

*REVIEW OF CAPALDI AND PROCTOR'S  
CONTEXTUALISM IN PSYCHOLOGICAL RESEARCH?  
A CRITICAL REVIEW*

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Capaldi and Proctor's (1999) book is about philosophical issues not directly related to applied behavior analysis, and readers of this journal might therefore ask why a review of it is published here. I would give two answers, one pragmatic and the other biased. The pragmatic answer is that both applied and basic behavior analysis, which of course are intimately interrelated (e.g., Baer & Pinkston, 1997; Sulzer-Azaroff & Mayer, 1991, pp. xv–xvi), are largely misunderstood and therefore mistakenly criticized in mainstream psychology. Capaldi and Proctor's book contributes to the misunderstanding and in doing so provides new ersatz fuel for the critics. Consequently, even though behavior analysts' applied or basic work will proceed as before—and indeed it should not be affected by Capaldi and Proctor's book or by this review of it—behavior analysts who are asked to justify their approach, or to respond to criticisms of it, should benefit from seeing where the critics err. The biased answer is that I am profoundly interested in contextualism, which is the philosophy that Capaldi and Proctor attack, and I think that behavior analysts should find contextualism at least somewhat interesting because it seems to be the philosophy that underlies behavior analysis (e.g., S. C. Hayes, Hayes, & Reese, 1988; Morris, 1988, 1997; and chapters in S. C. Hayes, Hayes, Reese, & Sarbin, 1993).

Capaldi and Proctor (1999) describe and evaluate “philosophic” contextualism and two “modified” contextualisms that are

called by their advocates “developmental contextualism” and “functional contextualism.” These modified contextualisms are used in approaches to, respectively, developmental psychology (e.g., Lerner, 1989, 1993) and behavior analysis (e.g., Biglan & Hayes, 1996; L. J. Hayes, 1993; S. C. Hayes, Blackledge, & Barnes-Holmes, 2001), but some developmental psychologists and behavior analysts have endorsed Pepper's (1942) contextualism rather than specifically the “developmental” or “functional” version (e.g., P. B. Baltes, Reese, & Lipsitt, 1980, p. 80; Barnes & Roche, 1994; Labouvie-Vief & Chandler, 1978; Morris, 1988, 1997; Reese, 1996).

Capaldi and Proctor conclude that philosophic contextualism is inconsistent with what they call “academic” or “mainstream” psychology and that, although the developmental and functional versions are consistent in some ways with this psychology, they are inconsistent in a number of important ways but might nevertheless make important contributions to this psychology. The book is generally well written but is substantively flawed by misunderstanding of worldviews, science, academic mainstream psychology, and contextualism. These problems are discussed in the following sections. All page citations refer to Capaldi and Proctor's book unless specifically attributed to another source.

MISUNDERSTANDING  
OF WORLDVIEWS

Although many philosophers have written about worldviews, Pepper's (1942) and

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Kuhn's (1962) analyses have had the most influence in psychology and are the ones that psychologists usually discuss. The majority of philosophers have ignored Pepper's analysis, to their discredit, but most have discussed Kuhn's analysis. Unfortunately, many of them and some psychologists, including Capaldi and Proctor, have misunderstood Kuhn's analysis (and Pepper's, if they considered it) and have either ignored or rejected attempts by Kuhn to dispel their errors.

#### *Consistencies Between Pepper and Kuhn*

Pepper's (1942) analysis of worldviews was consistent with Kuhn's (1962) later analysis of "paradigms." Two major consistencies are discussed in the following paragraphs.

*Irrationality.* According to both Pepper and Kuhn the adoption of one worldview instead of another is to some extent irrational, that is, based to some extent on nonevidential grounds. A point that needs to be emphasized because it is often misunderstood—by many psychologists such as Capaldi and Proctor as well as by many philosophers—is that neither Pepper nor Kuhn said that adopting any particular worldview is completely irrational. Rather, for both Pepper and Kuhn the reasons for adopting a particular worldview can be based in part on the available relevant evidence, but because this evidence is incomplete, evidence cannot fully justify the choice.

