Mechanism and Contextualism in Behavior Analysis: Just Some Observations

Edward K. Morris
University of Kansas

I was honored to have had my manuscript "Behavior Analysis and Mechanism: One Is Not the Other" (Morris, 1993a) and apparently several others (e.g., Morris, 1988a, 1992a) receive published peer commentary in this journal last spring (Baer, 1993; Carr, 1993; Delprato, 1993; Lee, 1993; Marr, 1993; Reese, 1993) and again this fall (Blackman, Shull & Lawrence, and Staddon, this issue). The commentators were divided over whether or not behavior analysis is mechanistic in worldview, and I suspect the same is true of the readership. Agreed or disagreed, however, we are having an important discussion. For too many years, we answered the question "What is radical behaviorism?" with something like "The philosophy of the science of human behavior" (see Skinner, 1974, p. 3). For too many years, we answered the question "What is the philosophy of radical behaviorism?" by describing (a) its products, among them, the analysis of verbal behavior (Skinner, 1957) and the behavior of scientists (Skinner, 1945, 1947, 1950, 1956, 1957, pp. 418–431) and (b) varieties and subvarieties of other *isms* (e.g., naturalism, functionalism, pragmatism, empiricism, positivism, and operationism; Day, 1980; Smith, 1986, pp. 257–297; Zuriff, 1985). These answers, however, do not describe behavior analysis in a way that integrates what we know about (a) behavior (e.g., its prediction and control; see S. Hayes & Brownstein, 1986) and (b) our terms, concepts, and constructs for describing behavior (e.g., our theory; see Skinner, 1947, 1950) with (c) our fundamental assumptions about behavior (e.g., the theory of our theory, our metatheory, or worldview; see Delprato & Midgley, 1992; S. Hayes, L. Hayes, & Reese, 1988).

The value of such integration lies in successful working. That is, just as we seek effective action through the prediction and control of behavior in basic and applied research, so too do we seek effective action in the conceptual analysis of behavior, working in part with our own behavior as our subject matter. Among the consequences of such analyses are, for example, those Skinner (1979) described for himself: "the discovery of uniformities, the ordering of confusing data, the resolution of puzzlement" (p. 282). This is the stuff of constructive thinking, problem solving, and intellectual self-management (Skinner, 1953, pp. 242–256; see Morris, 1992a).

Over time, the selective action of the consequences of conceptual analysis will work for and against certain features of behavior analysis as a discipline and a profession, as well as for and against its survival among the natural, behavioral, and social sciences, and within the culture at large. As Skinner (1981) pointed out, selection operates not only at the level of the phylogeny of organisms and the ontogeny of their behavior, but also at the level of cultural practices. Science and its philosophy are exemplars of the latter (Skinner, 1953, pp. 437–449, 1971; see Batts & Crawford, 1991, for a review of evolutionary accounts of science). But selection does not operate in a void. In our case, the behavior-analytic conceptual system will not evolve without conceptual analyses to be selected for and against. This is what is important about

publications and peer commentary, and now, I hope, in my reply.

With all due respect to the commentators, my reply does not take up their every point. That task was made too difficult when some of them spoke to manuscripts and issues in addition to (and sometimes other than) my mechanism paper, most specifically with respect to context and contextualism (e.g., Morris, 1988a, 1992a). There was, apparently, an abundance of verbal behavior about context and contextualism at considerable strength for some reason or other. Because of this and the time constraints on submitting this manuscript, I do not address as many issues as I should, or address them as well as I would like. Among the issues I do not address, for example, are (a) the different (and sometimes inappropriate) criteria that appear among those who agree that behavior analysis is not mechanistic in worldview or that it is contextualistic; (b) the usefulness of Pepper's (1942) "root metaphor" system for comparing and contrasting fundamental assumptions; (c) how the structure of our language conditions us to see the world either organismically or mechanistically, not contextualistically (see Hackenberg, 1988; cf. Lee, 1993); (d) the importance of familiarity with primary sources on mechanism and contextualism (e.g., Pepper, 1942) rather than secondary sources alone; and (e) the ontology–epistemology distinction (see Reese, 1993) and tensions between behavior-analytic ontology and epistemology (see S. Hayes et al., 1988, p. 103).

What I am left with are some broader issues, which I restate where they need additional emphasis and clarify where there has been misunderstanding. The issues I address are basically just two: the nature and meaning of (a) mechanism and mechanisms and (b) context and contextualism. In addressing these issues, I point to topics in need of elaboration and to new problems in need of analysis, inviting readers and commentators alike to pursue them. Before I begin, however, I would like to review my purpose in writing the mechanism paper. My apparent failure to clarify my purpose seems to have caused some misreading of my intent (i.e., of the variables that controlled my behavior).

