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On behaviorism, pragmatism and scientific theories

Sobre el conductismo, el pragmatismo y las teorías científicas

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Abstract

The relation among theories, theoretical concepts, and scientific knowledge is a recurrent theme in scientific epistemology. On a pragmatic view, knowledge implies effective action. This view implies that theories and theoretical concepts should be evaluated in terms of how well they promote effective action, including prediction and control. Instrumentalism and realism are two further, although contrasting considerations in the interpretation of theories and theoretical concepts. Instrumentalism holds that one theory and its concepts should be regarded as better than another theory and its concepts if they make broader and more accurate predictions, irrespective of whether the theoretical concepts actually exist in nature. Realism holds that theoretical concepts should be regarded as real entities that actually exist in nature. The theoretical concepts identify the essential properties of the phenomena in question. In what has developed as the traditional approach to doing science, pragmatic, instrumental, and realist principles are all intermixed in scientific theories, particularly as the theories involve hypothetical constructs. The present chapter argues that pragmatism does not entail a commitment to instrumentalism, realism, or hypothetical constructs. An explanation is a statement that occasions successful action, including prediction and control. The key to an understanding of scien-

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tific theories lies in an understanding of the principles of verbal behavior, and of the contribution of those principles to the verbal activity called theorizing. The principle of pragmatism emphasizes that if a so-called theoretical concept proves valuable, the stimulus control over the term in question needs to be clarified, so that scientists do not subsequently make various kinds of errors.

Key words: behaviorism, pragmatism, instrumentalism, realism, hypothetical constructs, theories, verbal behavior

Resumen

La relación entre teorías, conceptos teóricos y el conocimiento científico es un tema recurrente en la epistemología científica. En una visión pragmática, el conocimiento implica acción efectiva. Esta visión implica que las teorías y conceptos teóricos deberían evaluarse en términos de cuán bien promueven la acción efectiva, incluyendo la predicción y el control. El instrumentalismo y el realismo son dos consideraciones adicionales, aunque contrastantes, en la interpretación de la teorías y los conceptos teóricos. El instrumentalismo sostiene que una teoría y sus conceptos pueden considerarse mejores que otra teoría y sus conceptos si hacen predicciones más amplias y precisas, irrespectivamente de que los conceptos teoricos existan realmente en la naturaleza. El realismo sostiene que los conceptos teóricos debieran considerarse entidades reales que existen realmente en la naturaleza. Los conceptos teóricos identifican las propiedades esenciales de los fenómenos en cuestión. En lo que se ha desarrollado como el enfoque tradicional para hacer ciencia, se han entremezclado en las teorías científicas los principios pragmáticos, instrumentales y realistas, particularmente en la medida en que las teorías comprenden constructos hipotéticos. Este capítulo sostiene que el pragmatismo no implica un compromiso con el instrumentalismo, el realismo o los constructos hipotéticos. Una explicación es un enunciado que ocasiona acción exitosa, incluyendo la predicción y el control. La clave para una comprensión de las teorías científicas yace en una comprensión de los principios de la conducta verbal, y de la contribución de estos principios a la actividad verbal llamada teorización. El principio del pragmatismo hace énfasis de que si un concepto teórico resulta valioso, debe aclararse el control del estímulo sobre el término, de manera que los científicos no cometan subsecuentemente varias clases de errores.

Palabras clave: conductismo, pragmatismo, instrumentalismo, realismo, constructos hipotéticos, teorías, conducta verbal

Among the scientific community, the most controversial feature of B. F. Skinner's radical behaviorism is undoubtedly its stance on "theories" (e.g., see Day, 1969b, pp. 502-505). Much of the controversy probably stems from Skinner's own writings on the topic of theories. A representative example is Skinner's (1950) provocative paper "Are theories of learning necessary?", in which he explicitly rejected the hypothetico-deductive epistemological strategies that dominated mid-century psychological theorizing. Largely as a result of such pieces, radical behaviorism has come to be portrayed as "atheoretical" and as representing a "Grand Anti-Theory" (see Skinner, 1969, p. vii). Ironically, Skinner's stance was actually quite theoretical, although the nature of that theory and the epistemology upon which it is based have not always been appreciated (Moore, 1984).

The purpose of the present chapter is to compare and contrast certain of the epistemological features of Skinner's radical behaviorism, particularly as those features concern scientific theories and scientific verbal behavior, with features of the more traditional approach. By so doing we hope to stimulate a more critical appreciation of radical behaviorist epistemology than currently exists. We begin with a brief review of the historical record, to establish a context for the review.

The historical record

Concerns with scientific epistemology are often linked to logical positivism. As many know, logical positivism was an influential philosophical movement that had roots in the Anglo-Germanic empirical tradition and that virtually defined the philosophy of science beginning in the second and extending into the third quarters of the twentieth century (Moore, 1985). Indeed, some say that the influence of logical positivism continues today, primarily in the social and biological sciences, rather than professional philosophy.

Logical positivism became prominent during the late 1920s, when the spectacular achievements of quantum mechanics and relativity theory in the "new physics" forced a reexamination of fundamental scientific concepts. No longer was it meaningful to claim that scientific knowledge possessed an enduring, absolute quality, based on direct observation of natural events. At the very least, the new physics indicated that science needed to consider both observable and unobservable phenomena in its theories. The question that dominated episte-

mological discussions of the time was how to guarantee that the unobservable phenomena being proposed in the theories were scientifically meaningful.

In answering this question, the logical positivists argued that scientific theories should consist of three sorts of terms: logical terms, observational terms, and theoretical terms. Logical terms refer to such logical operations as conjunction, disjunction, negation, and implication. Observational terms are first-order epistemological phenomena that refer to publicly observable physical objects or attributes of physical objects. Theoretical terms are higher-order epistemological phenomena that are inferred according to the established principles of logic from observational terms (Hempel, 1958; Maxwell, 1962; Smith, 1986). Given these three sorts of terms, the logical positivists argued that unobservables should be handled as theoretical terms, so that "metaphysical" propositions would not degrade the "cognitive significance" of the scientific enterprise.

The original logical positivist interpretation was that theoretical terms must be *explicitly and exhaustively reducible* to publicly observable events. In particular, many of the theoretical terms were to be treated as "dispositions," that is, as statements of publicly observable events that would obtain in specific circumstances. However, an approach requiring exhaustive reduction created its own set of problems, as Rudolf Carnap, a leader of the logical positivist movement, recognized by the mid-1930s. For one thing, concepts in science were flexible and probabilistic, not fixed and static as required by exhaustive definitions. For another, dispositions had no observable manifestations except under particular test conditions. What was the status of the disposition in the absence of the test conditions? Finally, if theoretical terms were exhaustively reducible to publicly observable operations, with no remainder, then they added nothing, and were logically superfluous. Hempel (1958) ultimately identified this third problem as one horn of the "theoretician's dilemma."

To resolve these problems, Carnap developed an alternative interpretation of the meaning of theoretical terms that involved partial rather than exhaustive definitions (Carnap, 1936, 1937; see Zuriff, 1985, pp. 59 ff.). The upshot was that logical positivists dropped the requirement that theoretical terms be exhaustively defined with reference to observable events, and required only that theoretical terms be logically derived from observable events. Indeed, Smith (1986, p. 28) suggests that the whole movement after the introduction of these changes during the mid-1930s should be known as "logical empiricism," rather than logical positivism, to mark this rather significant revision in doctrine.

