In a recent paper by Hayes, Adams, and Dixon (1997), Skinner's radical behaviorism was criticized on conceptual and philosophical grounds. The criticisms centered around certain types of alleged confusions between observed events and the verbal constructions used in the description of those events. In examining the arguments presented by Hayes et al. (1997), however, certain inconsistencies may be seen regarding the central issues, and problems are found in the application of the arguments to Skinner's work, given a thoroughgoing behavioral view of language. For example, the word "cause" might be viewed in Skinner's writings as implying hidden metaphysical forces, but such an interpretation would ignore the historical influence of Mach in Skinner's usage of the term as well as Skinner's repeated refutation of such metaphysical implications.

In a recent article entitled "Causal Constructs and Conceptual Confusions," Hayes, Adams, and Dixon (1997) proposed and described a number of conceptual and philosophical problems that they contended were derivable from radical behaviorism as a scientific system. The authors have carefully considered a number of extraordinarily complex issues in their critique of radical behaviorism, and their efforts constitute a welcome contribution to the growing literature of this important and productive variation of psychological science (e.g., Chiesa, 1994; Leigland, 1992, 1997; Smith, 1986; Todd & Morris, 1995). The purpose of this commentary is to examine the principal issues and problems in the context of their presentation in the Hayes et al. (1997) paper, as well as in the context of the literature of radical behaviorism.

Essentially, the argument of Hayes et al. (1997; all page numbers given below without citation are taken from this source) is that an important problem arises in science when observed events become confused with the verbal constructions used in the description of those events.

This paper is dedicated, with many thanks, to Linda and Steve Hayes, sabbatical hosts extraordinaire. Thanks also to two anonymous reviewers for helpful and constructive comments. Address correspondence to Sam Leigland, Department of Psychology, Gonzaga University, Spokane, WA 99258-0001 (E-mail:leigland@gonzaga.edu).
events, and that this is a problem which occurs in radical behaviorism as a system of science. A salient example is offered by the authors in the radical behaviorist interpretation of causality, where causal powers or forces are said to be implied which are not included in the observed events themselves.

The starting point for Hayes et al. (1997) is with a discussion of the goals of science. In the second sentence of the paper, Kantor's (1953) view of science is described as follows: "Science is an enterprise directed at increasing our knowledge of the world and such is accomplished by describing confrontable events and elaborating upon our descriptions so as to produce what we may call explanations for the forms and operations of those events" (p. 97). This characterization was presented in contrast to that of Skinner (e.g., 1953), who was said to assume that the goals of science are prediction and control.

Although it is certainly true that there are a number of instances in Skinner's writings in which prediction and control are emphasized or identified with the goals of science, the picture becomes more complex when viewed in the larger context of Skinner's scientific system. For example, few scientists would disagree with a general statement that a major goal of science is to increase our knowledge and understanding of the world, but depending upon the given meanings of such key terms as "knowledge," "understanding," and "world," there is no doubt that many, perhaps most, philosophers and theologians would not only join with the scientist in signing on to such a statement, but would also vigorously defend the relevant events in their own fields as fully "confrontable."

Given that "knowledge" and "understanding" may be identified in different ways and with respect to different goals, how are we to characterize the knowledge that is produced by science? Skinner (1957) has described this issue in the context of the analysis of verbal behavior in the following way:

> The "understanding" of verbal behavior is something more than the use of a consistent vocabulary with which specific instances may be described. It is not to be confused with the confirmation of any set of theoretical principles. The *criteria* are more demanding than that. The extent to which we understand verbal behavior in a "causal" analysis is to be *assessed* from the extent to which we can predict the occurrence of specific instances and, eventually, from the extent to which we can produce or control such behavior by altering the conditions under which it occurs. (Skinner, 1957, p. 3; emphasis added)

Generally speaking, the language of "criteria" and "assessment" are being used in the sense of identifying the conditions under which the radical behaviorist is likely to say that the analysis provides scientific "understanding," where the latter term is simply being used in its mundane, everyday language sense, rather than as a special psychological or mental state, and so forth. (In a point to be developed
below, a similar argument will be made regarding Skinner's usage of the term “causal,” which, it should be noted, also appears in quotes in the passage above). Skinner (e.g., 1957) differentiated science in “a merely Baconian way” (to borrow a phrase from Rorty, 1991, originally used in a different context). That is, from a radical behaviorist perspective, science is not viewed as a special window to reality, a special means of accessing Truth, nor a special Method involving a special Epistemology. It is rather another type of human activity. If the products of science, including scientific knowledge and understanding, are not to be differentiated according to criteria such as prediction and control, however, and if we are to exclude traditional, largely mentalistic criteria such as “objectivity,” “truth,” and “realism” and so on, in agreement with Hayes et al., 1997, then how is the differentiation to be made?