Purpose

Mechanism. My reason for writing the paper was to describe and correct various representations of behavior analysis as mechanistic in character and in worldview. I have not known these representations to be especially accurate (see, e.g., Mahoney, 1989; contra Morris, 1990; Todd & Morris, 1992), except on a generous interpretation (nor had I known behavior analysts who avowedly called themselves mechanists before now, except for my close colleague Don Baer; see Baer, 1993). In trying to understand the nature and source of these misrepresentations in society, philosophy, and psychology, I turned to ordinary-language and professional dictionaries (e.g., *Webster's Ninth New Collegiate Dictionary,* 1987) and to literature on behavior analysis (e.g., Reese & Overton, 1970). My interest was in the meaning of *mechanism* for those who call behavior analysis mechanistic, *not* in the meaning of *mechanism* for those who call *themselves* mechanists. Admittedly, my findings would have been more representative if I had interviewed ordinary-language speakers, scholars, and professionals about what they currently mean when they called behavior analysis mechanistic. That now becomes a project someone else might pursue.

Contextualism. Although I mentioned contextualism but briefly in my mechanism article (only in a footnote at the end of the last sentence; Morris, 1993a, p. 39), it evoked extensive commentary, so a word or two. My interest in contextualism is not new. It dates back to the early 1980s, in my comparison and contrast of radical behaviorism and interbehavioral psychology (Morris, 1982, p. 193). Since then, contextualism has been, for me, a means for clarifying the behavior-analytic conceptual system and for understanding parallels and antiparallels with other perspectives in the biological, behavioral, and social sciences (see

Midgley & Morris, 1992; Morris, 1988b, 1991, 1992a). In no case, however, was this a "career move." Career moves consume too much time and energy, and detract from the excitement of "doing science."[1]

I realize that some of these comments are more personal than an archival journal and its readership should have any great interest in. Behavior analysis, though, *is* the behavior of behavior analysts and its products. An increase in understanding what controls our behavior—in this case, mine—is thereby an increase in understanding its products.

MECHANISM AND MECHANISMS

As I mentioned, the commentators were divided over whether or not behavior analysis is mechanistic in worldview. Among those who argued that it is, their most consistent and thoughtful point was that mechanism comes in varieties. In particular, Dennis Delprato (1993) distinguished between "discrete" and "consolidated" mechanism, and Richard Shull and Scott Lawrence (this issue) distinguished between "simplistic" and "sophisticated" mechanism. Dennis's distinction was based on his reading of Pepper (1942), whereas Richard and Scott's was based on their reading of the history of science and psychology. Derek Blackman (this issue), in contrast, seems divided within himself over mechanism, contextualism, and behavior analysis, and

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[1] Relatedly, several seemingly ad hominem remarks (and apsalts on Pepper) by a commentator or two smarted a bit. For instance, although what I have written about contextualism may sometimes be opaque to those who pursue the experimental analysis of behavior (and sometimes to me), publications in the *Journal of the Experimental Analysis of Behavior (JEAB)* do not always sparkle with lucidity for readers unfamiliar with its complexities and technicalities. However, I attribute my occasional struggles through *JEAB* (e.g., the recent Special Issue on Behavior Dynamics, 1992, Vol. 57, No. 3) to my unfamiliarity with terms and concepts (and maybe more). It never occurred to me that its occasional opaqueness might be context free, the phault of the writer, not the reader. It never occurred to me to separate the knower from the known, to play a game of "epistemological chicken" with the meaning of meaning.

so has stepped back (or closer) to comment astutely on the irony that "behavioral mechanisms" are today considered "contextualistic explanations" in behavioral pharmacology. In what follows, I comment first on the varieties of mechanism and second on the irony.

Varieties of Mechanism

Discrete versus consolidated mechanism. Dennis Delprato (1993) is most observant and thoughtful in his analysis of Pepper (1942). He is correct: I was largely arguing that behavior analysis is not mechanistic in the sense of discrete mechanism, although I had not self-consciously made the distinction between discrete and consolidated mechanism. I should have. Although Dennis wrote that my argument would be "hollow" should I admit this, I respectfully disagree. First, the criticisms of behavior analysis as mechanistic are almost always criticisms of it as a form of discrete (not consolidated) mechanism. These are the criticisms I sought mainly to address. Thus, Pepper's point about discrete mechanism being long ago dead is, I think, moot. Second, when Pepper said discrete mechanism was long ago dead, he was speaking of the physical sciences. In contrast, he wrote that the "psychology of discrete mental elements [discrete mechanism] is the neatest and, in that respect, the most intellectually satisfying psychology that has been developed" (Pepper, 1942, p. 219). Discrete mechanism is alive and well in psychology today.

