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On Behaviorism, Theories, and Hypothetical Constructs

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The present paper explores some of the characteristics and implications of the approach to scientific theories and theoretical concepts that developed under the auspices of mediational S–O–R neobehaviorism during the middle of the present century. Of special interest is the evaluation of scientific theories and theoretical concepts, notably "hypothetical constructs" and "intervening variables," in terms of realism, instrumentalism, and pragmatism. The paper argues that many contemporary behavioral theorists who embrace the aforementioned approach often fail to understand the verbal processes by which scientific verbal behavior develops. The result is an unfortunate confusion of epistemological principles that does not adequately distinguish among realism, instrumentalism, and pragmatism.

In a recent paper, Moore (1996) suggested that as mediational S–O–R neobehaviorism developed during the second and third quarters of the twentieth century, so also did a particular approach to scientific theories and theoretical concepts. He concluded that with only slight differences, much of this approach remains influential today, not only in neobehaviorism but also ironically in cognitive psychology. For ease of reference, this approach is designated here as the "traditional approach."

The purpose of the present paper is to explore further some of the characteristics and implications of this traditional approach. The first portion of the paper attempts to lay out some fundamental principles of the traditional

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approach. The second addresses the evaluation of theories and their associated theoretical concepts, particularly "hypothetical constructs" and "intervening variables," in terms of realism, instrumentalism, and pragmatism. The third and largest portion proposes a more general view by which to understand the contribution of theories and theoretical concepts to scientific epistemology. This view is derived from the view of verbal behavior associated with the late B. F. Skinner.

The Traditional Approach to Doing Science

Let us begin by formulating six fundamental principles that characterize the traditional approach. To remain faithful to the mid-twentieth century period, when theorists were systematically working through many of these concerns, we will cite sources from that time period as much as possible. Nevertheless, the continuity between the mid-century sources and more recent ones is evident, as cited examples will show. The principles are expressed as an advocate of the traditional approach would presumably express them.

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. 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 (e.g., Stevens' 1957 "psychophysical law"). 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 (e.g., Hempel and Oppenheim's, 1948, "covering law" model of explanation). One premise of the argument is a statement or description of relevant antecedent conditions. 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, elec-

trons, 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 assumptions about 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 be specified a priori? 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 are permitted to 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, so long as the theoretical concepts can be "operationally defined" (Bridgman, 1928; Stevens, 1939). As Killeen (1988) puts it, "We should learn from the history of the more advanced sciences" (p. 330).

6. Technically, most elements that refer to unobservable phenomena in psychological theories should be regarded as hypothetical constructs, rather than as intervening variables (MacCorquodale and Meehl, 1948; Maxwell, 1962; Tolman, 1949). Hypothetical constructs are assumed to actually exist in the world at large, although they are currently unobservable and have to be inferred from observed evidence. In contrast, intervening variables are purely logical devices constructed for the purpose of mediating accurate predictions. They are summary labels that have no meaning beyond their immediate role in the equation or scientific statement in which they appear. 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. 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 is 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.

Examples of the Traditional Approach From the Recent Literature

The recent behavioral literature contains many examples of this traditional approach to doing science. For example, consider the three statements below, all from nominally behavioral theorists. 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 concepts 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. (pp. 50; 59)

Setting aside for the moment the question of whether Skinner's views are appropriately represented in Williams', Killeen's, and Amsel's statements above, the statements nevertheless clearly illustrate the continuing influence of the traditional approach to doing science.

One Consideration in Evaluating the Traditional Approach to Doing Science: Interpretations of Scientific Theories

How are the merits of the traditional approach to doing science typically evaluated? One consideration that is relevant is interpretations of scientific theories. Generally, analyses of scientific epistemology recognize two interpretations of scientific theories. The first is realism, sometimes also called essentialism. The second is instrumentalism, sometimes also called conventionalism. Let us now briefly consider these two interpretations of scientific theories.

Realism

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 that lie beyond appearances; such theories are *ultimate explanations*, and to find them is the ultimate aim of the scientist (Suppe, 1977, p. 168).

Originally, the realists were scholastic philosophers who, while 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 and Donahoe, 1992, p. 1345).

