

*THE DEMISE OF BEHAVIORISM—EXAGGERATED
RUMOR?: A REVIEW OF MACKENZIE'S
BEHAVIOURISM AND THE LIMITS OF
SCIENTIFIC METHOD¹*

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The appearance of this article is a paradox. On the one hand, it reviews a book explaining the failure and ultimate collapse of behaviorism as an influential force in psychology. On the other hand, it appears in an obviously healthy journal recognized as a reporting organ for research strongly in the behaviorist tradition. From all appearances, this research, a lineal descendant of the "neo-behaviorist" experimental program begun in the 1930s, is far from moribund. Of course, reports of the death of behaviorism have been common over the past fifty years, the first occurring within a few years of Watson's declaration of the movement. Mackenzie, however, provides more: an obituary, an autopsy, and a diagnosis of the terminal disease.

Whether one agrees with Mackenzie's conclusions or not, the book is an important one on several counts. First, it is the first full-scale analysis of the philosophy of science underlying the behaviorist movement from its inception to the present. Second, several of Mackenzie's observations about behaviorism are of significance independent of the role they play in his overall argument. Third, using the behaviorist school as a "case study," the book elaborates an interesting general thesis in the philosophy of science, addressing the question of how scientific theory develops. In the next section, an outline of Mackenzie's central argument will be presented although not in the same sequence as that of the book.

THE ARGUMENT

Background

Mackenzie traces the origins of behaviorism to the conceptual crisis arising in comparative psychology at the turn of the last century. In Europe, animal psychologists such as Romanes, Morgan, and Hobhouse developed a comparative psychology whose goal was to reconstruct the pattern of evolutionary development of mind, i.e., capacities for increasingly complex adaptive behavior (pp. 55-73).² To deal with the inaccessibility of the animal mind, the argument from analogy was used: Whenever an animal shows behavior that is similar to that of a human, then the subjective experience accompanying that behavior in the human may be inferred to accompany that behavior in the animal. Although numerous objections to the argument from analogy ultimately discredited it, Mackenzie draws an important distinction among inferences from behavior (pp. 62-66). On the one hand are inferences as to the subjective experiences of the organism, i.e. "subjective inferences." In contrast are inferences about the organism's capacities, mental operations (functionally defined), and abilities, i.e. "objective inferences." If a particular capacity (e.g., for recognizing size relationships) is necessary for the performance of an action, and if the animal performs the action, then the capacity can be inferred. As method and theory were refined in European comparative psychology, increased emphasis was directed toward objective inference while subjective inferences played a smaller and more isolated role.

¹Mackenzie, B. D. *Behaviourism and the limits of scientific method*. Atlantic Highlands, N.J.: Humanities Press, 1977. Pp. xiv + 189, \$11.50.

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²Unless otherwise noted, all page references are to Mackenzie (1977).

In contrast to the European tradition, American Functionalism emphasized the role of consciousness, as conceptualized by Wundt and Titchener, in the adaptation of organisms to their environment. Hence the emphasis was necessarily on subjective inferences from behavior. However, with increasing sophistication as to the limitations of the argument from analogy, Functionalists became progressively more restricted in the kinds of inferences they could draw. The inferences inevitably came to be limited to a mere translation of the objective environment into the sensation language of introspective psychology. At the same time, this impoverished consciousness was supposed to serve as the most important determinant of the organism's behavior. Because Functionalists avoided objective inferences and restricted themselves instead to subjective inferences about mere sensations, behavioral observations became increasingly irrelevant to the interpretation of the animal mind. Consequently, there developed a gradual estrangement of data from the theoretical interpretations intended to make sense of them (pp. 73-87).

Classical Behaviorism

It was in reaction to this conceptual crisis that Watson declared his behaviorist manifesto. All concern with consciousness in comparative psychology would be eliminated, and only behavior itself would be studied. Mackenzie argues that Watson merely dropped consciousness out of Functionalist psychology but retained everything else including the Functionalist conception of behavior. Since for the Functionalist everything that gives meaning to behavior, e.g., its adaptiveness or purposefulness, is attributed to consciousness, behavior is what is left—the mere meaningless movement of an object in space. Indeed, Mackenzie claims that many of Watson's assumptions (e.g., the adoption of a stimulus-response model, the tendencies toward environmentalism and minute analysis) all stem from the elimination of consciousness from the conceptual framework of Functionalist comparative psychology (pp. 87-95).

