Symposium on Operationism cited in the introduction of the present review suggests an alternative to methodological behaviorism called radical behaviorism. Radical behaviorism differs in many respects from Interpretations 1, 2, and 3 above, even if we acknowledge the differences among those interpretations. As we have seen, radical behaviorism views Interpretations 1, 2, and 3 as forms of methodological behaviorism, in recognition of their common assumption that if psychology is to be a scientifically respectable discipline, the terms and concepts in psychological theories and explanations should be based on phenomena that are directly observable, measurable, and capable of generating agreement. For radical behaviorism, some phenomena that are currently not accessible to individuals beyond those who are behaving can and should be included in psychology, even though the phenomena may not be directly observable, may not be directly measurable, and may not generate agreement. However, the phenomena should not be regarded as from a nonbehavioral dimension (e.g., acts, states, mechanisms, etc., from a mental, cognitive, subjective, etc., dimension that cannot be described in behavioral terms or in the same terms as environmental variables and relations) simply because the vantage point of another person means they may not be directly observable, not directly measurable, and not capable of generating agreement. As Skinner put it,

No matter how clearly these internal events may be exposed in the laboratory, the fact remains that in the normal verbal episode they are quite private. . . . There is, of course, no question of whether responses to private stimuli are possible. They occur commonly enough and must be accounted for. But why do they occur, what is their relation to private stimuli, and what if any, are their distinguishing characteristics? (1945, p. 273)

And again,

Behaviorists have, from time to time, examined the problem of privacy, and some of them have excluded so-called sensations, images, thought processes, and so on, from their deliberations. . . . The strategy is, however, quite unwise. It is particularly important that a science of behavior face the problem of privacy. It may do so without abandoning the basic position of behaviorism. . . . An adequate science of behavior must consider events taking place within the skin of the organism, not as physiological mediators of behavior, but as part of behavior itself. It can deal with these events without assuming that they have any special nature or must be known in any special way. The skin is not that important as a boundary. Private and public events have the same kinds of physical dimensions. (1969, pp. 227-228)

Day commented in several places on the critical importance of private events in distinguishing between radical and methodological behaviorism. Here is one:
The lack of careful study of Skinner’s work has led to professional absurdities too numerous to review in detail. Strange blends of Skinner and conventional behaviorism abound. I would rather not identify the even relatively prominent Skinnerians who fail to concede that private events have any place in a natural science. Others view *Science and Human Behavior* as somehow beneath their empirical dignity; the word is passed around that the sticky parts of the book are to be excused because it is, after all, no more than a sophomore level text—this in spite of the fact that in a work as crucial as *Verbal Behavior*, Skinner refers the reader back again to *Science and Human Behavior* for his most thorough analysis of the issue of private experience (1957, p. 130). Mentalism among Skinnerians is rampant, and they are quickly trapped by the operationism of Boring and Stevens. Unfortunately, only very few people have an accurate idea of what Skinner means by operational definition. I have taken the liberty of speaking here directly to some of those who preach most loudly a supposedly Skinnerian line. (1969b, pp. 325-326)

Of course, we do not know whether Day had either Baum or Rachlin in mind when he wrote the passage above (e.g., Baum, 2011a, 2011b; Rachlin, 2012), given that both Baum and Rachlin are graduates of the Harvard department when Skinner was there. In any case, here is another passage from Day:

Since radical behaviorism is different from methodological behaviorism, we should not be surprised to find, then, that radical behaviorism has a special account to give of *privately* observable behavior; and indeed Skinner’s discussion of the distinction between radical and methodological behaviorism turns on this issue. (1983, p. 90)

We say a great deal more about private behavioral events and their relation to both radical and methodological behaviorism in the remaining sections of the present review.

For now, we can return to the previously mentioned point that radical behaviorism takes a different approach to the analysis of verbal behavior than does traditional psychology. More specifically, radical behaviorism does not distinguish between observational and theoretical terms in the same fashion as does traditional psychology. Consequently, for radical behaviorism psychological terms need not be understood as referring to initiators of behavior, or to mediating organismic phenomena in a neobehaviorist S-O-R model. When methodological behaviorists and mentalists do appeal to such initiating or mediating terms and concepts, those terms and concepts need to be examined on a case-by-case basis to determine what that talk is actually about. If the verbal behavior appeals to mentalistic explanatory fictions, then the verbal behavior may be analyzed to determine what social-cultural factors are responsible for its emission. In a more positive and constructive vein, some instances of behavior are private or covert, in that they are accessible to only the behaving person. However, these instances may still be incorporated into a naturalistic science of behavior using the same terms and concepts as behavior that is accessible to others. What remains important throughout is the pragmatic
question: To what extent does any term help to promote effective interaction with nature?

What Do Methodological Behaviorists Say is the Basis for Their Position on Verbal Processes and Meaning?

We may now more closely examine what methodological behaviorists say is the basis for their position on verbal processes and meaning. We focus on how methodological behaviorists dealt with the meaning of the mediating organismic variables in Interpretations 2 and 3 above.

As mentioned earlier in the present review, starting in the 1930s researchers and theorists amended an S-R formulation by inserting mediating organismic variables between stimulus and response, thereby creating the S-O-R neobehaviorist formulation. The researchers believed that this move allowed them to correct the inadequacies of classical S-R behaviorism and accommodate such observations as the variability and seeming spontaneity of behavior. These mediating variables were then given the status of unobservable theoretical terms to contrast them with observable variables that could be measured with standard scientific instruments.

In a mediational approach, some sort of organismic mediator is assumed to be inside the organism in some sense, as part of its psychological make-up. The organismic mediator is neither behavioral nor environmental. Rather, it is an unobservable feature of another dimensional system. At issue is whether the mediator operates according to the same behavioral laws that govern observable stimuli and responses (e.g., Zuriff, 1985, pp. 104, 156). Typically, the answer was no. Its operating characteristics were held to differ qualitatively from those of behavioral systems. In light of the negative answer to this and comparable questions, the mediator has the status of a functionally autonomous causal entity in a nonbehavioral system that underlies behavior, and with respect to which an explanation of behavior is properly sought. In some versions of a mediational approach the organismic variables are presumed to be causal by affording “competence” and making the behavior in question possible. In other versions the environment is held to activate or trigger in some complex but systematic way an organismic variable which, in turn, is held to activate or trigger in some complex but systematic way an eventual response. The organismic variable is causal in the sense that the mediators are what are temporally contiguous with the response. The organism’s behavior is therefore understood as a function of the mediating entity or entities, rather than the environmental circumstance that triggers the chain of mediating events. Regardless, the causal nature of the organismic mediator, such as its features and operating characteristics, is taken as the proper focus of psychological science, rather than a functional relation between environmental stimuli and response.

In many versions the mediators are explicitly assumed to be in a mental or cognitive dimension, as when an explanation of one or another sort of behavior consists of postulating one or another sort of unobservable but underlying mental
act, state, mechanism, process, entity, structure, or faculty as a cause of the behavior. Terms from contemporary cognitive psychology such as consciousness, representations, icons, subjective interpretations, cognitions, expectancies, memories, attention, arousal, encoding, storage, and retrieval often illustrate this approach. In this regard, organisms are said to discriminate or generalize or interpret or attend to stimuli, where such terms imply a mediating inner and explicitly nonbehavioral process that precedes and is causally responsible for the eventual observable behavior. Sometimes the processes are expressed in the form of gerunds to make the causal relation appear more dynamic, as in discriminating, generalizing, interpreting, or attending. The exact language may take any of several forms, but at heart it exemplifies a commitment to mediating internal states or processes. As the mediational neobehaviorist E. C. Tolman (1951) later put it,

A theory, as I shall conceive it, is a set of intervening variables. These to-be-inserted intervening variables are “constructs” which we, the theorists, evolve as a useful way of breaking down into more manageable form the original complete...function. (pp. 150-151)

So we have it that many theories in psychology came to consist of appeals to these intervening organismic entities, inserted to mediate the relation between observable stimulus and response variables. The important question was how to ensure that the mediational approach was scientifically respectable in light of the methodological behaviorist tradition of terms and concepts that were based on observable stimuli and behavior. Here is where Interpretations 2 and 3 of methodological behaviorism came in. Methodological behaviorists argued that the principle of operationism provided the required respectability. Typically, the mediating organismic variables were given the status of unobservable “theoretical terms” that needed to be “operationally defined” by logically connecting them to the publicly observable variables entailed in their measurement. By arguing that the meaning of their theoretical terms could be safely expressed by logically connecting them to observable variables or data via operational definition, methodological behaviorists believed their terms and concepts met the requirements for observability, measurability, and agreement. Tolman’s “operational behaviorism” (e.g., 1936) was an early example of this approach, though not the only one.

The Principle of Operationism

We now need to say a bit more about operationism. Operationism was a principle for evaluating the meaning of scientific terms and concepts promoted by P. W. Bridgman (1882–1961), a Harvard physicist and Nobel laureate who investigated the properties of matter under high temperature and pressure. In his landmark book, The Logic of Modern Physics, Bridgman (1927) wrote that

We mean by any concept nothing more than a set of operations; the concept is synonymous with the corresponding set of operations. . . . In principle the
operations by which length is measured should be *uniquely* specified. If we have more than one set of operations, we have more than one concept, and strictly there should be a separate name to correspond to each different set of operations. (pp. 5, 10)

Operations came in two categories: (a) instrumental operations, in which scientific instruments were used; and (b) paper and pencil operations, in which calculations might be involved, often involving the products from instrumental operations.

One of Bridgman’s examples was length or distance. Suppose we are interested in determining the length of a plot of land. One means of doing so is to use a meter stick and measure the plot by repeatedly moving the meter stick end-to-end along one dimension of the plot. This means of determining length would be an instrumental operation. Another means is to use surveyors’ instruments and compute the desired dimension from a known angle and another dimension. This means would be a paper and pencil operation. Importantly, for Bridgman, we need to recognize that at least initially, the concept of length or distance as measured by a meter stick differed from the concept of length as computed by triangulation because different operations were involved. Thus, there were separate meanings of the term “length.” Each meaning was exhaustive, in the sense that it was restricted to particular circumstances.

Einstein’s work on relativity influenced Bridgman enormously. Einstein’s work had demonstrated that the meaning of scientific terms and concepts was not eternal, fixed, constant, and absolute. For example, consider the concept of time. For Newton, time was eternal, fixed, constant, and absolute throughout the universe. In contrast, Einstein had shown that time was relative to the vantage point of the observer: Time meant one thing for a stationary observer and another thing for an observer moving at near the speed of light. The meaning of time for each observer was unique in the sense that it pertained to its own circumstances of use with no necessary implications for other circumstances of use.

Bridgman saw his work as primarily methodological and pragmatic. He disliked labeling his thesis as an –ism, fearing it would be taken as dogma. He much preferred to speak of his thesis as “the operational outlook” or “the operational point of view.” As Bridgman (1936) later put it, “The ultimately important thing about any theory is what it actually does, not what it says it does or what its author thinks it does, for these are often very different things indeed” (p. 5). What Bridgman wanted to emphasize was the experience of the behaving scientist. However, he bordered on phenomenalism. He argued that in the final analysis, science was only his private science, and knowledge was only his personal knowledge (Bridgman, 1936). For Bridgman (1927), “From an operational point of view it is meaningless to attempt to separate ‘nature’ from ‘knowledge of nature’” (p. 62).
Operationism as Interpreted By Others

Bridgman’s thesis attracted a great deal of attention from both philosophers and scientists. In philosophy, the thesis was congenial with what the developing school of logical positivists was saying about how to verify the meaning of terms and concepts in scientific theories, as the logical positivists sought to rationally reconstruct science in a way that would elucidate how terms and concepts were cognitively significant. Operationism meant that the observational and empirical basis of terms and concepts could now be made clear. As Feigl (1959) put it, “Operationism, just like behaviorism or other forms of radical positivism, has had an undeniably helpful influence in the development of recent science. Untestable assumptions have been relegated to the limbo of metaphysics” (p. 126). As Hempel (1954) put it,

The emphasis on ‘operational meaning’ in scientifically significant discourse has unquestionably afforded a salutary critique of certain types of procedure in philosophy and in empirical science and has provided a strong stimulus for methodological thinking. (pp. 215-216)

In the sciences, operationism was noted in physics, being the product of a fellow physicist, but it proved much more influential in the social sciences. For present purposes it is important to note that for psychology, the conventional interpretation of operationism led, in turn, to methodological behaviorism: “Applied to psychological concepts, operationism becomes methodological behaviorism” (Bergmann, 1954, p. 213). Tolman (e.g., 1936), Stevens (e.g., 1935a, 1935b, 1936, 1939), and Bergmann and Spence (e.g., 1941) were especially active in promoting operationism in psychology. For example, according to Stevens (1939),

[S]cience seeks to generate confirmable propositions by fitting a formal system of symbols (language, mathematics, logic) to empirical observations, and . . . the propositions of science have empirical significance only when their truth can be demonstrated by a set of concrete operations. . . . Science, as we find it, is a set of empirical propositions agreed upon by members of society. . . . Only those propositions based on operations which are public and repeatable are admitted to the body of science. Not even psychology knows anything about private experience, because an operation for penetrating privacy is self-contradictory. . . . What becomes acceptable psychology accrues only when all observations, including those which a psychologist makes upon himself, are treated as though made upon ‘the other one.’ . . . A term denotes something only when there are concrete criteria for its applicability; and a proposition has empirical meaning only when the criteria for its truth or falsity consist of concrete operations which can be performed on demand. (pp. 222, 227-228).