During the first quarter of the century, significant changes also occurred in psychology. In 1913, John B. Watson launched his classical behaviorism. Gone was the concern with analyzing "subjective" experience and specifying the contents of consciousness through introspection. In its place was a more practical, "objective" concern with analyzing behavior and specifying its determinants through experimentation. However, by the 1930s most psychologists found Watson's classical S-R behaviorism wanting. For one thing, stimuli and responses were not always correlated in the way that classical behaviorism required. For another, the S-R model does not easily accommodate how individuals come to use subjective terms to describe various conditions inside their bodies.

As a result of the inadequacies of classical S-R behaviorism, psychologists began to appeal to mediating "organismic" variables that intervened between stimulus and response (Koch, 1964). Psychology then entered the era of S-O-R mediational neobehaviorism. In doing so, behaviorists faced a problem that was analogous to the one faced by the logical positivists: How did one guarantee that the mediating organismic variables being proposed in the theories were scientifically meaningful? Although Smith (1986) has pointed out that most neobehaviorists tended not to be in direct contact with technical logical positivist philosophy, mediational neobehaviorism nevertheless followed a similar tactic: The internal, psychological states—the mediating "organismic" variables—were interpreted as "theoretical terms" that required "operational definition" (Gergen, 1985, p. 536; Moore, 1995b). The trend is apparent in the writings of the "grand learning theorists," such as Tolman and Hull, during the 1930s, although it is prevalent in other aspects of the discipline as well (Gergen, 1985).

The original interpretation of operationism was that theoretical terms were to be exhaustively reducible to observables. For example, with respect to psychology, any "mental" term must be regarded as a theoretical term that referred to nothing more than either publicly observable behavior or "dispositions" to behave. Interestingly, by the late 1940s, continuing discussions about the nature of operationism vis-a-vis theoretical terms prompted psychologists to engage another problem analogous to one that logical positivists engaged some years earlier: Was it in fact necessary to exhaustively reduce theoretical terms in psychology to observables?

In a well-known article, MacCorquodale and Meehl (1948) proposed a solution. They pointed out that many theories in psychology had actually tended to employ two sorts of theoretical terms without explicitly realizing it. The first sort did require that theoretical terms be exhaustively reducible to observable

events, without remainder. However, the second sort allowed for meaning beyond observable events. MacCorquodale and Meehl proposed a linguistic convention, where instances of the first sort, such as Hull's "habit strength," would be referred to as intervening variables. Instances of the second, such as Hull's (1943) "oscillation factor" (pp. 304 ff.) and "afferent neural interactions" (pp. 349 ff.), and Tolman's (1948) "cognitive maps," would be referred to as hypothetical constructs. Importantly, intervening variables admitted no surplus meaning, whereas hypothetical constructs did (see also Turner, 1967, p. 259, who organizes some 40 references into five groups, each of which is associated with a slightly different way of characterizing the differences between intervening variables and hypothetical constructs). These moves were similar to those of Carnap (1936, 1937) in the decade before.

The impact of MacCorquodale and Meehl's distinction was dramatic. For example, Tolman (1949), who was as responsible as anyone for introducing theoretical terms to psychology in the early 1930s, immediately and explicitly abandoned his original intervening variable approach in favor of the hypothetical construct approach:

> 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 Meehl and MacCorquodale'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. (p. 49)

The result was that most psychologists came to employ hypothetical constructs rather than intervening variables in their theories.

Two quotes below from the recent literature suggest that this same approach to theorizing and the status of theoretical terms continues in contemporary psychology. The first is from 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:

Antecedent — Mentalistic — Behavior
Conditions Concepts
Independent — Intervening — Dependent
Variables Variables Variables

Obviously, there is nothing in this formula to exclude mentalistic concepts. In fact, the whole point of it is to admit unobservables. (p. 316)

The second is from Killeen (1987), who calls for an "emergent behaviorism": "Our behaviorism would be emergent because it recognizes the causal relevance of mental states, and thus utility of having theoretical terms within the system to refer to those states" (p. 231). Presumably, Kimble (1985) should have used "hypothetical constructs" instead of "intervening variables." Nevertheless, the two quotes express the same position: The work of behavioral scientists may be judged as scientific to the extent it uses theoretical terms such as hypothetical constructs, and it is acceptable for those terms to appeal unselfconsciously to conventionally accepted concepts, even if they are mentalistic, provided conventionally accepted safeguards are observed.

Of course, B. F. Skinner's radical behaviorism was evolving during the 1930s and 1940s, at about the same time as many of the original debates on operationism and the meaning of theoretical terms were playing out. Nevertheless, his position differed appreciably from that of mediational neobehaviorism and its tendency to retain conventionally accepted concepts (Day, 1969a; Moore, 1995). Accordingly, Skinner's behaviorism was distinctly unsympathetic with the debates:

> 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.... This was never good behaviorism.... The position is not genuinely operational because it shows an unwillingness to abandon fictions. (Skinner, 1945, pp. 292-293)

Clearly, Skinner rejected the mainstream view of scientific epistemology and its associated views on scientific verbal behavior and scientific theories. In their place he advanced his own views (e.g., Skinner, 1950), which continue to be the subject of ongoing controversy (Catania & Harnad, 1988).

The traditional approach to doing science

The foregoing is an admittedly brief history of theorizing and the interpretation of theoretical concepts during the first half of the century. Given this history, we may identify six principles of what has now emerged as a traditional approach to doing science, particularly as it concerns theories and theoretical concepts in psychology.

1. Theories are the ultimate objective of science. A theory may be regarded as a set of propositions concerning some natural phenomena and consisting of symbolic representations of (a) observed relations among observed events, (b) observed mechanisms and structures underlying observed relations, and (c) inferred underlying mechanisms and structures intended to account for observed relations (e.g., Marx, 1976, p. 237). Theories have two interrelated functions. The first is to explain the phenomena at hand. The second is to provide the basis for making predictions about the phenomena. Theories explain by one of two strategies: by instantiation or by higher-order deductions (Turner, 1967, p. 178). According to the first strategy, a particular event is said to be explained when it can be expressed as some value of a variable in a more general theoretical proposition. According to the second strategy, a particular event is said to be explained when it can be expressed as the conclusion of a logical argument. One premise of the argument is a statement of relevant conditions antedating the phenomenon in question. The other premise is a general "covering law," which may itself be an actual law, a lawlike generalization, or even a hypothesis within a theory. The second function of theories is to provide the basis for making predictions about those phenomena, according to the form of the logical argument described above. In sum, theories are the principal vehicle of scientific knowledge, and are involved in virtually all aspects of the scientific enterprise.

2. Theories have elements that refer to both observable and unobservable phenomena. For example, chemists and physicists postulated atoms, electrons, neutrons, and quarks without anyone ever having observed these phenomena. The same is true for biologists, who postulated genes and receptor sites. The history of the sciences suggests that progress depends on postulating unobserved phenomena, and theories should not be restricted to only observable phenomena.

3. Theories are necessary for explaining events in two ways:

(a) They cannot be avoided in practice. According to this argument, one must engage in theoretical activity if one is to truly explain any phenomenon. Anything else is merely description.

(b) Theories are uniquely appropriate to the logical processes by which knowledge is acquired. This argument is similar to (a) above, except that this argument concerns assumptions about what knowledge is, and how scientists become knowledgeable, rather than the subject matter.

4. Theories should be evaluated according to the following criteria (e.g., Estes et al., 1954, pp. xiii-xv; Marx, 1976, pp. 249-252):

(a) Testability: Can data inconsistent with the theory can be specified a priori, and does a method exist for checking the theory against these data? A theory that cannot specify the data and the method by which they are to be checked is regarded as inadequate in principle.

(b) Validity: Does the theory do an acceptable job of accounting for the phenomenon in question? One theory is regarded as better than another if it provides a better (e.g., more valid, better organized, internally consistent) accounting for the phenomenon. Indeed, scientific activity that is not coordinated with a theory is deemed to be risky at best, or bankrupt otherwise, because the activity is not tied together by a framework that effectively organizes it.