In recent explorations of the World Wide Web, I entered a “Science” website on a popular browser, and thence to “Physics,” and “Theoretical Physics.” I was then faced with three possible sites; “Relativity,” “Quantum Theory,” and “New Physics.” Thinking that the last might be a summary of new developments in theoretical physics, I entered only to find that “new physics” was a kind of new-age babble only tenuously and tangentially related to what would normally be considered physics in academic settings. Yet how are we to differentiate between the "new age" and "academic" physics, since they both may be defended given Kantor's characterization of science noted above? For example, they both may be said to “increase knowledge” of some sort, certainly both fields have complexities regarding the “confrontability” of the events of interest, and so on.

The prestige of physical science is derived from its demonstrated power as a source of abstract rules for the effective interaction between people and the rest of nature, where “effective” is a term which may be cashed out in terms of the prediction and control of events. As noted above, these are not the only human goals, of course (e.g., see Skinner, 1957, on literary vs. scientific verbal behavior), but they are goals which are certainly important and well-served by the products of science (Morris, Todd, & Midgley, 1993; cf. Rorty, 1991).

The perspective on science which is advocated by radical behaviorists is certainly a Baconian/Machian/Skinnerian one, but it is a defensible one (e.g., Chiesa, 1994; Day, 1983; Leigland, 1997). To emphasize “prediction and control” is simply to say, from such an antimentalist and antifoundationalist perspective, that scientific activity is differentiated on the basis of practical or effective knowledge, or that science is an activity in which, in Bacon's words, “human knowledge and human power meet in one” (as quoted in Smith, 1995, p. 41).

Relativism

One of the central positions adopted in the critique by Hayes et al. (1997) is a version of relativism, The authors describe their position in the following way:
We maintain that no formulation is any more true or accurate than any other in an absolute sense. . . . Our position is more relativistic than [Kantor's]. From our perspective, different scientific formulations are not able to be judged more or less valid as no irrefutable criteria by which to make this evaluation are available. . . . Instead, formulations are just more or less satisfying in relieving our uncertainties, or more or less useful in accomplishing specific aims at particular times over the course of cultural evolution. (p. 98)

We have not argued that some interests are more legitimate than others nor that some philosophical systems are more valid than others. (p. 108)

Descriptions . . . are constructed for different purposes and one purpose is as good as another. (p. 109)

Because this perspective is important to the general character of the critique presented by Hayes et al. (1997), it is worthwhile to examine briefly some questions arising from it.

First, this general line of argument in favor of relativism is widely considered in philosophy to be self-refuting (e.g., Rorty, 1991). For example, as a statement of relativism, a statement that all scientific formulations, philosophical systems, or purposes, are equally valid, legitimate, or good, amounts to a declaration not of relativism but rather an ontological statement of "absolute equality" among whatever "value" that is being described, which in turn seems to necessitate an "absolutist" perspective from which the equality of the values may be assessed (e.g., Rorty, 1989). For present purposes, it may simply be said that the issues surrounding such statements are extraordinarily complex (for detailed discussions see, e.g., Meiland & Krausz, 1982).

Second, the article by Hayes et al. (1997) seems to include statements and/or implications which contradict the sort of relativism being advocated. For example, in a passage which shortly follows the first statement of relativism cited above, the authors state that the more general issue [regarding Skinner's formulation of science] is the violation of certain rules of governance upheld in the discipline of scientific philosophy . . . . (Kantor, 1969) . . . . These rules of governance provide a means of evaluating particular scientific views as to their adequacy at a specific point in time. Our aim is to examine and evaluate the causal constructions of radical behaviorists along these lines. (p. 99; see also p. 102, "a more adequate scientific understanding"; & p. 109)

The authors' previously quoted denial that some philosophical systems are more valid that others appears to be contradicted by this statement, which takes one system, perspective, or set of assumptions as a means of evaluating another (and not surprisingly, the latter is found lacking). The "discipline of scientific philosophy" which is given priority in the
Hayes et al. (1997) critique is not explicitly identified, although a reference to a paper by J. R. Kantor (1969) is given. The reference to “the discipline” is not likely a general reference to philosophy of science, for the emergence of any generally accepted “rules of governance” in that field could be regarded as nothing short of miraculous (for an excellent overview, see Bechtel, 1988).