Dennis's compelling point remains that I did not address whether or not behavior analysis is a variety of "consolidated" mechanism, for it presents an alternative to behavior analysis as contextualistic in worldview. In distinguishing between discrete and consolidated mechanism, for instance, Pepper (1942) pointed out how the latter sometimes gravitates toward contextualistic categories, for instance, toward an operational theory of truth. But he eventually resolved these categories back into mechanism, for instance, to a causal-adjustment theory of truth—prediction without control. The slope be-

tween consolidated mechanism and contextualism is a slippery one, and clearly in need of further consideration vis-à-vis behavior analysis.

Simple versus sophisticated mechanism. Shull and Lawrence (this issue) distinguish between a "simplistic mechanistic description" of behavior analysis and a "sophisticated mechanistic approach." The former is presumably Pepper's (1942) discrete mechanism. The latter they identify with the variety of mechanism described by Jack Marr (1993). Whether this is Pepper's consolidated mechanism, neither they nor Jack say, but at least it is not contextualism to them.

Richard and Scott quite rightly argue for sophisticated mechanism over simplistic mechanism. They then argue for sophisticated mechanism over contextualism. Their latter argument is based on internal considerations in the history of science and psychology, which are almost always preferable to external considerations in the politics of science. External considerations sometimes go no deeper than to ask, for instance, which will play better in Peoria, sophisticated mechanism or contextualism? Richard and Scott's internal reasons for selecting mechanism over contextualism are among those most strongly and commonly voiced within behavior analysis: (a) Mechanism has been productive in many other sciences (i.e., in enhancing the prediction and control of their subject matters), and (b) it is the scientific tradition within which behavior analysis developed. These matters are not unequivocal, so let me address them directly.

The Productivity and Tradition of Mechanism

Productivity. As for the productivity of mechanism (simplistic or sophisticated), there is no argument. The evolution of the sciences from self-actional stages (e.g., from creationism, vitalism, and mentalism) to mechanistic stages has always revolutionized (or will) our understanding of the inanimate and animate world in which we live (see, e.g., Thompson, 1984). If science is a cultural practice, however, we cannot argue that mechanism must be its final stage, its usefulness to date notwithstanding. Evolution is just not that sort of process. In *The Evolution of Physics,* for instance, Einstein and Infeld (1938/1966) illustrated how the "mechanical view" of Galileo and Newton gave way to "the field as representation" in the work of Faraday, Maxwell, and Hertz (cf. Marr, 1993), a view they did not equate with "mechanical." Einstein and Infeld have been cited by philosophers (Dewey & Bentley, 1949) and psychologists (Pronko & Herman, 1982; cf. Kantor, 1946) alike, who suggest that psychology is evolving (or should) along a similar course (Morris, 1992a). Such an evolution does not signal the eclipse of the mechanical view, for the conceptual systems available at different stages of scientific evolution may fill different niches. But which they will fill best, and how many, are empirical matters, as is their relative usefulness across different subject matters (e.g., physics and psychology).

Einstein and Infeld's (1938/1966) observations warrant analysis in our discussions of the behavior-analytic worldview. First, a retrospective review of *The Evolution of Physics* might be a useful basis for considering how behavior analysis has progressed as a science, both internally and with respect to its place in the behavioral and social sciences as a whole. Second, just as Einstein and Infeld's analysis was driven by empirical considerations in the field of physics, so too should claims that behavior analysis holds not to a mechanical view, but to something like a field-theoretic perspective. Perhaps we should move away from metaphors drawn from physics to the subject matter of behavior analysis itself. Third, we all seemingly agree that behavior analysis does not adhere to Einstein and Infeld's "mechanical view" or a "simplistic mechanistic description" or to Pepper's (1942) "discrete mechanism," but are they one and the same? I guess so, but this might be looked into. We are also seemingly agreed that be-

havior analysis holds to something else: to Einstein and Infeld's field-as-representation, a sophisticated mechanistic approach, Pepper's (1942) consolidated mechanism, or Pepper's (1942) contextualism. But here, we are not agreed on which one, which ones overlap, or where they overlap, if they do. This is a project worth pursuing, but by someone else, please—I am not a rocket scientist.

The Mechanistic Tradition

The other argument for taking mechanism over contextualism is that the former is the tradition within which behavior analysis developed. This tradition may be parsed two ways: into an empirical lineage and a philosophical lineage.