(c) Utility: Does the theory synthesize a large number of findings? One theory is regarded as better than another if it synthesizes a larger number of findings.

(d) Parsimony: Is the theory simple? One theory is regarded as better than another if it is simpler.

(e) Heuristic value: Does the theory suggest new lines of research? One theory is regarded as better than another if it suggests more new lines, with more novel predictions.

5. Psychology should formulate theories, and these theories should have elements that refer to unobservable phenomena. As noted above, scientific progress appears to depend on the postulational nature of the enterprise, and psychology should not be restricted in this regard. As Killeen (1988) puts it, "We should learn from the history of the more advanced sciences" (p. 330).

6. Technically, the elements that refer to unobservable phenomena should be regarded as hypothetical constructs, rather than intervening variables. Hypothetical constructs are an indispensable component of doing science. They provide a means of doing science that is not otherwise available, and the sciences that have made extensive use of them are the ones that have advanced. The review at the outset of this chapter notes their historical importance in

psychology. Three arguments in favor of using hypothetical constructs in psychology may be summarized as follows (e.g., Zuriff, 1985, chapter 4):

(a) They simplify the subject matter under consideration (McGuire, 1969; Miller, 1959). As Zuriff (1985, p. 70) explains, suppose that the theoretical term in question relates m independent variables to n dependent variables. Without the theoretical term, $m \times n$ different equations are required. With the theoretical term, only $m + n$ different equations are required. Hence, according to this argument, the use of the theoretical term reduces the number of equations that are necessary. [Of course, the simplification may be more apparent than real. Readers may recall Hempel's (1958) "theoretician's dilemma," as well as the possibility that adding another level to the equation may actually increase its complexity, rather than decrease it.]

(b) They are heuristics. According to this argument, they organize, integrate, and direct both current and future research. This argument suggests that an examination of the history of science reveals many cases where new findings have followed the postulation of various constructs, and where new lines of research have developed.

(c) They mediate causality. According to this argument, the constructs bridge spatial and temporal gaps between independent and dependent variables, thereby achieving better explanations.

The current literature contains many examples of this traditional approach to doing science. For example, consider the three quotes below. The first is from Williams (1986), who advocates the consideration of unobservables as hypothetical constructs and contends that

> all theoretical terms, including those commonly used by radical behaviorists who believe themselves to be following Skinner's positivistic dicta, involve the postulation of unobservable entities or processes as causes of behavior. In other words, theory construction inherently entails conjectures about a level of reality not available for direct empirical observation. (p. 112)

The second is from Killeen (1995), who addresses the synthesizing function of theories and hypothetical constructs:

> Theoretical constructs are as necessary for a science of behavior as they are for any other science (Williams, 1986); this was recognized by Skinner throughout his career, beginning with his argument for the generic nature of the concepts of stimulus and response (Skinner, 1935), through his defense of drive as a construct that can make a theory of behavior more parsimonious overall (Skinner, 1938), to his final writings. The issue, as Skinner and others

> (Feigl, 1950; Meehl, 1995) have stated, is not whether such constructs are hypothetical, but whether they pay their way in the cost-benefit ratio of constructs to predictions. (p. 407)

The third is from Amsel (1989), who emphasizes unobservables, parsimony, and the validity of theoretical constructs in neobehaviorism:

> Killeen [1987] writes, "We should study behavior, but we should also study what goes on inside organisms" (p. 225). As to what goes on inside organisms, Killeen agrees with Zuriff (1976, p. 51) that: "The decision as to whether to limit the psychological theory to stimulus-response relationships ... or to admit non-behavioral theoretical terms would seem to depend only on the heuristic values of the two approaches." These two observers are, then, in agreement that it is time for followers of Skinner to abandon his rigid prohibition against explanation in terms of empirical constructs. But in the context of this conversion experience, Killeen writes as though the constructs ... representing "what goes on inside organisms," the "nonbehavioral theoretical terms" of Zuriff, were not already a part of other behaviorisms. It has never been debatable—certainly not among neobehaviorists—that explanations should involve constructs, and that explanatory success (and I would add economy of explanation) should be the coin of the realm of theories of behavior. 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." .... [Misconceptions of neobehaviorism fail] to understand that such theoretical terms, be they behavioristic or cognitive in the recent sense, [should] be judged less by any surplus meaning they may appear to have, and more by the rigor of their definition and their significance—by their success in organizing that segment of the describable world they set out to organize. (p. 50, 59)

Leaving aside for the moment the question of whether Skinner's views are appropriately represented in Amsel's and Killeen's quotes above, the quotes clearly illustrate the traditional approach to doing science, as described above.

Considerations in evaluating the traditional approach to doing science: theories of truth

What kinds of considerations are relevant in evaluating this traditional approach to doing science? Let us begin by considering theories of truth.

Generally, analyses of epistemology recognize three theories of truth. The first is the pragmatic theory. The second and third are the correspondence theory and the coherence theory.

Pragmatism

Pragmatism holds that the truth of a statement is a matter of how well the statement promotes effective action. Pragmatism is an influential, distinctly American orientation, having its background in the philosophical pragmatism of John Dewey, William James, and C. S. Peirce. As Morris (1988) notes, it is also an aspect of Stephen Pepper's contextualism. B. F. Skinner (1974) embraced pragmatism as follows: "[A] proposition is true to the extent that with its help the listener responds effectively to the situation it describes" (p. 235). The relation between pragmatism and Skinner's radical behaviorism is reviewed in Day (1983), Hayes and Brownstein (1986), and Zuriff (1985). Pragmatic concerns are evident in the traditional approach to doing science described above (e.g., among the criteria used to evaluate theories in principle #4 above, as well as among the arguments in favor of hypothetical constructs in principle #6 above). They are also evident in the quotes above from Amsel (1989) and Killeen (1995).

Correspondence Theory and Coherence Theory

As stated earlier, the other two theories of truth are the correspondence theory and the coherence theory. We can mention them here for the sake of completeness, although we are not concerned with formally evaluating them. The correspondence theory holds that the truth of a statement is a matter of how well the statement agrees with some fact. Correspondence theory is derived from logical atomism, which was a precursor to logical positivism (Turner, 1967, pp. 81-103). A statement is regarded as true to the extent it reflects the structure of the world at large.

The coherence theory holds that the truth of a statement is a matter of how well the statement agrees with other statements in a system of statements. It was the position eventually endorsed by the logical positivists, who rejected correspondence theory because of difficulties with verifying statements about past personal experiences. In the end, the logical positivists argued that truth can only be regarded as a matter of establishing agreement between (a) verifiable statements about current states of affairs and (b) verifiable statements about past states of affairs. Thus, the logical positivists could be said to have argued that coherence theory is really an improved version of correspondence theory.

Considerations in evaluating the traditional approach to doing science: interpretations of scientific theories

Interpretations of scientific theories are also relevant in evaluating the traditional approach to doing science. Generally, analyses of scientific epistemology recognize two interpretations of scientific theories. The first is instrumentalism, sometimes also called conventionalism. The second is realism, sometimes also called essentialism.

Instrumentalism

According to instrumentalism, a theory is an instrument that enables scientists to derive statements about observables from other statements about observables (Suppe, 1977, pp. 29, 167). Any theoretical concepts included in a theory need not be construed as referring to phenomena that actually exist. Theoretical concepts are devices that are conventionally accepted for the purposes of prediction and explanation. Anything that mediates accurate predictions is acceptable. For example, "as if" psychology, where the theory supposes that it is "as if" some proposed entity has a particular set of properties, is entirely acceptable because no commitment is made to the actual existence of the entity involved. The goal of science is to generate new concepts that mediate prediction and explanation. Given MacCorquodale and Meehl's (1948) distinction between intervening variables and hypothetical constructs, instrumentalism interprets theoretical terms as intervening variables.