In this regard, given a distinction between the semantics and the syntax of a theory, operationism provided the semantic basis for empirically verifying the content of a theory (e.g., Bergmann & Spence, 1941, p. 1; Stevens, 1939, p. 223).
In the division of scientific labor, philosophy would then be more concerned with the syntax of the theory.

Stevens (1936, 1939) saw psychology as the propadeutic science. Within psychology, the study of sensation was fundamental. Discrimination became the fundamental operation of all science. Consequently, the fundamental business of science was to measure the discriminatory capacities of a behaving organism. Stevens took it for granted that these sensory contents could be dealt with scientifically even though they were unobservable and “subjective” in their own right. A critical question was how to achieve a degree of public agreement in light of the unobservable character of sensory content and experience. Stevens’s (1939) answer was through a discrimination procedure:

When we attempt to reduce complex operations to simpler and simpler ones, we find in the end that discrimination, or differential response, is the fundamental operation. Discrimination is prerequisite even to the operation of denoting or ‘pointing to,’ because whenever two people reduce their complex operations for the purpose of reaching agreement or understanding, they find that unless they can each discriminate the same simple objects or read the same scales they still will not agree. Agreement is usually reached in practice before these most elementary operations are appealed to. (p. 228)

Thus, if a participant’s behavior in a psychophysical judgment task was inferred to be mediated by the magnitude of the participant’s subjective sensation—clearly a private phenomenon—then that sensation needed to be made public through the operation of measuring a verbal report in a discrimination procedure, so that it could be agreed upon. For the purposes of science, the verbal report was the sensation.

Concerns about Operationism in the Mid-20th Century

Despite the seeming virtues of operationism, many raised concerns. Benjamin (1955) documented a large number of these concerns in the mid-20th century, while Feest (2005), Grace (2001), and Green (1992) reviewed concerns from a more recent point of view. One concern was whether operationism was only relevant to evaluating concepts that had already been developed, rather than to developing new concepts. How did operationism relate to scientific ingenuity and insight, if at all? With its emphasis on what was observable, did operationism have a chilling effect and thwart the development of new concepts and, indeed, scientific progress? For his part, Bridgman (1954) was perplexed:

In general, I think that there need be no qualms that the operational point of view will ever place the slightest restriction on the freedom of the theoretical physicist to explore the consequences of any free mental construction that he is ingenious enough to make. It must be remembered that the operational point of view suggested itself from observation of physicists in action. (p. 226)
A second concern was even more controversial: How literally should philosophers and scientists take the principle that different operations entailed different concepts? Many philosophers and scientists had taken Bridgman’s thesis to imply that operational definitions were particular and unique. After all, hadn’t Bridgman claimed that the meaning of a concept was “synonymous” with the operations by which it was measured? Hadn’t Bridgman claimed that concepts should have uniquely different names if they corresponded to uniquely different operations? Didn’t operationism imply exhaustive definitions where the meaning of a concept was exhausted when a given scientist carried out his or her particular operations? If a concept is always to be defined by an operation, and operations were particular acts performed at particular times and places by particular individuals, aren’t meanings restricted to particular instances and not to general systems that applied across situations?

These questions are complex and require some sorting out, if only to clarify Bridgman’s own position as well as that of others. Readers will recall that Bridgman (1927, p. 5) had used the phrase “set of operations.” Presumably, Bridgman recognized that an operation carried out on a different side of the experimental laboratory or by a scientist wearing a different-colored lab coat did not necessarily mean a different concept. Rather, he wanted to emphasize that even if we thought we were conducting the same operation at a different time or place, we could not be assured we actually were, as his own research showed: The same matter responded differently to some operation when the matter was under high temperature and pressure than when it was under low. Nevertheless, Bridgman recognized that if operations repeatedly yielded the same results we could conclude that we were dealing with something that actually existed—we had conducted “converging operations.” Presumably, we can state that for Bridgman, the question of whether different operations imply different concepts is ultimately an empirical question to be answered pragmatically by how well the resulting data corresponded to the concept. Interestingly, for Bridgman operations became a necessary but not sufficient condition for determining meaning.

In 1944 the psychologists Israel and Goldstein published an article in Psychological Review that was critical of operationism (Israel & Goldstein, 1944). The article caused such a stir that the Harvard psychologist E. G. Boring, long an advocate of operationism and a mentor of S. S. Stevens, suggested to the editor Herbert Langfeld that a symposium be convened to resolve some disputed points. Some representative questions were formulated to which participants could respond. The participants included many noted individuals from the physical sciences, social sciences, and philosophy (Boring, 1945). Question 2 reads as follows:

2. When the same construct is defined by two independent operations, should it be said that there are really two constructs? For instance, it has been said that tape-measured distance and triangulated distance are really two kinds of distance and should perhaps have different names.
Against this view it can be argued that these are operations for showing the equivalence of operations, e.g., for demonstrating the identity of taped and surveyed short distances (Langfeld, 1945, p. 241).

As evident, this question directly concerned whether different operations entailed different concepts. Boring (1945, p. 243) made a good faith effort to answer question 2 above, favoring an equivalence of concepts even though different operations were involved. However, most of the other participants danced around this and other questions and simply restated their entrenched positions. Their lack of agreement on important points was notable, and nothing was actually resolved.

**Skinner and Operationism**

Among the participants in the symposium was B. F. Skinner, who received his doctoral degree from Harvard in 1931. While at Harvard, Skinner had sparred with Boring over various matters related to his dissertation. Skinner’s dissertation was an operational analysis of the term reflex in historical-critical terms. Skinner was introduced to Bridgman’s operationism by a fellow student in mathematics, Cuthbert Daniel, and Skinner identified it as an important influence in his professional development. Skinner and Stevens were contemporaries in the Harvard department, and Skinner was well aware of Boring and Stevens’s advocacy of their own particular interpretation of operationism during the 1930s. By the time of Boring’s Symposium on Operationism, Skinner had clearly become disenchanted with the way traditional psychology had interpreted operationism, as evidenced by the passage in the introduction of the present review and his not particularly veiled criticism of his mentor Boring and his contemporary Stevens (Skinner, 1945, pp. 271, 292-294).

For Skinner, operationism involved the functional analysis of verbal behavior. At issue was how a scientist’s operations and contacts with data occasioned scientific concepts and led to effective scientific behavior. Verbal behavior that was under the control of incidental sources that were cherished for irrelevant reasons could be discarded:

But by the time Bridgman’s book was published, most of the early behaviorists, as well as those of us just coming along who claimed some systematic continuity, had begun to see that psychology actually did not require the redefinition of subjective concepts. The reinterpretation of an established set of explanatory fictions was not the way to secure the tools then needed for a scientific description of behavior. Historical prestige was beside the point. There was no more reason to make a permanent place for ‘consciousness,’ ‘will,’ ‘feeling,’ and so on, than for ‘phlogiston’ or ‘vis anima.’ On the contrary, redefined concepts proved to be awkward and inappropriate, and Watsonianism was, in fact, practically wrecked in the attempt to make them work.

Thus, it came about that while behaviorists might have applied Bridgman’s principle to representative terms from a mentalistic psychology (and were most competent to do so), they had lost all interest in the matter. They might as well
have spent their time in showing what an eighteenth century chemist was talking
about when he said that Metallic Substances consisted of a vitrifiable earth
united with phlogiston. There was no doubt that such a statement could be
analyzed operationally or translated into modern terms, or that subjective terms
could be operationally defined. But such matters were of historical interest only.
What was wanted was a fresh set of concepts derived from a direct analysis of
the newly emphasized data, and this was enough to absorb all the available
energies of the behaviorists. . . (Skinner, 1945, pp. 292-293)

In contrast, the traditional interpretation of operationism, such as advocated
by Boring and Stevens, was concerned with deploying a publicly observable
surrogate or proxy for some causal entity from a mental dimension, in a
continuation of mentalism and of preserving “explanatory fictions.” For Skinner,
the problem was that the traditional interpretation was predicated on an ineffective
mentalist conception of verbal behavior itself and the role of verbal behavior in
science that followed from that conception. Stevens had implicitly granted this
mental dimension in his commitment to a discrimination procedure as a proxy for
sensation: “Stevens has applied Bridgman’s principle to psychology, not to decide
whether subjective events exist, but to determine the extent to which we can deal
with them scientifically” (Skinner, 1969, p. 227).

Logical Positivism and Logical Behaviorism

We may now return to the previously mentioned events in philosophy at the
end of the first quarter and beginning of the second quarter of the 20th century, at
about the same time that psychology began to move from classical S-R
behaviorism to mediational S-O-R neobehaviorism. We are speaking here about
the rise of “logical positivism.” Logical positivism embraced the twin theses of
verificationism and physicalism. Verificationism means to establish the truth or
falsity of a proposition by specifying the conditions under which the proposition is
true and those under which it is false. The conditions are either (a) publicly
observable themselves or (b) logically connected to publicly observable conditions.
Statements are said to be cognitively significant and meaningful when they can be
verified according to these two criteria. Statements are meaningless when we
cannot specify the conditions according to which they are true or false.
Physicalism is a related thesis. It means that for every sentence P in the language
of a branch of science, including psychology, there must be a sentence Q in the
language of physics such that P and Q can be logically deduced from each other,
without remainder. Taken together, verificationism and physicalism were viewed
as surefire means for ensuring that scientific terms and concepts were meaningful
and appropriate.

The version of psychology that developed under the auspices of logical
positivism was called “logical behaviorism.” Logical behaviorism sought to
explicitly and exhaustively reduce the meaning of all psychological terms and
concepts to observable, physicalistic measures, so that the truth value of
propositions containing them could be assessed. Passages from articles by several
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Eminent logical positivists illustrate the verificationist, physicalist treatment of psychological terms in logical behaviorism. We begin with Carnap (1932–1933/1959):

Every psychological term is translatable into a statement about the physical state of the body of the organism. . . . We are not demanding that psychology formulate each of its sentences in physical terminology. For its own purposes psychology may, as heretofore, utilize its own terminology. All that we are demanding is the production of definitions through which psychological language is linked with physical language. We maintain that these definitions can be produced, since, implicitly, they already underlie psychological practice. . . . Every psychological property is marked out as a disposition to behave in a certain way. (pp. 166, 167, 186)

Dispositions were regarded as theoretical terms of a largely quantitative or mathematical nature: a probability of engaging in a specific form of behavior, given specific antecedent circumstances. This mathematical treatment of behavior was not always precise enough for the logical positivists, however. As a result, the logical positivists sought to have scientific concepts expressed in terms of the underlying physiological microstructure and become observational terms whenever possible. Behavior served as evidence of that microstructure, which was the proper concern of science. We can consider next the words of another logical positivist, Carnap (1932–1933/1959), who emphasized

a physical [micro]structure characterized by the disposition to react in a specific manner to specific physical stimuli. . . . ‘Person X is excited’ means ‘If, now, stimuli of such and such a sort were applied, X would react in such and such a manner’ (both stimuli and reactions being physical events). Here too the aim of science is to change the form of the definition; more accurate insight into the micro-structure of the human body should enable us to replace dispositional concepts by actual properties. (pp. 172, 186-187)

Presumably, information in psychology about the underlying physiological microstructure includes not only information about the central nervous system but also information about the peripheral nervous system, pulse, respiration, glandular secretions, and the like. All this would be revealed by public observation of readings on dials, pointers, and meters. In like manner, the meaning of all psychological terms pertaining to the mental would be verified through physicalistic measures. Hempel’s (1935/1949) comments are illustrative:

[All psychological statements which are meaningful, that is to say, which are in principle verifiable, are translatable [without loss of theoretical content or change of meaning] into statements which do not involve psychological concepts but only the [spatio-temporal] concepts of physics. (p. 18)

Indeed, in a stance reminiscent of Interpretation 1 of methodological behaviorism as discussed earlier in this review, Hempel (1935/1949) explicitly ruled out ontological discussions of the concepts as metaphysical distractions:
Logical behaviorism claims neither than minds, feelings, inferiority complexes, voluntary actions, etc., do not exist, nor that their existence is in the least doubtful. . . The thesis developed here. . . by no means offers a theory belonging to the domain of psychology, but rather a logical theory about the propositions of a scientific psychology. Its position is that the latter are without exception physicalistic statements, by whatever means they have been obtained. Consequently, it seeks to show that if in psychology only physicalistic statements are made, this is not a limitation because it is logically impossible to do otherwise. (p. 381)

Logical Positivism and the Interpretation of Theoretical Terms

The logical positivists argued that physicalist definitions of psychological terms and concepts meant that those terms and concepts could be formally deployed in theories and explanations that had the same scientific status as those in other science, such as physics, by virtue of their logical and semantic validity. Many logical positivists found operationism compatible with their own view of verification: The operations one performed, for example, while measuring the phenomena of interest, were the conditions that could verify the meaning of scientific phenomena in physicalistic terms and contribute to establishing the truth value of statements that incorporated them. Importantly, the “without remainder” clause in physicalism implied exhaustive definitions, in the sense we discussed earlier.