This issue might be summarized in the following way. The authors first asserted that because no irrefutable criteria exist for evaluating scientific formulations, one formulation must be as good as another. The latter doesn’t necessarily follow, however, because there could be criteria, short of complete irrefutability, that might be applied to the evaluation of scientific formulations. In fact, Hayes et al. (1997) do apply criteria from one formulation to evaluate another. However, the scientific formulation from which criteria are derived in the critique of radical behaviorism is not specified, but is referenced indirectly in such phrases as “the discipline of scientific philosophy” (p. 99), and where a point is criticized as having “little merit from the standpoint of a philosophy of science” (p. 104).

As noted above, the authors reference the work of Kantor (see also p. 109), and at one point in which the implications of certain “philosophical traditions” are being summarized, an indirect reference is made to “interbehavioral philosophy” (p. 102). This is an important issue, however. If Hayes et al. (1997) are using one scientific formulation to criticize another (and in apparent violation of their relativistic perspective), then the basis of such priority should be described explicitly. If the evaluative criteria are derived from Kantor’s interbehavioral psychology (e.g., Kantor, 1969; other references to Kantor’s work are given later in the paper under the section “Causal Knowledge”; p. 109), then the scientific formulation should be described in enough detail to allow for useful comparisons, but even more importantly, a justification of the apparent superiority of Kantor’s formulation (i.e., given its evaluative priority) should be given. The authors’ stated relativism should lead us to assume that it is a matter of a simple preference between two equally valid formulations; but if this is the case, the force of their argument disappears. If there is more to it than simple preference, however, and if there is assumed to be an intrinsic superiority of Kantor’s formulation such that it is taken to be “foundational” in some sense, then it should be described and, importantly, defended as such.

The third point both relates to the previous two as well as raising an issue to be developed below. The issue of “relativism,” as presented by Hayes et al. (1997), gives rise to additional questions of ontology, which may be illustrated with the following example. As noted above, Hayes et al. (1997) take the position that “one purpose is as good as another” (p. 109), but surely this doesn’t mean that if we were in the position to develop a treatment program for an autistic individual with a behavior problem, that it would be “just as good” if we designed a treatment
program making use of strong electric shock in order to reduce all of the individuals' adaptive social and nonsocial behaviors. Clearly this is not what is meant by the statement of equal-valued purposes. The point is that Hayes et al. (1997) are not using the word "good" in that statement in the mundane, everyday, ordinary-language sense of the term. They mean rather that no purpose is better than another in the sense of "in ultimate nature of things," or in the sense of "reality." The electric shock program for appropriate behavior would not be good, certainly, but its purpose has no more, and presumably no less, ontological "goodness" than any other. The possible hazards of engaging ontological issues such as these are described below, along with certain strategies of ontological disengagement which are shared by both radical behaviorism and philosophical pragmatism (e.g., Rorty, 1991).

Causality

The principal target of the Hayes et al. (1997) critique is what the authors present as the radical behaviorist conception of causality, as when the authors state at the outset that

among the most serious of [scientific confusion] is the confusion of events with descriptive constructions. It is our contention that the radical behavioral interpretation of causality illustrates a confusion of this sort; and it is our aim to examine this confusion as to its sources and its implications for the science of psychology. (p. 98)

The heart of the problem for Hayes et al. (1997) may be illustrated in the following passage:

Causal relations are . . . implicated [for example] in the process of reinforcement. The suggestion here is that one event has some sort of power over another event, and further that events fall into dichotomous classes of causes and effects. There are no such powers or forces to be found among the events themselves, however, and neither do events exist in classes. Causes and effects are products of acts of classifying, and classifying is construction not observation. "Causes" and "effects" are reactions to events, not features of the events themselves. (p.105; emphasis added)

Several issues are raised in this passage, but the central issue for present purposes concerns how the term "cause" is to be interpreted in the literature of radical behaviorism. Historically speaking, Skinner's writings are the place to begin, of course, but when these writings are examined carefully, a clear and consistent picture emerges.