Realism is also related to a mechanistic empiricism that was prevalent in the late nineteenth century. A popular assumption at that time was that the world was an association of objects, moving about according to mechanical fields of force. If one employed the scientific method to observe nature in operation, one could not help but detect these mechanisms. If the whole process was done correctly, the language one used to describe these mechanisms would identify the essential characteristics of these objects and mechanisms. As Suppe (1977, pp. 7–11) pointed out, the "new physics" sprang from this epistemological context and conspicuously challenged its basic precepts.

In summary, then, the realist believes that the goal of science is to propose concepts that represent real phenomena in nature. On this view, any theo-

retical concepts included in a theory should be construed as real phenomena whose existence in the world at large is inferred on the evidence of their effects (for a brief discussion of realism and "existence claims," see Suppe, 1977, pp. 566–570). Given MacCorquodale and Meehl's (1948) distinction between intervening variables and hypothetical constructs, theoretical concepts on a realist view of theories are interpreted as hypothetical constructs.

Instrumentalism

The instrumentalist interpretation of theories holds that theories and their associated theoretical concepts should be understood as conventionally accepted instruments that enable scientists to derive new statements about observables from other statements about observables (Suppe, 1977, pp. 29; 167). The origins of the position are often associated with such figures as Poincaré and Duhem in the early twentieth century, as they reacted against prevailing mechanistic, realist tendencies. In particular, on an instrumentalist view, any theoretical concepts included in a theory need not be construed as referring to real entities that actually exist. Rather, they are only devices that mediate prediction and explanation. To talk about reality and existence is to descend into ontology and metaphysics, and can only lead to unwarranted speculation that will sidetrack the scientific enterprise. Given MacCorquodale and Meehl's (1948) distinction between intervening variables and hypothetical constructs, theoretical concepts on an instrumentalist view of theories are interpreted as intervening variables.

According to the instrumentalist view, then, the important question about a theory concerns its range of application, rather than whether it is true or false in the way that an empirical proposition is true or false. A theory is a statement of properties and relations pertaining to a given set of events. The application of a theory to a particular set of events is 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). We should no more ask whether one theory and its concepts are true and another false than ask whether a measuring system using meters and centimeters is true and another using feet and inches is false (e.g., Poincaré, 1905/1952).

Although the relation is often not formally recognized, instrumentalism is also related to Vaihinger's (1911/1952) influential continental philosophy of "as if." Vaihinger evidenced both Kantian and Machian lines of reasoning in his approach, and then added his own unique contribution. Following a Kantian line of reasoning, Vaihinger maintained humans can never know reality itself. Following a Machian line of reasoning, Vaihinger suggested humans can know only sensations and relations among sensations. Vaihinger's

own unique contribution was to suggest that humans invent "fictions" to make sense of the world, and then operate "as if" these fictions correspond to reality. For example, according to Vaihinger the elements of science are nothing but fictions (as is also the case for morality and religion, among other things). Although we can never know whether these fictions correspond to reality, we make sense of our world by acting "as if" they actually exist. The philosophy of "as if" and instrumentalism are related in that both avoid any commitment to what reality ultimately consists of. Instead, they both talk in terms of how constructs or fictions contribute to an individual's desire to create an understandable universe.

A Second Consideration in Evaluating the Traditional Approach to Doing Science: Views of Truth

A second consideration that is relevant in evaluating the merits of the traditional approach to doing science is views of truth. Generally, epistemological analyses recognize three views of truth: (a) the pragmatic view, (b) the correspondence view, and (c) the coherence view. Let us now briefly consider these three views of truth.

Pragmatism

Pragmatism holds that the truth of a statement is a matter of how well the statement promotes effective action. Pragmatism has its background in the "philosophical pragmatism" of William James, John Dewey, and C.S. Peirce. Suffice it to say that pragmatic concerns are inherent in the traditional approach to doing science described above, for example, among the criteria used to evaluate theories in principle number 4 above, as well as among the arguments in favor of hypothetical constructs in principle number 6 above.

Correspondence View

The correspondence view holds that the truth of a statement is a matter of how well the statement agrees with some fact. The correspondence view 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 corresponds to the structure of the world at large.

Coherence View

The coherence view 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 view eventually embraced by the logical positivists, who had to reject the correspondence view because of difficulties with verifying statements about personal experience. 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 the coherence view is really an improved version of the correspondence view.