By the mid-1930s the logical positivists had begun to work through some of the logical implications of their early embrace of exhaustive definitions, as well as operationism. Suppose we throw a stone at a pane of glass and the glass breaks. We ask “Why?” and answer “Because it is brittle.” What, then, does brittleness mean? Well, presumably it means that glass has a certain molecular structure, so that when we submit it to certain test conditions, such as throwing a stone at it, we can count the number of pieces into which it breaks. In alternative words, we might say brittleness means the glass has a disposition to break when we throw a stone at it.

Now the question becomes, “What is the status of the brittleness in the absence of the test condition?” Doesn’t the glass have the same molecular structure, regardless of whether we are currently throwing a stone at it and counting into how many pieces it breaks? Moreover, won’t the glass break regardless of the size of the stone we throw at it? Note that according to operationism and exhaustive definitions, to talk of brittleness in the absence of the operation of throwing a stone and counting the resulting broken pieces was nonsensical. Moreover, the brittleness of glass as measured by its breaking after being hit with a large stone was not the same as brittleness as measured by its breaking after being hit with a small stone, any more than length as measured with a ruler is the same as length as computed by triangulation. How could scientists develop a general theory about the molecular properties of substances, like brittleness, if their concepts applied in one and only one case?

In recognition of such problems, the logical positivist Carnap (1936, 1937) proposed that definitions (i.e., interpretations of theoretical terms) need not
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actually be exhaustive, in the original sense of logical positivism or operationism. Rather, they could be partial, in the modified sense that they came at the end of a logically valid chain of reasoning that started with observables. In fact, this change in position was so highly significant that Smith (1986, p. 28) suggested the logical positivist movement after this time be known as “logical empiricism” to highlight the change. In any case, what remained important was the logical validity of their terms in the context of justification. The logical validity was assessed by empirically testing predictions of the theories according to hypothetico-deductive methods.

In one place, the logical empiricist Feigl stated the revised position as follows:

Even the positivists have mended their ways. They have liberalized their erstwhile extremely restrictive meaning criterion. That is the main reason why most of them prefer to label themselves as “logical empiricists” or “scientific empiricists.” These new party designations are intended to convey the shift from verifiability to confirmability. That is to say that we no longer insist on complete and direct testing, but that we consider any statement as scientifically meaningful if it is at least incompletely and/or indirectly testable. (Feigl, 1959, pp. 123-124)

In another place, Feigl said that

Statements about mental events are not translatable into statements about (actual or possible) overt behavior. . . .The meaning of statements (at least in one very important sense of “meaning”) is to be identified with their factual reference, and not with their evidential basis. The slogans of early logical positivism and of ultra-operationism about meaning and verification—while helpful in the repudiation of transcendent metaphysics—despite their imprecision were far too restrictive to do justice to the actual conceptual structure of knowledge. Given this general outlook it becomes obvious that the naïve peripheralistic forms of behaviorism must be repudiated and their shortcomings remedied by the admission of central states and processes as the genuine referents of psychological terms. . . .Concepts such as memory trace may be taken to refer to (as yet very incompletely specified) central conditions. (Feigl, 1963, pp. 247-248, 252)

In such passages Feigl can be seen to depart from exhaustive definitions, which require that psychological terms refer only to something observable and peripheral with no implications for anything beyond the particular and unique case. As an alternative, he can be seen to embrace partial definitions, which allow psychological terms to refer to something unobservable and central that does have implications for other cases.

The logical positivist Carnap (1956) came to frame the problem as follows:

In a way similar to the philosophical tendencies of empiricism and operationism, the psychological movement of Behaviorism had, on the one hand, a very healthful influence because of its emphasis on the observation of behavior as an
intersubjective and reliable basis for psychological investigations, while, on the other hand, it imposed too narrow restrictions. First, its total rejection of introspection was unwarranted. . . . Secondly, Behaviorism in combination with the philosophical tendencies mentioned led often to the requirement that all psychological concepts must be defined in terms of behavior. . . . [T]he interpretation of a psychological concept as a theoretical concept, although it may accept the same behavioristic test procedure based on S and R, does not identify the concept (the state or trait) with the pure disposition. . . .

The distinction between intervening variables and theoretical constructs, often discussed since the article by MacCorquodale and Meehl, seems essentially the same or closely related to our distinction between pure dispositions and theoretical terms. “Theoretical construct” means certainly the same here as “theoretical term”, viz., a term which cannot be explicitly defined even in an extended observation language, but which is introduced by postulates and not completely interpreted. (pp. 70-71, 73)

Finally, Hempel (1966), one of founders of logical behaviorism noted earlier in the present review, defected from the original commitment to observables in the following way: “In order to characterize. . . .behavioral patterns, propensities, or capacities. . . . we need not only a suitable behavioristic vocabulary, but psychological terms as well” (p. 110). Here again we see that the logical empiricists had departed significantly from their original position regarding the role of observables in defining psychological terms and concepts.

**Traditional Psychology and the Definition of Theoretical Terms**

In traditional psychology, a formal embrace of partial operational definitions and hypothetical constructs at last removed any constraints regarding mentalistic views of the mediating theoretical terms of neobehaviorism, which in fact many in psychology had desired all along. Although not a neobehaviorist, Boring’s (1950) own comments are symptomatic:

> All the mentalistic entities come in as reduced to the operations by which they are observed. . . . Quite contrary to expectation, it turns out that the behaviorist can eat the cake of consciousness and have it too. He may not always know it, but he can. (pp. 658-659)

E. C. Tolman, who was a neobehaviorist and one of the early psychologists to advocate theoretical terms and operationism, wrote as follows:

> I am now convinced that “intervening variables” to which we attempt to give merely operational meaning by tying them through empirically grounded functions either to stimulus variables, on the one hand, or to response variables, on the other, really can give us no help unless we can also imbed them in a model from whose attributed properties we can deduce new relationships to look for. That is, to use MacCorquodale and Meehl’s distinction, I would abandon what they call pure “intervening variables” for what they call “hypothetical
constructs,” and insist that hypothetical constructs be parts of a more general hypothesized model or substrate. (Tolman, 1949, p. 49)

Later neobehaviorists continued in the same vein. For example, here is Kimble (1985):

Even in Watson’s day there were those, most notably Tolman, who attempted to bring mentalistic-sounding concepts back into psychology by means of what amounted to operational definitions. In a general way, the operational point of view did nothing more than insist that terms designating unobservables be defined in ways that relate them to observables. From there it proceeded to a further insistence that concepts defined in this way must have a relationship to behavior. In this way these concepts became intervening variables, ones that stand between observable antecedent conditions on the one hand and behavior on the other. The diagram below serves to summarize this point:

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Antecedent — Mentalistic — Behavior
Conditions          Concepts
Independent — Intervening — Dependent
Variables            Variables            Variables
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Obviously, there is nothing in this formula to exclude mentalistic concepts. In fact, the whole point of it is to admit unobservables. (p. 316)

And here is Amsel (1989):

It has never been debatable—certainly not among neobehaviorists—that explanations should involve constructs [representing nonbehavioral states and processes that are inside organisms] . . . And it is really not debatable either that stimulus—response theory refers, as it did in Hull’s 21 papers in Psychological Review, . . . , as well as his Principles of Behavior (1943), to hypothetical states and processes that “go on inside organisms.” [T]he fact is that for the present S-R theorist, as I think for Hull and certainly for Spence, the mediating machinery defined as hypothetical Ss and Rs are no more or less permissible, and no more or less observable, than are the cognitive constructs the “emergent behaviorists” are now willing to permit. . . . It is an essential contradiction to refer to models of observables; and as I indicated earlier, such a characterization of S-R models does not fit the neobehaviorist of Hull, Spence, Miller, or Mowrer—or any other version of neobehaviorism, including my own. (pp. 50-51, 71)

This view has continued to the present. Although cognitively-oriented psychologists and philosophers routinely disparage behaviorism, what they disparage is a methodological behaviorism that requires researchers and theorists to either (a) remain formally silent on the mental dimension, in the sense of Interpretation 1; or (b) exhaustively define theoretical terms or interpret them as intervening variables, in the sense of Interpretation 2. In this regard, we note that most traditional behaviorists (i.e., neobehaviorists) have actually not required
exhaustive definitions of mediating theoretical terms or interpreted them as intervening variables since the 1940s, as the preceding passage from Tolman (1949) testifies. Rather, they have advocated partial definitions and interpreted the terms as hypothetical constructs, in the sense of Interpretation 3.

Interestingly, Stevens (1939, pp. 230-232) took a nominally instrumentalist view and steadfastly asserted that operationism was a technic and not a positivism, behaviorism, monism, dualism, pluralism, or any other sort of –ism that asserts something about the ultimate nature of reality. This methodological stance allowed a disingenuous claim of scientific “agnosticism.” That is, the stance allowed an interpretation to the effect that the inferred phenomena actually are in a mental dimension rather than behavioral and actually do exist and actually do cause behavior. However, no formal commitment needs to be made one way or the other as to the ontology or actual existence of the phenomena. Rather, the resulting scientific efforts and their theories are meaningful because they can be agreed upon, by virtue of their logical connection to the world of observable events through partial operational definitions. Again, liberalized partial definitions according to a hypothetical construct interpretation allow methodological behaviorists to appeal indirectly rather than directly to mental terms in their theories and explanations and claim they have satisfied the requirements of good science.

Why Radical Behaviorists Oppose Methodological Behaviorism: Verbal Processes and Meaning

An earlier section of the present review questioned what methodological behaviorists say is the basis for their position. As an alternative, what do radical behaviorists say is the basis for methodological behaviorism, and why do radical behaviorists oppose methodological behaviorism?

The Meaning of Verbal Behavior in Methodological Behaviorism

As is evident throughout the present review, methodological behaviorism is intimately concerned with establishing the meaning of psychological terms. Radical behaviorists argue that establishing the meaning of terms is absolutely legitimate, but that methodological behaviorism goes about it the wrong way. Radical behaviorists argue that methodological behaviorism and operationism are committed to a symbolic, referential view of verbal behavior, which gives rise to a particular approach to the meaning of verbal behavior.

According to this symbolic, referential view of verbal behavior, we establish the meaning of the term by identifying the thing that is being symbolically represented or to which the term refers. Thus, any time a speaker uses a term, the assumption is that there must be something in the world at large to which the term refers or corresponds. If the something is not literally an object in the world, then it is a mental entity constructed in the mind of the speaker. Readers will recall that under Interpretation 1 of methodological behaviorism, it was permissible to assume that psychological terms meant unobservables that existed in a
nonbehavioral dimension. However, these unobservables were ruled out of bounds for direct consideration in science. Readers will recall that according to Interpretation 3, psychological terms meant unobservables that were also assumed to exist, again in a nonbehavioral dimension. However, they could be viewed as mediating organismic variables and theoretical terms. They were then operationally defined, albeit in Interpretation 3 with partial definitions according to a hypothetical construct interpretation in the S-O-R approach, in order to meet the presumed requirements of good science. They could then be indirectly considered.

