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SOME REFLECTIONS ON CONTEXTUALISM, MECHANISM, AND BEHAVIOR ANALYSIS

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Recent conceptual work in behavior analysis has argued that the discipline is not mechanistic, but contextualistic, in world view. This argument has been contested, however, and a mechanism-contextualism debate has ensued. In taking the side of contextualism, I offer four reflections on the controversy. These concern (a) confusions concerning Pepper's purpose in writing his book and its place in the debate, (b) misunderstandings about the meanings of context and contextualism, (c) the pragmatic implications of theories of truth in world views other than contextualism, and (d) the evolution of ontology from mechanism to contextualism. In the end, behavior analysis may benefit from this debate by evolving as a world view unto its own for its science of behavior. The two–the world view and the science–are inexorably interrelated.

In the 1930s and 1940s, B. F. Skinner (1938, 1945) established a science of behavior (the experimental analysis of behavior) and a philosophy thereof (radical behaviorism) in the tradition of Watson's (1913, 1930) classical behaviorism. Watson's behaviorism was commonly regarded as mechanistic and, as a consequence, so too was Skinner's. Although Skinner and others maintained that behavior analysis was not an S-R psychology (see Ringen, 1976; Skinner, 1969, 1974), the presumption that it was mechanistic remained the received view (Harré & Secord, 1973; Taylor, 1964), a view formalized in the 1970s by Reese and Overton (1970; Overton & Reese, 1973) on the basis of Pepper's book, *World Hypotheses: A Study in Evidence* (Pepper, 1942, pp. 186-231). Although behavior analysts continued to object to having their discipline described as an S-R

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psychology (Moxley, 1984), they did not formally contest Reese and Overton's (1970) characterization of it as mechanistic in world view until the 1980s, at which time an alternative among Pepper's views was proposed as more apt and accurate—contextualism (see Hayes & Brownstein, 1986; Hayes, Hayes, & Reese, 1988; Morris, 1982, 1988, 1993a).

The objection to mechanism and the proposal for contextualism were welcomed by some behavior analysts (e.g., Carr, 1993; Ruiz, 1995), but not all. A number argued that behavior analysis was actually both mechanistic and contextualistic (e.g., Blackman, 1993; Reese, 1993). Others argued against contextualism because it was not productive (e.g., Shull & Lawrence, 1993) and might even undermine a science of behavior (e.g., Staddon, 1993). Some argued that, although Pepper's "discrete mechanism" was unsuitable to a science of behavior, "consolidated mechanism" was not (e.g., Delprato, 1993). A few others argued for a more modern version of mechanism found in the contemporary physical sciences (e.g., Marr, 1993). Taken together, this might be called the mechanism-contextualism debate (see Morris, 1993b).

The debate is a complex one and extends beyond what can be reasonably summarized in this paper. Moreover, it may not be familiar to all the readership of this journal, as it has appeared mainly in *The Behavior Analyst* (*TBA*). I apologize, then, for my seemingly cursory coverage of this material, but my purpose is not to review the particulars, but to reflect on the debate. For further background, I refer readers to the two 1993 issues of *TBA* (see also Hayes et al., 1988; Morris, 1988).

In what follows, I take the side of contextualism and comment on four issues. The first pertains to confusions about Pepper's (1942) *World Hypotheses*, specifically, his purpose in writing the book and its place in the debate. The second concerns misunderstandings about the meanings of context and contextualism. The third involves theories of truth, specifically, how the theories of truth in world views other than contextualism become, in contextualism, varieties of successful working. The fourth relates to the possibility that the mechanism-contextualism debate may not be a debate between two relatively adequate world views, but rather the product of a process—evolutionary ontology.

Pepper's Purpose and Place

A thorough analysis of the mechanism-contextualism debate requires, of course, delving deeply into the history and philosophy of science, but for present purposes I go back no further than to Pepper's (1942) book. It is a source of debate for at least two reasons. First, Pepper is sometimes thought to have undertaken an impossible exercise in German Idealism and, second, his project is seen as nonsensical because it cannot be verified in the logical positivist tradition (see Place, 1994). These views and others, however, are in part the result of reading Pepper at more than face value. That is, we not only read *in* Pepper, but

into him as well, in accord (or dis-accord) with our already established views. And, where we do not read Pepper (1942) at all, but only the secondary sources, then the problem is compounded.