The Importance of Understanding Verbal Processes

The present paper takes an altogether different view of theories, theoretical concepts, and their evaluation. The basis of this view is an altogether different conception of verbal behavior, associated with thoroughgoing behaviorism of the late B.F. Skinner. This view emphasizes that much of scientific activity is verbal in nature. Importantly, this view argues that because scientific verbal behavior is operant behavior, it may be analyzed in terms of the antecedent circumstances that occasion it and the consequences that reinforce it. Thus, a scientific term is simply an instance of behavior that is under the discriminative control of aspects of its antecedent setting, just as a pigeon's key peck to a lighted response key 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).

Importance of Multiple Control

To be sure, a good deal of scientific verbal behavior is occasioned either directly or indirectly by operations and contacts with data, such as the pointer and meter readings of which the logical positivists were so fond. That is why the meaning of some scientific terms can be so definitively established. However, the present view recognizes that scientific verbal behavior is complex, perhaps the most complex subject matter that we can engage. It recognizes, therefore, that other factors beyond scientific operations may contribute to scientific verbal behavior. In short, it recognizes that scientific verbal behavior is often under "multiple control" (Skinner, 1957, pp. 227 ff.).

In this regard, Moore (1981) described a second source of control, arising from metaphors, preconceptions, culturally established patterns of speech, and social/cultural traditions that are cherished for irrelevant and extraneous reasons. Skinner identified the importance of spurious contingencies affect-

ing the use of scientific terms, particularly the contingencies that involve social/cultural traditions, 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 and 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 terms used in traditional psychology are related to inappropriate metaphors, generalizations from culturally established patterns of speech, and so on, none of which is appropriate from a strict scientific perspective (see also Day, 1969b, pp. 319–323; Hineline, 1984, p. 98; Marr, 1983, p. 12; Moore, 1983, 1990; Schnaitter, 1984, p. 7). Indeed, much of this approach is in the spirit of Bacon (e.g., Bacon's "Idols") and Mach (see analysis by Smith, 1986, 1995).

For the present view, the two fundamental questions that an analysis of scientific verbal behavior must engage are (a) what are the contingencies that are responsible for a given instance of scientific verbal behavior, and (b) what are the contingencies into which the verbal artifact subsequently enters as it exerts discriminative control? Therefore, the present view is not concerned with the difference between observational and theoretical terms, or between theoretical terms of any interpretation, such as whether a given term is an intervening variable or a hypothetical construct (Moore, 1992; cf. MacCorquodale and Meehl, 1948, and Zuriff, 1985). To ask about differences between observational and theoretical terms or about differences between interpretations of theoretical terms, without asking about the contingencies responsible for the verbal behavior in question, comprehensively fails to understand the nature of verbal behavior. The present argument is that such questions illustrate control arising from mischievous preconceptions and social/cultural traditions, and that this control must be stripped away, to reveal the factors from scientific operations that yield manipulation and control. Theoretical concepts tag no essential properties that impart a metaphysically real identity to a phenomenon. Moreover, there is no other dimension in which these purported essential properties reside, even if they did exist. In short, the present view argues that those who focus on a purported extrabehavioral role of theoretical concepts in science succumb to mischievous social/cultural metaphors that mislead and deflect meaningful scientific analysis. This whole view, emphasizing the analysis of sources of control over scientific verbal behavior, is what Skinner meant by "operationism" (Moore, 1975, 1981).

That the present argument rests on Skinner's operant analysis of verbal behavior is no doubt surprising to some readers who might assume Chomsky's

(1959) review of Skinner's (1957) book *Verbal Behavior* constitutes such a devastating critique of Skinner's behavioral perspective on verbal behavior that the perspective is fundamentally untenable. Cummins (1983) offers a representative example of this assumption in his widely read book on psychological explanation:

> B.F. Skinner . . . constantly represents psychology as seeking predictive nomic correlations. Yet his *Verbal Behavior* (1957) is a sustained attempt to apply a variant of Watson's analysis of habit to linguistic behavior. It is not even prima facie an exercise in prediction and control, and when Skinner tries to exhibit it as such, the results are ludicrous. See Chomsky's review (1959) for some amusing examples. (p. 205)