Numerous sources have documented that a major influence upon Skinner was Ernst Mach (e.g., Marr, 1985), and one of the most
conspicuous points of influence was Mach's conception of causality. Specifically, like Mach, Skinner employed the term "cause" to mean nothing more than an observed correlation between events; and thus, like Mach, Skinner took pains to remove any implication of underlying "forces" from his usage of the term, which appeared as a convenient summary-description of observed relations only. Detailed discussions of the relations between Mach and Skinner on this "causality" issue and related issues may be found in Day (1980), Chiesa (1994), Marr (1985), Scharff (1982), and Smith (1986, 1995).

Examples of this usage of "cause" may be seen throughout Skinner's writings. Skinner's doctoral dissertation, for example, was in part a functional analysis of the term, "reflex," and in the published version (originally published in 1931) we find Skinner's early advocacy of that more humble view of explanation and causation which seems to have been first suggested by Mach . . . wherein, in a word, explanation is reduced to description [cf. Kantor's views noted above] and the notion of function substituted for that of causation. (Skinner, 1961, pp. 337-338)

Similarly, in The Behavior of Organisms, Skinner also describes his conceptual "substitution of correlation for cause" (Skinner, 1938, p. 443), and also says that

In general, the notion of a reflex is to be emptied of any connotation of the active "push" of the stimulus. The terms refer here to correlated entities, and to nothing more. All implications of dynamism and all metaphorical and figurative definitions should be avoided as far as possible. (p. 21)

In Science and Human Behavior as well, Skinner (1953) describes his conception of "causality" in the following way:

The terms "cause" and "effect" are no longer widely used in science. They have been associated with so many theories of the structure and operation of the universe that they mean more than scientists want to say . . . . The old "cause-and-effect connection" becomes a "functional relation." The new terms do not suggest how a cause causes its effect; they merely assert that different events tend to occur together in a certain order . . . . There is no particular danger in using "cause" and "effect" in an informal discussion if we are always ready to substitute their more exact counterparts. (p. 23; emphasis in original)

In these passages as well as others (e.g., Skinner, 1957, p. 5; 1971, p. 5), Skinner is clearly not using the term "cause" in such a fashion as to imply hidden or underlying forces or powers, but is rather using the term as a shorthand way of describing observed correlations only. Note also that if a critic were to insist that any usage of the term "cause" must
always carry with it such implications of meaning, regardless of such numerous descriptions and definitions to the contrary, then the critic would be engaging a mentalistic view of language, because the criticism would amount to a statement that words possess a meaning as an inherent characteristic, apart from considerations of context and usage. Such a mentalistic view of language is clearly not the position adopted by Hayes et al. (1997).

Apart from a mentalistic view of language, the worst criticism that may be brought to bear is that for Skinner or other radical behaviorists to use the word "cause" is a poor or misleading choice of terms, because it may be mistaken for what may be a more common usage (i.e., underlying forces, etc.). If the latter is the criticism however, then the problem is not a "philosophical" one (as claimed by Hayes et al., 1997), but is merely a matter of effective communication.

An additional point is worth emphasizing in this context. Toward the end of the paper by Hayes et al. (1997) under a section entitled, "Causal Knowledge," the authors illustrate what they term, "the difference between a philosophical and practical interpretation of a causal situation" (p. 109) with an example from Kantor (1977). The example involves the interpretation of an instance of an explosion as the confluence of a variety of factors (e.g., highly flammable substance, medium such as oxygen, presence of particular temperature and pressure, a spark, etc.). The authors conclude that "from a philosophical perspective, no object or action of an object may be regarded as a causal factor prior to its presence in a specified field" (p. 109), and that "causal knowledge, from a philosophical standpoint, is simply knowledge of the pattern of events, nothing more (Kantor, 1950, p. 174; Kantor, 1970)" (p. 109).

Presumably, the "philosophical" perspective or standpoint being described is Kantor's interbehavioral psychology, but in any case it does not differ from Skinner's radical behaviorism, except perhaps in one way, to be noted below. That is, the authors make the point that the spark, for example, "may not be regarded as a causal factor" in any real or true sense of the word "cause" until it participates in the appropriate field. To state the matter in radical behaviorist terms, and to return to the first quote in the preceding paragraph, there is no useful distinction to be made between a "philosophical" and a "practical" interpretation of a "causal situation," because, as we have seen, the term "cause" is used only in the sense of observed correlation, and in this descriptive, observational sense, is not taken to imply underlying forces or "causal powers." Further, such correlations always and necessarily involve a confluence of many factors, as may be seen in the functionally defined concept of the discriminative stimulus function (e.g., Catania, 1992) which may be defined only in the context of other simultaneously interactive and functionally defined variables as an operant response class, a momentarily effective type of reinforcement, relevant establishing operations, and so on.