Pepper's Purpose

Matters might be clarified if we knew more about Pepper's (1942) purpose in writing his book, but we have little more than what we find in his Preface, where he wrote: "The origin of this book goes 'way back to a consuming personal desire to know the truth" (p. vii). Although this suggests that Pepper might have been searching for some one fundamental philosophy (see Leigland, 1994), after he surveyed the material he was going to cover, he was more circumspect:

> [H]ere is the solution that seems best to one man, living in the first half of the twentieth century, who has passed through most of the cognitive experiences we have been subject to: religious creed, philosophical dogma, science, art, and social revaluation. Possibly here is also a present crystallization of some twenty-five centuries' struggle and experience with the problem of how men can get at the truth in matters of importance to them. (p. ix)

Pepper was a modest man, not a "great philosopher." He was simply trying to make sense of the welter of philosophical and scientific ideas and concepts found in his day. His purpose was simply to describe the then "relatively adequate" world views, which he gave generic labels, and called world hypotheses—formism, organicism, mechanism, and contextualism.

In presenting these views, Pepper (1942) identified them in terms of their respective common-sense root metaphors and theories of truth. The root metaphor of organicism, for instance, is organic growth (e.g., development through integration; Piaget & Inhelder, 1969). Its theory of truth is coherence, for instance, the coherence of our understanding of one aspect of nature with our understanding of other aspects, with an emphasis on their logical relations. In Pepper's (1942) view, organicism was commonly called "absolute (or objective) idealism" (pp. 141-142) and was associated with Schelling, Hegel, and Royce. Mechanism's root metaphor, in turn, is the machine and its parts. Its theory of truth is a causal-adjustment version of correspondence, for instance, the correspondence of theories about nature with facts and evidence thereof, usually as predictions deduced from hypotheses to data. Pepper (1942) aligned mechanism with what was often called "naturalism or materialism and, by some, realism" (p. 141), as these were associated with Democritus, Galileo, Descartes, Hobbes, Hume, and Reichenbach. Pepper, himself, had earlier adopted and then rejected idealism and logical positivism (viz. mechanism) as fully adequate philosophies. In his book, though, he included them, along with formism and contextualism, as the four relatively adequate world views in history of his day.

All scientists work from a perspective, from their own histories, some of them long-standing traditions in philosophy. Pepper was not prescribing any one of these traditions in pointing this out, although he did later lean toward contextualism (see Pepper, 1966). His overall project might have been nonsensical on logical positivist grounds, but those grounds largely applied to only one view—mechanism—which Pepper was presenting, comparing, and contrasting with the three others, each with its own theory of truth. In any event, these logical positivist grounds are "universally discredited" in philosophy today (Place, 1994), although they persist among scientists.

Pepper's Place

Pepper's (1942) book is a source of debate for a second reason: the central role it has assumed in framing the issues. There is no necessary reason why this must have occurred, but some causes may be found in contingencies. First, Reese and Overton (1970; Overton & Reese, 1973) formalized the mechanistic characterization of behavior analysis in terms of Pepper's (1942) "study in evidence." Thus, the pros and cons of their analyses had to contend with Pepper (e.g., Morris, 1988). Second, Pepper's book was reviewed in the *Journal of the Experimental Analysis of Behavior* (Hayes et al., 1988), and thus the world views, as he described them, naturally became the basis for subsequent discussion (e.g., Hayes, Hayes, Reese, & Sarbin, 1992). Under these circumstances, it would have been peculiar if Pepper (1942) did not have a central place in the debate.