Cummins and others are certainly entitled to their opinions. Worth noting, however, are the following two points. First, Skinner (1950) rather conspicuously rejected the hypothetico–deductive practices of his mediational neobehaviorist contemporaries. To label Skinner as subscribing to orthodox nomic subsumption models of explanation is very clearly wide of the mark. Second, as most beginning students of psychology know, Watson was concerned with what we would now identify as S–R classically conditioned relations. Watson rejected the principle of reinforcement, and appealed to associative principles, particularly chains of responses mediated by intradermal peripheral cues. Skinner formulated his conceptions of reinforcement and operant behavior during the 1930s precisely as an alternative to the dominant S–R associative model, and one is hard-pressed to even find the term "association" in Skinner's writings. Thus, Skinner's position on verbal behavior as being an operant phenomenon is in no way linked to Watson's position on habit and association (see Moore, 1996, pp. 361–362, and Schnaitter, 1986, pp. 258–261, especially note 4, for additional development of this point).

Skinner (1984) in fact reported that he received an early copy of Chomsky's (1959) review, and actually began to read it. The first few pages were full of so many egregious inaccuracies that he stated

> I could not see how a review beginning that way could be of any value, and I stopped reading. A year later I received a thirty-two page version reprinted from the journal *Language*. When I saw that it was the same review, I put it aside again. (p. 153)

Readers of the present paper may be interested to know that eleven years after Chomsky's review, MacCorquodale (1970) systematically responded to Chomsky's "condescending, unforgiving, obtuse, and ill-humored" (p. 84) comments. Thus, the common perception that Chomsky's criticisms were so essentially valid that they were unanswerable from a behavioral perspective is actually quite distant from the truth (see also MacCorquodale, 1969).

The Evaluation of Traditional Scientific Epistemology

The Traditional Conception of Verbal Behavior

The present argument is that the traditional approach to scientific epistemology rests ultimately on (a) the conception of verbal behavior as a logical phenomenon that differs from a behavioral phenomenon; and (b) a reference or correspondence theory of meaning, wherein words are taken to be things that refer or correspond to other things. For example, consider the time-honored distinction between observational and theoretical terms, previously mentioned (cf. Achinstein, 1968). According to the traditional approach, an observational term refers to some entity or attribute that is publicly observable or intersubjectively verifiable, 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? If those other things are not in the intersubjectively verifiable world, then the other things must be internal and unobservable creations in the "subjective" world of the speaking individual. Because the referent is created internally, it is designated as a "theoretical term." According to the traditional approach, then, the meaning of a theoretical term must then be established through operational definition and logical analysis.

The traditional approach to verbal behavior also creates conspicuous problems with epistemological dualism and representationalism. Epistemological dualism is an orientation in which two dimensions are assumed, certainly in the knower if not also in the known (Moore, 1995b, pp. 65–67). Representationalism holds that immediately given phenomena are representative of events or processes taking place at some other level or in some other dimensional system, and that these other events or processes are the true subject of concern. Representationalism assumes an epistemological dualism, in which extra-dimensional processes on the part of the knower are assumed to be involved in forming or responding on the basis of the representation. The postulation of a theory is the essential means by which access to the other dimension is gained.

The Differences Between the Traditional Approach and the Present View

The magnitude of the distinction between the present view and the traditional approach is not often appreciated. At issue is whether scientific terms are to be regarded as things that have meaning because they stand for, symbolize, represent, or refer to objects either in the environment or in some "subjective" dimension unique to the scientist. To repeat, the present view regards a scientific term as an instance of verbal behavior. As such, it is pre-

sumably under the discriminative control of some property of the environment. Those properties determine its meaning. By all rights, the statement that a scientific term is a construct that symbolizes or refers to another thing ought to be just as odd as the statement that the pigeon's key peck to a lighted response key is a construct that stands for or refers to the light, that it is a "representation" of the key, or that it takes place in another dimension. If the statement 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)

In sum, the present view of verbal behavior leads to an entirely different treatment of the relation between theories and theoretical concepts, on the one hand, and their evaluation in terms of realism, instrumentalism, and pragmatism, on the other hand. First, the present view rejects the idea that hypothetical constructs are useful because they afford some unique logico-theoretical insight into the underlying structure of nature. That kind of insight implies representationalism. Neither such insight nor that kind of structure exists for anybody, especially those who appeal to hypothetical constructs in explanations.

