

Name of Author: Edward K. Morris

Title of Manuscript: `Some reflections on Contextualism,
Mechanism and Behavior Analysis'

Date Received: August 21, 1996

Date Review completed: September 19, 1996,

Have you written comments
on the ms. that you wish the
author to see: NO

Paragraphs 1 and 2

I take it that not all readers of *Psychological Record* are members of ABA and that, of those that are, not all will be familiar with, let alone subscribers to, the curious metaphysical doctrines of the late Stephen C. Pepper, doctrines which, since Skinner's death, appear to have acquired a powerful hold over certain sections of the behavior analytic community. If I am right in this, to describe J. B. Watson as holding "a mechanistic world view" before explaining what is meant by this piece of Pepperian jargon, as Morris does in his opening paragraph, is premature.

It is equally tendentious to suggest, as he does in the same paragraph, that it was only thanks to their adoption of Pepper's views that behavior analysts began to challenge the early behaviorist conception of behavior as consisting of a string of mechanical reflexes or stimulus-response connections. Morris is quite right to point out that in his early work and, to some extent, throughout his career Skinner's writing retains many features which reflect this earlier "mechanistic" model, for example, the use in his early work of the term "reflex", later abandoned, his retention to the very end of the terms "stimulus" and "response", despite his insistence that operants are spontaneously "emitted" and that discriminative stimuli are "occasions" for the emission of the responses they control, and his retention of a purely mechanistic push-button account of respondent (i.e., Pavlovian) conditioning. But to attribute the emancipation of behavior analysis from this mechanistic tradition to the adoption of Pepper's ideas is to ignore the extent to which Skinner himself broke away from it towards a much more flexible and multi-factorial analysis of the interaction between behavior and its environmental context. This appears in his early identification of the phenomenon of intermittent reinforcement which led to the identification of the so-called "schedules of reinforcement" and forces us to look beyond the individual stimulus-response connections emphasized by trial by trial learning procedures, in his insistence, already mentioned, that operants are "emitted" and that discriminative stimuli are the "occasions" for that emission, the later replacement of the concept of conditioning by that of the "contingency-shaping" of

behavior, etc., etc.

What I am suggesting is that instead of plunging us straight into the spurious mechanism-contextualism debate - "spurious" because no one now would want to defend mechanism in the sense in which Pepper uses that term, if indeed they ever did - it would be better to begin by pointing out

- (a) the extent to which behavior analysis has moved away from the conception of behavior as a chain of mechanical reflexes,
- (b) that the critics of behaviorism systematically refuse to believe that any such change has or even could take place,
- (d) that adopting Pepper's Contextualism as their scientific ideology appeals to behavior analysts, because it enables them to put "clear blue water" between their current position and their mechanistic past.

But before proceeding to (c) a thumbnail sketch of Pepper's doctrine is needed, together with an explanation which is presumably provided by (c) of why, of the four "world views" described by Pepper, it is only mechanism and contextualism that seem to interest behavior analysts. Morris does in fact eventually give us an exposition of Pepper, but only *after focusing on the mechanism-contextualism "debate" which would be unintelligible to anyone who was not already familiar with Pepper's views.

Pepper's Metatheoretical Project (ms. p. 2)

The first sentence of this section, implying as it does that Pepper is a major figure in the history and philosophy of science during the twentieth century, alongside names such as Popper, Lakatos, Kuhn and Feyerabend, is to invite ridicule from anyone with a professional commitment to that field of enquiry. I don't have access to a history of the history and philosophy of science during twentieth century, if indeed one exists; but I would be surprised if Pepper were to receive more than the briefest of mentions in such a work. In the only history of nineteenth and twentieth century philosophy on my bookshelves, the 2nd (1968) edition of J. A. Passmore's *A Hundred Years of Philosophy*, Pepper is mentioned only in a footnote to a brief discussion of the German metaphysician, Nicolai Hartmann in a chapter (Chapter 13) on 'Recalcitrant Metaphysicians'.

The second sentence in this section cites my 1994 presentation to an ABA symposium on 'The Bogy of Mechanism' as the source for the view that Pepper's

"project is seen as nonsensical because it cannot be verified in the logical positivist tradition."