A mid-20th century example of this symbolic, referential view of verbal behavior is Benjamin (1955):

What, then, gives such an operation cognitive significance? The answer is simple and clear-cut. The event which is produced by the operation must refer to that which was involved in its creation in that unique way which is characteristic of all symbols. Symbols are a special kind of sign. A sign is defined as that which has the property of referring to, or indicating, something else; this “meaning relationship” is probably unique and indefinable. But it can be readily exemplified: an overcast sky means rain, a flag at half-mast means the death of an important figure, the reading on a thermometer means that something is hot or cold, the fact of the light being on means that someone has pushed the switch, and the existence of a cake means that someone has baked it. Whenever any two physical events are frequently associated either may become the sign of the other, and the presence of one may lead a person to think of the other. But not all signs are symbols. A sign becomes a symbol when it is taken away from the physical situation and made into an instrument of communication; this involves making it a part of a language system having syntactical, semantical, and pragmatical rules. In the great majority of cases it also involves replacing the physical event by an arbitrarily chosen symbol, such as a word or number. In pictorial languages the character of the physical event is retained in the symbol; for example, one could easily construct a language in which heat would be represented by the picture of a thermometer with the mercury standing at a high point on the scale. But in most modern languages this element of resemblance between the symbol and its referent has been lost, and the associative tie is established simply by learning the language. (pp. 97-98)

A 21st century example is Dickins and Dickins (2001):

Signs... have meaning by virtue of their ability to orient an organism’s behavior toward some actual or potential environmental feature. This meaning is acquired through discrimination learning. Symbols are distinctive in that their meaning is shared by a social group—the verbal community that uses them. Symbols are not reliant upon each individual having directly experienced the features that they symbolize. Symbols are attached to their referents by arbitrary social convention and maintained by linguistic and error correction practices prevalent in the social group. Specifically, symbols are artificial and acquire their status due to their role in human social life. The final difference between signs and symbols is that signs are related to their referents asymmetrically, such that spots can be the (natural) sign of measles but measles cannot be considered a sign of spots—signs are indicative of something... However, symbols are attached to
their referents symmetrically. This means that on presentation of the word “cake,” for instance, a person can pick “cake” from a mixed array of stimuli, and on the presentation of a cake they can equally pick the word “cake” from an array of words, or generate it themselves. (pp. 223-224)

Our point in citing such passages is to emphasize how ingrained a symbolic, referential view of verbal behavior is, not that we couldn’t translate the language or find a sentence here or there that appears consistent with radical behaviorist view of verbal behavior. In this regard, the study of equivalence relations and the circumstances that give rise to them is certainly one of the most heavily investigated topics in the literature (e.g., Sidman, 1994). Dickins and Dickins point out the importance of equivalence relations in the conclusion of the passage above, and correctly so. Of concern, however, is that equivalence is framed in the context of a referential process: Words are taken as “symbols” that are “attached to their referents.” The behavioral basis for such relations is not given.

In any event, Stevens (1939), as we have seen one of the early advocates of operationism in psychology, fully subscribed to this symbolic, referential view:

A sign has semantical significance when an organism will react to it as it would to the object which the sign supplants. The psychologist works out the laws under which different stimuli evoke equivalent reactions. Signs, as stimuli, can be combined and utilized extensively in the control and direction of behavior, both individual and social. The entire activity of the scientist as a sign-using organism constitutes, therefore, a type of behavior for which behavioristics seeks the laws. (p. 250)

The Meaning of Verbal Behavior in Radical Behaviorism

For radical behaviorism, verbal behavior is not at heart a symbolic, referential process from a nonbehavioral dimension. Terms are not things that refer to or symbolically represent other things. The meaning of a term is not established by finding its referent. Speakers do not express inner meanings by their choice of terms. Rather, it is operant behavior established through interaction with the verbal community (Skinner, 1957).

As reviewed earlier, the 1945 Symposium on Operationism was precipitated by concern about the meaning of psychological terms and how that meaning, in turn, affected research and explanatory practices in psychology. With regard to the meaning of psychological terms, early in his contribution to the Symposium, Skinner (1945) argued that

A considerable advantage is gained from dealing with terms, concepts, constructs, and so on, quite frankly in the form in which they are observed—namely, as verbal responses. There is then no danger of including in the concept that aspect or part of nature which it singles out. . . . Meanings, contents, and references are to be found among the determiners, not among the properties, of response. (p. 271)
For radical behaviorists, the determiners of verbal behavior are the elements of the contingencies that govern the emission of the response, as an instance of verbal behavior. Particularly important are the antecedent circumstances that occasion the verbal response in question. In everyday language, the questions are: With what variables and relations is the speaker in contact? What is the speaker actually talking about and why? In another portion of his contribution, Skinner argued that “If it turns out that our final view of verbal behavior invalidates our scientific structure from the point of view of logic and truth-value, then so much the worse for logic, which will also have been embraced by our analysis” (p. 277). This entire orientation is decidedly at odds with a traditional orientation, certainly within philosophy but also psychology, according to which language gains meaning by fitting into a superordinate logical template.

Moore (e.g., 2001b, 2009, 2010) suggested that terms from a nominally psychological or mental vocabulary often imply five sources of control, either singly or in combination: (a) private behavioral events, (b) physiology, (c) dispositions, (d) behavioral relations, or (e) explanatory fictions. With respect to the first source, some so-called mental talk might be about private behavioral events. Private behavioral events are concerned with the influence of feelings, sensations, and covert operant behavior. The notion of private behavioral events allows radical behaviorists to understand how those events participate in contingencies controlling subsequent operant behavior, whether verbal or nonverbal. When a private behavioral event does contribute functionally to public behavior, some prior experiences are necessary for the private event to do so. Nevertheless, responding with respect to private or covert stimuli is lawful and alike in kind to responding with respect to public or overt stimuli. Private stimuli may be interpreted as simply additional independent variables in the same dimensional system as public stimuli (see Moore, 2008, for detailed discussion).

With respect to the second source, some so-called mental talk might be about physiology. This talk engages the role of physiological structures and pathways that participate in any form of behavior. After all, we have two gaps in a behavioral account. One is within a behavioral event, from the time a stimulus impinges on an organism until the organism then responds. The second is between behavioral events, from the time an organism has certain experiences until the organism then behaves differently as a result of those experiences. Physiological events take place during these gaps that can be known about and that can be used as a basis for prediction and control. However, this talk runs the risk of confounding causal and explanatory categories. Although physiology necessarily participates in behavioral events, physiological events are not the same type as behavioral events, public or private. On this view, an organism’s physiology is a material cause. To portray physiology as an autonomous, initiating, or efficient cause, as traditional psychology often does, creates a variety of explanatory problems (Moore, 2002).

With respect to the third source, some so-called mental talk might be about dispositions. This talk does not reflect anything literally mental. Rather, dispositional talk reflects the probability of behavior engendered by contingencies.
Dispositional talk is about effects instead of causes or intervening variables, as traditional psychology often portrays them.

With respect to the fourth source, some so-called mental talk might be about behavioral relations. This talk might be occasioned by the relation between particular antecedent circumstances and behavior, as when we say persons are “paying attention” to what they are being told. The paying attention simply indicates what they are being told is exerting discriminative control over the behavior in question. The behavior need not be understood as mediated by some cognitive “attentional” process.

Finally, with respect to the fifth source, some so-called mental talk is little more than an appeal to fanciful explanatory fictions. This talk, common in traditional psychology, owes its strength to language patterns and the everyday social reinforcement inherent in “folk psychology.” The talk surrenders to mentalism and methodological behaviorism, notwithstanding any claims that it is “theoretical.”

For radical behaviorism, then, the functional analysis of verbal behavior, including that of the scientist, clarifies many of the concerns about establishing meaning. Terms are instances of behavior emitted under specific circumstances and having a certain function in the speaker’s life. It makes no more sense to say that a term symbolically represents or refers to something else than it does to say that stepping on the accelerator at a traffic intersection symbolically represents or refers to a green light, or that drivers express inner meanings by stepping on the accelerator. In both cases, the meaning of the behavior is a function of the circumstances in which it is emitted. The meaning of stepping on the accelerator is the presence of a green light and drivers’ being able to proceed safely through the intersection. The meaning of a term from a speaker’s point of view is the circumstances that occasion it. The meaning of a term from a listener’s point of view is the extent to which the term allows the listener to take action that obtains certain consequences. Importantly, these are not measures of meaning, where meaning is construed as some mental or logical entity in a different dimension. Rather, they are what meaning means.

It is perfectly reasonable to seek to establish the meaning of a psychological term, and hence its function in scientific inquiry. However, we need not assume that the term symbolically represents or refers to acts, states, mechanisms, etc., that literally exist in and cause behavior from a nonbehavioral dimension. As we have reviewed, the mediational version of methodological behaviorism clearly does so assume. Thus, methodological behaviorism unselfconsciously assumes the existence of another dimension, with its set of acts, states, mechanisms, etc., to which psychological terms are supposed to symbolically refer. The assumption is that this dimension can’t be accessed directly by science, so we have to deal with it indirectly and inferentially. For radical behaviorists, such an approach is attributable to a variety of extraneous considerations rather than legitimate scientific practices. These considerations include language patterns of converting adjectives and adverbs into nouns, inappropriate metaphors, and a generally mentalistic if not dualistic explanatory orientation in the culture at large (e.g.,
Moore, 2008). As stated earlier, on the radical behaviorist view, operationism consists in the functional analysis of verbal behavior: What circumstances occasion the verbal behavior in question? It may well be that the verbal behavior is functionally related to whatever scientific operations the researchers have conducted and the contacts with the data that result from such operations. However, it may also be that the verbal behavior is functionally related to language patterns of converting adjectives and adverbs into nouns, inappropriate metaphors, and a general mentalistic if not dualistic explanatory orientation of folk psychology in the culture at large. Analysis and interpretation of the verbal behavior in question will clarify why scientists speak as they do.

As we noted, radical behaviorists see a tight connection between mentalism and methodological behaviorism even though mentalism appears to differ from methodological behaviorism. The connection is that both mentalism and methodological behaviorism appeal to mental causes, albeit not always in identical ways. In the final analysis, radical behaviorists oppose mentalism and methodological behaviorism on pragmatic grounds. Radical behaviorists argue that a critical examination of mentalism and methodological behaviorism reveals that both are based on an entire series of mischievous assumptions about the nature of verbal behavior, the role of verbal behavior in producing knowledge, and the role of theories in knowledge and explanation. These mischievous assumptions ultimately lead people to accept ineffective mentalistic answers to questions about the causes of behavior. More specifically, radical behaviorists argue that mentalism and methodological behaviorism obscure—and indeed actively impede—the search for important details about the genuinely relevant relations between behavior and environment, they allay curiosity by getting us to accept fanciful “explanatory fictions” as causes, they misrepresent the facts to be accounted for, and they give us false assurances about the state of our knowledge. Consequently, mentalism and methodological behaviorism interfere with effective prediction, control, and explanation of behavior.

The Role of the Unobserved in Explanations

What, then, about the role of unobservables in explanations? In point of fact, one of the distinctive features of radical behaviorism is its position on unobservables:

Behaviorists have, from time to time, examined the problem of privacy, and some of them have excluded so-called sensations, images, thought processes, and so on, from their deliberations. When they have done so not because such things do not exist but because they are out of reach of their methods, the charge is justified that they have neglected the facts of consciousness. The strategy is, however, quite unwise. It is particularly important that a science of behavior face the problem of privacy. It may do so without abandoning the basic position of behaviorism. Science often talks about things it cannot see or measure... An adequate science of behavior must consider events taking place within the skin of the organism, not as physiological mediators of behavior, but as part of
behavior itself. It can deal with these events without assuming that they have any special nature or must be known in any special way. The skin is not that important as a boundary. Private and public events have the same kinds of physical dimensions. (Skinner, 1969, pp. 227-228)

A science of behavior must consider the place of private stimuli as physical things, and in doing so provides an alternative account of mental life. The question, then, is this: What is inside the skin, and how do we know about it? The answer is, I believe, the heart of radical behaviorism. (Skinner, 1974, pp. 217-218)

Skinner wrote numerous times about the importance of private events (e.g., Skinner, 1953, chapter 17 on “Private events in a natural science”; Skinner, 1957, chapter 19 on “Thinking”). Expositions of these writings are available elsewhere and need not be reviewed here (e.g., Moore, 2008). Two important considerations are that (a) some factor need not be regarded as from another dimension just because it is unobservable from the standpoint of another observer, given the current state of our technology; and (b) the factor need not be incorporated into a scientific explanation in a different way than observable stimuli and responses. The commitment to another dimension that requires such factors be treated in a special way is attributable to social–cultural factors, as in folk psychology. Thus, a critical feature is whether a putative cause of behavior is held to be from the behavioral dimension or from another dimension, such as the mental, psychic, or conceptual dimension, rather than how many people can observe the cause. Regarding this matter, Skinner (1953) suggested that

The line between public and private is not fixed. The boundary shifts with every discovery of a technique for making private events public. . . . The problem of privacy may, therefore, eventually be solved by technical advances. But we are still faced with events which occur at the private level and which are important to the organism without instrumental amplification. How the organism reacts to these events will remain an important question, even though the events may some day be made accessible to everyone. (p. 282)

In sum, it is clear that radical behaviorism has always considered causal factors that are not publicly observable. It does consider them differently than does traditional psychology, however. Radical behaviorism argues for understanding their contribution at a descriptively consistent level, whereas traditional psychology conceives of the factors as from a different dimensional system. Are there factors that are “private in principle”? Matters of ontology and metaphysics are notoriously difficult to resolve. Suffice it to note that Skinner (1945) did not see how such factors could exist so as to be involved in the determination of behavior because the necessary process of differential reinforcement could not lead anyone to talk about them:

There is apparently no way of basing a response upon the private part of a complex of stimuli. A differential reinforcement cannot be made contingent on

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*the property of privacy.* This fact is of extraordinary importance in evaluating traditional psychological terms. (p. 275, italics in original)

Why, then, is talk of private, covert behavioral events not itself mentalism? First, the events are in the behavioral dimension. They may therefore be talked about in the same terms as environmental events and relations. Second, they are executed by the same response systems as public responses, just reduced in magnitude. They may even be incipient or inchoate. Third, they are functionally related to prior environmental circumstances. Public and private events are caused by the same sort of variables and relations. Fourth, although subsequent behavior may well show their influence, that influence is conditional on further variables and relations. Their influence is not the mechanical or inevitable relation implied in an S-O-R mediational account. Thus, the functional role of private, covert events is homogeneous and consistent with that of public, overt events (Moore, 2008).