Second, the present view rejects the idea that hypothetical constructs are useful because they correctly tap into underlying, logico-theoretical, epistemological processes of the scientist that differ from behavioral processes. On the present view, to the extent that there are "underlying epistemological processes," those processes involve operant stimulus control, discriminated verbal behavior, and so on, and differ appreciably from the inferred logico-theoretical processes portrayed by those who appeal to hypothetical constructs in explanations. As Skinner put it, "The hypothetico–deductive method and the mystery which surrounds it have been perhaps most harmful in misrepresenting ways in which people think" (in Catania and Harnad, 1988, p. 102).

Third, the present view rejects the idea that instrumentalism and accompanying appeals to theoretical concepts are legitimate, and perhaps even necessary components of scientific knowledge. This idea assumes that scientific knowledge consists in the manipulations of "fictions," and any term that does not refer to an item that is publicly observable is such a fiction. Indeed, according to this idea, in one respect all terms must exist as fictions, created or constructed by scientists to make sense out of their observations (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; cf. Vaihinger, 1911/1952). The present view rejects this idea because it assumes that knowledge consists in the manipulation of subjective/cognitive constructs in the theorist's mental world, apart from the theorist's behavioral world. This idea is epistemological dualism. Various analysts have criticized the idea over the years: (a) Skinner (1947): "No one today seriously uses a fictional explanation as a theory" (p. 28); (b) Scriven (1964): "Do we have to feed ourselves fibs as fuels for our forward movement?" (p. 177); and (c) Suppe (1977): "[I]nstrumentalism leaves one in the uncomfortable position of holding that theoretical terms are necessary, but they do not mean anything or refer to anything" (p. 34). The important point is that just as we do not say that the pigeon's key peck to a lighted key is a fiction or a construct in a mental world apart from a behavioral world, so should we also not say that a verbal term is a fiction or a construct in a mental world apart from a behavioral world.

Given the preceding three points, a fourth and final point assumes major significance. The present view unhesitatingly accepts pragmatism: "[A] proposition is true to the extent that with its help the listener responds effectively to the situation it describes" (Skinner, 1974, p. 235; see also discussions of pragmatism in Day, 1983; Hayes and Brownstein, 1986; Morris, 1988; Zuriff, 1985). Importantly, however, the present view dissociates pragmatism not only from realism and the use of hypothetical constructs, but also from instrumentalist justifications of certain kinds of theories, from representationalism, and from epistemological dualism more generally (cf. Killeen, 1987, 1988, 1995; Williams, 1986). The important question about pragmatism may then be phrased as follows: If a theoretical term promotes effective action, such as prediction and control, what is the basis by which it does so? The answer has nothing to do with the logical status of the theoretical term, or with any of the other issues that the traditional approach raises. Scientific language and theoretical terms are not essentially logical phenomena that correspond to objects somewhere else and that need to be validated through a logical analysis. 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).

Rather, because scientific language, theoretical terms, and explanation are verbal phenomena, the answer has everything to do with the contingencies that control the occurrence of the term, as an instance of verbal behavior. Thus, one may meaningfully ask, "In what sense is the 'theoretical term' an abstraction or an extended tact?" The issue is not simply the percent of variance accounted for by some variable 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. Therefore, a meaningful analysis views the issue as 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 helps to occasion effective action in the world at large.

To be sure, behavioristic theorists of both the past and present have adopted what *appear* to be pragmatic positions. For example, Spence (1936) talked about justifying theoretical constructs "wholly from the pragmatic standpoint that they serve as an aid to the integration and comprehension of the observed phenomena" (p. 447). Similarly, Amsel (1989) continues Spence's theme and argues that neobehaviorist theoretical terms should be judged "by their success in organizing that segment of the describable world they set out to organize" (p. 59). Killeen (1995) recapitulates the position of Spence and Amsel and argues from an ostensibly pragmatic perspective when he suggests that the important issue concerning constructs is "whether they pay their way in the cost–benefit ratio of constructs to predictions" (p. 407).