Since that paper has, unfortunately, never been published, I have no documentary evidence, other than what is on my computer, to support the claim that that

was not what I was saying. I do not and did not in the paper subscribe to the verification principle in the form in which it was held by the logical positivists, in other words, the view that the meaning of a statement is its method of verification which implies that a statement that is in principle unverifiable is meaningless nonsense. I mentioned the principle in the paper, but only to dismiss it as now "universally discredited". What I did claim was that Pepper, along with the whole Hegelian tradition to which he belongs, is conceptually confused. I focused in particular on two conceptual confusions in Pepper:

- (1) a confusion about the nature of the causal relation, and
- (2) a confusion between the nature of truth (what it means to say of statement that it is true) and the criteria we use to discriminate true propositions from false ones.

Of these only first was actually discussed in the paper. The second was, however, raised in the somewhat acrimonious discussion that followed. On the issue of causation I suggested that the confusion manifests itself in a failure to appreciate that causes are always multiple and that a mechanical causal relation in which the same cause invariably produces the same effect occurs only when all other factors are held artificially constant as in a controlled experiment. That mechanical causation of this kind applies within the nervous system at the level of the synapse cannot be denied; but at the molar behavioral level where the organism must adapt to multi-factorial causal relations in the contingencies operating in the environmental context only a contextually sensitive system can hope to succeed. The two types of causation apply at different levels of analysis. Properly understood, there is no conflict between them.

The second confusion, that between the nature of truth and that by which we discriminate it manifests itself

- (a) in the mistaken belief that the correspondence theory of truth is a "truth criterion", when in fact it is an account of what it is* for a statement to be true, and
- (b) in the equally mistaken supposition that it makes sense to separate out the different truth criteria and allocate them to different "world views".

I will not attempt to repeat the arguments for thinking that these beliefs are mistaken here. What I will do is send Morris a copy of the 1994 paper, together with a copy of a paper which has recently appeared in *Communication and Cognition* with the title 'The picture theory of meaning and its implications for the theory of truth and its discrimination'. Morris heard that paper later in the same year when it was presented under the title 'Linguistic behaviorism as a philosophy of empirical science' at the Second International Congress of Behaviorism and the Behavioral Sciences at Palermo, Italy, in October 1994. Though not specifically aimed at

the Pepperian position, it aims to disentangle the kind of confusion which underlies Pepper's account of "truth-criteria". Another recent publication which may be relevant, because it includes both an account of truth and its discrimination and* a theory of causation is a paper with the same title as the Palermo presentation ('Linguistic behaviorism as a philosophy of empirical science') which has just appeared as Chapter 9 of W. O'Donohue and R. Kitchener (Eds.) *The Philosophy of Psychology*. London: Sage, pp. 126-140. It may be that by citing these publications, Morris can avoid the awkwardness of having to cite a presentation which is unpublished and likely to remain so.

In replying to what he takes to have been my criticism of Pepper's position, Morris claims that behavior analysts have read more into Pepper than can be derived from the text itself. This may well be the case; but since what I was criticizing was what I have learned about Pepper's work from behavior analysts, rather than from reading Pepper himself, this is beside the point. It does, however, reinforce the need to explain to the reader what it is that attracts behavior analysts to Pepper and what it is that they are inclined to read into his account that is not in the text itself.

PP.3-4

I don't think it is acceptable to describe "organicism and mechanism" as "long-standing traditions in philosophy". There is certainly a long tradition going back to Descartes of using mechanical models in explanation; but there is no association between this approach to explanation and the adoption of a particular "truth criterion". Descartes, for example, held what Pepper would classify as a coherence theory of truth. There is no such tradition associated with the term "organicism". Flew's (1979) *A Dictionary of Philosophy* mentions organicism as a species of holism which holds that

"some systems that are not literally organisms are nevertheless like organisms, whose parts can only be understood in relation to their functions in the complete and ongoing whole."

The names of Plato, Hegel, Marx and Jan Smuts are mentioned in connection with holism. No names are given in connection with organicism.

P. 4

The rhetorical question

"What is science if not applied philosophy?"

should be deleted. Not only is a rhetorical question such as this out of place in a serious piece of philosophical exegesis; but this particular question assumes, contrary to fact, that the claim that science is applied philosophy, even if we knew what that means, is

uncontroversial which it most certainly is not.