Note that from the perspective of radical behaviorism, at exactly what point we decide to call any of these factors a "cause" is nothing
more than an issue of human verbal behavior in observed interaction with the same world as the phenomena being described. It may be regarded as merely a "language" issue, with no implications of dualism, ontology, or of other "philosophical" problems (the latter issues would be considered merely verbal/behavioral as well, because the "metaphysics" is not, of course, contained in the words themselves, but would be viewed as well in terms of interaction, "usage," context, and so on; e.g., Leigland, 1996; Skinner, 1945, 1957).

If there is an important difference between a Kantorian and a Skinnerian construal of the explosion example, it would be in how the two interpretations regard the "practical" implications of their interpretations. Hayes et al. (1997) summarized their view in the quotation above by saying that "causal knowledge . . . is simply knowledge of the pattern of events, nothing more" (p. 109). In behavior-analytic science, however, there is a great deal more, in that the "knowledge" of the patterns of interaction may suggest the arrangement of similar types of events on future occasions. Such arrangements need not be regarded as violating any metaphysical first principles, nor do they necessarily imply metaphysical dualisms, expose underlying "mechanistic" philosophical assumptions, nor any other such problems. They may in fact be construed as the development of another interbehavioral field, inclusive of another type of interbehavior on the part of the human participant. In whatever terms the interactions are described, however, the products of the applied or technological areas of science may be described in turn as examples of such "arranged" interactions, and an emphasis upon such "practical" implications has led behavior-analytic science into remarkable achievements in a wide variety of areas of human application (e.g., Biglan, 1995; S. Hayes, Strosahl, & Wilson, in press; Johnson & Layng, 1992, 1994; Martin & Pear, 1996).

Ontology

The general line of argument that is used by Hayes et al. (1997) in their critique of radical behaviorism may be characterized as follows: (a) To use certain words, terms, or concepts (e.g., "cause") necessitates the implication of the existence of certain characteristics or properties of a corresponding real thing or entity, in cases where a corresponding thing or entity actually exists; (b) but all such terms/concepts are merely verbal constructions; (c) as constructions, the implied corresponding characteristics or properties do not exist in the things themselves; so (d) to use the terms/concepts is to speak as though they do exist, which is false, and thus confused. A complicating factor in this line of argument is its strongly ontological character; that is, the argument depends strongly upon determining which kinds of things or entities, or their associated characteristics or properties, in fact do or do not actually exist.

Although issues of existence are the philosophical domain of that branch of traditional metaphysics known as ontology, some psychologically oriented approaches treat terms such as "real" or "exist" as properties (i.e.,
discriminable characteristics) of verbal interactions which may be subjected to a scientific analysis (e.g., Leigland, 1996; Skinner, 1945; cf. Rorty, 1989, 1991). Nevertheless, Hayes et al. (1997) appear to engage the issue of “existence” in full ontological literality, as shown in the following passages: “In some cases, confusion occurs . . . because the events, in fact, do not have any actual existence” (p. 99; emphasis added), and from the passage quoted above, “There are no such powers or forces, to be found among the events themselves, however, and neither do events exist in classes” (p. 105; emphasis added).

One of the problems arising from such ontological engagement concerns the criteria by which “actual existence” is to be assessed. For example, early in the paper Hayes et al. (1997) state that “no one stands outside of his or her idiosyncratic and cultural circumstances such as to constitute an observer with [independent, objective] characteristics. Science and philosophy are human enterprises and the products of those enterprises unavoidably reflect the circumstances and histories of individual scientists and philosophers” (p. 89). Now if all of the constructions of science are inextricably tied to human culture, context, and language (cf. Murphy, 1990), then how is it possible to speak of the “actual existence” or “nonexistence” of various entities, in and of themselves, as the latter seems to imply the very objectivist stance being rejected? Here the authors adopt a straightforward physicalistic stance, as seen in the following passages:

The organization of event fields, including the relations among field factors, are not aspects of the events themselves, however. Organizations of factors and relations among them have no substantive structure, which is to say, they do not have properties by which they occupy space. They are not thereby among the events to be described. Rather, they are aspects of our descriptions of those fields, not aspects of the fields themselves. (p. 102; emphasis added)