*Incommensurability.* According to both Pepper and Kuhn, the adherents of different worldviews see the world differently to some extent. For example, Pepper's analysis showed that the four relatively adequate worldviews that he identified (contextualism, formism, mechanism, and organicism) entail different conceptions of such basic concepts as the nature of substance and change and the criterion by which truth is determined. These differences in turn make any eclectic mixing of worldviews futile

(Pepper, 1942, 1943; Reese, 1999). They also lead to the use of different vocabularies, often with the same word referring to different concepts. These features of worldviews, according to both Pepper (1942, chap. 5; 1973/1980) and Kuhn (1970b, 1977a, pp. xxii–xxiii), lead to a difficulty of communication across worldviews—a difficulty that Kuhn called "incommensurability." Many critics, including Capaldi and Proctor, have argued that Kuhn was wrong because incommensurability is obviously partial rather than complete. However, although his view may have been unclear in the first edition of *The Structure of Scientific Revolutions* (Kuhn, 1962), Kuhn later clarified his view, pointing out that he always conceptualized incommensurability as partial (Kuhn, 1970a, 1970b, 1970c, 1977a, 1977b). Feyerabend (1978, chap. 15) agreed.

According to Kuhn, incommensurability means that if A can learn B's language, A can communicate with B in that language; but—and here Kuhn agreed with Feyerabend (or vice versa)—some of B's language is not translatable into A's language and therefore A cannot take some of the communicated information back home. Stripped of the metaphor of conversation, the view is that one can potentially understand all paradigms fully, but one cannot give a full and faithful description of any one paradigm using the terms of any other paradigm. A concrete example is Dollard and Miller's (1950) stimulus–response learning-theory interpretation of part of Freudian psychoanalytic theory. This interpretation was incomplete because some psychoanalytic terms and principles are not translatable into the language of stimulus-response learning theory. The basic reason is that the underlying worldviews are different: Dollard and Miller's theory is consistent with the mechanistic worldview and, although Freud's theory is consistent with mechanism in some ways (e.g., the

hydraulic metaphor of id, ego, and super-ego), it is mentalistic in some important ways (e.g., the concepts of reaction formation, the Oedipus complex, and identification).

Capaldi and Proctor get the point straight from time to time, in referring to incommensurability as difficulty rather than impossibility of communication (e.g., pp. 36, 148, 154), but they comment in a summarizing statement (p. 154) that the difficulty may sometimes be insurmountable, and they often refer to the difficulty as complete and always insurmountable (e.g., p. 137). They also say that incommensurability is “the idea that it is difficult for individuals who ascribe to different theories to communicate effectively with each other” (p. 148; I think they meant “subscribe” instead of “ascribe”). They are overgeneralizing, because Kuhn’s concept of incommensurability refers to difficulty of communication only for theories reflecting different paradigms. Theories that are consistent with the same paradigm, or worldview, are in the same “family,” and their advocates can communicate with one another relatively easily (e.g., Pepper, 1973/1980; Reese, 1999; Reese & Overton, 1970). For example, behavior analysts should have more trouble communicating with psychologists in the mechanistic academic mainstream than with workers in such contextualistic disciplines as behavioral economics, cultural materialism, some versions of ethology (e.g., regarding foraging), and sociobiology (e.g., S. C. Hayes et al., 1988; Reese, 1999).