However, on the view presented here, these positions actually reflect instrumentalism, representationalism, and epistemological dualism, rather than pragmatism. There is no effort to discern why a given theoretical concept mediates accurate prediction. Moreover, when the theorists then embrace a hypothetical construct interpretation of theoretical terms, which they do when they talk of surplus meaning of the terms, the positions then mischievously shift to realism. One example is Williams (1986), previously cited, who indicates that "theory construction inherently entails conjectures about *another level of reality*" (p. 112, italics added). A second example is Killeen (1987), who holds that his kind of 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). The point is that theorists then set out in pursuit of all sorts of fanciful entities in other dimensions that are only marginally related to what can actually be manipulated to control events in the world at large, and the scientific enterprise does not advance as effectively as it should.

The Theoretical Stance of a Thoroughgoing Behaviorism

What Does the Term "Theory" Mean?

No doubt the present view — that some theoretical relevance may be extracted from Skinner's brand of behaviorism — is perplexing to some readers. The pivotal issue is what is meant by the term "theory." On the present view, theories are regarded as verbal behavior. The verbal behavior is occa-

sioned by certain antecedent conditions, and reinforced by its consequences (Skinner, 1957, chapter 18). Zuriff (1985) suggests that such a 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 . . . theory is, therefore, a simple, comprehensive, and abstract description of a corpus of data. (p. 89)

Smith (1986, 1995) points out the continuity among Bacon, Mach, and Skinner in their views of theories. Theories of the above sort 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. The theories help discover uniformities, order confusing data, and resolve puzzlements (Skinner, 1979, p. 282). In any event, always at issue are the contingencies governing the verbal behavior regarded as "theoretical" (Moore, 1990, pp. 25 ff.).

By way of contrast, the present view decidedly rejects the traditional, representationalist view of theories as formal statements that appeal to causal events and entities that reside 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). Moreover, the present view further rejects the assumption that causal explanation in psychology, and scientific knowledge in general, necessarily consist in framing such theories with such representational constructs. Such assumptions further illustrate the same mentalistic problem. These are the kind of theories of which one must be wary (Skinner, 1950).

In place of a traditional theory, a different kind of theory is appropriate. Certainly such a theory would not represent acts, states, mechanisms, and processes from another dimension. This new kind of theory would presumably have at least the following concerns:

1. What aspects of behavior are significant?
2. Of what variables are changes in these aspects a function?
3. How are the relations between behavior and its controlling variables to be brought together in characterizing the organism as a system?
4. What experimental methods are appropriate to the study of such a system?
5. Under what conditions does such an analysis yield a technology of behavior, and what issues arise in its applications?
6. Why do scientists examine and explore a given subject?

7. What rate of discovery will sustain their behavior in doing so?
8. What precurrent behaviors improve their chances of success and extend the adequacy and scope of their descriptions and interpretations?
9. What precurrent behaviors interfere with their chances of success and restrict the adequacy and scope of their descriptions and interpretations?
10. What steps do scientists take in moving from protocol to general statement?

Consequently, in a piece that is not widely known, Skinner (1947) suggested that

> 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]ith proper operational care, they need be nothing more than that. But they have a wider generality which transcends particular facts and gives them a wider usefulness . . . . [E]xperimental psychology is properly and inevitably committed to the construction of a theory of behavior. A theory is essential to the scientific understanding of behavior as a subject matter. (pp. 27–29)

In addition, Skinner also stated

> 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 . . . . If hypotheses commonly appear in the study of behavior, it is only because the investigator has turned his attention to inaccessible events — some of them fictitious, others irrelevant . . . . Cognitive psychologists have promoted the survival of another inaccessible world to which deductive methods seem appropriate . . . . The Freudian mental apparatus has also required a deductive approach, as have the traits, abilities, and factors derived from "mental measurements." We can avoid hypothetico–deductive methods in all these fields by formulating the data without reference to cognitive processes, mental apparatuses, or traits. (in Catania and Harnad, 1988, p. 103)

The Importance of Interpretation

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 interpretations of a vast number of facts, in one

case about the origin of species and in another about the nature of the earth's crust. They use 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 (Catania and Harnad, 1988, pp. 207–208).

Skinner's Behaviorism: Atheoretical or Just Bad Theory?

Some critics regard Skinner's behaviorism as "atheoretical," if not "merely descriptive." Other critics condemn Skinner's behaviorism as nevertheless being "theoretical" in the traditional sense, despite Skinner's protestations to the contrary. The latter critics argue that Skinner's approach is just not very good theory, and it would be manifestly better if it was repaired so that it more formally adhered to traditional principles (see Day, 1969a, pp. 502–505, for examples and discussion). Let us now examine these criticisms of the theoretical nature of Skinner's behaviorism.