The empirical tradition. The argument from the empirical tradition is especially compelling because of some formal correspondences between behavior-analytic research practices and how we describe them (e.g., $R = f(S)$; see Skinner, 1931) and the mechanistic worldview (e.g., dependent and independent variables). But we should be wary about characterizing our research practices (and descriptions thereof) as mechanistic on the basis of correspondences among formal or linguistic features alone. Preferably, the relation between research practices and worldviews should be based on correspondences among functional relations between them, that is, on relations between the controlling variables over the behavior of research scientists and the behavior of those who pursue conceptual analyses within a particular worldview. It may be that a research program that appears mechanistic in form may actually be contextualistic in function (cf. Reese, 1993). This point is difficult to articulate and to appreciate because formal research practices are so concrete, whereas their functional relations to metatheory and worldviews are less so. They are so much less so that we often look through them and, in so doing, think that metatheory and worldviews are trivial or nonexistent. But this is positivism gone awry. "Seeing" a worldview is an operant.

Let me try to make this point in the context of the behavior of other organisms. Here, the form or topography of their responding is tangible—discernible in concrete instances. Its function, however, is not. The form or topography of responding (or of its mechanical transduction) is what we observe and measure. Its function is an inference. It is a relation detectable only over time, not in an instance—a relation we may sometimes look through and thus not find (Hineline, 1990). Ironically, what is important in the experimental analysis of behavior is rarely the tangible, discernible forms or instances of responding, but rather their relations and functions (this also applies to the stimulus environment). This lesson was one of the first that Skinner (1931, 1935, 1938) taught us. Ted Carr (1993) taught us this lesson again in his commentary: Behavior as response form is not our subject matter. Behavioral function is.

Just as this is true of the behavior of other organisms, so is it true of our own behavior. We should not decide on the function or the variables that control our research practices on the basis of their form alone. The controlling variables are difficult to observe and define, and the functional relations between them and our behavior as researchers are difficult to see. Discerning the functional relations, not the form of our research practices, however, is the more useful basis for recognizing the behavior-analytic worldview. It also has the positive effect of making the name of our worldview—whatever it might be—a summary term for a set of functional relations between our behavior and its controlling variables, not a reified thing.

This argument does not come down on the side of either mechanism or contextualism. It is not meant to. It simply suggests the kinds of evidence we need in order to be secure in arguing one way or the other. Perhaps this is the basis for yet another paper, for which Bill Verplanck (1954) has given us a lead. He wrote:

Because of the existence in Sherrington and Pavlov of sets of data of the kind [Skinner] believes are

needed [for his system], he has adopted many of their terms and applied some of their laws in defining behavior. As a consequence, he has been misinterpreted. In his choice of terminology, Skinner has assured that his work and those of his followers will be read easily by the followers of Hull and Guthrie and only with emotion, if not with difficulty, by those who have selected the organismic-field-Gestalt-force family of words to work with. Skinner's conditioned responses seem to many readers just as *mere* as those of Pavlov and Hull, with the extraordinary result that he has been classed with Hull rather than with Tolman, with Guthrie rather than with Lewin, in his general position. (p. 307; see also Krechevsky, 1939, pp. 406–407)

Lewin (1935) had an affinity for a field-theoretic orientation, and Tolman (1932) was some variety of a contextualist, at least at the start (Pepper, 1934; see Smith, 1986, pp. 67–145). Invoking Tolman suggests even further that the supposedly mechanistic language of dependent (R) and independent variables (S) and of functional relations—where, for instance, $R = f(S)$ (Skinner, 1931)—is not inherently determinative of mechanism. Tolman (1938) used such designations himself, as in $B = f(S, H, T, P)$, of which Skinner (1987) commented:

B represents behavior, S stimulus conditions, H hereditary make-up, T past training (my "conditioning"), and P appetite or aversion (my "drive"). R. S. Woodworth later pointed to the similarity of the equations. In addition to the stimulus, I had called the conditions of which response strength was a function "third variables" [as in $R = f(S, A)$, Skinner, 1931, p. 452], but Tolman called them "intervening." That may have been the point at which the experimental analysis of behavior parted company with what would be cognitive psychology. (p. 208)

This would also be the point at which Tolman's intervening variables became hypothetical constructs (Tolman, 1949; see Morris, Higgins, & Bickel, 1982) and the point at which he began parting company with contextualism: Mediational and representational cognitive psychology is mechanistic in worldview (Costall & Still, 1987; Gillespie, 1992; see Morris, 1989).

As for the formula, $R = f(S)$, however, it follows easily from conceptual practices, of which Skinner (1935) wrote:

In the description of behavior it is usually assumed that both behavior and environment may be broken down into parts, which may be referred to by name, and that these parts will retain their identity from experiment to experiment. (p. 40)

These practices are reductionistic in the elementaristic sense (see Branch, 1977) and are largely in keeping with the mechanistic worldview. But this view was not Skinner's view or what it would become (Coleman, 1984; Schraff, 1982):

The analysis of behavior is not an act of arbitrary subdividing, and we cannot define the concepts of stimulus and response quite simply as "parts of behavior and environment" without taking account of the natural lines of fracture along which behavior and environment actually break. (Skinner, 1935, p. 40)

Analysis per se is not inherently antithetical to contextualism (albeit perhaps anathema to some varieties thereof). The categorical complaint is about the type of "analysis into parts" that Skinner (1935) described in the first of the two quotations above (S. Hayes & Brownstein, 1986; S. Hayes et al., 1988; see Dewey, 1896; Lee, 1993).