On this view, the important question about a theory concerns its range of application, rather than whether it is true or false in the way an empirical proposition is true or false. A theory is a statement of properties and relations pertaining to a given set of events. Just what the set of events is, and just what is the range of events over which the theory applies, are to be decided on the basis of empirical evidence. Statements about the range of a theory's application can be true or false, but not the theory itself (e.g., Toulmin, 1953; Turner, 1967, p. 251).

Realism

The second interpretation of theories is realism, or essentialism. Originally, the realists were scholastic philosophers who, quibbling over the exegesis of Aristotle, held that categories are defined by essential properties that transcend

specific instances of the categories. For example, the category of white things is defined as those elements that possess the property of "whiteness." Whiteness is an essence, a "thing" that is known in its own right through experience with white objects. Instances within a category might vary widely, but they were all seen as variants of a single template. Individual variability is explained as the outcome of less fundamental factors—accident, random processes, or other vicissitudes. The origin of the template itself is generally unexplained. The position can be traced back, in some form, to Plato and Parmenides (Palmer & Donahoe, 1992, p. 1345).

The realist interpretation of theories has three premises: (a) the aim of science is to find a true theory or description of the world (and especially of its regularities or "laws"), which shall also be regarded as an explanation of the observable facts; (b) science can succeed in finally establishing the truth of such theories beyond all reasonable doubt; and (c) the best and truly scientific theories describe the "essential properties" of things—the realities which lie beyond appearances; such theories are *ultimate explanations*, and to find them is the ultimate aim of the scientist (Suppe, 1977, p. 168).

The realist thinks that the goal of science is to discover new concepts that represent real objects in nature. On this view, any theoretical concepts included in a theory are construed as real entities that actually exist in the world at large and that may be observed either directly or indirectly through their effects (for a brief discussion of "existence claims," see Suppe, 1977, pp. 566-570). Given MacCorquodale and Meehl's (1948) distinction between intervening variables and hypothetical constructs, realism interprets theoretical concepts as hypothetical constructs.

An alternative perspective on scientific epistemology based on Skinner's radical behaviorism

The six principles outlined earlier in connection with the traditional approach to doing science, as well as the quotes by Amsel and Killeen illustrating that approach, reflect aspects of *both* the instrumental and realist views of scientific theories. Hardly any contemporary theorist adopts a purely instrumental or purely realist view. Instrumentalist aspects are shown in Amsel's (1989) statements that the basis for evaluating theoretical concepts is whether they serve to organize the existing data and mediate accurate predictions, rather than whether they actually exist. Pragmatic concerns are often intermingled here

with instrumentalism. Realist aspects are shown in Killeen's (1987) statements that the theoretical concepts are to be regarded as hypothetical constructs, which do have some basis in reality, and that that reality happens to be mental.

An alternative perspective based on Skinner's radical behaviorism views matters altogether differently. An alternative perspective seeks to address the assumptions underlying assertions that pragmatic considerations justify the use of hypothetical constructs and the disregard of questions of ontology. Presumably, the hypothetical construct is not useful because it affords some unique logico-theoretical insight into the structure of nature. That kind of insight doesn't exist for anybody, including those who appeal to hypothetical constructs in explanations. Similarly, the hypothetical construct isn't useful because it correctly taps into the underlying epistemological processes of the scientist. Those processes don't exist either, especially among those who appeal to hypothetical constructs in explanations, despite their statements to the contrary. As Skinner (1969) put it, "The hypothetico-deductive method and the mystery which surrounds it have been perhaps most harmful in misrepresenting ways in which people think" (p. x).

Instrumentalism and the use of theoretical concepts are often justified by arguments that knowledge of any sort is actually derived from the manipulation of subjective/cognitive entities in the theorist's mental world, apart from the theorist's behavioral world. Therefore, employing fictions is perfectly acceptable; fictions are all there are for anyone to employ anyway. That is what human mind creates when it tries to become knowledgeable (e.g., "[O]ne of the components of theory is the generation of useful fictions. That's what theories are about", George Mandler in Baars, 1986, p. 255).

The point here is that both instrumentalist and realist interpretations of theories and theoretical concepts entail a tacit commitment to a reference theory of language and a correspondence theory of truth. The treatments assume that a theoretical term is a thing that must refer or correspond to another thing. Indeed, it represents a return to a kind of logical atomism. Because scientists can invoke a term, there must be an entity at another level of reality for which the term stands, and the job of the scientist is to find out about it. In one sense, there is hardly any distinction between instrumentalism and realism, despite what is conventionally regarded as an adequate means for distinguishing between them.

Moreover, the traditional approach to doing science is inherently representational, rather than presentational. Representationalism holds that immediately given phenomena are representative of events or processes taking place at

some other level or in some other dimensional system. The truly important realities of nature are to be found at this deeper level. The theory is the essential device for gaining access to the deeper level. In contrast, presentationalism holds that immediately given phenomena need to be addressed and understood at their own level, just as one encounters them (Schnaitter, 1986a, p. 300). They are not simply tokens of another level of reality.

On the basis of the alternative perspective presented here (which we argue is actually the appropriate interpretation of Skinner's position; cf. Killeen, 1987, 1988, 1995, and Williams, 1986), pragmatism can and must be dissociated from instrumentalist justifications of certain kinds of theories as well as from realism and the use of hypothetical constructs. The important question about pragmatism is as follows: If a theoretical term promotes effective action, such as prediction and control, why does it do so? The answer does not have anything to do with the logical status of the theoretical term, or with any of the other issues raised by the traditional approach. Scientific language, theoretical terms, and explanation are not essentially logical phenomena. They are verbal phenomena. As Skinner (1945) said, "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).

Because scientific language, theoretical terms, and explanation are verbal phenomena, the answer has to do with the contingencies that are involved in the verbal processes in question. For example, one may meaningfully ask, "In what sense is the 'theoretical term' an abstraction, an extended tact, or a constructed tact?" The fundamental question is, "What is the stimulus control over the term, as an instance of verbal behavior?" The issue is not simply the percent of variance accounted for in a covering law explanation. The issue is that if a given theoretical term aids the scientific endeavor, it does so because of the discriminative repertoires involved. That is a matter of identifying the stimulus control involved (a) in the origin of the term, as an instance of verbal behavior, and (b) in the application of the term among the scientists for whom it facilitates effective action in the world at large.

Logical positivists distinguished between the context of discovery and the context of justification. The context of discovery was a matter for the psychology, sociology, or history of science, as members of those disciplines sought to examine the source of ideas. However, logical positivist philosophers thought such enterprises did not contribute to an understanding of the meaningfulness

of scientific statements. Only a logical analysis of the context of justification was relevant to the understanding of science (Suppe, 1977, p. 524).

In traditional terms, given the distinction between the context of discovery and the context of justification, a significant question is whether the context of discovery can be so lightly dismissed. Some factors exert control over the verbal behavior; what are they? Fictions do not exist for anybody, and verbal behavior does not create the fictitious phenomena spoken about. Thus, fictions cannot be the things that exert discriminative control over effective verbal or nonverbal behavior. What are the factors in question? It is those factors that will ultimately be involved in prediction and control, and they need to be clearly identified for the scientific statements to be effective.