Pepper's (1942) renderings of these views, though, need not constrain the debate; indeed, they should not. The debate would be better informed if scholarly inquiry were updated and extended. For instance, it needs to be enlightened about the nature of mechanism as it is contrasted with functionalism (see Chiesa, 1994; Moxley, 1992) and perhaps about mechanism as found in the modern physical sciences, for example, in dynamical systems theory (see Marr, 1993). For instance, when Marr (1982) wrote, "The abandonment of mechanistic determinism should not be viewed by behaviorists with despair, but rather be looked upon as liberating (as it has been for physics)" (p. 207; contra. Baer, 1993; Vaughan, 1983), he was writing about an earlier, Newtonian version of the mechanistic world view, not about a newer one. Marr did not mean we should abandon modern instantiations of mechanism (see Marr, 1996), although I doubt we have abandoned the old ones (Morris, 1993a). The debate also needs to be illuminated by advances in philosophy, for instance, in theories of truth (Hocutt, 1994) and philosophical pragmatism (e.g., Rorty, 1982). Moreover, theories of truth and truth criteria are sometimes conflated (see Place, 1996, 1997). This work will be necessary and challenging, but at present it is beyond my purview. Thus, I turn to misunderstandings about the meanings of context and contextualism.

Contextualism and Context

Contextualism

As with the other world views, Pepper (1942) declined, at first, to identify contextualism with any of the classical philosophers and philosophies. Later on, however, he was more revealing. He noted that contextualism was commonly called "pragmatism," as found in the work of Charles S. Peirce, William James, Henri Bergson, John Dewey, and George Herbert Mead (cf. Blackman, 1991; Pronko & Herman, 1982). Philosophical pragmatism was (and is) America's unique contribution to philosophy.

Why Pepper (1942) did not name pragmatism "pragmatism," but instead "contextualism," he never said. My guess is that although pragmatism specified a theory of truth—successful working—it did not suggest a common-sense root metaphor by which the world view could be identified. "Contextualism" did suggest just such a metaphor—context—but context has more than one family of meanings. The possibility that these meanings might be conflated may have prompted Pepper to choose the more specific (albeit opaque) root metaphor of the "historic event," yet retain contextualism as the name for the world view. The evidence for this goes briefly as follows.

As far as I know, Pepper (1932) first used the term "contextualism" in a 1932 reference to John Dewey's pragmatism where Dewey (1931) was writing of "context." In writing of context, Dewey's primary referent was the historical context of action, that is, the historical situatedness of the meaning and function of behavior. Even more specifically, Dewey was emphasizing that the historical context was not a place or a thing, or an arrow of time, but an ever-changing dynamic relation, as present meanings and functions become the past for more present. This is aptly captured in the aphorism attributed to the Greek philosopher, Heraclitus: "No one ever steps into the same river twice." What did Dewey think of Pepper's analysis of his work? Dewey (1939) wrote that he was a "contextualist."

An aside: In contextualism so construed, individuals are historical entities, whose analysis yields natural history, not natural science. This does not preclude, though, a natural science of behavior, yielding "ahistorical" laws or principles (e.g., reinforcement), even though those principles actually may evolve—operant processes are the product of natural selection.

Context

In any event, as noted, the meaning of context in contextualism was (and is) the "historic event," which is also, as I mentioned, the defining root metaphor of contextualism. Another family of meanings for context, though, is found in the behavioral, social, and cognitive sciences, which has confused matters greatly (Morris, 1993b). These meanings are aptly described in my WordPerfect 5.1 thesaurus as "background, circumstances, conditions, framework, setting, and situation." These

emphasize context-as-place, not context-as-history. When these meanings are taken to define contextualism, then contextualism may be little more than a complex version of mechanism (e.g., Shull & Lawrence, 1993; contra. Morris, 1993b). Let me illustrate this with an analysis of causal relations.

The scientific methodology of mechanism is said to be one in which, when other things are equal or when context-as-place (e.g., establishing operations) and as-history (e.g., reinforcement history) are held constant, causes produce completely determined states of affairs. To accept "other things being equal" or to hold context constant is, of course, to acknowledge that behavior is multiply determined. This is not doubted by anyone, but multiple causation is not thereby contextualism, where multiple causes are considered context for every otherwise completely determined state of affairs. Just because we can point to multiple causes and call them context does not mean we have identified the world view of contextualism—as a reading of Pepper (1942) would clarify. Where contextualism is mistaken to mean multiple causation (see Place, 1994), then debates about whether behavior analysis is mechanistic or contextualistic cannot help but arise.