In this view, then, behavior might be defined generically as $B = (R \leftrightarrow S)$ (see Midgley & Morris, 1988) or, more specifically, as the three-term contingency: $B = (SD \leftrightarrow R \leftrightarrow SR)$. In any event, further consideration of our unit of analysis and how we depict it, either mechanistically or contextually, seems warranted (Morris, 1992a; cf. Moxley, 1982, 1984, 1987), as would a closer account of the affinity between the behavior-analytic research tradition and the two worldviews.

The philosophical tradition. Although the argument for taking mechanism over contextualism on the basis of tradition is usually based on an empirical tradition, we should not overlook philosophical traditions. Central to the latter are our assumptions about the nature of nature (i.e., ontology) and about how we can know it (i.e., epistemology) (see Reese, 1993), to the degree that they are ever separable in practice. Unlike the empirical tradition out of which behavior analysis evolved, its philosophical tradition is, I think, more obviously contextualistic (cf. Chiesa, 1992; Moxley, 1992). This observation remains to be worked out in detail, but some preliminary sketches are

available (see Morris, 1988a, pp. 293–299, 1993b). In addition, a comparison of contextualism with informed presentations of the philosophical-historical antecedents of behavior analysis, such as those by Willard Day (see Day, 1980), lends further credence to the relation between behavior analysis and contextualism, although further scholarship remains to be completed. (I wonder what Willard would say about all this.)

Of Mechanisms and Mechanism

Not only are there varieties of mechanism as a worldview, but there are also "behavioral mechanisms." As for the latter, Derek Blackman (this issue) comments on the irony that, in behavioral pharmacology, behavioral mechanisms (e.g., rate dependency) are the contexts in which pharmacological agents operate. Behavioral-mechanisms-as-context explain, in part, how those agents function and, as such, constitute "contextualistic," as opposed to reductionistic or mechanistic, explanations of drug effects.

I take up difficulties that this "context-contextualistic" distinction raises in the next section, but I first want to comment on whether talk of behavioral mechanisms is necessarily tied to mechanism as a worldview. The homology notwithstanding, I think not. A *behavioral mechanism* is a generic construct for more specific, predictive functional relations between responses and stimuli (e.g., reinforcement). Neither the generic nor the more specific constructs require that we take any particular ontological stance with respect to their character, for instance, that they operate mechanically. Such constructs are only efficient, pragmatic means for describing predictable (and often controllable) behavioral relations— relations as we know them at this point in the evolution of what we understand about behavior.

Still, the term *behavioral mechanism* suggests an analogy to something that operates mechanically, and thereby an analogy to the root metaphor of the mechanistic worldview—the machine (Pepper, 1942, pp. 186–191). Perhaps we should take a more neutral stance, and refer instead to behavioral "processes," not mechanisms. This fits better our conceptualization of behavior as a process, not a thing.

CONTEXT AND CONTEXTUALISM

Most of the commentators touched on context or contextualism in one way or another. Some supported the recently increased interest in context in the analysis of behavior; others doubted there was anything new, or anything new that did not already come from a mechanistic worldview. Some supported the contention that behavior analysis was contextualistic in worldview; others raised grave doubts that this was true or even possible. Although this material is nominally outside the purpose and purview of my original paper, I would be remiss in not replying. So, I address what I think are two fundamental misunderstandings: one in appreciating the variety of contextualisms and another in understanding the context–contextualism distinction.

Varieties of Contextualism

Several of the commentators were concerned that contextualism is perhaps antithetical to science, to natural science, to the kind of science behavior analysis represents. Like mechanism, however, contextualism exists in several varieties. Perhaps the varieties of contextualism are not as distinct as those of mechanism (e.g., discrete vs. consolidated mechanism) or as obvious to those committed to the categories of the mechanistic worldview, but some important distinctions do exist (see S. Hayes, L. Hayes, Reese, & Sarbin, 1993) and need to be made (yet another paper). For instance, the contextualism exemplified in the writings of behavior analysts (e.g., S. Hayes et al., 1988; Morris, 1988a, 1993b) differs in important ways from that in the writings of personality and social psychologists (e.g., Rosnow & Georgoudi, 1986; Sarbin, 1973). Although I can understand what the social and personality psychologists are saying (and much of it

is important), I have my own doubts about some of their program, especially where it seemingly slips into mentalism (Morris, 1988b, 1989). But I have found it worth the effort to make the discrimination (see Morris, 1993b, pp. 153–156). The discrimination can be made on several dimensions, each deserving a paper of its own. Let me touch on just one.