The events upon which scientific descriptions are constructed are constituted of a multitude of component factors undergoing continuous change with respect to their composition and their locations in space. (p. 103; emphasis added)

An organism is a substantive entity, however, while reinforcement is only a descriptive construction, and a construction cannot change a substantive entity, let alone in a substantive way. (p. 106; emphasis added)

These passages, together with those cited above, indicate that the only entities which maybe regarded as having existence are those that may be described with the verbal/social constructions of “substance” and which may be said to “occupy space.” It could be said that such constructions have some sort of special or privileged relationship with the “real world” in some fashion, but in order to defend such a statement, one would again have to adopt an objectivist stance (i.e., one would
have to have special access to that "world" in order to make a case for a special relationship regarding "physical" terms, etc.; for discussions on these and related matters, see Rorty, 1979, 1989, 1991.

From the argument presented by Hayes et al. (1997), some scientific products or constructions are apparently given ontological priority over others. That is, the constructions of physical science or certain physical dimensions are viewed as foundational for the purposes of "existence," while others are merely verbal constructions only. The problem is that there seems to be no way of allowing such privileged status to certain constructions while maintaining either the anti-objectivist position described by Hayes et al. (1997) or their argument regarding event- construction confusion.

The variety and depth of the linguistic traps that await those who travel the philosophical territory explored by Hayes et al. (1997) may be illustrated with the following example. In making their argument regarding conceptual confusions in radical behaviorism, Hayes et al. (1997) assume that many terms and verbal distinctions carry with them ontological implications. In addition to the examples above, the authors conclude the following regarding the notion of reinforcement as "selection by consequences":

There are no causal powers among the events themselves, regardless of how they are conceptualized. Furthermore, as a causal process, selection implies two categories of events, those that select and those that are selected. *Nature is not divided into categories*, though. Categories are products of categorizing acts—they belong to the *domain of reactions* to events, not the *domain of events*. (p. 107; emphasis added)

We have already seen that Skinner's earliest writings, and those throughout his career, have explicitly rejected the notion of "causal powers" when speaking of "causes" as functional relations. In this passage, however, we see a term criticized for the implication of ontological categories, followed by a statement that there are no such divisions into categories, followed by clear a statement of two types of clearly distinct and divided "domains"; one of reactions and one of events (see also p. 105, "time is a metric—the product of an act of measurement—and, as such, it belongs to the domain of constructions, not events"). Applying the same criteria provided by the authors to the authors' own categories, there are clear implications of a dualism throughout the argument of Hayes et al. (1997). Indeed, the distinction between descriptions/constructions-as-reactions versus events may be seen in several of the quotations above as well as others throughout the paper (e.g., "This is an issue of relations among events, and relations . . . are not among the events themselves. They are aspects of our descriptions of events," p. 107; "descriptions are descriptions, not events," p. 105).

Beyond the seeming inconsistency of an implied dualism, the distinction raises additional problems. For example, because the authors
assert that "categories . . . belong to the domain of reactions . . . not to the domain of events" (p. 107), their own categorization between reactions and events must belong in the domain of reactions as well, so that any talk of events, in and of themselves, must constitute exactly the sort of confusion the authors are attempting to criticize.

Further, it is not clear why any such distinction must be made by Hayes et al. (1997) in the first place. Certainly "categorizing acts" may be regarded as examples of "events," and, as noted above, if one is to adopt the anti-objectivist stance described by Hayes et al. (1997), then it is very difficult to see how all "events," so identified by an observer, are not themselves, in every case, "reactions," "descriptions," and the "products of categorizing acts." One can say that the events "themselves" have ontological status apart from the act of observing or describing, and that their defining properties may be described as they are, in and of themselves, but to defend such a position would require a privileged, context-free, culture-free, descriptive vocabulary of some sort (the language of physics is a frequent candidate; e.g., Rorty, 1989, 1991), and it would also necessitate the abandonment of the anti-objectivist stance adopted by Hayes et al. (1997).

Summary and Conclusions

The types of problems that Hayes et al. (1997) seek to expose in radical behaviorism may be seen only if one is willing to believe a particular set of metaphysical views. In this case, the origins of the views appear to be related to Kantor's interbehavioral psychology, but were not described explicitly in the Hayes et al. (1997) critique. More importantly, the reasons why such views were given evaluative priority over other perspectives in science were not addressed in the critique, but such priority appears to be inconsistent with the authors' statement of the relativism (or perhaps more accurately, the absolute evaluative equality) of different scientific perspectives.