#### *Pepper’s Theory*

I address here only one of many misunderstandings of Pepper’s theory. Others are discussed throughout this review. Capaldi and Proctor say that “dispersive theories . . . lack precision” and that “integrative theories . . . lack scope” (p. 47). As evidence, they cite Pepper’s (1942) Figure 3.1, in which

Pepper characterized contextualism and organicism as synthetic, formism and mechanism as analytic, contextualism and formism as dispersive, and organicism and mechanism as integrative. However, Capaldi and Proctor ignore Pepper’s explanation that the figure shows tendencies or emphases rather than absolutes, leading them to exaggerate his position: “Contextualism, as described by Pepper, is a *nonanalytic*, dispersive theory that also *lacks precision*” (p. 48; italics added). They add, “In our view, a system of this sort cannot possibly provide a basis for science” (p. 48). I agree, as Pepper did also, that a system that lacks precision cannot be adequate for scientific purposes; however, Pepper’s contextualism is not such a system. First, analysis is given a major role in Pepper’s contextualism; second, dispersiveness is not emphasized in some contextualistic theories; and third, although animism (among the worldviews Pepper described) lacks precision, contextualism does not (nor do formism, mechanism, and organicism).

#### *Ontological Role of Worldviews*

Worldviews have two major roles, one ontological and the other epistemological. The ontological role is metaphorical because reality cannot be directly known and therefore can only be modeled or represented. (Granted, the premise that reality cannot be directly known is theoretical rather than empirical—if it is true, it precludes an empirical demonstration that it is true. However, the premise is consistent with all four of the relatively adequate worldviews that Pepper described.)

In the mechanistic ontology, contrary to Capaldi and Proctor, every event is represented as completely determined, causality is represented as one-to-one and unidirectional, and the organism is represented as reactive (passive) rather than active. These points are discussed in the following paragraphs.

*Complete determination.* Aristotle differen-

tiated between causes of *becoming*—efficient, final, and incidental (chance) causality—and causes of *being*—material and formal causality. Causes of becoming are emphasized in both mechanism and contextualism, but causes of being are at least implicitly given a determinative role. That point having been made, the word *cause* and its cognates are used hereafter to refer to causes of becoming.

Complete determination means that every event had a determinate cause, but not necessarily a determinable cause, and in mechanism it means further that chance has no ontological reality. In mechanism, chance is an epistemological concept meaning that the true cause is unknown, and perhaps that it is unknowable (as in some philosophical versions of the physical principle of indeterminacy). That is, although I suspect that all scientists have referred at least once to “chance variations” in a phenomenon, the mechanists among them would have been speaking loosely because mechanism entails that anything attributed to chance had some other actual cause.

Complete determinism is also accepted in contextualism, but chance is ontologically real in contextualism and can have causal efficacy. That is, variations in a phenomenon may truly be effects of chance and may be unexplainable by any other kind of cause. The categorical acceptance of the possibility of chance causality leads to the contextualistic concepts of dispersiveness and novelty, which are discussed later. (A feature is *categorical* in a worldview if it is entailed in that worldview, that is, required to be true by the basic metaphor of that worldview.) However, the random responding that Neuringer (e.g., 1986, 1992) has demonstrated is not attributable to chance because it occurs only after specific operant training (e.g., Neuringer, 1993). In fact, even though Skinner sometimes attributed a causal role to chance, a fundamental premise of most behavior-analytic research is that chance is not a real

cause. In this respect, the usual ontology in behavior analysis is consistent with mechanism rather than contextualism (Reese, 1996, 1999).

*One-to-one causality.* One-to-one causality means that one cause has only one effect, with “one cause” understood to include an additive or interactive combination of analytically separable constituents and “one effect” understood to include an additive combination of analytically separable constituents. This is the only kind of causality recognized in the mechanistic worldview, in which it leads to the in-principle complete predictability of any future event and postdictability of any past event. An important point is that one-to-one causality is a *principle* in mechanism even though it is often not actually demonstrated in *practice*.

In contextualism, causality may be one-to-one in some cases and one-to-many or many-to-one in other cases, but in all these cases the type of causality found is understood to be changeable into any other type because the laws of nature can change.