Subject matters. The discrimination between the varieties of contextualism that perhaps distinguishes them most clearly is what they take as the subject matter of psychology—the individual or behavior. In some varieties of contextualism (see, e.g., Mancuso, 1993; Sarbin, 1993), the subject matter of psychology is the individual. The individual-in-context is its unit of analysis, which is its goal to understand. In this view, psychology cannot be a natural science, where natural science is defined in terms of discovering "general principles." Psychology cannot be such a science because its statements about individuals are spatiotemporally restricted, local, and particular to time and place. The individual is a unique and historical event, usually described in the language of behavioral "content" (i.e., in ordinary-language terms; e.g., personality, sociality). Psychology, in this sense, is natural history, not natural science (see Gergen, 1973).

In other varieties of contextualism (e.g., S. Hayes & Brownstein, 1986; S. Hayes et al., 1993), the subject matter of psychology is behavior—behavior considered generically. Behavior-in-context is its unit of analysis, which is its goal to understand. In this view, psychology can be a natural science because its experimental analysis generates principles that are taken to be spatiotemporally "universal" (e.g., reinforcement), principles that are not local and particular to time and place. Behavior, as such, has generic qualities that are replicable, qualities usually described in the language of "process" (i.e., technical terms; e.g., reinforcement). This is natural science, and thus is a variety of contextualism distinct from that which is natural history.

Natural science and natural history are *different* ways of "knowing." Moreover, natural science makes important contributions to altering and explaining natural history. Where we have both prediction *and* control in behavior analysis, altering natural history is the purview of applied behavior analysis; for instance, with respect to personality (e.g., response repertoire) disorders and social deviance (Baer, Wolf, & Risley, 1968). Where we have only prediction (and no control), natural history is the purview of behavioral interpretation; for example, the interpretations of verbal behavior (e.g., Skinner, 1957) and cultural practices (e.g., Skinner, 1971).

Truth in science. This distinction between the natural-history and natural-science varieties of contextualism raises the issue of what we consider to be "true" in science. This will help to clarify this distinction. One of the difficulties natural scientists have with contextualism is its opposition to the mechanistic ideal of *discovering* universal laws and principles that exist independently of the behavior of scientists (see Kantor, 1953). In contextualism, laws and principles are *constructed* from interactions among scientists and their subject matters. They are not eternal truths or universal essences, although we may behave towards them as though they were. As Skinner (1974) noted, science is "a corpus of rules for effective action and there is a special sense in which [the rules] could be true if [they] yield the most effective action possible" (p. 259). In other words, the laws and principles of natural science are not discovered, universal truths that exist independently of the behavior of self-actional scientists (or logical positivists). Rather, laws and principles are constructs that are useful means towards the end of successful working with the subject matter (e.g., the prediction *and* control of behavior) and inductive theory construction (see Skinner, 1947). This is Skinner's empirical epistemology, his pragmatic theory of truth (see Zuriff, 1980). The natural-history varieties of contextualism are not necessarily hostile to what we call "the principles of behavior," as long as we recognize the contingent nature of those principles-as-constructs and the possibility of change, no matter how long they have been useful.

This discrimination between these two varieties of contextualism is not a particularly difficult one to make, even as their root metaphor—the "historic event"—remains the same. The truth criterion of both is pragmatic, but here they differ with respect to their philosophical pragmatism. A closer examination of the differences among the philosophical pragmatists with respect to their truth criteria—for instance, from experiential (e.g., "That makes sense to me") to experimental (e.g., prediction and control)—would contribute importantly to making distinctions among the varieties of contextualism.

The Context–Contextualism Distinction

A major source of misunderstanding about contextualism lies in its relationship to "context," a misunderstanding that leads mechanists to dismiss contextualism as slippery and superficial and leads even "contextualists" to misapply the name of that worldview. The misunderstanding is evident in some of the commentaries on my mechanism article, as well as elsewhere in the current behavior-analytic literature (and in psychology; see Houts, 1991). I will not at this point, however, name names beyond the current commentary, because my research into this topic is incomplete, but I can briefly sketch some basic contours of the misunderstanding (see Morris & Peterson, 1993).

The basic problem is that context and what Pepper (1942) described as contextualism are treated as though they are *necessarily* from the same logical category, when they are not. To be specific, context is not restricted to just the worldview of contextualism; it finds a home in other worldviews as well. As for contextualism, it is not the empirical or conceptual analysis of context. It is a worldview, a metatheory. These distinctions can be better appreciated if we examine their respective sources and lineages.