However, Watson did not limit his reforms to comparative psychology. Instead, he extended his repudiation of the mental to all of psychology. According to Mackenzie, Watson was able to carry out this wholesale ex-

ension by appeal to an ideal standard of objectivity exemplified by the natural sciences. Thus, Watson's behaviorism was more than just a reaction to conceptual problems in comparative psychology. His rejection of mentalism throughout psychology and his adoption of the methodology of physics did not logically follow from the failures of Functionalist comparative psychology. Instead, his behaviorism was based on the a priori beliefs that no inferences about the mental can be objective and that the methods of the natural sciences are appropriate for psychology (pp. 10-15, 96-100).

Thus, Watson's influence was chiefly to introduce into psychology a change in methodology. He did not, however, provide a set of scientific achievements that could provide model problems and solutions to a community of practitioners. That is, Watson did not provide what Kuhn (1962) has called a scientific "paradigm" (pp. 4-8, 18-23). Mackenzie argues that the incorporation into behaviorist psychology of the work of Thorndike and Pavlov also did not provide a paradigm for American behaviorism because, although their methods were adopted, their general theories were not (pp. 5-9). Thus, from its inception, behaviorism was not a school of psychology committed to what is regarded by its members as a common store of substantive knowledge growing by cumulative accretion (pp. 3-5, 19). Instead, it was only a commitment to a set of positivistic *methodological* principles, the most important of which were objectivity and the repudiation of unobservables (p. 23).

Neobehaviorism

In the 1930s, Watson's brand of psychology developed into the "neobehaviorism" of Hull, Tolman, Guthrie, and Skinner. These later behaviorists were not bound by Watson's polemical rejection of consciousness. Instead, they were concerned with developing behaviorist theory and needed some method to insure that their theoretical concepts were objective. They sought this objectivity in the adoption of formal and explicit decision procedures providing criteria to determine whether a term, concept, or theory meets the standards of objectivity and observability. For these procedures, they turned to the logical positivists who had developed decision criteria through their analysis of the failure of Newtonian physics and the scientific revolution that en-

sued. Two of their major decision procedures were the verificationist criterion of meaning and the use of operational definitions, both adopted and refined by neo-behaviorists. It was the behaviorists' central belief that the use of these formal procedures would generate the development of scientific theory in psychology and the convergence of diverse positions within the field. This would occur, they held, even in the absence of any common initial commitment to a substantive theory or generally agreed upon store of knowledge (pp. 15-23, 101-113, 116).

It is Mackenzie's major thesis that this commitment to positivistic methodology without a commitment to a common paradigm necessarily led to fragmentation in psychological theory rather than to convergence, and to continued controversy over the basic questions in the field rather than to gradual resolution. This failure of positivistic methodology followed from two sources. The first is the set of internal philosophical problems logical positivism was never able fully to overcome in meaning its own stated goals (pp. 116-134). The second source is pragmatic—Mackenzie argues that positivism is simply inappropriate for the development of scientific theory.

Positivism

Positivism's fundamental tenet—the repudiation of unobservables—is in apparent conflict with what is commonly regarded to be the role of scientific theory: to tell us about reality. Scientific theory contains statements about the world, and these statements, strictly speaking, transcend observation both in their universality and in their assertions of truths about the reality underlying observations. Therefore, a natural corollary of positivism is conventionalism, the view that scientific theories are neither true nor false but are rather conventions, more or less useful. As instruments they may be used to get from one observation to another, but they say nothing about a reality beyond (pp. 28-36). This conventionalism contrasts with realism, the view that scientific inquiry is capable of revealing truths about the world, and this truth is not limited by the limitations of experimental procedures. A realist orientation allows the scientist to maintain a theory even though the observational evidence for the theory is not currently compelling (pp. 28-43).