To be sure, many of the criticisms of the theoretical nature of Skinner's behaviorism are justifiably attributable to difficulties in Skinner's rhetorical style. For example, Skinner (1938) suggested that

> So far as scientific method is concerned, the system set up in the preceding chapter may be characterized as follows. It is positivistic. It confines itself to description rather than explanation. Its concepts are defined in terms of immediate observation and are not given local or physiological properties . . . . They are not hypotheses, in the sense of things to be proved or disproved, but convenient representations of things already known. (p. 44)

In addition, Skinner's (1950) provocative article "Are Theories of Learning Necessary?" in which he rejected the hypothetico–deductive practices of mediational neobehaviorism, has been the source of continuing confusion. Finally, both Bacon and Mach exerted a profound influence on the development of Skinner's intellectual orientation, which is again at variance with the hypothetico–deductive strategies that characterize mediational neobehaviorism (see discussion in Smith, 1986, pp. 264–275).

Further difficulties arise when Skinner objects to traditional theories in psychology, but then states 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. 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. (in Catania and Harnad, 1988, p. 102)

Such statements seem to imply that the public observability of the subject matter is of fundamental concern. 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, so long as things are "operationally defined."

Critics understandably find support in these latter 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, although his own are quite impoverished and his position suffers accordingly (e.g., Killeen, 1988, 1995; Williams, 1986). In this latter regard, Lana (1995) has suggested that laws relating to reinforcement schedules, a topic of fundamental importance in the Skinnerian approach, have the potential to be developed into a more traditional axiomatic system, but have not yet been so developed, perhaps to the detriment of Skinner's approach. In any case, how can these matters be resolved?

The Importance of Contingencies in the Development of Scientific Theories

The present view is that a resolution of these matters turns on an understanding of the importance of contingencies in the development of scientific theories. The present view holds that 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).

Three important steps occur in the development of a theory (Skinner, 1947, pp. 34 ff.). The first step is to identify the basic data. This step is sur-

prisingly 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. The expression of these relations typically takes the form of the "laws" of the science.

The third step is to develop abstract concepts. Relying on Mach, 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, p. 38). 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.

Again relying on Mach and Bacon, Skinner (1953, pp. 13–14) suggested 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 or idiosyncratic 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).

An important issue here is whether many current "theoretical" statements in psychology have gone through anything remotely resembling this sort of developmental process, three steps or otherwise. On the present view, they have not. Rather, the verbal responses themselves are controlled to a large extent by factors that are cherished for irrelevant and extraneous reasons. They are the product of many mentalistic or even dualistic factors, of unfortunate metaphorical extensions, and so on. As a result, the stimulus control over what are hailed as advanced activities is suspect. 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.

With regard to the successful theoretical contributions of those who are said to have followed the traditional hypothetico–deductive approach, the question is whether the theorists would 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 from the present perspective 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 engage in their verbal behavior if the relevant foundation had not been established earlier.

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. Given its "vast vocabulary of ancient and non-scientific origin" (Skinner, 1945, p. 271), psychology is no exception.

The Contexts of Justification and Discovery

Logical positivists distinguished between the context of discovery and the context of justification, and their distinction may be appropriately reviewed here. On the logical positivist view, 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 positivists thought such enterprises did not contribute to an understanding of the meaningfulness of scientific statements. In contrast, therefore, only a logical analysis of the context of justification was relevant to the understanding of science (Suppe, 1977, p. 524).

Given the distinction between the context of discovery and the context of justification, the present analysis raises the question of whether the context of discovery can be so lightly dismissed in a critical analysis of scientific epistemology. Some factors exert control over the scientific verbal behavior; what are they? Instrumentalist interpretations and intervening variables are

interesting, but strictly speaking intervening variables do not mediate control and manipulation of events. The phenomena spoken about are presumed not to exist. If they do not exist, how can they be manipulated in a scientific operation? Moreover, fictions cannot be the things that exert discriminative control over effective scientific verbal or nonverbal behavior. Fictions do not exist for anybody, and verbal behavior does not create the fictitious phenomena spoken about. 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 illustrate this general perspective. For example, consider the following two passages 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. (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. (1964, p. 96)

Similarly, consider the following, somewhat longer passage from the late W.N. 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 ori-

> gins 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, nor will it succeed in practice. (pp. 337–338)

Topics for scientific investigation can come from any one of a number of sources: "hunches," the breakdown of one's apparatus, convenience, luck, serendipity (Skinner, 1956). Regardless of the particular source, one must recognize that something acts to generate the to-be-investigated topics, and that they do not spring full-blown from the forehead of Zeus. By recognizing that the topics are derived from the experience of the scientist, one can then proceed to investigate them in an orderly and effective way.