**Radical Behaviorism and Theories**

An important point is that radical behaviorist concerns about methodological behaviorism and the meaning of psychological terms does not turn simply on the extent to which an approach is deemed “theoretical.” For radical behaviorism, theories are instances of verbal behavior. They are occasioned by certain antecedent conditions and reinforced by certain other conditions. Theories are important as a form of discriminative stimulation that guides future action by promoting either (a) direct manipulation of environmental events or (b) action when direct manipulation is not feasible.

One passage that illustrates the common but mistaken view that radical behaviorism is merely atheoretical and descriptive instead of theoretical and explanatory is from the mediational neobehaviorists Kendler and Spence (1971):

[T]he radical positivistic position, at times enunciated by Skinner (1950), [is] that theories are unnecessary. The scientist’s task is to manipulate events that are directly observable to discover the facts as they are, and nothing more. . . .

Skinner’s atheoretical position has generated much confusion simply because it is itself unclear. It is one thing to state that at a particular time in the history of psychology the systematic collection of data without any theoretical preconceptions, but with a desire to control phenomena, may be more productive that self-conscious efforts to erect theoretical structures that cannot be supported by available empirical evidence. It is quite another thing to state that theories are unnecessary and should be ignored as a scientific goal. Skinner seems to maintain both of these positions, frequently arguing in favor of the latter, but defending it in terms of the former. . . .

In truth, the denial of the significance of the theoretical goal in psychology has been more of a debator’s point than a controversy that reflects in actual practice two forms of scientific effort. Skinner himself has theorized extensively, in his
efforts both to systematize the variables that determine behavior and to
generalize from the operant conditioning situation to all aspects of life. But his
theorizing has been covert and thus he has felt no need to defend his
speculations. (pp. 21-22)

In truth, Kendler and Spence seriously misunderstand radical behaviorism,
just as do most mediational neobehaviorists. For radical behaviorism, theories are
based on descriptions of the functional relation, or the contingency in the case of
operant behavior, between behavior and its controlling variables in the
environment. The resulting theories will economically and abstractly describe
uniformities in those contingencies across many different circumstances, using a
minimum number of terms. The theories are descriptively consistent in the sense
that they remain within a single dimensional system in all respects. In Skinner’s
(1972) words,

Behavior can only be satisfactorily understood by going beyond the facts
themselves. What is needed is a theory of behavior. . . .[T]heories are based
upon facts; they are statements about organizations of facts. . . .[W]ith proper
operational care, they need be nothing more than that. But they have a wider
generality which transcends particular facts and gives them a wider
usefulness. . . .[E]xperimental psychology is properly and inevitably committed
to the construction of a theory of behavior. A theory is essential to the scientific
understanding of behavior as a subject matter. (pp. 301-302)

What radical behaviorism rejects are the mentalistic theories of
methodological behaviorism as well as of dualism that appeal (either indirectly or
directly) to causal events and entities that are somewhere else, at some other level
of observation, that must be described using different terms and concepts, and that
are measured if at all in a different dimensional system (e.g., “mental,” cognitive,
subjective). As Skinner (Catania & Harnad, 1988, p. 89) pointed out, a common
use of the term “theory” is to postulate supposed intervening events in another
dimension. These intervening events are intended to fill the gap between
observable events—the traditional independent and dependent variables. For
radical behaviorism, the appeal to phenomena in another dimension with exactly
the causal properties necessary to explain an instance of behavior creates a false
sense of security and an unwarranted diversion from further study.

Some psychologists justify their position by claiming that science has always
progressed by hypothesizing “unobservables” in theories and explanations, and
that it is not appropriate to limit scientific theorizing. For example, according
to the noted cognitive psychologist George Mandler, “I mean that one of the
components of theory is the generation of useful fictions. That’s what theories are
about” (as cited in Baars, 1986, p. 255). Radical behaviorists argue that the chief
feature of this claim is that it is precisely dead wrong and uninformed: “No one
today seriously uses a fictional explanation as a theory, but all sciences have done
so at one time or another” (Skinner, 1972, p. 302). Radical behaviorists emphasize
that, to the contrary, science has generally progressed by dispensing with fanciful
explanatory fictions. Three conspicuous examples are (a) phlogiston in chemistry,
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(b) vital spirits in biology, and (c) a luminiferous ether in physics. At issue are the dimensions of the unobservables. Science progresses by ridding itself of unobservables of uncertain dimensions. There is nothing limiting about this process. For radical behaviorism, mentalistic constructs are examples of things of uncertain dimensions. As such, they are irrelevant at best and an unwarranted, counterproductive diversion at other times.

Finally, we can note that operationally defining a mental or cognitive construct by “logically connecting” it to some physiological structure or mechanism doesn’t really aid in identifying the cause of behavior because methodological behaviorists have still assumed the ultimate cause of behavior is from another dimension. For example, consider this passage from Pinker (1997):

The mind is what the brain does; specifically, the brain processes information, and thinking is a kind of computation. The mind is organized into modules or mental organs, each with a specialized design that makes it an expert in one area of interaction with the world. The modules’ basic logic is specified by our genetic program. The operation was shaped by natural selection to solve the problems of the hunting and gathering life led by our ancestors in most of our evolutionary history. (p. 21)

In other words, all methodological behaviorists have done is to hypothesize at an abstract and conceptual level about the machinery that the “ghost in the machine” operates, but they still assume there is a ghost that originates or mediates the behavior in question. As Day (1976, 1983) noted, the origin of the theoretical terms in methodological behaviorist theory is in the mentalistic conceptual system commonly employed in our culture—folk psychology. The theory is given honorific status, but the actual causes of the dependent measure may only be incompletely or inaccurately identified in the theory. Something taken as an adequate explanation is actually not. The inherent mentalism of folk psychology prevails. As we see, given the dominance of the hypothetical construct interpretation of theoretical terms, legitimate question may be raised as to whether methodological behaviorism really should be called a behaviorism after all, or whether it is nothing but a thinly disguised version of mentalism if not dualism. For example, Bergmann, a methodological behaviorist if there ever was one, adopted a version of psychophysiological parallelism that fully endorsed minds and mental phenomena that were qualitatively different from publicly observable behavior. Bergmann argued that to do otherwise was “silly” and “a lot of patent nonsense” (Bergmann, 1956, p. 266). Indeed, Natsoulas (1984) points out that Bergmann (a) admits mental episodes that differ from physical episodes (p. 52), (b) admits mental causes for behavior (p. 63), and (c) concedes that mental variables may legitimately be invoked to explain behavior (p. 64). Moreover, Natsoulas (1983) discusses extensively “the mind–body dualism of methodological behaviorism” (p. 13) and how methodological behaviorism considers “conscious content to be mental as distinct from physical” (p. 5).

Earlier we identified a passage from Tolman (1949) on the use of theoretical terms. Skinner (1989) commented on Tolman’s use of theoretical terms as follows:
I had called the conditions of which reflex strength was a function “third variables,” but Tolman called them “intervening.” That may have been the point at which the experimental analysis of behavior parted company from what would become cognitive psychology. . . . Although as a behaviorist Tolman thought his intervening variables replaced mental processes, they have been taken over by cognitive psychologists as elements of mind. (pp. 109-110)

Skinner is here using the term “intervening variable” in the generic sense of a theoretical term that intervenes between independent and dependent variables. The broader context of his comments suggests he is referring more to the hypothetical construct interpretation of theoretical terms than to the specific intervening variable interpretation, in the sense of MacCorquodale and Meehl (1948). In any event, the passage highlights the important problem that Interpretation 3 creates: It promotes mentalism.

For the mediational form of methodological behaviorism with hypothetical constructs, methodological behaviorists can deploy publicly observable stimuli and behavior as surrogates or proxies for whatever unobservable mental or cognitive phenomena they want to incorporate into their theories and explanations. Thus, methodological behaviorists can indirectly incorporate terms and concepts thought to refer to unobservables in their theories and explanations by invoking logical or theoretical constructs. These constructs can then be operationally defined using publicly observable data in order to claim scientific respectability. The operational definition is only a partial definition, as a hypothetical construct. In addition, the fundamental nature of these constructs can be the same as in other sources, such as folk psychology or even orthodox mind–body dualism. Thus, we have the mediational form of methodological behaviorism that may be interpreted as consistent with the prescription to talk only about publicly observable stimuli and behavioral data. The prescription is interpreted to permit talk that involves indirect appeals to mental phenomena, provided that the mental phenomena are logically connected to observables, even if the mental phenomena are only partially defined according to the liberalized definition of hypothetical constructs mentioned above.

Radical Behaviorism and Explanations

How, then, does radical behaviorism view the issue of explanation? Day (1969a) has noted that the issue is

whether explanations and predictions are properties of scientific systems in themselves or whether they are aspects of human functioning. Are predictions about what is to be observed properties of formally organized words and symbols or are they varieties of human behavior? . . . For Skinner, the preference is to view explanations and predictions as aspects of human behavior. (p. 504)

In particular, for radical behaviorism, explanations and predictions are aspects of human verbal behavior. As such, critical questions about explanation are engaged in the same way as critical questions about other forms of complex
operant behavior: What discriminative stimuli occasion the explanation, what reinforcers support the explanation, what are the contingencies involved in generating the explanation, and what are the contingencies according to which the explanation exerts a discriminative effect?

When once asked whether the term “explanation” was a meaningful term as it related to behavior analytic practices, Skinner (1964) answered “When I said ‘explanation,’ I simply meant the causal account. An explanation is the demonstration of a functional relationship between behavior and manipulable or controllable variables” (p. 102). Thus, causal explanation plays a central role for radical behaviorism, given the fundamental concern with practical outcomes.

Theories as Verbal Behavior

As we have seen, for radical behaviorism, theories and explanations function as forms of discriminative stimulation that guides future action through either (a) direct manipulation of environmental events or (b) action when direct manipulation is not feasible, as in some cases of prediction and interpretation. Does the foregoing mean that prediction and control should be regarded as the only goal of scientific activity? The answer is not always, of course, but often enough to warrant the point. Skinner (1979) identified other factors that might maintain the behavior of explaining:

Was not confirmation the be-all and end-all of science? It was a question concerning my own behavior, and I thought I had an answer: “...What is the motivational substitute for thing-confirmation? Pretty important in teaching method to graduate students. Resulting order instead of confirmation?” My reinforcers were the discovery of uniformities, the ordering of confusing data, the resolution of puzzlement. (p. 282)

In any event, always at issue in any analysis of explanation are the contingencies governing the verbal behavior regarded as explanatory (Moore, 1990a, pp. 25 ff.).

What radical behaviorism does reject is the traditional view of theories as formal statements that appeal to causal events and entities in other dimensions (e.g., neural, psychic, “mental,” cognitive, subjective, conceptual, hypothetical) with observational and theoretical terms, in which the latter are operationally defined as either intervening variables or hypothetical constructs (cf. Zuriff, 1985, Chapters 4 and 5; Moore, 1996, 1998). It further rejects the position that an event is explained when it follows as a logically valid deduction in an argument with a law or theory as a major premise and a statement of antecedent conditions as minor premise (Hempel & Oppenheim, 1948), where the law or theory appeals to causal events in other dimensions. These traditional modes of explanations are commonly called “covering law” or “deductive-nomological” explanations, and their liabilities are discussed elsewhere (Moore, 2003; Salmon, 1984, 1989; Sosa & Tooley, 1993). Indeed, radical behaviorism argues the assumptions that (a) psychological knowledge necessarily consists in the formulation of such theories and
explanation consists of nomic subsumption are further illustrations of the longstanding mentalistic problem. Skinner put it as follows:

The theories to which objection is raised here are not the basic assumptions essential to any scientific activity or statements that are not yet facts, but rather explanations which appeal to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions. . . . Theory is possible in another sense. Beyond the collection of uniform relationships lies the need for a formal representation of the data reduced to a minimal number of terms. A theoretical construction may yield greater generality than any assemblage of facts; such a construction will not refer to another dimensional system. (Catania & Harnad, 1988, p. 77)

To be sure, many sets of variables, rather than just one set of variables, often control scientific verbal behavior. Thus, verbal behavior regarded as explanatory is often under “multiple control” (Skinner, 1957, pp. 227 ff.). As Moore (1981) noted, some of the stimulus control derives from experimental operations and contacts with data. Other control derives from other sources.