Mackenzie argues that the conventionalism of positivism inhibits theory development. First, conventionalism cannot deal adequately with anomalies, i.e. observations contrary to the predictions of the theory. Any developing theory will encounter such anomalies, and they require judgments: Should the theory be modified? Should it be rejected on the basis of the anomalies? Should the consideration of the anomalies be postponed until the theory is more fully developed? Answering these questions necessitates judgments going beyond the available observational evidence to assess the significance of the anomaly in the context of knowledge that has not yet been acquired. Such judgments, therefore, are inconsistent with a positivistic orientation (pp. 120-1). Furthermore, the positivist commitment to formal features of theory rather than its substantive ontological claims encourages the accommodation of theory to anomalies through ad hoc and trivial adjustments of the theory. If a theory is merely a convention, then adjustments improving the theory's utility are justified regardless of how ad hoc or trivial they are (pp. 122-126).

Implications

Mackenzie argues that it was precisely these features of positivism in behaviorism that led to the continuing fragmentation of behaviorist theory. Each behaviorist school, consistent with its commitment to formal and explicit decision procedures, modified its theory in order to maintain the theory in the face of the negative findings from experiments performed by other schools. Hence, although the various behaviorist theories came to make similar predictions and thus to "converge empirically," there was no comparable theoretical convergence. Since each theory could be made empirically defensible, behaviorism's positivist orientation ensured theoretical disunity and the impossibility of agreement on a paradigm (pp. 143-4).

Furthermore, positivism rules out the kind of logically indefensible ontological reference or unformalized substantive insight permitted by realism. However, Mackenzie argues, these references and insights are indispensable for theoretical development. They provide the only possible basis for many crucial decisions (e.g., how to interpret anomalies, what variables to implicate in operational definitions).

Therefore, it was necessary for behaviorists to incorporate some such references in their theories if these theories were to develop at all. However, because of behaviorism's aversion to such references, they were incorporated *covertly*. It is only through painstaking analysis, as exemplified in the 1954 *Modern Learning Theory* (Estes, Koch, MacCorquodale, Meehl, Mueller, Schoenfeld, & Verplanck, 1954), that these references can be made explicit. As long as they remained covert and outside the scrutiny of the scientific community, they were not subject to adoption or criticism. Because these unexamined covert ontological references differed from theory to theory, they further prevented theoretical convergence and agreement among the various behaviorist schools (pp. 144-148).

Although positivism is thus inappropriate for the development and elaboration of a theory (the context of "construction"), Mackenzie suggests it is appropriate for the period of transition from one theory to another (the context of "reconstruction"). In the context of reconstruction, attention is focused on the variables and concepts of the theory primarily as components of the theory and only secondarily as attributes of the world. There is thus conceptual economy in a positivist examination of these as they occur, without the realist assumption of external reference independent of their observational content (pp. 50-53). Because positivism withholds any attribution of "truth" to a theory, it provides the flexibility necessary in the context of reconstruction to pursue tentative, diverse, and even incompatible routes toward the solutions of fundamental conceptual problems that become apparent during scientific revolutions. Such flexibility encourages flights of imagination since there is no commitment to the "reality" of the theory (pp. 134-141).

However, Mackenzie claims, the positivism of behaviorism did not play a salutary role even in the context of reconstruction. Because Watson extended his positivism beyond comparative psychology, calling for the elimination of mentalism even in human psychology, his positivism was no longer a useful methodology for the reconstruction of specific troublesome concepts in comparative psychology. Rather, in later behaviorism, it became a "weapon" directed at all mental concepts, divorced from its specific context of application.

Instead of ridding psychology of theoretical terms with empirically unspecifiable references, it forced the references to become covert and separated from their central position in theory development (pp. 148-152).

Thus the failure of behaviorism—its theoretical fragmentation, its lack of agreement over fundamental issues, its inability to develop a basic paradigm—is shown to be the inevitable consequence of its methodological commitment to positivism. Indeed, Mackenzie suggests that the chief *systematic* contribution of behaviorism lies in its practical demonstration that a science based solely on a positivist methodology will fail (pp. 154-156). In his opinion, the chief *unsystematic* contribution of behaviorism is behavior modification, although he questions whether behavior modification is really an outgrowth of behaviorism (pp. 158-159). A further *unsystematic* contribution of behaviorism is the development of perceptual skills as exemplified by Skinner and his followers (pp. 160-170). Verplanck (1954) had already noted that seeing response rate changes in a cumulative record is an acquired skill. Koch (1964) further remarks that many of the terms close to the observation base in behavior research are similarly acquired. Mackenzie carries these ideas one step forward and suggests that "Stimuli and responses are what one has learned to see as stimuli and responses. They are not initially but they *become*, directly observable." (p. 167)

DISCUSSION

Mackenzie's argument is tight, well constructed, and developed in carefully reasoned steps. On the one hand, the appeal of such arguments is their elegance. Their weakness, on the other hand, is that any false move has serious consequences for the entire closely knit fabric. In what follows, several of these questionable moves will be presented. Although these problems may not be fatal for Mackenzie's argument, they do flaw the elegance of the argument, and, in some cases, limit its generality.