Context. As an ordinary-language word, the etymology of *context* lies in the Latin words *textere,* meaning "to weave," and *con,* meaning "together," giving us

the meaning of context something like "to weave together, interweave, or join together." This is how the term came down through French and into Middle English in the mid-1400s. By the late 1500s, context had acquired a meaning related to language and literature that is today usually its first-listed dictionary entry. This 16th century meaning was "the weaving together of words and sentences; construction of speech, literary composition" (*Oxford English Dictionary* [*OED*], 1989, p. 820). Today's comparable, more complete definition is

The whole structure of a connected passage regarded in its bearing upon any of the parts which constitute it; the parts which immediately precede or follow any particular passage or "text" and determine its meaning. (*OED,* 1989, p. 821)

The second-listed dictionary entry under context is usually a more generic one, something closer to everyday use, for example, (a) "circumstances in which an event occurs: a setting" (*American Heritage Dictionary of the English Language,* 1992, p. 407), (b) "the interrelated conditions in which something exists or occurs: Environment" (*Webster's New Collegiate Dictionary,* 1977, p. 245), and (c) the "circumstances in which a particular event occurs: Situation" (*Webster's II New Riverside University Dictionary,* 1984, p. 304).

Dictionaries of philosophy and psychology usually define context more functionally. According to Reber (1985), for instance, context is

1. generally, those events and processes (physical and mental) that characterize a particular situation and have an impact on an individual's behavior (overt or covert). (p. 153)

This definition, like the earlier ones that specify setting, environment, and situation, I hereafter refer to as the "context-as-place." It is, I think, the usual, but mistaken, meaning of context in discussions of contextualism.

Contextualism

Contextualism is rarely listed in ordinary-language, philosophical, or psychological dictionaries and, when it is, it does not much illuminate contextualism as

Pepper (1942) described it, which is the contextualism in question. Although Pepper was not the first to use the term (see, e.g., Laird, 1929), he may have been the first to apply it explicitly to philosophical pragmatism, which he did first in a 1932 paper, "How to Look at Causality—An Example of Philosophic Method," a paper influenced strongly by Dewey's (1931) paper, "Context and Thought."

Although Dewey's (1931) paper is saturated with "context" (and its cognates), context was not formally defined. Context included, however, "background" and "selective interest." Selective interest referred to the individuality and uniqueness of psychological activity and its products. Background was both "spatial" and "temporal." For Dewey, spatial background referred to the "contemporary setting" and the effects of setting on action and events—context-as-place. This meaning is similar to the ordinary-language meaning of context as "environment" or "situation," except that, for Dewey, spatial background encompassed not just the local physical environment, but reached out broadly, as far as the culture at large. Temporal background, in turn, referred to the "state" of a situation as a function of its past and the effects of states on action and events. Action and events, then, are the products of the process of history—context-as-time—the time in the life or evolution of an individual, cultural practice (e.g., science), or the culture at large. (A closer look at Dewey's philosophy would, I think, yield material supportive of behavior analysis; cf. Blackman, 1991.)

Context-as-time is suggestive of what Pepper (1942) referred to as the root metaphor of contextualism—the "historic event"—and thus is closer to what is definitional about context in contextualism. In Dewey (1931), context-as-history, however, is the key, but not as the mechanistic "arrow of time." Rather, context-as-history is key in the sense that all events are "historic" in that they occur in the context of ongoing change, as the present continuously becomes past for more present (see L. Hayes, 1992). This is the

context in Dewey that Pepper (1942) took as categorical to contextualism, even if we find other senses of context as well. For further support for this interpretation, see Schilpp's (1939) *The Philosophy of John Dewey,* specifically the chapters by Savery (1939), Pepper (1939), and Dewey (1939) (see also Pepper, 1973).

Still further support for this interpretation may be found directly in Pepper's description of contextualism (Pepper, 1942, pp. 232–279). The opening lines of his chapter on contextualism, under the heading "The Contextual Root Metaphor," should suffice:

When we come to contextualism, we pass from an analytical into a synthetic type of theory. It is characteristic of the synthetic theories that their root metaphors cannot satisfactorily be denoted even to a first approximation by well-known common-sense concepts. . . . We are too likely to be misunderstood at the start, even though the basic synthetic concepts do originate in common sense or are, at least, discoverable there. The best term out of common sense to suggest the point of origin of contextualism is probably the historic event. (p. 232)

Pepper presumably could have chosen "context" as the root metaphor of contextualism, for it was certainly a commonsense concept. The commonsense concept Pepper chose was instead the "historic event," which, as I have described, characterizes behavior as ever-evolving, historically contingent on its ever-changing past (Pepper, 1942, pp. 232–233).