Several passages in the literature of radical behaviorism illustrate this position. For example, consider the following two quotes from Skinner's own writing:

> We may quarrel with any analysis which appeals to ... an inner determiner of action, but the facts which have been represented with such devices cannot be ignored. (Skinner, 1953, p. 284)
>
> No entity or process which has any useful explanatory force is to be rejected on the ground that it is subjective or mental. The data which have made it important must, however, be studied and formulated in effective ways. (Skinner, 1964, p. 96)

Similarly, consider the following passage from Schoenfeld (1969):

> Current emphasis upon deductive elaborations in psychology proceeds from the comforting, but I think mistaken, belief that the physical sciences owe their modern pragmatic successes to their constructional theoretical systems.... What is not pointed out is the companion difficulty of deductive practice when it is described this way, namely, to say where the axioms or postulates come from in the first place. To reject this question as irrelevant or *ad hominem*, and to argue that only the ultimate correctness of the postulates is of interest, is to deny that human behavior is involved. It puts the origin of postulates into the sphere of disembodied whimsicality and mentalism, and thereby makes it impossible to instruct anyone in how to go about the business of doing science.... It is not the form of the proposition that is at issue, but how the proposition has been arrived at. The inductive generalization openly declares itself to be based on previously ascertained facts, even if particular ones. But where does the postulate come from? It is plain silly to imagine any rational scientist actually doing what some have claimed he does or should do, or what he is praised for doing as a "deductivist-constructivist": that is, close his eyes and reach into a grab bag of possible postulates, come up with whatever ones he chances upon, explore their logical consequences, put those logical consequences to experimental test, and then, if necessary, revise those postulates or go back to the grab bag for others.... That position, literally interpreted, not only removes the choice of postulates from connection with established knowledge, but it gives the fool equal rights

> with the scientist in the choice; it means that we yield any hope of acquiring new knowledge, since the chances of pulling a "good" postulate are vanishingly small because the contents of the grab bag are infinite in number; it means that even "good" postulates, being sentences of infinite length, are doomed to be wrong when endlessly tested against an infinite world; it means that our purpose becomes one of proving propositions right or wrong, rather than of learning something about the world; and so on. Into blind alleys of this sort are we led by a defense of the disembodied origin of the postulate. This remoteness of origins and sources, their divorce from actual human behavior, is intended to give postulates some unassailable rational status. But the intention does not square with reason, not will it succeed in practice. (pp. 337-338)

The difficult question, of course, is whether psychologists will find it worthwhile to continually assess stimulus control over the mentalistic language found in most instrumentalist or realist theories involving hypothetical constructs. Could not the time given to this task be better used by simply attempting to move forward on one's own, and by attempting to discover new facts and relations? The question is not any easy one. From the standpoint of Skinner's radical behaviorism, scientific language is usually under multiple control of both (a) operations and contacts with data and (b) social/cultural traditions (Moore, 1981, p. 61). Thus, despite its inclinations, even the most mentalistic sounding theory might contain something of value. The value would derive from the theory's implicit contact with operations and data, rather than its contact with social/cultural traditions. On the one hand, if the psychologist entertains the mentalistic theory, the psychologist runs the risk of finding out later that time and resources have been wasted by entertaining something trivial at best. On the other hand, if the psychologist rejects the mentalistic theory, the psychologist runs the risk of missing something of genuine value. Skinner (1969, pp. 93-94) suggests that an emphasis on basic dimensions will help in making such decisions. Graphs in the research related to the theory should not ordinarily show changes in behavior from trial to trial, in terms of time or number of errors required to reach a criterion, or in terms of amount remembered. In addition, dimensions are probably suspect if the work was done with mazes, jumping stands, or memory drums. Perhaps the choice will also involve the "track record" of individual scientists in individual laboratories.

Caveat emptor: the case of phlogiston

In the final analysis, no science has ever operated according to the picture presented by those who advocate hypothetical constructs, despite the claims that all sciences have used them. For one thing, language is simply not the symbolic, logical activity implied by those who advocate using hypothetical constructs. When hypothetical constructs have proved useful, they have done so because they are extended tacts and abstractions, rather than because of their status as logical entities that capture an essential property.

For another thing, history suggests that sciences have not simply retained a "construct" simply because it accounted for an agreeably high percentage of the variance. If so, then science would have stayed with vis anima, caloric, ether, and so on. The story of the phlogiston theory of combustion in chemistry is a representative case history (Read, 1961, pp. 117-144).

In the 1600s, chemistry as it then existed owed considerable allegiance to Aristotle: The world was presumed to consist of four types of substances—air, fire, water, and earth. The study of combustion was a particularly conspicuous demonstration of these phenomena. When a piece of wood burns, air is a necessity, fire is produced, water is a byproduct, and ashes (earth) are left. Indeed, animal life was presumed to depend on some sort of combustion, as animals' metabolic processes produced heat inside their bodies and liberated the energy that made life possible. Fecal waste products were the remainder, as in the colloquialism "night soil."

The theory of phlogiston developed as a theory of combustion in this context. Johann Becher (1635-1682) originated the theory in 1669, when he proposed that metals contained an inflammable principle, which is released into the air by the fire. A pupil and successor, Georg Ernest Stahl (1660-1734), advanced further details of this process in 1697, and named the essence "phlogiston," derived from the Greek term for burnt, or inflammable. Joseph Priestly (1733-1804), an otherwise brilliant English amateur of science, was ironically a dedicated phlogistonist and tried to sustain the theory into the latter quarter of the 18th century, even though his work with oxygen laid the foundation for its eventual overthrow.

According to the theory, phlogiston was a substance that was contained in materials in their natural state. When the materials were heated, they released their phlogiston to the outside air, thereby accounting for combustion. Air made no direct contribution to combustion, other than as a vehicle for carrying away the phlogiston. It was simply an agent or a catalyst. Prior to being burned,

substances were regarded as compounds of the essential material (e.g., a vitrifiable earth) plus phlogiston. The "pure" substance remained after combustion.

The theory attempted to make sense of a number of observations. Casual observation of a burning piece of wood suggested that the flames reached upward, in a metaphorical pose hinting at the release of vapor from the wood. In addition, substances typically weighed less after combustion. The loss was attributed to the loss of its phlogiston, rather than say a loss of water vapor. If a substance was burned in an environment that we would now recognize as oxygen (although oxygen wasn't recognized as an element at the time), the substance would burn exceedingly well. The phlogistonists referred to the oxygen (again, they didn't know what the gas was) as "dephlogisticated air." They believed that such air was exceedingly devoid of any phlogiston, and would readily absorb any phlogiston that was liberated from the burning substance. The phlogistonists did not regard oxygen as being added to the burned substance; they did not recognize the role of oxygen in combustion, or that the process of combustion actually forms an oxide, rather than the pure form of the substance.

If someone tried to burn a substance in a confined environment in which something had already been burned and had gone out, the substance would obviously not burn. We would now recognize the air in the confined environment as consisting primarily of carbon dioxide and nitrogen (although the phlogistonists did not recognized air as being composed of these gases). The phlogistonists referred to the carbon dioxide as "fixed air," and the nitrogen as "spent air" or "phlogisticated air," meaning that it was already highly saturated with phlogiston, and would not absorb any more from the substance, thereby inhibiting any further combustion.

Even at the time the theory was proposed, two problems were apparent. The first was that the weight of metals (e.g., tin) increased when heated, rather than decreased. The phlogistonists dealt this problem by adopting various ad hoc hypotheses. Sometimes, for example, they held that the phlogiston in these elements had a negative weight. Prior to combustion, the metal possessed the phlogiston, and was lighter. When the phlogiston was released during combustion, the substance actually became heavier. Other times they held that phlogiston affected the density of substances, and that the lighter weight after combustion was simply an artifact of changes in density. At still other times they regarded the fire material as passing through the glass of the container as the material was being heated, thereby adding to the weight of the material.