Theories of Truth

As for contextualism's theory of truth—successful working—Pepper (1942, pp. 268-279) sometimes seemed to equivocate over whether it captured all of the theory of truth in this view. He implied that "successful working" might also include (a) "qualitative confirmation," that is, the confirmation of what we understand about how the world works with what else we understand about it (e.g., understanding our understanding) and (b) "verified hypotheses," that is, the verification of hypotheses about how the world works through predictions derived from theory (e.g., the hypothetical-deductive method).

This implication raises a problem: Qualitative confirmation is close to the organicist's coherence theory of truth, while verified hypotheses is akin to the mechanist's causal-adjustment version of correspondence. If contextualism adopts theories of truth that belong to the other world views, then so much the worse for the distinctiveness of contextualism. I suggest, however, that in contextualism these other two theories are varieties of successful working and that we quite naturally use all three theories, depending on purposes (cf. Place, 1996). Given that pragmatism was the view Pepper (1942) called contextualism, these variations in successful working are a dimension along which contextualism (and the behavior of scientists) varies (cf. Hayes et al., 1992). Let me try to support this.

C. S. Peirce was the first to define pragmatism. For him, it was a matter of *finding* the truth of a belief. This truth was found, he wrote in "its conceivable bearing on the conduct of life" (Peirce, 1905), where the test was public, social, and objective. In behavior analysis, this is

effective action, typically conceptualized as prediction and control (Morris, Todd, & Midgley, 1993). For William James (1907), in contrast, pragmatism became a matter of *making*, not finding, truths. For James, too, the truth of a belief was also found in its conceivable bearing on the conduct of life, but for him the conduct of life was not only public, social, and objective, but also private, personal, and subjective. The latter, for instance, included beliefs that simply "made sense" or that, in his words, "produced intellectual satisfaction." This Jamesian theory of truth is close to that of coherence, and thus seemingly at odds with successful working.

Something like coherence, however, is found in behavior analysis. I reference here, for instance, comments Skinner made about why he wrote *Verbal Behavior* (Skinner, 1957):

> I was *interpreting* a complex field using principles that had been verified under simpler, controlled conditions . . . . I decided to leave out all experimental data. (An interesting question then arose: what survived to reinforce writing or reading the book? Was not [experimental] confirmation the be-all and end-all of science?) It was a question concerning my own behavior, and I thought I had an answer . . . resulting *order* instead of *confirmation*? (My reinforcers were the discovery of uniformities, the ordering of confusing data, the resolution of puzzlement.) (emphasis in the original; Skinner, 1979, p. 282)

These are the consequences of successful behavioral interpretation or of conceptual analysis, more generally. They may be found as well in productive thinking, problem-solving, decision-making, and self-control, about which Skinner wrote often (e.g., Skinner, 1953, pp. 227-294; 1969, pp. 133-171; 1974, pp. 113-131). These activities lead to the discovery and control of variables of which our own behavior is a function—and are reinforced because they do so. When the activities are effective, we are able to act on facts and events in new and more useful ways—for ourselves, at first, and then maybe later for others. Let me organize these points more formally below.

Three Pragmatic Theories of Truth

Coherence. What I have described is, in part, a pragmatic theory of a coherence theory of truth—truth through qualitative confirmation or coherence in behavioral interpretation. This is prediction and control in the service of understanding not our subject matter, but our behavior as scientists (i.e., its non-contradiction). Increases in coherence, for instance, in its scope and precision, strengthen and maintain this activity. This is close to the pragmatism of James (1907), where making truth was private, personal, and subjective, and related to a common-sense, ordinary-language meaning of "understanding."

In behavior analysis, we pursue a truth criterion of this sort when we can do no better. For instance, when we cannot predict or control the world around us, we must be satisfied with interpretation (see Skinner,

1974, p. 176). Still, coherence through qualitative confirmation is only provisionally acceptable as successful working. It is not the most useful theory of truth where other, more effective theories are available, for we will want to ask after the truth of coherence, qualitative confirmation, and behavioral interpretation.