For example, some explanatory verbal behavior simply manifests “control by ordinary language habits, extensive chains of familiar intraverbals, and one or another preconception about the inherent nature of scientific explanation” (Day, 1969b, p. 323; see also Moore, 1990a). As Day (1969b, p. 319) noted, the traditional conception assumes that the chief function of language is to identify the Platonic nature of the thing spoken about. It assumes that any time we do speak, the words we use must be things that refer to other things in the world at large that have actually been declared as metaphysically real and permanent, by virtue of the inherent properties that give the things their essential identities. Speakers then assume they have correctly isolated the things talked about. At best, such reification only illustrates the “Formalistic Fallacy” (Skinner, 1969, p. 265; see also the discussion of “realism” in Moore, 1998, p. 220).

Other explanatory verbal behavior manifests control by metaphors and social/cultural factors that are cherished for irrelevant and extraneous reasons. In sum, radical behaviorism is concerned with the contingencies that are responsible for a given instance of verbal behavior, and the contingencies into which the verbal artifact subsequently enters as it exerts discriminative control among those who entertain it. The argument is that we must strip away control arising from mischievous social and cultural contingencies, leaving only the factors producing such things as manipulation and control, to understand the validity of a scientific explanation. Skinner (1957) put it as follows:

The scientific community encourages the precise stimulus control under which an object or property of an object is identified or characterized in such a way that practical action will be most effective. . . . Generic extensions are tolerated in scientific practice, but metaphorical, metonymical, and solecistic extensions are usually exinguished or punished. Metaphorical extension may occur, but either the controlling property is quickly emphasized by additional contingencies which convert the response into an abstraction or the metaphor is
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robbed of its metaphorical nature through the advent of additional stimulus control. . . In ruling out the effects of other consequences of verbal behavior the contingencies established by the scientific community work to prevent exaggeration or understatement, misrepresentation, lying, and fiction. . . . Scientific verbal behavior is most effective when it is free of multiple sources of strength; and humor, wit, style, the devices of poetry, and fragmentary recombinations and distortions of form all go unreinforced, if they are not actually punished, by the scientific community. . . . In general, however, practices are designed to clarify the relation between a verbal response made to a verbal stimulus and the nonverbal circumstances responsible for it. The community is concerned with getting back to the original state of affairs and with avoiding any distortion due to the intervening verbal linkage. (pp. 419-420)

The Contribution of Physiology

To be sure, this stance does not mean that physiology is irrelevant in a science of behavior (Moore, 2002). Physiological phenomena relate to the second of the two questions above. As we reviewed earlier, a behavioral account has two unavoidable gaps. Information about the events that take place during these gaps will be provided by physiologists, rather than psychologists, although psychologists might inform the physiologists of what they should look for. However, only the science of physiology can fill those two gaps. In doing so it provides additional information that will guide efforts to predict and control behavior. Physiological information is not necessary for a valid account of behavior as a process. Behavior analysis and physiology provide mutual and reciprocal support for each other; physiology does not provide the logical grounds for validating behavior analytic explanations. Behavior analysis and a theoretical behavioral neuroscience are therefore complementary sciences. In a more practical vein, physiological information, such as how an organism has been changed by interactions with its environment, can compensate for a possibly inadequate behavioral specification of those interactions as a basis for making predictions. Overall, behavior analysis gives neuroscience one of its directions, just as Mendel’s studies of the traits of successive generations of pea plants gave the study of the gene one of its directions (Catania & Harnad, 1988, p. 470).

Radical Behaviorism and Pragmatism

Pragmatism is the general position that the utility of an idea is the best criterion for determining its validity. On the present view, pragmatism emphasizes that if a particular explanation proves valuable, the contingencies underlying the development and application of the explanation in question need to be critically examined. This sort of examination would preclude both a facile instrumentalism and a facile realism, notwithstanding that instrumentalism and realism are typically taken to be opposing points of view. A facile instrumentalism would hold that if an explanation “works” to some approximation, for example, by generating predictions that are verified, we should accept that the explanation is satisfactory and the purposes of science have been accomplished. A facile realism would hold
that just because an explanation “works” to some approximation, even if the explanation appeals to acts, states, mechanisms, processes, and the like in another dimension, we should assume that the explanation has disclosed something essentially “real” in the world at large for which science should go off hunting. The latter unfortunate problem is reification, against which Skinner (1945, p. 271) cautioned. The present view is that a critical examination of contingencies would reveal how and to what extent verbal behavior that successfully guides future actions is derived from contacts with events, so that scientists do not subsequently make various kinds of either instrumentalist or realist errors (Moore, 1998, pp. 220-222).

Skinner’s approach to explanation entails pragmatic concerns with prediction and control, as shown in the following passage:

Science is in large part a direct analysis of the reinforcing systems found in nature; it is concerned with facilitating the behavior which is reinforced by them. . . . The point of science . . . is to analyze the contingencies of reinforcement found in nature and to formulate rules or laws which make it unnecessary to be exposed to them in order to behave appropriately. (Skinner, 1969, pp. 143, 166)

Elsewhere, Skinner (1974, p. 123) suggested that early scientific laws probably emerged from the lore of craftsmen. Other scientific laws may have arisen because they supplemented the natural contingencies of the physical world. A formal statement of the law of the lever permitted the principle to be used in situations where contingency-shaped behavior was unlikely or impossible to develop without contrived intervention. Thus, by learning the laws of science, a person is able to behave effectively under the contingencies of an extraordinarily complex world.

In addition, Skinner (1953) spoke about the developmental continuity in science as follows:

[Science] is a search for order, for uniformities, for lawful relations among the events of nature. It begins, as we all begin, by observing single episodes, but it quickly passes on to the general rule, to scientific law. Something very much like the order expressed in a scientific law appears in our behavior at an early age. We learn the rough geometry of the space in which we move. We learn the “laws of motion” as we move about, or push or pull objects, or throw and catch them. If we could not find some uniformity in the world, our conduct would remain haphazard and ineffective. Science sharpens and supplements this experience by demonstrating more and more relations among events and by demonstrating them more and more precisely. As Ernst Mach showed in tracing the history of the science of mechanics, the earliest laws of science were probably the rules used by craftsmen and artisans in training apprentices. The rules saved time because the experienced craftsman could teach an apprentice a variety of details in a single formula. By learning a rule the apprentice could deal with particular cases as they arose.

In a later stage science advances from the collection of rules or laws to larger systematic arrangements. Not only does it make statements about the world, it
METHODOLOGICAL BEHAVIORISM

makes statements about statements. It sets up a “model” of its subject matter, which helps to generate new rules very much as the rules themselves generate new practices in dealing with single cases. A science may not reach this stage for some time.

The scientific “system,” like the laws, is designed to enable us to handle a subject matter more efficiently. What we call the scientific conception of a thing is not passive knowledge. Science is not concerned with contemplation. When we have discovered the laws which govern a part of the world about us, we are then ready to deal effectively with that part of the world. By predicting the occurrence of an event we are able to prepare for it. By arranging conditions in ways specified by the laws of a system, we not only predict, we control: we “cause” an event to occur or to assume certain characteristics. (pp. 13-14)

The hallmark of radical behaviorist explanations, then, is pragmatism, understood as the possibility of effective practical action, apart from the structure of a logical argument. Effective practical action requires knowledge of factors that exist in space and time and that can be manipulated to produce (i.e., to cause) a given outcome. An operational analysis of the factors that engender effective scientific behavior, verbal or otherwise, can make that behavior even more effective in the future (see also the comparison between pragmatism and instrumentalism in Moore, 1998, pp. 228-229).

Causal Explanation and Description

One place where Skinner speaks extensively of the causal analysis of behavior is in Chapter 3 of Science and Human Behavior (Skinner, 1953):

The terms “cause” and “effect” are no longer widely used in science. They have been associated with so many theories of the structure and operation of the universe that they mean more than scientists want to say. A “cause” becomes “a change in an independent variable” and an “effect” a “change in a dependent variable.” The old “cause-and-effect connection” becomes a “functional relation.” The new terms do not suggest how a cause causes its effect; they merely assert that different events tend to occur in a certain order. This is important, but it is not crucial. There is no particular danger in using “cause” and “effect” in an informal discussion if we are always ready to substitute their more exact counterparts.

We are concerned, then, with the causes of human behavior. We want to know why men behave as they do. Any condition or event which can be shown to have an effect upon behavior must be taken into account. By discovering and analyzing these causes we can predict behavior; to the extent we can manipulate them, we can control behavior. (p. 23)

Moore (1984, pp. 87 ff.) suggested a parallel between (a) Aristotle’s doctrine of the four senses in which the word “cause” is used and (b) the identification of “causes” in behavior analysis. According to this suggestion, the final cause may be
interpreted as another way of talking about the reinforcing consequence of a response, the formal cause as another way of talking about discriminative control, the material cause as another way of talking about the physiology of the sentient organism, and the efficient cause as another way of talking about the contingency itself. To be sure, this interpretation imparts a modern reading to Aristotle and may well challenge the bounds of legitimacy. Nevertheless, the interpretation does indicate that for behavior analysis, causality is not conceived of as an inexorable power or force that sucks events toward some inescapable outcome. Rather, causality is conceived of as a natural process concerned with the arrangement and rearrangement of factors in an event field as a function of their properties and conditions (see also Moore, 1990b).

Causal Explanation and Prediction

Some misunderstanding perhaps exists with respect to the issues of explanation and prediction in radical behaviorism. Part of the misunderstanding no doubt arises from Skinner’s own writings. For example, Chapter 2 of The Behavior of Organisms (Skinner, 1938) opens as follows: “So far as scientific method is concerned, the system set up in the preceding chapter may be characterized as follows. It is positivistic. It confines itself to description rather than explanation” (p. 44). In addition, Coleman (1987) has documented the essentially descriptive quantitative nature of Skinner’s early research. Largely as the result of Skinner’s early writings, then, his system was deemed an atheoretical “descriptive behaviorism” for many years (e.g., Kendler & Spence, 1971).

Of course, we should not suppose that an emphasis on observable processes constitutes a naive commitment to a post-hoc psychology, using only events that can be observed by at least two people. Consider the following passage from Skinner (1961):

A science must achieve more than a description of behavior as an accomplished fact. It must predict future courses of action; it must be able to say that an organism will engage in behavior of a given sort at a given time. (p. 70)

This passage is significant because it indicates Skinner’s commitment to the practical concerns of doing science in the tradition associated with Bacon and Mach. Predictions based on knowledge of functional relations are of the utmost practical concern. The statement above also indicates Skinner’s rejection of the distinctively unpragmatic and unparsimonious flavor of traditional mediational neobehaviorism, which seeks to validate the appeal to phenomena from other dimensions such as psychic events, habits, wishes, attitudes, and so on in terms of intersubjectively verifiable phenomena such as behavior or neurological states. Absent among mediational neobehaviorists is any concern that their appeals to phenomena from other dimensions can interfere with doing science (see discussions of this issue in Smith, 1986, pp. 271-272; Zuriff, 1985, pp. 68-69, 257-261).
We often say that the job of a scientific psychology is to predict and control (or more generically, influence) behavior. Let us accept understanding and explaining as terms functionally synonymous with prediction and control, in the sense that we cannot reasonably be said to understand and explain behavior unless we can predict and control it. Presumably, we can also accept just predicting behavior when actual intervention leading to control is not feasible. We can still take some measure of effective action based on the prediction even if we can’t intervene directly to produce a given outcome. Beyond prediction and control, we can achieve a consistent and helpful account through interpretation, which is the use of scientific terms and principles in talking about facts when too little is known to make prediction and control possible, or again when precise manipulation is not practical.

When we predict and control, we are behaving. We are talking here about our behavior as we “do science.” We can analyze this behavior in terms of the contingencies that are responsible for it: the antecedent discriminative stimuli that set the occasion for it, and the reinforcers that maintain it. The reinforcers presumably come from the actual control we have over behavior, by making an organism do what we want.

What discriminative stimuli are involved in predicting and controlling? In other words, what do we as researchers need to take into account to predict and control the behavior of a subject/participant?

1. The first set of variables and relations we can take into account are from the history of the subject’s interactions with environment during its lifetime. By knowing the history of a subject’s interaction with the environment, and the presence or absence of these variables and relations in a given environment, we can predict and control behavior in that environment. For example, we can determine if the behavior is respondent or operant, what stimuli control the behavior in question, and how to produce behavior with a given set of properties.