Classical Behaviorism

Mackenzie argues that Watson's positivism could not serve a helpful role in the behaviorist revolution because Watson did not limit his positivist reforms to the conceptual crisis

in comparative psychology but instead extended it to human psychology where, Mackenzie claims, there was no comparable conceptual crisis. For example, he states that

The dispute between the Wundtians and the Wurzburgers . . . did not constitute a methodological failure generally of all introspective methods. . . . It could be seen as a general failure only by those who were already prepared to repudiate introspection on other grounds. (p. 11)

He shows how introspectionist methods and theories could have been saved despite the controversy over imageless thought. There are two problems with this account. First, what is said about introspectionism could be said about nearly any theory prior to its being "overthrown" in a scientific revolution. Nearly any theory can be modified or reinterpreted so that its problems do not constitute absolutely compelling reasons for discarding it. Therefore, the claim that introspectionism could be "saved" does not imply that there was no serious conceptual crisis in introspectionist human psychology for which a positivist revolution would have been appropriate.

Second, whether or not there was a conceptual crisis in introspectionist human psychology, the early behaviorists certainly perceived that there was. Nearly every major behaviorist in the early behaviorist revolution discusses the problems inherent in introspectionism and attempts to reinterpret the introspectionist experiment within the behaviorist framework (cf. Tolman, 1922a, b; Watson, 1924; Weiss, 1917, 1918). For their part, Structuralists (e.g., Washburn, 1922) responded in kind. The concerns evidenced in these writings and the controversy they elicited indicate that the behaviorist positivist reformation of human introspectionist psychology was not merely an unwarranted a priori extension from comparative psychology. It was, at the least, a response to perceived serious deficiencies in introspectionist methodology and theory.

Perhaps, nevertheless, the behaviorist reformation was an overextension in its repudiation of what Mackenzie terms "objective inferences" of mental operations in addition to its rejection of "subjective inferences" (cf. supra). This important distinction is particularly relevant to contemporary behaviorism. Repeated attempts over the past fifty years to introduce

objective inferences into behaviorist theory have met with continued resistance. A particularly good illustration of this dispute emerged recently in the pages of this journal between Shimp (1976) and Branch (1977). Shimp argues for the introduction of the concept of "short-term memory" into the experimental analysis of behavior. For him, short-term memory is a theoretical quantity not directly observed but mathematically derived from observables in a context where a speaker using ordinary language would find the word "memory" relevant (Shimp, pp. 120-121). Branch, echoing fifty years of behaviorist thinking, rejects Shimp's suggestion on pragmatic rather than logical grounds. Although there may be nothing philosophically objectionable about such objective inferences, they tend to interfere with research and theory development. Concepts such as memory tend to be reified; they soon come to serve as explanations rather than descriptive generalizations; they are readily attributed properties and interpretations beyond those objectively inferred from behavior; and they divert attention away from the external environment, the ultimate independent variable in the functional analysis of behavior. Indeed, these divergent views of the heuristic value of objective inferences account for much of the disagreement between modern cognitivist and behaviorist positions (cf. Skinner, 1977a).

Neobehaviorism

Mackenzie's treatment of the transition to neobehaviorism includes certain questionable assumptions. He claims that in neobehaviorism the early antimentalism of Watsonian behaviorism was subordinated to a commitment to formal decision criteria. This implies, according to Mackenzie, that mental concepts could be admitted to theory as long as they met the explicit criteria of objectivity. Mackenzie cites Hull's (1937) A.P.A. presidential address as evidence for this assertion. Although Mackenzie's reading of Hull is a plausible one, a careful consideration of Hull's statement within the context of his overall philosophy of science suggests an alternate interpretation in which a priori antimentalism is still present, but implicit and presupposed by the criteria of objectivity. Although Hull was willing to admit mind into psychological theory, its status would be that of a theoretical postulate. As such its meaning would be, according to

Hullian methodological principles, exhausted by equations linking it to environmental variables on the one hand and to behavioral variables on the other. Furthermore, it would necessarily be a hypothetical entity, not one known directly through experience. It is not clear how such a theoretical entity is at all related to anything resembling the common concept of consciousness.