"Pepper was not prescribing a fundamentalist philosophy"

The terms "fundamentalism" and "fundamentalist philosophy" are not terms any philosopher would use. The term "foundationalism" has recently acquired some currency as a description of those epistemological theories which hold that a statement is true only in so far as it can be deduced from or otherwise "reduced" to self-evident first principles or incorrigible observation statements. But that is not what you are referring to here. What you mean is that Pepper was not, in the 1942 book at least, endorsing any one of the four "world views" he describes.

"He might have been nonsensical on logical positivist grounds, . . ."

As already mentioned, this is not what I am accusing him of being.

"but those grounds apply only in one view -- in mechanism -- . . ."

This would appear to assume

- (a) that what Pepper's critics (of whom I am the only one named) are claiming is that his views are nonsense because unverifiable,
- (b) that the kind of verification being demanded is verifiability in accordance with a particular truth criterion (the so-called "correspondence truth criterion") which, according to Pepper, is recognized only by those committed the mechanist "world view", and
- (c) that consequently those who adopt another world view (contextualism) can safely ignore any such argument.

Since both the premises of this argument, (a) and (b), are false, the conclusion (c) is unsupported.

P. 6

Morris' account of the scientific methodology of mechanism is an admirably clear exposition of the experimental method, as described by Mill (1843) in his method of "Concomitant Variation" and as brilliantly applied to the experimental analysis of behavior by Murray Sidman (1960) in his *Tactics of Scientific Research*. However, he then goes on to challenge, though without indicating its source, the claim I made, in the 1994 ABA presentation referred to earlier, that contextualism is nothing more than an emphasis on the causal role of the multiple causal factors which the experimentalist must hold constant in order to evaluate the effect of the particular independent variable he or she is studying:

"Just because we can point to all the causes

called context does not mean we have identified the world view of contextualism. Contextualism belongs to a different logical category."

But what "logical category" *does* Pepper's contextualism belong to? What differentiates that logical category from that to which the multiple causal factor story consigns it? What theory of logical categories is at work here? Aristotle's? Kant's? We are not told. What we *are* told is that

"When the categories are confused, debates about whether behavior analysis is mechanistic or contextualistic cannot help but arise."

This invites the retort that it is only when mechanism and contextualism are treated as distinct "world views" that any conflict between them appears. If we accept that contextualism is simply the view that every causal relation involves a multiplicity of causal factors both historical and immediate and that mechanism is, as Morris describes it, the procedure whereby all those background causes must be held constant in order to evaluate the effect of any one of them, there is no conflict between the two. They are two sides of the same coin.

P. 7

The phrase

"mechanism's correspondence theory of truth"

implies that you believe with Pepper

- (a) that the correspondence theory of truth is a "truth criterion", i.e., a theory about how we tell* whether or not a statement is true, the theory that we can tell that it is true, if it corresponds to observational evidence for it,
- (b) that the correspondence theory in this sense is one of three alternative truth criteria between which we can choose when deciding how to conduct the scientific enterprise, the other two being the coherence criterion and the pragmatic criterion, and
- (c) that the use of mechanical models for the purpose of explanation in science is invariably associated with the adoption of the so-called "correspondence" truth-criterion.

I believe that all these three propositions are false, and I would suggest that other readers would share that view. If so, it would be wrong to use the phrase "mechanism's correspondence theory of truth" without acknowledging that both this use of the term "correspondence theory of truth" and the alleged connection between what Pepper refers to by that expression and mechanistic approaches to explanation are controversial.

P. 10

The paragraph on the correspondence theory of truth

confuses two senses of the term:

- (a) the correspondence theory of what it *means* to say that a statement is true, and
- (b) the correspondence theory of *how we tell* whether it is or not.

In (a) the correspondence is between the event or state of affairs specified in the statement and an event or state of affairs which actually exists at the space-time location specified in the statement. In (b) the correspondence is between the statement and the observational evidence for it, evidence, in other words, that a state of affairs corresponding to that specified by the statement actually exists at the relevant location in time and space. It should be obvious that correspondence between a statement and the observational evidence for it, is no final guarantee that the event or state of affairs for whose existence it provides evidence actually exists and that the statement is, therefore, true. That applies on *any* account, not just "on a behavior analytic account".