The second problem was that a confined volume of air contracts when a substance is burned in it, rather than expands, as would be required by the

addition of phlogiston to the surrounding environment. The phlogistonists dealt with this problem by holding that sometimes phlogiston had a negative volume. When released into the confined air during combustion, the air would decrease in volume because of this property.

Despite the ad hoc nature of the phlogiston theory of combustion and the added hypotheses, the theory remained dominant in the field of chemistry until the end of the 18th century. What was lacking until that time was a comprehensive approach to understanding gases, in particular the gases that made up the air. The theory began to run into real trouble when Henry Cavendish (1731-1810), the millionaire grandson of an English duke, showed in the early 1780s that water could be produced by passing an electric spark through a container filled with oxygen and hydrogen. The volumes of gases involved revealed that the process was an orderly one, and suggested the integral role of gases such as oxygen. Undaunted by contrary data, some dedicated defenders of phlogiston theory seized on these results and suggested that electricity was actually phlogiston. Others thought hydrogen was.

Ultimately, Antoine Lavoisier (1734-1794), a brilliant Frenchman, administered the coup de grace to phlogiston theory. In a simple experiment, Lavoisier first heated liquid mercury in a closed vessel until a red powder, which we now recognize as an oxide of mercury, formed on its surface. He then took advantage of the chemical properties of this particular compound by heating the oxide still further. This second heating liberated the oxygen that had formed the oxide in the first heating. Measurement showed that the same volume of gas was involved. Thus, Lavoisier was able to demonstrate that a gas was either added or subtracted from the substance, and that no such phenomenon as phlogiston was liberated during combustion. For these and other demonstrations, Lavoisier is regarded as one of the founders of modern chemistry, although he was unfortunately a victim of the French revolution. He was executed in 1794, having been found guilty of adding water and other ingredients detrimental to the health of citizens to tobacco.

In sum, the actual progress of science is a function of conditions that differ appreciably from those portrayed by those who advocate instrumentalism as well as realism and the use of hypothetical constructs (cf. Amsel, 1989; Killeen, 1995; Williams, 1986). The argument that other sciences have used them to good advantage, so psychology should too, is bogus. The key lies in an understanding of verbal behavior and the theoretical stance that follows from that understanding. Let us now examine these two issues.

The basis of radical behaviorist epistemology: its conception of verbal behavior

Perhaps the greatest differences between the alternative perspective, presented here, and the traditional approach to doing science lie in the respective conceptions of verbal behavior. For the traditional approach, the distinction between observational and theoretical terms rests ultimately on a reference theory of meaning and the view of verbal behavior as a logical phenomenon. An observational term is taken to refer to some entity or attribute that is publicly observable, by virtue of its primary qualities. However, individuals obviously speak about other qualities and attributes of their environment. What is the referent of these qualities and attributes? According to the traditional view, the referent must be something internal and unobservable, constructed by and existing solely for the individual in question. Because the referent is created internally, it is designated as a "theoretical term." The meaning of a theoretical term must then be established through operational definition and logical analysis. Worth noting is that this whole position is predicated on the notion that words are things that refer to other things. If those other things are not in the intersubjectively verifiable world, then the other things must be creations of the speaking individual, in the "subjective" world of that individual. The representational problems with mentalism and epistemological dualism, in which two dimensions are assumed certainly in the knower if not also the known (Moore, 1995b, pp. 65-67), are extensive here.

For the alternative perspective, the important concerns are the contingencies involved with the verbal behavior in question. One set of contingencies is involved with the speaker, and the production of the verbal behavior in question. A second set of contingencies is involved with the listener, and with the contribution of the verbal behavior in question to discriminative control over the listener's behavior. Some factors that enter into these contingencies are troublesome. For example, from the radical behaviorist perspective, much verbal behavior that is called "theoretical" in traditional psychology is derived from a set of social/cultural assumptions about the subject matter and methods of psychology that are not related in any significant way to the relation between behavior and the circumstances in which it occurs (Moore, 1981, 1990). 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 identities. Speakers then assume they have correctly isolated the things talked about. At best, such activity only illustrates the "Formalistic Fallacy" (Skinner, 1969, p. 265).

Other 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). The argument is that we must strip away the intraverbal contribution derived from ideas that are cherished for extraneous and irrelevant reasons, such as social/cultural preconceptions about the phenomenon itself or about what is necessary to make oneself knowledgeable.

The magnitude of the distinction between the traditional approach and the alternative perspective is not often appreciated. At issue is whether scientific terms are to be regarded as things that stand for, symbolize, or refer to objects either in the environment or in some "subjective" dimension unique to the scientist. According to the alternative perspective, a given instance of verbal behavior may be under the discriminative control of an object, but no scientific term is a thing or construct that stands for, symbolizes, or refers to another thing in any dimension. By all rights, the statement that a scientific term is a construct that symbolizes or refers to an another thing ought to be just as odd as the statement that a pigeon's key peck to a lighted response key is a construct that stands for or refers to the light; that the statement perhaps does not sound as odd is ample testimony to the pervasiveness of nonbehavioral approaches to verbal behavior. Skinner (1945) criticized the conception of verbal behavior as a symbolic, logical process as follows :

> Attempts to derive a symbolic function from the principle of conditioning ... have been characterized by a very superficial analysis.... Modern logic, as a formalization of 'real' languages, retains and extends this dualistic theory of meaning and can scarcely be appealed to by the psychologist who recognizes his own responsibility in giving an account of verbal behavior. (pp. 270-271).

Rather, a scientific term is simply an instance of behavior that is under the discriminative control of aspects of its antecedent setting, just as the pigeon's response is an instance of behavior that is under the discriminative control of aspects of its antecedent setting. The meaning of a scientific term for the speaker derives from the conditions that occasion its utterance. The meaning for the listener derives from the contingencies into which the term enters as a discriminative stimulus (Moore, 1995a). Importantly, the alternative perspec-

tive does not distinguish between observational and theoretical terms. Radical behaviorism is therefore not concerned with the difference between theoretical terms of any interpretation, such as whether a given term is an intervening variable or a hypothetical construct (Moore, 1992; cf. MacCorquodale & Meehl, 1948, and Zuriff, 1985). 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.

Skinner identified the importance of contingencies supporting the use of scientific terms, particularly the contingencies that involve a social/cultural dimension, when he talked about the origin of cognitive terms: "[T]he reasons for the popularity of cognitive psychology ... have nothing to do with scientific advances but rather with the release of the floodgates of mentalistic terms fed by the tributaries of philosophy, theology, history, letters, media, and worst of all, the English language" (Skinner in Catania & Harnad, 1988, p. 447). In fact, much of Skinner's later writing was concerned with elucidating the prevalence of this form of stimulus control over the verbal behavior called "cognitive" (e.g., Skinner, 1989, 1990). Thus, many of these terms are related to inappropriate metaphors, culturally established patterns of speech, and so on, none of which are appropriate from a strict scientific perspective (see also Hineline, 1984, p. 98; Marr, 1983, p. 12; Moore, 1983; Schnaitter, 1984, p. 7).