Second, although Hull was also willing to admit mind into psychology as a dependent variable, his method for explaining this variable presupposed a behaviorist concept of the mental. Explaining the mind, according to Hullian methodology, consisted of two stages. First, it was necessary to find those behaviors allegedly manifesting mind (e.g., insightful behavior). Second, it was necessary to explain those behaviors by deriving their occurrence from the behavioral theory. Presupposed in this methodology is the assumption that the mental is nothing more than a feature of certain classes of behavior. Explaining the behavior means explaining the mind. Contrast this approach with one that assumes the existence of mental contents and therefore admits the qualia of experience as legitimate dependent variables for psychology to investigate and explain. Thus, although it may appear that Hull substituted explicit decision procedures for Watson's polemical antimentalism, the antimentalism is there, nevertheless, presupposed by the methodology, and the polemics, too, are not hard to find (e.g., Hull, 1933, 1937).

Another problem in Mackenzie's treatment of Hull relates to the question of unobservables. A critical point in Mackenzie's thesis is that neobehaviorists, while explicitly repudiating indefensible ontological references in their theories, nevertheless included such references covertly. However, the truth of this assertion turns on an interpretation of MacCorquodale and Meehl's (1948) distinction between "intervening variables" and "hypothetical constructs." According to MacCorquodale and Meehl, the latter are not exhaustively reducible to observable empirical relations. Furthermore, they claim, certain Hullian episodic theoretical terms (e.g., the anticipatory goal reaction), as opposed to state-variable terms (e.g., habit strength), qualify as hypothetical constructs. If so, then Hullian theory does contain explicit ontological references from a

realist orientation. Indeed, while a good case can be made to show that Tolman, Guthrie, and Skinner held a conventionalist orientation, it is not clear that Hull did, and Mackenzie supplies no evidence for Hull's conventionalism. To be sure, over the past thirty years, the validity of MacCorquodale and Meehl's distinction and its relationship to behaviorist theory have been a source of much contention (cf. Spence, 1948, 1957), but the issue is critical for Mackenzie's thesis. However, Mackenzie's remarks on the problem are limited to one long, but unpersuasive, footnote (p. 186, n. 67), surely too cursory a discussion for such an important point.

The most serious problem with Mackenzie's treatment of neobehaviorism is that, from beginning to end, nearly all of Mackenzie's thesis simply does not apply to the most influential of the neobehaviorists—B. F. Skinner. First, Skinner (e.g., 1945, 1974) has been quite clear that his use of operationism does not exclude unobservables from behaviorist psychology. To the contrary, he has elaborated, at considerable length, a theory showing how private events can be included in the experimental analysis of behavior. These private events have by no means been "covertly" introduced into Skinner's theory. Second, Skinner (1950) is quite explicit in his rejection of the hypothetico-deductive method as well as any formal decision procedures for theory development. Instead, he has placed greater emphasis on hunches, faith, luck, and intuition than on rules of inference (Skinner, 1956). However, according to Mackenzie's thesis (pp. xiii, 43), such an approach is incompatible with the positivist neobehaviorist stance. Yet Skinner is surely a positivist in the Machian tradition.

The trap Mackenzie falls into, that of treating all neobehaviorists as if they were Hullians and ignoring the major differences between Skinner and Hull, is quite common. The most egregious example is that of Koch (1964) who, at a symposium where Skinner was the only behaviorist participating, mounted a severe attack on "behaviorism" while practically ignoring Skinner's position. Indeed, it is no accident that much of Mackenzie's analysis of behaviorism's adoption of formal decision procedures is based on Koch's discussion.