Correspondence. A more satisfactory theory of truth is one in which coherence through qualitative confirmation affords verified hypotheses or prediction, that is, a causal-adjustment, correspondence theory of truth. This constitutes *not* confirmations or predictions concerning one's own behavior, but confirmations and predictions concerning the world with which we interact. This too is sometimes the best we can do. We do this when we cannot demonstrate controlling relations, but only make predictions. Even this theory, though, is provisional. Eventually, we want to know about the truth of correspondence via causal adjustment—correspondence achieved through verified hypotheses. That is, we ask about the truth of the predictions we make about behavioral relations.

Operational. For this, a more satisfactory theory of truth is one in which correspondence through verified hypotheses affords an operational theory of truth through successful working, that is, effective action through experimental control. This third criterion offers a measure of success akin to Peirce's (1905) pragmatism. On this view, the test of the truth of correspondence and verified hypotheses lies in their bearing on our control of our subject matter, that is, on experimental analyses of behavior reinforced by the discovery of functional relations.

Summary

Although Skinner (1979, p. 282) informally embraced a coherence theory of truth, and presumably as well correspondence through causal-adjustment, he was ultimately a philosophical pragmatist of a Peircian sort, whose theory of truth was successful working or, more specifically, effective action through prediction-and-control (see Zuriff, 1980). Although Skinner did not formally write about theories of truth, he often spoke about the "the goodness of a concept" in ways that cohere with the three theories:

> The ultimate criterion for the goodness of a concept is not whether two people are brought into agreement, but whether the scientist who uses the concept can operate successfully on his material—all by himself if need be. What matters to Robinson Crusoe is not whether he is agreeing with himself but whether he is getting anywhere with his control over nature. (p. 285)

Contextualism, then, embraces the theories of truth of the other world views as varieties of successful working, that is, as pragmatic ends in themselves. Understanding the diversity among these theories allows us to appreciate better the variations we find in the world view of

contextualism (see Hayes et al., 1992), for instance, in the humanities (e.g., history; see Bohan, 1990), the social sciences (e.g., social psychology; see Sarbin, 1977), and the natural sciences (e.g., behavior analysis; see Morris, 1988).

Evolutionary Ontology

Evolutionary concepts are found throughout behavior analysis (see Glenn & Madden, 1995). Skinner (1966, 1975), for instance, drew an analogy between (a) natural selection in evolutionary biology and (b) the reinforcement of operant behavior. He referred to the former as "phylogenic contingencies" (e.g., natural selection), which explained phylogenic (or innate) behavior, and to the latter as "ontogenic contingencies" (e.g., positive reinforcement), which explained ontogenic (or learned) behavior. He later applied the analogy to the social sciences in order to explain cultural practices and their survival (Skinner, 1981). Given that science is both a behavioral and a cultural practice, then it too is presumably subject to a selectionist account.

This is not news in the philosophy of science. A related view was expounded in the first postpositivist analysis of science—Kuhn's 1962, *The Structure of Scientific Revolutions.* Kuhn (1962) argued that science was shaped and maintained not only by facts and logic, but also by personal, social, and professional practices. He called these practices "paradigms," of which there were two components: shared exemplars and a disciplinary matrix. Shared exemplars are models for the conduct of science, for instance, experimental preparations for studying operant behavior (see Ator, 1991) and conceptual tools such as the three-term contingency (Skinner, 1938). In turn, the disciplinary matrix comprises a paradigm's ontological and epistemological assumptions. Ontological assumptions are presuppositions about the nature of nature, for instance, about monism, materialism, and determinism. Epistemological assumptions, in turn, are presuppositions about the nature of knowing, sound knowledge, and truth, for instance, assumptions concerning positivism, empiricism, and pragmatism. When epistemology and ontology are formally systematized, they constitute philosophies of science; when they are informally organized, they constitute world views; when they are unknown, we do not speak of them, but they are not thereby irrelevant to describing the behavior of scientists.

Kuhn's (1962) analysis of scientific change in terms of paradigmatic "revolutions" has now given over to more evolutionary accounts, for instance, those offered by Toulmin (1972), Laudan (1977), and Hull (1988). If science is accounted for in evolutionary terms, then so too should its disciplinary matrix and then, by inclusion, its epistemology and ontology. The observation that epistemology is subject to a selectionist account is also not new: Popper (1972) founded a branch of philosophy called "evolutionary epistemology," which accounted for knowledge on the basis of a "scientific understanding" of human nature and human

behavior, wherein knowledge is a function of trial-and-error learning and selective elimination through falsification in theory construction (see also Campbell, 1974).