2. The second set of variables and relations we can take into account are physiological states, events, and changes associated with the subject’s body that have been brought about during the history of the subject’s interactions with environment. These physiological events and changes occur at two stages of a behavioral event. The first stage is within an event, from contact with the stimulus to the resulting behavior. The second stage is between events, from one event to the next. By knowing the prevailing physiological state of an organism, or by knowing the underlying physiological mechanisms, we can take advantage of this knowledge to predict and control its behavior in a given environment with given variables and relations.

3. The third set of variables and relations we can take into account are physiological states and mechanisms that have been selected during the evolutionary history of species of which the subject is a member. This aspect of an organism’s physiology allows us to know what stimuli the subject is sensitive to and what responses it can make to those stimuli, thereby giving rise to prediction and control about its behavior in a given environment with given variables and relations.
All types of information can be used to predict and control. Depending on how specifically we desire to predict and control, the more that we know about one of the first two types above, the less we need to know about the other to predict and control.

The first type of information, about the variables and relations that come from the subject’s interactions with the environment during its lifetime, is at two levels: overt and covert. The overt variables and relations are accessible to all. Covert variables and relations are not. However, we need not suppose that covert variables and relations differ qualitatively from overt variables and relations, or that they follow qualitatively different rules. In this way we can understand the contribution of such processes as are commonly called thinking to behavior. Note that we may have to infer the presence or absence of covert variables and relations on any given occasion, but the influence of even these covert factors can be traced back to some history of interaction with the environment during the lifetime of the behaving organism. Some history is responsible for the covert activity in question. Whether this covert activity actually does influence behavior is an empirical question. Even if it is present, it might not influence behavior.

The second type of information comes about through explicitly physiological techniques (e.g., ablations, lesions, recordings, measurements, biochemical interventions) rather than as inferences from only behavior. However, behavioral methods obviously need to be used to determine the effect of physiological manipulations on behavior. Moreover, this second type of information is redundant if sufficient information is already available from observation. Indeed, this second type of information can only be gathered with appropriate apparatus. If that apparatus is not available, this second type of information obviously cannot be gathered and used to predict or control.

The third type of information comes from an intensive study of the evolutionary history of the species and such disciplines as ethology and behavioral genetics. Nevertheless, even this information can be traced to the observation of behavior.

In short, we need not regard organisms as empty or as black boxes. At issue is the nature of the information with which we are in contact when we predict and control. If we do not have information about the state of an organism’s physiology, we must use what has already been observed as the organism has interacted with the environment during its lifetime. Similarly, if we do know the state of an organism’s physiology, then we don’t have to bother with reconstructing an organism’s past. But again, we need behavioral methods to determine how physiological states are related to the behavior that does or does not occur in a particular environment given particular antecedent events, variables, and relations.

Radical Behaviorism and Dispositions

As reviewed earlier, dispositional analyses were at the heart of logical behaviorism in the 1930s. Dispositional analyses are further regarded as at the heart of “philosophical behaviorism” and a methodological behaviorism.
One of the foremost philosophical behaviorists is Gilbert Ryle. Ryle (1949; see also Place, 1999, from which much of the following is taken) argued that traditional psychology makes a “category mistake” when psychological terms, in particular verbs, are used to designate special mental activities taking place prior to behavior in a special dimension apart from the behavioral dimension. Philosophical behaviorists argue that such words actually relate to the probability of or a particular way of engaging in publicly observable behavior. In this regard, Ryle distinguished among three types of psychological verbs: (a) dispositional verbs (Ryle, 1949, pp. 116-135); (b) activity verbs (Ryle, 1949, pp. 135-149); and (c) achievement verbs (Ryle, 1949, pp. 149-153). Dispositional verbs relate to the propensity to do something from time to time. An actor can be disposed to do X, and may even do X habitually, though the actor may not be doing X currently. Activity verbs mark continuous occurrences or engaging in tasks. Achievement verbs mark not only engaging in some task, but also the result of doing so: Perceptual verbs such as “To see” mark not only an activity but the accomplishment of perceiving the thing seen.

Relevant for present purposes is Ryle’s distinction between dispositional verbs and activity verbs. For Ryle, taking a dispositional verb as an activity verb is a mistake and leads to conceptual confusion. For example, suppose one says one “believes” that London is the capital of England. If “believes” really is an activity verb in the sense that the traditional mental doctrine would have us accept, then it would make sense to hold that just as one can begin to whistle or stop on demand, where whistle is a noncontroversial activity verb, so also should one be able to start or stop “believing” on demand. But this locution doesn’t make sense. Beliefs just aren’t the sort of occurrences that are switched on and off on demand, as is whistling. Hence, it follows that to believe is not an activity verb but rather a dispositional verb. The word indicates a propensity or disposition of speakers who are said to believe “London is the capital of England” to so assert, perhaps in a loud voice, in a wide variety of circumstances. Comparable arguments can be made for the use of such terms as “knowing” and “understanding.” Hence, Ryle argued they should be rendered as dispositional verbs rather than activity verbs. Ryle’s (1949) expert application of this technique was the reductio ad absurdum argument, showing that many uses of psychological terms from our everyday language as implying mental activities were muddled and just didn’t make sense.