The theoretical stance of radical behaviorism

The alternative perspective based on Skinner's radical behaviorism is not opposed to theories in principle (despite claims by some critics that Skinner's behaviorism was "atheoretical" and merely "descriptive," some of which are presumably attributable to Mach's influence on Skinner; see discussion in Smith, 1986, pp. 264-275). As Skinner (1947/1972) once put it,

> the cataloguing of functional relationships is not enough. These are the basic facts of a science, but the accumulation of facts is not science itself. There are scientific handbooks containing hundreds of thousands of isolated facts—perhaps the most concentrated knowledge in existence—but these are not science. Physics is more than a collection of physical constants, just as chemistry is more than a statement of the properties of elements and compounds.... 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]with 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)

The pivotal issue is what is meant by the term "theory." On a Skinnerian view, theories are regarded as verbal behavior. They are occasioned by certain antecedent conditions, and reinforced by certain other conditions (Skinner, 1957, chapter 18). Zuriff (1985) suggests that a Skinnerian theory is

> a formulation using a minimal number of terms to represent a large number of experimental facts.... As the theory develops, it integrates more facts in increasingly more economical formulations. Theoretical concepts thus merely collate observations and do not refer to nonbehavioral processes. A Skinnerian theory is, therefore, a simple, comprehensive, and abstract description of a corpus of data. (p. 89)

Such theories function as a form of discriminative stimulation that guides future action through either (a) direct manipulation of environmental events or (b) correlated action when direct manipulation is not feasible, as in some cases of prediction and interpretation. Always at issue are the contingencies governing the verbal behavior regarded as "theoretical" (Moore, 1990, pp. 25 ff.).

To be rejected is the traditional, representationalist view of theories as formal statements that appeal to causal events and entities in other dimensions, with observational and theoretical terms, where the latter are operationally defined as hypothetical constructs (cf. Zuriff, 1985, chapters 4 and 5). In particular, to be rejected are the sorts of mentalistic theories that appeal to unobserved events and entities that are somewhere else, at some other level of observation, in a different dimension (e.g., a neural, psychic, "mental," subjective, conceptual, or hypothetical dimension), where those entities must be described in different terms (Skinner, 1950, p. 193). To be further rejected is the assumption that causal explanation in psychology, and scientific knowledge in general, consists in framing such theories with such representational constructs. Indeed, to be rejected is the assumption that scientific knowledge necessarily consists in the formulation of such theories. Such assumptions are but further illustrations of the same mentalistic problem.

Rather, a different kind of theory seems appropriate. Certainly such a theory would be more presentational than representational. This new kind of theory would have the following concerns: (a) What aspects of behavior are significant? (b) Of what variables are changes in these aspects a function? (c) How are the relations among behavior and its controlling variables to be

brought together in characterizing the organism as a system? (d) What methods are appropriate to the study of such a system experimentally? (e) Under what conditions does such an analysis yield a technology of behavior, and what issues arise in its applications? (f) Why do scientists examine and explore a given subject? (g) What rate of discovery will sustain their behavior in doing so? (h) What precurrent behaviors improve their chances of success and extend the adequacy and scope of their descriptions? (i) What precurrent behaviors interfere with their chances of success and restrict the adequacy and scope of their descriptions? (j) What steps do scientists take in moving from protocol to general statement?

An important component of the resulting theoretical verbal behavior will be "interpretive." Interpretations are the use of scientific terms and principles in talking about facts when too little is known to make prediction and control possible, or when precise manipulation is not feasible. Two examples of interpretation are (a) the theory of evolution and (b) the theory of plate tectonics. These theories are not philosophy. Rather, they are interpretations of a vast number of facts, in one case about species and in another about the state of the earth's crust, using terms and principles taken from much more accessible material and from experimental analyses and their technological applications. The basic principles of variation, selection, and retention can be studied in the laboratory under controlled conditions, but their role in explanations of the evolution of species is interpretation. Similarly, the basic principles governing the behavior of material under high pressure and high temperature can be studied in the laboratory under controlled conditions, but their role in explanations of the formation of surface features of the earth is interpretation (taken from Skinner in Catania & Harnad, 1988, pp. 207-208)

A variation on the usual criticism that the radical behaviorist approach is atheoretical is that despite what Skinner said, radical behaviorism is in fact "theoretical," but the theory is just not very good. On this view, Skinner's approach could be put in necessary order if radical behaviorists just recognized and paid more attention to the requirements of good theory. The comments above by Killeen (1987, 1995) and Williams (1986) illustrate this variation.

Some years ago, Day (1969a) remarked on the difficulty that some psychologists have in accepting Skinner's treatment. He provided a representative quote critical of Skinner, and then indicated that the quote

> misses its mark by speaking as if any specialized use of language in interpretation and explanation must be taken to be theoretical. Ontological properties are attributed not only

> to theory, presumably as distinguished from description, but also to such entities as logical reasoning and extrapolation, possibly taken either as mental processes or as *a priori* forms of knowing.... It is true that many scientists *think* they know what science is (to speak in terms of the name-relation), or *think* they know what is involved in legitimate scientific explanation and in the application of scientific method. However, their *thoughts* in this respect have still to be accounted for just as much as Skinner's even if the value of trusting such conventional views were not to be trusted.... To fail to view the problem of explanation in this fashion, as *inescapably* an empirical and behavioral problem, is perhaps to miss the force of what may well be Skinner's major contribution to psychological thought. (pp. 503-505)

Although Day was addressing a different matter, his remarks are as relevant now as when they were written, if comments by Killeen (1988, 1995) and Williams (1986) are any indication.

Verbal processes and the development of theories

To be sure, Skinner has noted that "the achievements of the hypothetico-deductive system, where appropriate, have been brilliant. Newton set the pattern in his *Principia*, and the great deductive theorists who follow him have been given a prominent place in the history of science" (Catania & Harnad, 1988, p. 102). Elsewhere, he noted that although neural, mental, and conceptual theories were objectionable, he did not seek "to show that any of these theories cannot be put in good scientific order, or that the events to which they refer may not actually occur or be studied by appropriate sciences. It would be foolhardy to deny the achievements of theories of this sort in the history of science" (Catania & Harnad, 1988, p. 89). At first blush, these statements seem at odds with much of the foregoing analysis. How can they be reconciled?

Skinner has espoused a view whereby much of the control over the sophisticated verbal behavior called theoretical develops over time as a result of interactions with the environment, just as does the control over any other kind of sophisticated verbal behavior. For example, Skinner (1953) wrote that science "is a search for order, for uniformities, for lawful relations among the events in nature. It begins, as we all begin, by observing single episodes, but it quickly passes on to the general rule, to scientific law.... 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 makes statements about statements" (pp. 13-14).

In a related piece, Skinner (1947/1972, pp. 305 ff.) more explicitly addressed the three important steps in the development of a theory. The first step is to identify the basic data (p. 305). He noted that this step is surprisingly difficult, and many sciences have started off on the wrong foot precisely because they have incorrectly identified their basic data.

The second step is to express uniform relations among the data (Skinner, 1947/1972, p. 307). The expression of these facts typically takes the form of the "laws" of the science.

The third step is to develop abstract concepts (Skinner, 1947/1972, p. 307). Skinner identified "acceleration" and "force" as examples. These concepts are something more than the second step laws from which they are derived. Importantly, these concepts "are peculiarly the product of theory-making in the best sense, and they cannot be arrived at through any other process" (Skinner, 1947/1972, p. 307). They help the scientific statement go beyond the expression of uniform relations by providing "a formal representation of the data reduced to a minimal number of terms" (Skinner, 1950, p. 216). They can even be conjoined with laws for ever more complex statements.

Skinner (1953, pp. 13-14) endorsed Mach's position that the first laws and theories of a science were probably rules developed by artisans who worked in a given area. As these individuals interacted with nature, they developed skilled repertoires. Descriptions of the effects brought about by relevant practices were then codified in the form of verbal stimuli. The verbal statements, often taking the form of maxims or other informal expressions (e.g., "rules"), supplemented or replaced private forms of stimulus control. The verbal stimuli became public property, and were transmitted as part of the culture, enabling others to behave effectively. Many scientific laws and theories therefore have the character of statements that specify or imply responses and their consequences. Scientific laws and theories are not statements that are obeyed by nature. Rather, scientific laws and theories are statements that exert discriminative control over individuals who need to deal effectively with nature. On this view, the formula $s = 1/2\ gt^2$ does not govern the behavior of falling bodies in nature. Rather, it is a rule that governs predictions made by individuals who are concerned about the speed of a falling body at a given time (Skinner, 1969, pp. 138-142).