To be sure, Mackenzie recognized that much of his thesis did not apply to Skinner. Consequently, two alternatives were available to

him. The first was somehow to force his paradigm onto Skinner, willy-nilly. The second was to use Skinner's example as evidence for his central theses but to limit these theses to a subset of behaviorists. Skinner's example can be taken to support Mackenzie's central theses because it shows that a behaviorist who does *not* adopt formal decision procedures as the sole method to develop theory, who does *not* repudiate private events in psychology, and who is *not* inconsistent in his conventionalism, can develop a science of behavior *not* doomed to failure. Had Mackenzie chosen this route, several happy consequences would have ensued. First, Mackenzie would not have been embarrassed by the obvious vitality of what is variously known as "the experimental analysis of behavior," "operant psychology," or "Skinnerian psychology," and is clearly a direct outgrowth of Skinner's neobehaviorism of the 1930s. Second, Mackenzie could have included "operant psychology" among the "unsystematic positive contributions of behaviorism" along with behavior modification instead of being forced to ignore such a fruitful area of psychological research. Third, this review would not have been a paradox.

Unfortunately, Mackenzie chose the former route—Skinner is forced into the Procrustean bed of Mackenzie's thesis. According to Mackenzie:

the covert substantive principles in Skinner's position . . . function . . . as basic meta-systematic orienting assumptions. These assumptions are fairly prominent in Skinner's system, and account for all of its general systematic (although atheoretical) character. . . . The assumption of environmental generality, to put it excessively crudely, asserts that the Skinner box is representative of all environments. The assumption of species generality, equally crudely, asserts that the pigeon is representative of all species of organisms. (p. 160)

It is here that Mackenzie's argument is most unsatisfactory. For one thing, he totally ignores Skinner's own discussions of these issues (e.g., Skinner, 1966, 1977b). He also ignores recent developments in operant theory (e.g., Herrnstein, 1977; Schwartz, 1974) designed as a rapprochement with the "biological boundaries of learning"—the term used to refer collectively to the issues Mackenzie raises. Second,

even if Mackenzie is correct in attributing "covert substantive principles" to Skinner's position, for Mackenzie's thesis to apply, these principles would have to be responsible for the failure of theoretical convergence within behaviorism. However, while it may be plausible to view certain controversies between Hull and Tolman (e.g., latent learning, S-S versus S-R learning) as failing to resolve theoretical questions because of covert substantive references, it is difficult to see how Skinner's alleged "meta-systematic orienting assumptions" have done the same, and Mackenzie gives no examples. Controversies between ethologists and learning theorists, for example, have dealt with metasystematic assumptions quite explicitly, and cross-disciplinary enrichment rather than fragmentation seems to be the initial consequence. Similarly, while the interminable controversies between behaviorists and cognitivists rarely lead to resolution, this failure does not seem to be related to the "covert substantive principles" Mackenzie finds in Skinner. Nor do the current controversies within behaviorist psychology seem, for the most part, to stem from them. Finally, even granted that these covert principles exist, and even granted that they somehow have prevented the development of behaviorist theory, how does Mackenzie explain the apparent continued success of the programs Skinner initiated, both in their experimental and applied forms? Mackenzie's suggestion that operant psychology is proto-scientific but not yet science (p. 170) is unconvincing and presupposes a prescriptive concept of science in need of explicit defense.

Evaluation

Despite the objections raised here, Mackenzie's book represents an important and pioneering achievement. Displaying a remarkable knowledge of several disciplines (although Chapter 2 seems excessively dependent on secondary sources) he adeptly threads his way among philosophy of science, psychology, and intellectual history. The resulting synthesis is an interesting and revealing case study. To help the reader follow the closely reasoned argument, Mackenzie is careful to provide effective transitions—summaries and introductions—between major points. Moreover, his writing style is lucid, and he is skilled in presenting overviews that capture the essence of a theory without getting caught in the details.

Many of his ideas merit notice independent of their role in his argument about the failure of behaviorism. His thesis concerning the appropriateness of realism in the context of construction and positivism in the context of reconstruction is especially deserving of further attention. His distinction between "objective" and "subjective" inference is important and was discussed above. His analysis of Watsonian behaviorism as American Functionalism minus the concept of consciousness adds a new twist to an old truism. And his discussion of the "phenomenology of behavior" in behaviorism (i.e., the acquired perceptual skill of seeing stimuli and responses), although at first counterintuitive, is extremely suggestive.

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