In contextualism, causality is *one-to-many* if a given cause can have multiple effects and *many-to-one* if multiple causes can have the same effect. “Multiple” does not refer here to the number of constituents a cause or an effect has; it means that many causes can have the same effect and one cause can have alternative effects if chance intervenes or the context changes. Skinner gave little attention to context, although he acknowledged that it has a role (e.g., Skinner, 1953, pp. 141–145); but many behavior analysts have taken it seriously, for example in the essentially similar concepts of “setting factors,” “setting events,” and “establishing operations” (e.g., commentaries on Michael, 1993, in *The Behavior Analyst*, 1993, 16, No. 2, pp. 207–236, 347–349).

*Unidirectional causality.* Unidirectional causality has two meanings. One is that causality works like Time’s Arrow, from the past

to the present and from the present to the future. In this sense, it means that teleological (“backward”) causality is impossible. Unidirectional causality in this sense is accepted in both mechanism and contextualism—and in behavior analysis (for brief discussion and references, see Reese, 1997). The other meaning is that causes and effects do not exhibit simultaneous reciprocal interaction. The elements in a causal interaction combine nonadditively to produce an effect. In a reciprocal interaction, each element is both a cause and an effect; in a simultaneous reciprocal interaction, each element is *at the same time* both a cause and an effect. Thus, simultaneous reciprocal interaction makes the distinction between cause and effect arbitrary and rather useless.

Unidirectional causality in the second sense is categorically accepted in mechanism. Capaldi and Proctor deny that it is (p. 96) and quote an article by Ford and Lerner as an example of misunderstandings of the mechanistic position—“A typical view is that if A causes B, B cannot simultaneously cause A.” Capaldi and Proctor cite Newton’s third law of motion—to every action there is always opposed an equal reaction—as evidence of reciprocal causality in mechanism. The problems are that they misunderstand the nonmechanistic concept of simultaneous causality, which is defined above, and misunderstand Newton’s law. In the familiar use of billiards to illustrate this law, the motion of a cue ball leads to contact of the cue ball with an object ball, and the contact causes the object ball to move with a predictable velocity in a specified direction. Simultaneously, the contact causes a predictable change in the velocity and direction of movement of the cue ball. The simultaneity is expressed in vector addition; simultaneous reciprocal causality as defined above is not involved.

The point is that in mechanism, causes can interact with one another to produce an

effect, but causes and effects cannot interact except in the sense that they can occur in a chain in which each effect becomes a cause of a subsequent effect, which in turn becomes a cause of a further effect, and so on. Whenever a cause and an effect appear to exhibit simultaneous reciprocal interaction, the mechanistic interpretation should be that the sequential transitions in the chain occurred too rapidly to be perceived as temporally sequential. This is the standard interpretation in behavior analysis (e.g., M. M. Baltes & Reese, 1977). In contrast, contextualism accepts the reality of simultaneous reciprocal interactions as well as cause–effect chains.

*Reactive versus active organism.* Capaldi and Proctor say that the mechanistic organism is active, and in support they give quotations (pp. 95–96) from four “so-called mechanistic sources”—a 1951 article by Lashley, a 1972 article by Postman (titled “A *Pragmatic View of Organization Theory*” [italics added]), a 1980 textbook by Klatzky, and a 1994 textbook by Spear and Riccio. On the basis of these quotations, they conclude, “Thus, from at least the early 1950s to the present, it has been continuously recognized that learners are active rather than passive in the processing of information” (p. 96). A more justifiable conclusion would be, “Thus, from at least the early 1950s to the present, it has been implicitly recognized that the mechanistic worldview provides an inadequate model of the processing of information.” In support of this revision, I could cite many sources indicating that the mechanistic organism is reactive (one example is M. M. Baltes & Reese, 1977), but does anyone really believe that a machine can decide what information it will process, whether and how it will behave in light of the outcome of the processing, and so on?