Further, the metaphysical views being offered by Hayes et al. (1997) are purchased at the cost of the creation of additional problems of a "philosophical" nature. One of the goals of this essay was to point out some of the difficulties that arise when psychological scientists address problems of metaphysics through the use of the very same language game that, historically, produced those problems in first place. For example, to force "metaphysical import" onto the usage of a term such as "cause" in a verbal context where the latter term (as in the case of Skinner) has repeatedly been defined as a descriptive term concerning observed correlations only, is to create an unnecessary "metaphysical" problem out of a relatively simple verbal context. To press the point to the usage of the term itself is to place oneself in a traditional, mentalistic language game in which terms possess meanings irrespective of context and usage. As another example, if critics who deny the existence of categories are to force ontological categories onto verbal discriminations (e.g., selection vs. selected), then those critics must beware of the same practice in their own writings (e.g.,
constructions vs. events). Further, the assessment of entities which “actually exist” as distinct from those which do not is incompatible with the stated anti-objectivist stance of Hayes et al. (1997).

Although space does not permit a proper description here, Skinner's radical behaviorism shares a number of important features with philosophical pragmatism (a preliminary, indirect path may be traced by examining the relations between Skinner's radical behaviorism and the later work of Wittgenstein; e.g., Day, 1969; and the relations between the later Wittgenstein and contemporary pragmatism; e.g., Rorty, 1979, 1991; see also Day, 1980, 1983; Zuriff, 1980). Pragmatism is a complex philosophical perspective with a number of historical variations (Murphy, 1990). A principal theme of pragmatism which is shared with radical behaviorism is that of opposition to “representationalism”; that is, opposition to the traditional view in Western philosophy that the mind or language represents, with different degrees of accuracy, a real world that exists and can be known by way of these representations apart from human language, history, and culture. Philosophical pragmatism preserves the notion of a person in the context of, and in interaction with, a world, but disputes the notion of a mind, language, or vocabulary as being able to represent that world (specific examples of Skinner's anti representationalism may be found as early as Skinner's doctoral dissertation, 1961; see also Skinner, 1953, 1964, 1974). In following the implications of such an antirepresentationalist view, contemporary pragmatic philosophers such as Richard Rorty (e.g., Rorty, 1979, 1989, 1991) have sought to deconstruct the traditional epistemological and metaphysical problems of philosophy, and to rid the philosophical agenda of such traditional philosophical distinctions as appearance vs. reality, mind vs. body, language vs. fact, realism vs. antirealism, and so on.

One of the more general implications of antirepresentationalism, when viewed from the scientific perspective of radical behaviorism, is that the traditional problems of philosophy (such as the family of “mind-body” problems or those of metaphysics), become problems appropriate for psychological science, in that they involve complex issues of language, differing vocabularies, contextual variables, and so on. They become, in other words, problems of verbal behavior (e.g., Leigland, 1996; Skinner, 1945, 1957). Such problems would not be “solved” by such a scientific analysis, as any “solution” would necessarily involve the direct engagement of the very vocabulary that has framed the problem. Nor would the scientific analysis involve the “reduction” of the terms or language in which the problem was framed to some sort of “foundational” terms or language (e.g., from “mentalistic” to “physical” language). Stating the matter informally, the analysis would involve a reformulation of such problems such that they may be dismissed or pursued as the language in which the problems were framed becomes clarified (e.g., Leigland, 1996; cf. Wittgenstein, 1953).

As a final point, it should be emphasized that the position taken here is not that the sort of pragmatic interpretation of radical behaviorism being offered is the only possible interpretation, nor is the interpretation
in some sense considered to be "foundational," or what radical behaviorism "really is." It is an interpretation which is consistent with the broadest themes and issues arising from Skinner's work over a period of 60 years. In particular, such an interpretation follows directly from the implications of Skinner's work on verbal behavior (e.g., Skinner, 1945, 1957). Beyond consistency, however, there are advantages to such an interpretation, not the least of which are the reformulation of traditional philosophical problems into issues of verbal interactions and verbal/cultural practices. In following the implications of the verbal practices of radical behaviorism in this way, we find the pathway from science to philosophy and metaphysics to be a pathway that leads inevitably back, allowing us to start again with new questions, new tools, and new directions.

References