Such an analysis works well with verbs and corresponding nouns related to “propositional attitudes” such as “to believe” and “belief,” “to intend” and “intention,” and so on. For example, here is a relevant passage from Skinner (1957):

```plaintext
With respect to a particular speaker, the behavior of the listener is also a function of what is called “belief.” We may define this in terms of strength of response. Our belief that there is cheese in the icebox is a function of, or identical with, our tendency to go to the icebox when we are hungry for cheese, other things being equal. Our belief that there is a substantial table in front of us varies with our tendency to reach for it, place things upon it, and so on. If we have just spent some time in a house of mirrors in an amusement park, our belief in this simple
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fact may be shaken, just as our belief about the cheese may be quickly dispelled by an empty icebox. Our belief in what someone tells us is similarly a function of, or identical with, our tendency to act upon the verbal stimuli which he provides. If we have always been successful when responding with respect to his verbal behavior, our belief will be strong. If a given response is strictly under the control of stimuli with little or no metaphorical extension and no impurity in the tact relation, and if the speaker clearly indicates these conditions. . . , we will react in maximal strength. In this sense we will “take his word for it” implicitly. It does not matter whether or not he is a specialist. We believe that the expert will tell us all about it, but the nonexpert is equally well believed if the above specifications hold, for he will simply stop talking when he does not know what he is talking about. (pp. 159-160)

What, then, about such other verbs and corresponding nouns as “to think” and “thoughts”? Dispositional approaches seem limited here, but radical behaviorism is not. As outlined earlier, thinking may be construed as a kind of activity that affects subsequent behavior. For Skinner (1957),

There is no point at which it is profitable to draw a line distinguishing thinking from acting [on a continuum ranging from overt to covert forms of action]. . . .So far as we know, the events at the covert end have no special properties, observe no special laws, and can be credited with no special achievements. . . .A better case can be made for identifying thinking with behaving which automatically affects the behaver and is reinforcing because it does so. This can be either covert or overt. (p. 438)

Thus, although many instances of behavior are peripheral and publicly observable, not all are. Some instances of behavior entail activity within the skin and inaccessible to others. Like other instances of behavior, these instances are functionally related to a particular set of environmental circumstances. Then, once these instances have occurred, a further set of environmental circumstances is responsible for their influences on subsequent behavior.

The functional analysis of verbal behavior, including that of the scientist, clarifies many of the concerns about establishing meaning. Clearly, many psychological terms are dispositional and do describe the probability of behavior in particular circumstances. However, not all do. If we are interested in a causal account of the behavior in question, we still need to specify the variables and relations that cause the disposition in the first place. Consequently, to regard radical behaviorism as some sort of philosophical behaviorism committed to dispositional analyses in the rendering of psychological terms is mistaken.

**The Compatibility Between Mentalism and Methodological Behaviorism**

We earlier suggested that many mentalists and methodological behaviorists fail to appreciate the compatibility between mentalism and methodological
To qualify as a behaviorist in the broad sense of the term that I shall employ, one need only believe that the following proposition expresses a necessary truth: For each mental predicate that can be employed in a psychological explanation, there must be at least one description of behavior to which it bears a logical connection. I shall henceforth refer to this proposition as P... A mentalist is, then, simply someone who denies ‘necessarily P’. ... The distinction between mentalism and behaviorism is both exclusive and exhaustive. (pp. 51, 55)

Fodor acknowledges that one interpretation of “logical connection” is that “theoretical terms in psychological explanations must, in principle, be eliminable in favor of (definable by) terms that designate observables” (p. 51). Well, yes, Fodor’s mentalism does differ greatly from Skinner’s radical behaviorism. However, as argued in the present review, what Fodor and others are disagreeing with is either Interpretation 1 or 2 of methodological behaviorism above, especially a commitment to a dispositional view in Interpretation 2 that requires exhaustive definitions. Perhaps only a minority of contemporary psychologists actually advocate Interpretation 1 or 2. As a result, mentalists are critical of, at best, a minority position. The clear majority of contemporary psychologists advocate partial definitions and hypothetical constructs of the mediational neobehaviorism and methodological behaviorism in Interpretation 3, which ironically is equivalent to mentalism. Ironically, we can see that mentalists such as Fodor fail to acknowledge the equivalence.

Of course, we can make the analogous point in the opposite direction. Many psychologists who self-identify as behaviorists say they disagree with mentalism. These psychologists typically adhere to the mediational neobehaviorism and methodological behaviorism of Interpretation 3. As before, their position is actually equivalent to mentalism. However, many neobehaviorists fail to acknowledge the equivalence.

In another case, the cognitive psychologist Baars (1986) advocates a kind of mentalism that he attempts to distinguish from behaviorism:

Cognitive psychologists also have a claim about the domain of scientific psychology—essentially, it is that psychologists observe behavior in order to make inferences about underlying factors that can explain that behavior. They agree with behaviorists that the data of psychology must be public, but the purpose of gathering this data is to generate theories about unobservable constructs, such as “purposes” and “ideas,” which can summarize, predict, and explain the data. ... A psychological theory is a network of such constructs, serving to summarize empirical observations, predict new results, and explain them in an economical way. Like behaviorism, cognitive psychology is primarily a metatheory for psychology, one that simply encourages psychologists to do theory. ... No longer is it thought necessary for theoretical constructs to resemble visible stimuli and responses, or to adhere to rigid concepts of theoretical parsimony. ... By the same token cognitive psychology is
an act of imagination that permits wider latitude in imagining explanations for behavior. Whereas behaviorists taught psychologists to respect empirical evidence, the cognitive metatheory may make it possible to do good theory. (pp. 7, 144-145)

The point in such passages is that the writers are rejecting the positions expressed in Interpretation 1 and perhaps Interpretation 2 as well. However, the hypothetical constructs and partial definitions inherent in Interpretation 3 are entirely consistent with what they are saying.

Three who actually do recognize the compatibility between Interpretation 3 and contemporary mentalism are Suppes (1975), Nelson (1989), and Leahey (2000). Here is Suppes:

[In neobehaviorism as opposed to classical behaviorism it is quite appropriate to postulate a full range of internal structures, ranging from memory hierarchies to language production and language comprehension devices that cannot be, from the standpoint of the theory, directly observed. . . .It is my view that the approach of cognitive psychologists or of psychologists interested in complex problem solving or information processing (Newell and Simon, 1972, is a good example) could be fit within a neobehaviorist framework if a proper amount of structure is assumed and not mastered from scratch. . . .There is not a formal inconsistency between the two viewpoints. (pp. 270, 279-280)

Here is Nelson:

My own view. . . is that [hypothetical constructs] fall within the pale of the computable, i.e., of [T]uring automata. So if you admit hypothetical constructs, you’ve got machines, in principle. (p. 310)

Finally, here is Leahey:

Although it was a major theoretical position in the 1950s, mediational behaviorism ultimately proved to be only a bridge linking the inferential behavioralism of the 1930s and 1940s—Hull’s and Tolman’s theories—to the inferential behavioralism of the 1980s: cognitive psychology. . . [note: Leahey defines behaviorism as the attempt to predict, control, explain, or model behavior, where in so doing one may or may not refer to conscious or unconscious mental processes. Behavioralism is aimed at behavior; consciousness—the mind—is not the object of study, although it may be called on to explain behavior.] The mediationalists’ commitment to internalizing S-R language resulted primarily from their desire to achieve theoretical rigor and to avoid the apparently unscientific character of “junkshop psychology.” In essence, lacking any other language with which to discuss the mental processes in a clear and disciplined fashion, they took the only course they saw open to them. However, when a new language of power, rigor, and precision came along—the language of computer programming—it proved easy for mediational psychologists to abandon their mediating response life raft for the ocean liner of information processing. (p. 479)
The explication of the information-processing “paradigm” by Lachman et al. [Lachman, Lachman, & Butterfield, 1979] makes clear that information-processing psychology is a form of behavioralism with strong affinity to all but radical behaviorism. . . . In short, although Lachman et al. specifically deny it, information processing adopted a modified logical positivism from neobehaviorism. (p. 508)

Although it’s quite different from radical behaviorism, information-processing psychology is a form of behavioralism. It represents a continuing conceptual evolution in the psychology of adaptation. . . . Herbert Simon, one of the founders of modern information-processing psychology, revealed the continuity of information processing with behavioralism, and even its affinity with behaviorism. . . . Information-processing psychologists share many important behaviorist assumptions: atomism, associationism, and empiricism. On the philosophical side, information processing espouses materialism, holding that there is no independent Cartesian soul, and positivism, continuing to insist on operationalizing all theoretical terms. . . (p. 510)

Baars (1986) and Fodor (1968) seem to view mentalism as inherently superior to any kind of behaviorism. According to the present argument, mentalism is entirely compatible with the methodological behaviorism of Interpretation 3. Many mentalists simply want to “climb onto the behavioristic band-wagon unobserved,” as Skinner (1945, p. 292) put it in a different context, by relating terms to behavior. At issue is whether they do so well enough. Typically they don’t. Suppes (1975), Nelson (1989), and Leahey (2000) show more sophistication and at least acknowledge the compatibility.

**Recapitulation**

Table 1 below summarizes the various features of methodological behaviorism from the point of view of radical behaviorism. Along the vertical axis of the table are the criteria that can apply to the various interpretations of methodological behaviorism. These criteria are summarized as follows:

Criteria

1. The terms and concepts in scientific theories and explanations, including those in psychology, should be based on phenomena that are directly observable, measurable, and capable of generating agreement.

2. Phenomena such as causal mental states and processes that don’t meet the criteria of being observable, measurable, and capable of generating agreement may not even exist.

3. Phenomena such as causal mental states and processes that don’t meet the criteria of being observable, measurable, and capable of generating agreement do exist but can’t be directly included in psychology; psychology must remain silent on them.

4. Phenomena such as causal mental states and processes that don’t satisfy the criteria of being observable, measurable, and capable of generating agreement do exist but still can’t be directly included in psychology; however, psychology need
not remain silent on them: They can be included indirectly in psychology. Identifying the source of the mental phenomena is less important than understanding their causal contribution, if it is important to identify their source at all.

5. Psychology should distinguish between theoretical and observational terms, where terms referring to causal mental states and processes are theoretical rather than observational terms; the causal status of the theoretical terms ranges from initiating to mediating in an S-O-R model.

6. Causal mental states and processes should be regarded as theoretical terms and on this basis included indirectly in psychology; the sense of based on is that the theoretical terms are logically connected to observable stimuli and responses by exhaustive operational definitions.

7. Causal mental states and processes should be regarded as theoretical terms and on this basis included indirectly in psychology; the sense of based on is that the theoretical terms are logically connected to observable stimuli and responses by partial operational definitions.

Along the horizontal axis of the table are the interpretations that are taken to characterize behaviorism. According to radical behaviorism, these interpretations constitute one or another version of methodological behaviorism. Interpretation 1-A is silent on mental phenomena because it denies mental phenomena even exist. Interpretation 1-B is silent on mental phenomena because it wants to satisfy what it considers the requirements of good science, even though it implicitly acknowledges that mental phenomena exist and are causal. Interpretation 2 admits causal mental phenomena but requires exhaustive operational definitions of the mental phenomena in terms of observables. Interpretation 3 admits causal mental phenomena but requires only partial operational definitions of the mental phenomena in terms of observables.

Table 1. Features of methodological behaviorism from the point of view of radical behaviorism.

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Figure 1 below is a Venn diagram of the relations described in this review among mentalism, dualism, and the three interpretations of methodological behaviorism. As before, Interpretation 1-A is silent on mental phenomena because it questions whether mental phenomena even exist, as it does with any unobservable. Therefore, we provisionally place Interpretation 1-A outside of mentalism. However, just because the interpretation is outside doesn’t mean that it is acceptable—it still yields an incomplete account of a behavioral event because, as radical behaviorism argues, some unobservables are in the behavioral dimension and can therefore be legitimately included in psychology. Interpretation 1-B is silent on mental phenomena because it wants to satisfy what it considers the requirements of good science, even though it implicitly acknowledges that mental phenomena exist and are causal. Interpretation 2 admits causal mental phenomena but requires exhaustive operational definitions of mental phenomena in terms of observables. Interpretation 3 admits causal mental phenomena but requires only partial operational definitions of mental phenomena in terms of observables. Interpretations 1-B, 2, and 3 are mentalistic because they accept mental causes, even though they try to deal with those mental causes in different ways. These interpretations represent an institutionalized mentalism. Finally, all dualists are mentalists, though some mentalists are not dualists.

Figure 1. The relations among mentalism, dualism, and the interpretations of methodological behaviorism.
Summary and Conclusions

As we have seen, methodological behaviorism is a strongly prescriptive orientation to how psychologists should do psychology. Its principal feature is that psychologists should talk only about observable stimuli and behavior in their theories and explanations. Initially, this feature was interpreted to mean that psychologists should remain silent and not appeal directly to unobservable psychological terms and concepts at all (Interpretation 1). Later, this feature was interpreted to mean that psychologists could appeal to unobservables in their theories and concepts after all, but only indirectly, after the meaning of those unobservables has been established by logically connecting them to observable stimuli and behavior through operational definitions. At first, the definitions had to be exhaustive and not admit surplus meaning (Interpretation 2). Subsequently, the definitions could be partial and admit surplus meaning (Interpretation 3).

Is the present review guilty of the same distasteful concerns that it has laid at the feet of methodological behaviorism, namely, of prescribing a particular ideology to doing psychology? To be sure, radical behaviorism questions the entire set of assumptions underlying methodological behaviorism, arguing that methodological behaviorism is unpragmatic and based on extraneous considerations that are cherished for incidental and irrelevant reasons. The important consideration for radical behaviorism is pragmatism, not ideology. Skinner (1953) stated the matter as follows:

The scientific “system,” like the law, is designed to enable us to handle a subject matter more efficiently. What we call the scientific conception of a thing is not passive knowledge. Science is not concerned with contemplation. When we have discovered the laws which govern a part of the world about us, we are then ready to deal effectively with that part of the world. By predicting the occurrence of an event we are able to prepare for it. By arranging conditions in ways specified by the laws of a system, we not only predict, we control: we “cause” an event to occur or to assume certain characteristics. (pp. 13-14)

Hence, as noted throughout the present review, radical behaviorism is not simply interested in post-hoc description. Radical behaviorism is clearly interested in prediction, but by virtue of its implications for practical, direct action rather than as a property of a logical argument. Therefore, private behavioral events may legitimately be incorporated in the interpretation and explanation of behavior. Moreover, the explanation of behavior itself may be understood as a behavioral matter, especially when it comes to the verbal practices of psychologists. The failure in methodological behaviorism to directly recognize the contribution of private behavioral events constitutes an incomplete psychology because a legitimate participant in the causal process is not acknowledged. In addition, an attempt to indirectly recognize mediating organismic variables as theoretical terms constitutes an “institutional mentalism,” where conventionally accepted practices are taken to validate appeals to causal factors in other dimensions. Factors in the behavioral dimension go unheeded.
As we have seen, operationism has played a major role in the development of methodological behaviorism. Earlier, the present review mentioned that Grace (2001) and Feest (2005) recently addressed the history and influence of operationism in psychology. Their reviews critically examine many important and traditional topics, but regrettably not the functional analysis of verbal behavior generally, and scientific verbal behavior especially. Both cite Boring’s (1945) Symposium on Operationism, but Feest does not cite Skinner’s (1945) contribution to the Symposium at all. To his credit, Grace (2001, pp. 18-22) devotes several pages to Skinner’s view of operationism. However, even though Grace does acknowledge that Skinner was, in effect, proposing a radical behaviorist epistemology based on verbal processes, which Skinner clearly was, Grace ultimately suggests that Skinner was offering a metaphysical argument based on untestable metaphysical assumptions. Skinner would presumably agree with both Grace and Feest that the conventional interpretation of operationism under Skinner’s contemporary Stevens led nowhere. The reason it led nowhere was that it was based on a symbolic, referential view of verbal behavior. Unfortunately, neither Grace nor Feest points out the liabilities of the symbolic, referential view. For example, Feest provides an exemplary summary of Stevens’ position, and why Stevens was committed to his view of operationism, but throughout Feest assumes the symbolic, referential view of verbal behavior is essentially correct rather than the source of the problem.

Skinner (1945) offered an alternative interpretation of operationism based on an alternative account of verbal behavior. According to this alternative interpretation, operationism implies the functional analysis of verbal behavior, including scientific verbal behavior. Meaning was a function of the determiners, not properties of a verbal response as traditionally conceived in a symbolic, referential account of verbal behavior. The matter of logically connecting psychological terms to observable events through operational definitions, regardless of whether those definitions are exhaustive or partial, or whether the theoretical terms are interpreted as intervening variables or hypothetical constructs, is simply beside the point. All of these traditional matters assume a symbolic, referential view of verbal behavior, which is mistaken. The present paper has outlined the problems associated with the traditional view. Stevens and others since were simply mistaken to accept the verbal report in a discrimination procedure as veridically referring to an internal sensation in another dimension. At the very least, one needs to account for how verbal behavior under the control of private events can even develop in light of the problem of privacy (Skinner, 1945, p. 273). Verbal behavior results from the reinforcing action of the verbal community. In everyday language, the problem of privacy is how does the verbal community know when to provide a reinforcer when the verbal community isn’t in contact with the discriminative stimulation, which is private? Without such an account the door is open for mentalism, if not outright Cartesian mind–body dualism. The principal problem regarding verbal reports of so-called “subjective events” is in the verbal field—how does verbal behavior develop under the discriminative control of private stimulation? The traditional solutions such as those of Stevens and others
were uncritically meditational in character and led to preserving “a vast vocabulary of ancient and non-scientific origin” (Skinner, 1945, p. 271) and “the old explanatory fictions unharmed” (Skinner, 1945, p. 292). Indeed, verbal behavior ostensibly about private events may suffer from two limitations. First, it actually might not be very accurate, even though its form superficially suggests it is, because the verbal community has great difficulty differentially reinforcing verbal responses under the control of sensations and feelings and generating precise stimulus control. Second, there is the possibility of fictional distortion, where verbal behavior is actually under the control of “drives associated with their consequences rather than antecedent stimuli” (Skinner, 1945, p. 275). An operational analysis of the contingencies governing the emission of the verbal response will sort out the possibilities, and this approach is very different from anything that Grace (2001) and Feest (2005) review (see Moore, 1975, 2008).

Finally, a position in philosophy called “philosophical behaviorism” is sometimes linked with methodological behaviorism in psychology. Philosophical behaviorism is also known as analytic philosophy or conceptual analysis, and it is often associated with the positions of Ludwig Wittgenstein and Gilbert Ryle, among others, in mid-20th century philosophical thought. Philosophical behaviorism is intimately concerned with establishing the meaning of psychological terms and concepts, as was methodological behaviorism. The main feature of philosophical behaviorism is that psychological terms and concepts should be taken to refer to “dispositions to behave.” Psychological terms and concepts should not be taken to refer to inner workings such as those of a “ghost in the machine,” as Ryle (1949) famously put it. For Wittgenstein, publicly observable behavior, for example moaning and groaning, was a criterion that justifies the application of putatively mental terms like pain. On a radical behaviorist reading, this position may be taken to mean that publicly observable behavior constitutes the conventionally accepted circumstances in a given language community in which the putatively mental term is ordinarily used. Thus, in one sense, philosophical behaviorism did render an understanding of psychological terms by referring to observable behavior, as did methodological behaviorism. Nevertheless, the relation between philosophical behaviorism in philosophy generally, the specific form of analytic philosophy advocated by Wittgenstein, and methodological behaviorism in psychology is a topic of much debate (see Moore, 2001a, 2001b, 2008, and Zuriff, 1985). At issue is whether the statement “John intends to do X” is occasioned by the same circumstances as “John is disposed to do X,” and whether the statement “Mary is in pain” is occasioned by the same circumstances as “Mary is disposed to moan and groan.” Could individuals be meaningfully said to have an intention but not act, or to be in pain but not moan and groan? Is a disposition nothing more than an observable index to some causal entity from a mental dimension? More recent positions in philosophy with a mentalistic orientation, such as philosophical functionalism, strenuously criticize dispositional treatments of psychological terms, arguing that they are derived too closely from verificationist accounts of physicalism and logical behaviorism. A full evaluation of philosophical functionalism in
contemporary cognitive psychology vis a vis philosophical behaviorism, analytical behaviorism, methodological behaviorism, and radical behaviorism remains a tale for a different telling.

References


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