Of course, the difficult question from the present perspective is whether psychologists will find it worthwhile to continually assess stimulus control over the mentalistic language found in most instrumentalist or realist theories involving theoretical 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. As noted earlier, 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 preconceptions, metaphors, and social/cultural traditions. On the one hand, if psychologists entertain the mentalistic theory, then psychologists run the risk of finding out later that time and resources have been wasted by entertaining something trivial at best. On the other hand, if psychologists reject the mentalistic theory, then psychologists risk missing something of genuine value, even though the value is not what the mentalist thinks it is. Skinner (1969, pp. 93–94) suggested 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. Even the most accepting of positions, however, would respond far differently to most recent "theoretical" statements in the discipline.

Summary and Conclusions

In summary, then, the present view regards much research in traditional psychology as a kind of sophisticated census-taking at best. That is, many tra-

ditional psychologists study behavior engendered in our culture by common contingencies, but then do not attribute the behavior to the contingencies. Instead, in the fashion of "folk psychology," traditional psychologists uncritically attribute the cause of the behavior to some underlying neural, mental, or conceptual act, state, mechanism, or process. The underlying act, state, mechanism, or process, which often may only be metaphorical, is then given the status of the mediating hypothetical construct, embedded in a broader mentalistic theory. A thoroughgoing behaviorism holds that the resulting theories of behavior (a) are incomplete and vague; (b) obscure important details; (c) allay curiosity by getting us to accept fictitious way stations as explanatory; (d) impede the search for relevant environmental variables; (e) misrepresent the facts to be accounted for; (f) falsely assure us about the state of our knowledge; (g) misrepresent the processes involved in becoming knowledgeable; and (h) lead to the continued use of scientific techniques that should be abandoned, for example, because they are wasteful (e.g., Catania and Harnad, 1988, p. 102). In short, positions linked with the traditional approach actually interfere with effective prediction, control, and explanation of events, notwithstanding arguments to the contrary. They interfere precisely because they lead investigators to search for acts, states, mechanisms, and processes in another dimension. The argument here is that there are no acts, states, mechanisms, and processes of the sort identified by the traditional approach, and there is no other dimension in which to search for them. Rather, talk of such acts, states, mechanisms, and processes, as well as the dimension in which they are said to belong, is occasioned by various misconceptions (Moore, 1990).

The present view of scientific epistemology emphasizes the importance of the contingencies associated with scientific terms. On the one hand are matters of stimulus control over the development of scientific verbal behavior. On the other hand are matters of how verbal statements function as discriminative stimuli to occasion actions that produce reinforcers from nature via prediction and control. Effective action, success, usefulness, efficiency, productivity, practical consequences, expedience, and workableness are all matters of reinforcers in contingencies according to this second sense.

The present view differs from traditional, representational viewpoints in two ways. First, it does not hold that verbal behavior in general consists of logical entities that correspond to or symbolize 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 the 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, the present view regards pragmatism as an issue that is conceptually orthogonal to realism, instrumentalism, and the appeal to theoretical con-

cepts. The study of behavior, including verbal behavior, is not simply a means for making inferences about causal entities from another dimension. A theory of behavior, including a theory of verbal behavior, is ultimately concerned with behavior and the circumstances of which it is a function, as a subject matter in its own right. What is desirable is that scientists recognize the factors that influence their own scientific verbal behavior. 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, rather than validate it (Skinner, 1957, pp. 419–420). Given that some success has accompanied the traditional approach to doing science, as it surely has, the question is whether the traditional approach is successful for the reasons it says it is. The present view is that it is not. Rather, the present view argues that an answer lies in an analysis of the contingencies associated with the verbal behavior in question.

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