Context Again

Although Pepper (1942) makes context-as-history the definition of his contextualism, context-as-place appears as well, with its ordinary-language meaning. This meaning comes up in his discussion of the "quality" and "texture" of an event, that is, respectively, an event's "intuited wholeness or total character" and its "details and relations." Texture (the details and relations) is comprised of "strands" and "context" (Pepper, 1942, pp. 246–252), of which Pepper wrote, "By way of definition we may say that whatever directly contributes to the quality [total character] of a texture may be regarded as a strand, whereas whatever indirectly

MECHANISM AND CONTEXTUALISM contributes to it will be regarded as context" (p. 246). This meaning of context is the meaning we find often (and often only) in behavior analysis. It is the source of misunderstanding for equating contextualism with the study of context (or when context is said to have evolved into contextualism; Marr, 1993, p. 59).[2]

In some cases, of course, context makes no contact with contextualism as a worldview in the behavior-analytic literature. For instance, context may be found in the most recent edition of Charlie Catania's (1992) glossary, where it is defined as "the constant features of an experimental situation" (p. 368). Context has also been used to describe more specific behavioral relationships, for instance, in discussions of the matching law. Here, context refers to the alternative sources of reinforcement—multiple control. And, in the classical conditioning and instrumental learning literatures, context has found extensive use with slightly more technical meanings, often as conditional control of a sort (see Balsam & Tomie, 1985; Mandell, 1986).

Context, however, is becoming perilously close to being used in behavior analysis as a term for which we already have a technical vocabulary. For instance, we find "context" used along a continuum of molarity: (a) sometimes in reference to discriminative stimuli as the "occasion" or "setting" for operant behavior, (b) sometimes in reference to the "three-term contingency" itself, and (c) sometimes in reference to "setting events" and terms that share some meaning with "establishing operations." Worse yet, context is sometimes no more than a vague, nontechnical term that specifies

what we cannot otherwise be precise about (Morris, 1992b, 1992c; Morris & Peterson, 1993; see Marr, 1993; Shull & Lawrence and Staddon, this issue). From my perspective, context-as-place may be most useful if we restrict it to two meanings: (a) one formal, as in initial and boundary conditions (cf. Marr, 1993), and (b) the other functional, as in conditions that alter functional relations within the three-term contingency (e.g., establishing operations for reinforcement; Michael, 1982; see Morris, 1992a). Only the latter can be integrated into our unit of analysis as a term for a process or principle (see Shull & Lawrence, this issue).

Context and Contextualism

My point is a simple one: Contextualism as a worldview is often mistakenly equated with a concern for the contextual determinants of behavior, where context means largely context-as-place. Although this sense of context is categorical in contextualism, it is not defining. Indeed, context-as-place may arise in any of Pepper's (1942) worldviews (i.e., context can be countenanced in mechanism). What is defining about contextualism are its root metaphor (i.e., the historic event) and its truth criterion (i.e., pragmatic, successful working). As terms and concepts, *context* and *contextualism* need to be distinguished. Contextualism is a worldview, whereas context is many things, only some of which are useful in a science of behavior. That some behavior analysts (or psychologists) should take context into account, either empirically or conceptually, does not thereby make them contextualistic in worldview, or "contextualists." It does not make them contextualists any more than someone who studies behavior is necessarily a behavior analyst.

CONCLUSION

I am honored to have had my mechanism article submitted for peer commentary and to be allowed a response. I want to thank my colleagues—Don, Derek, Ted, Dennis, Vicki, Jack, Hayne, Richard and Scott, and John—for the

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[2] I never contended that Skinner was a "contextualist" because he systematically analyzed the effects of "facilitating conditions" or "third variables" on behavior (Skinner, 1930, 1931; see Skinner, 1980, p. 194), or that he ignored "contextualism" when he later dropped these terms (see Marr, 1993, p. 59). This, again, would confuse the study of context with contextualism. Skinner's radical behaviorism, however, did come to have features of contextualism as a worldview (S. Hayes et al., 1988; Morris, 1988a). Only in the latter sense was Skinner a "contextualist."

time and effort they put into the project. They have applied consequences that will differentially strengthen (and weaken) certain aspects of my thinking about mechanism and contextualism in the future. For behavior analysis as a whole, they have offered the variety of conceptual analysis and opinion necessary for selection by consequences to operate on the future of our field. Who among us knows the truth of these matters—of mechanism and contextualism—we cannot say, but I close with a caveat from Skinner (1979, p. 346):

Regard no practice as immutable. Change and be ready to change again. Accept no eternal verity. Experiment.

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