Ontology, in contrast, has no current evolutionary account, perhaps for the following *wrong* reason. Epistemological claims about knowing, sound knowledge, and truth may be said to reflect the behavior of scientists in interaction with their subject matter and, as such, can be experimentally tested, the consequences of which differentially strengthen or weaken those claims. Ontological claims, in contrast, are taken to reflect less the behavior of scientists and more the nature of nature. These claims are presumably either true or not—and are not subject to test. If they are not subject to test, they cannot evolve.

On the evolutionary account I am suggesting, this latter view is, as I said, mistaken. Ontology does indeed reflect the behavior of scientists—the behavior of making and acting on (or in accord with) ontological assumptions. These assumptions are subject to empirical test in terms of their usefulness and effectiveness (or not) in the long run, and thus they, too, evolve. If ontology evolves, then the world views described by Pepper may represent different phases in the evolution of science. To support this, I turn to three accounts of the history of science that converge on a common lineage. These may be found, one each, in physics, psychology, and philosophy (see Delprato, 1986, pp. 65-68; cf. Comte, 1975, on three stages in the human history of explaining how the world works—theological, metaphysical, and scientific).

The Evolution of Science

The first modern statement of the evolutionary perspective I am suggesting was probably Einstein and Infeld's 1938 *The Evolution of Physics*, which described how physics had evolved from substance theory, to the mechanical view, and then to field theory (Einstein & Infeld, 1938). A second account was offered by Kantor in his 1946 article, "The Aim and Progress of Psychology." He described corresponding changes along a "scale of scientific progress" from the substance-property stage, to the statistical-correlational stage, to integrated field theory (Kantor, 1946). The third example is found in Dewey and Bentley's 1949 philosophical collaboration—*Knowing and the Known*—where they described three "levels of action"—self-action, interaction, and transaction (Dewey & Bentley, 1949; see Pronko & Herman, 1982).

Integrating across these accounts, we have, in the first phase, physical events produced by their own self-contained, self-actional substances whose inherent properties account for them. Examples of self-action include phlogiston in physics, vitalism in biology, and soul, psyche, and mind in psychology. In the second phase, we find the mechanical view of causal determinism, where causes lie in factors acting on objects in absolute time and space. Examples of mechanism include the stimulus-response psychologies and computational models of the mind. The former involve muscle twitches and the latter on-off

switches; there is not much conceptual difference. In the third phase, events and actions occur at particular points in the ever-changing interrelation of all their interdependent conditions, in their evolving functional relations in a field or system of factors—as reciprocal, transactional relations.

With these three parallel descriptions of the evolution of science as precedents for an evolutionary ontology, I do Pepper (1942) the disservice of suggesting that his world hypotheses are not as "relatively adequate" as he had surmised. Their adequacy has perhaps been changing, possibly reflecting the evolution of ontology across formism/organicism, mechanism, and contextualism in ways that parallel the three just-mentioned accounts of the evolution of science. If evolutionary ontology is a viable concept, then this may explain certain aspects of the mechanism-contextualism debate, for behavior analysis is presumably still evolving—perhaps from mechanism to contextualism—with tensions, strains, and debates in the process (and sometimes "red in tooth and claw").

Conclusion

In the foregoing, I have offered some reflections on the mechanism-contextualism debate, specifically, on Pepper's (1942) project and its place in the controversy, the meanings of context and contextualism, contextualism's theories of truth, and evolutionary ontology. Each topic deserves, of course, separate and extended treatment. In pursuing this, behavior analysts will advance the evolution of the science of behavior and a philosophy thereof, that is, a science (a) of the behavior of organisms and (b) of the behavior of analyzing the behavior of organisms. The two proceed together hand-in-hand in a "bootstrap" fashion (see Skinner, 1945, p. 277). One possible consequence of this is that behavior analysis will enunciate its own unique world view. Pepper (1942), of course, is not logically necessary for arriving at this end, but I see no virtue in ignoring antecedents in the history and philosophy of science that might engender variants for the selection of more successful working, both with our subject matter and with ourselves.

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