The objection is that many current "theoretical" positions in psychology have not gone through anything remotely resembling a developmental process, three steps or otherwise. The theoretical positions have simply appeared at what might be regarded as the advanced stage (usually by the theorists themselves), but without having adequately gone through the necessary earlier steps. As a

result, the stimulus control over what are hailed as advanced activities is suspect. The verbal responses themselves are controlled by factors that are cherished for irrelevant and extraneous reasons. They are the product of many mentalistic or dualistic factors, of metaphorical extensions, and so on. The necessary base needs to be established before useful third-step concepts will appear, and psychology is so mentalistic that it has not gone through the necessary prior steps to establish that base.

In acknowledging the theoretical contributions of those who are said to have followed the hypothetico-deductive approach, Skinner pointed out that the theorists would not have gone far without a foundation of basic experimental findings. Newton feigned no hypotheses about the existence of fictitious entities in his mechanical universe (even though he was ultimately concerned with illustrating how the laws established by his Christian deity were expressed in that universe). The important point is that Newton's verbal behavior was enough under the stimulus control of step 1 and step 2 events that an adequate foundation existed. Similarly, in the development of quantum mechanics and relativity theory in physics, Boltzmann, Planck, Einstein, Bohr, and Born theorized about events taking place somewhere else, at some other level of observation, described in different terms, and definitely measured in different dimensions from the experimental set up. Again, the important point is that such theorists would not have been able to emit their verbal behavior if the relevant foundation had not been established earlier.

To be sure, Skinner's own words on this topic are sometimes equivocal. For example, in material immediately preceding his acknowledgment of Newton's deductive contributions, Skinner (1969) stated that

> When a subject matter is very large (for example, the universe as a whole) or very small (for example, subatomic particles) or for any reason inaccessible, we cannot manipulate variables or observe effects as we should like to do. We therefore make tentative or hypothetical statements about them, deduce theorems which refer to accessible states of affairs, and by checking the theorems confirm or refute our hypotheses. (p. 102)

In another place, Skinner stated that behavior "is not one of the subject matters (like the very small or the very large) requiring theory" (Catania & Harnad, 1988, p. 469). Similarly, in material immediately following his acknowledgment of Newton's deductive contributions, Skinner (1969) argued that

> Behavior is one of those subject matters which do not call for hypothetico-deductive methods. Both behavior itself and most of the variables of which it is a function are usually conspicuous. (p. 103)

Such statements seem to imply that Skinner's fundamental concern is purely with the public observability of the subject matter. The statements seem to concede that hypothetico-deductive methods as conventionally understood are called for in some cases, such as when the subject matter is not publicly observable.

The problem is that critics find support in such statements, in which Skinner appears to surrender his point on a technicality. Critics need only say that either (a) their subject matter is unobservable (e.g., cognitive processes), rather than publicly observable (e.g., behavior), and that they are actually investigating them in accordance with Skinner's suggestions, or (b) that Skinner is being inconsistent and that he believes in "theories" after all, albeit not very sophisticated ones (e.g., Killeen, 1988, 1995; Williams, 1986).

The resolution is to recognize that the fundamental concern resides in the nature of the stimulus control over the verbal behavior in question. Probably there is multiple control over the behavior. As Moore (1981) noted, how much of the stimulus control derives from experimental operations and contact with data, relative to that which derives from metaphors and social/cultural factors that are cherished for irrelevant and extraneous reasons? For the most part, the experimental operations and contacts with data are publicly observable, and the fundamental concern is with the stimulus control they exert, relative to the other sources of stimulus control. Kantor (1938) characterized the problem as follows:

> Construction begins with elementary reference.... We may, however, demand that all constructions be connected with the primary data or events by a substantial link of observation and observational procedure.... The exigencies of scientific work may be such as to attenuate the thread binding the construction with events to a very thin calibre.... But it is an established maxim that this thread can never be broken. When the ratio of construction to observation is very large we may still regard the speculative construction as scientific, but when the observational factor is zero we have no other alternative than to characterize the speculation as unscientific or non-scientific. (pp. 11-12)

Kantor (1945) continued on this same theme by noting that "The lesson is plain—namely, by the frail process of language manipulation ideas are established with which no scientific enterprise is in any manner concerned", to which he added the following footnote: "Excepting, of course, the psychological and cultural investigation of the origin and maintenance of such theories" (p. 148).

Schnaitter (1986b, p. 262) has suggested Skinner implicitly rejected theories appealing to hypothetical entities on the basis of two criteria:

1. The criterion of intraverbal distance. According to this criterion, as the amount of verbal behavior intermediating between the world and a conclusion about the world increases, the opportunity for a faulty inference increases.

2. The criterion of metaphoric contamination. According to this criterion, metaphoric extension invariably leads to misdirection and conceptual error in science.

Again, according to the present interpretation, these two criteria apply to ostensible third-step entities that have been proposed without the benefit of the first two steps. In these instances, the necessary stimulus control may be lacking, and the resulting scientific product is suspect. To be sure, verbal manipulation can generate supplemental stimulus control in efforts to derive more reinforcers from nature. However, such manipulations would be regarded in terms of the stimulus control over verbal behavior (Skinner, 1957, chapter 18), rather than in terms of nomological networks and hypothetical constructs. Moreover, one must be cautious about attempts to manipulate the verbal behavior prematurely, without establishing an adequate foundation in the first two steps. As suggested by the story of phlogiston, sciences are vulnerable to such premature leaps. Given its "vast vocabulary of ancient and non-scientific origin" (Skinner, 1945, p. 271), psychology is no exception.

Summary and conclusions

An alternative perspective of scientific epistemology emphasizes the importance of the contingencies associated with scientific terms. On the one hand, there are matters of stimulus control over the production of scientific verbal behavior. On the other hand, there are matters of how verbal statements function as discriminative stimuli to occasion actions that produce reinforcers from nature via prediction and control. Matters of effective action, success, usefulness, efficiency, productivity, practical consequences, expedience, or workableness are matters of contingencies in this second sense.

This alternative perspective differs from traditional, representational viewpoints in two ways. First, it does not hold that utterances in general consist of logical entities that correspond to things somewhere else, either in the world at large or in the "immediate experience" of the scientist. Second, it does not regard logic as some superordinate system that determines truthfulness or

meaningfulness of utterances. If anything, logic is a dependent variable—a property of verbal behavior, rather than an independent variable—a determiner of verbal behavior.

Thus, pragmatism is not synonymous with the instrumentalist view of theories or realism or the use of hypothetical constructs. A theory of behavior, including verbal behavior, is ultimately concerned with behavior and the circumstances of which it is a function, as a subject matter in its own right. To argue for instrumentalism, realism, or the use of hypothetical constructs is to go off in search of mental way stations, to impede search for relevant environmental variables, to obscure important details, to misrepresent the facts to be accounted for, to give false assurances about the state of our knowledge, and to continue the use of scientific techniques that should properly be abandoned, for example, because they are wasteful. None of these are desirable.

What is desirable is that scientists recognize the factors that influence their scientific verbal behavior. As with Mach, scientists can also recognize that some knowledge may start out as serendipitous. In any event, fictions and metaphors mislead and distort eventually. The techniques of science are intended to promote supplemental control that reduces the metaphorical nature of scientific statements (Skinner, 1957, pp. 419-420). The traditional view of scientific epistemology based on instrumentalism, realism, and hypothetical constructs is at once mischievous and deceptive.

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