P. 11

It is worth pointing out that in the quotation at the top of the page, Skinner is talking about the goodness of a concept, not about the truth of a statement or theory. Concepts cannot be true or false; so no truth criteria apply. Some concepts, such as that of a unicorn or a centaur have no instances other than in legend and pictorial representation; but this is seldom, if ever, an issue in deciding whether or not to adopt a particular concept for scientific purposes. Here pragmatic considerations (what works best) are the only ones that count. Of course, there is a connection between the goodness or otherwise of a concept and the truth of the statements it allows us to formulate. But the relation is not a simple one. The goodness of a concept is not just a matter of how many true statements it allows us to generate. Their quality is important too. No doubt the quality of such statements is partly, as Skinner suggests, a matter of their utility in allowing a Robinson Crusoe to get somewhere "with his control over nature". But in pure science it may simply be a matter of what yields the best predictions of what is observed, the best overview of the field or the most comprehensive links between the statements from its different subdivisions.

P. 13

I am puzzled by the claim that ontology, unlike epistemology, "has no current evolutionary account". Evolutionary epistemology, I understand it, is the claim that knowledge, in *any* field of enquiry, evolves through a process of variation and natural selection or trial-and-error learning. There is no suggestion that it should be restricted to knowledge of epistemological matters, i.e., to the knowledge we have of the process whereby knowledge itself is acquired. So why should it be supposed to exclude such knowledge as we have (it is

not obvious that we have *any*) of matters ontological? Ontology is a science, if that is the right word, within which there is notoriously little agreement between the philosophers who take a position on such matters. But, like Morris, I can see no reason to suppose that, if and when such agreement is achieved, it will not emerge from the same kind of trial-and-error-correction process as applies in other fields of enquiry. As Morris seems to be suggesting, the principal reason for the failure to reach agreement on ontological issues (issues concerning what different kinds of entity can and cannot be said to exist) is that there are no obvious tests that can be applied in deciding between the various alternative ontologies that have been proposed. He seems to think, however, that, given a selectionist epistemology, the absence of such tests should be no barrier to progress in this direction. But, if there are no tests that can be agreed, there is no way of telling when an ontology has succeeded and when it has failed, and hence no way that the right ontology can be identified and evolve into an accepted body of knowledge.

PP. 14-16

There is clearly something important and valuable in the suggestion that, as science evolves, it begins with explanations which rely on the distinction that Aristotle draws between a substance (using that term in the special sense in which Aristotle uses it) and its properties (Pepper's formism and organicism). Not that such explanations have been rendered otiose by subsequent developments. Dispositional properties or "capacities", as Nancy Cartwright (1989) calls them, are an essential ingredient in any scientific explanation. As I pointed out in my contribution to Modgil and Modgil (1987) Skinner does his best to avoid such explanations in his science of behavior; but all he succeeds in doing is in disguising his use of them. In fact dispositional properties are everywhere in science. Even the quark, the smallest entity known to current physics, has its distinctive dispositional property, its so-called "charm". Nevertheless, a further stage in the evolution of science has been reached when mechanical type explanations are adopted (Pepper's mechanism). A final stage in the evolution of science is reached when mechanical explanations are increasingly replaced by explanations of the field theory type (Pepper's contextualism). My only cavil with Morris' description of this process is with his suggestion (perhaps it's Pepper's - it's not clear) that this evolutionary process is a matter of ontology. It is true that the substance-property distinction is an ontological distinction; but what is at issue here is the use that is made of that distinction, or, to be more precise, of the concept of a dispositional property, in scientific *explanation*. According to the traditional scholastic classification, Metaphysics was divided into two sub-divisions: Ontology and Cosmology. Ontology was concerned with the problem of existence, with answering the question 'What different

kinds of things are there?' Explanation was the province of Cosmology. It asks the next question, the question 'Why are there the things that there are, rather than those that there aren't?'

P. 16

To the suggestion in the penultimate sentence before the 'Conclusion' that

"behavior analysis is its own unique and evolving world view"

I can't resist the temptation to respond:

'That's absolutely right! - but in that case, why do we need Pepper?'

[Signed] Ullin T. Place