#### *Epistemological Role of Worldviews*

The basic metaphor of contextualism is a purposeful activity that is actually ongoing

here and now, that is, in a real physical-social-personal-temporal context. Such an activity may be experienced as a chain of events or, apparently less often, as a simultaneous reciprocal interaction. Cause–effect chains are therefore more expected than simultaneous reciprocal interactions, and they are easier to conceptualize. Consequently, contextualism emphasizes cause–effect chains and encourages attempts to analyze interactions that appear to be simultaneously reciprocal into cause–effect chains. Whether the activity-in-context is interpreted as truly a cause–effect chain is irrelevant, because analysis is always conceptual rather than physical in contextualism; physical analysis would destroy the wholeness of the activity-in-context. The results of the analysis are consequently useful only if they are used for a conceptual synthesis of the whole, and this synthesis is useful only if it yields a better understanding of the whole. In other words, in contextualism both analysis and synthesis are epistemological rather than ontological. Consequently, neither analysis nor synthesis requires use of experimental methods.

#### MISUNDERSTANDING OF SCIENCE

##### *The Nature of Science*

Capaldi and Proctor use the word *science* in a way that leaves out such nonexperimental sciences as astronomy, human population genetics, sociobiology (widely but usually inappropriately criticized as nonscientific), and many versions of fields such as anthropology, sociology, and psychology. In fact, even the natural sciences—properly, physics, chemistry, and biology—are not fully experimental, leading me to wonder whether Capaldi and Proctor sometimes use the word *experimental* to mean *empirical*. If they do, then their criticisms of dramaturgical, hermeneutic, phenomenological, and some other nonmechanistic approaches are not entirely justified,

because all these approaches are empirical in a way that excludes literary and religious “empiricism.”

Capaldi and Proctor say that only mechanism provides an adequate basis for science, and they attribute the same view to Pepper: “Pepper sees each of these worldviews [i.e., contextualism, formism, mechanism, and organicism] as being adequate in its own distinctive domain, with only mechanism being the worldview of science” (p. 40) and “Pepper sees mechanism as being the basis of science” (p. 47). They do not cite a page, or even a work, and I have not found any statement by Pepper that supports this point, or the point about each worldview being adequate only in *its distinctive domain*. Pepper did make statements that would support the point that only mechanism is adequate for science if *science* is interpreted to mean *physics*. However, this interpretation would take away the implication Capaldi and Proctor want because physics is obviously not the only science.

Their claim about Pepper’s view therefore seems to be unjustified, especially because they acknowledge that “one’s philosophy of science, whether explicit or implicit, affects one’s practice of science” (p. 20). This acknowledgment is also implicit at the end of the following quotation, to which I have added bracketed index numbers and italics.

[1] If philosophic contextualism were to prevail in psychology, the changes it would produce would not only be numerous but also fundamental. [2] On the one hand, many problems currently recognized as fundamental in psychology would come to be ignored and neglected. [3] On the other hand, psychology would come to concern itself with a variety of new issues that are not scientific in character, *at least by any conventional standard*. (Capaldi & Proctor, pp. 86–87)

Statements 1 and 2 are true (but “philosophic” in 1 is unnecessary and either “ignored” or “neglected” in 2 is redundant). Statement 3 is also true, but it is irrelevant to *contextualistic* science and to formistic and organicist sciences.

### *The Nature of Scientific Progress*

Some philosophers have pondered the question of whether evolution, or culture, or science is *progressive*. Capaldi and Proctor attribute to “Kuhn and other relativists” the conclusion “that progress does not occur in science, that successor theories are not superior to but merely different from the theories they displace” (p. 34; see also p. 146). They attribute the same idea to “contextualists and social constructionists” (p. 137). In support, they cite Kuhn’s 1962 book, but they do not cite any references to “other relativists” nor to “contextualists and social constructionists.” They quote statements in which Kuhn compared science and evolution and concluded that science is not demonstrably progressive (pp. 146–147) because, in Capaldi and Proctor’s words, “In neither evolution nor science is there a goal” (p. 146). Actually, of course, although evolution has adaptive outcomes but not goals, not only does scientific work have outcomes but also this work is done because “science,” as embodied in scientists, has goals. Some scientific outcomes are “adaptive” in the sense that they attain the goals, that is, the problems that scientists worked on were solved.

According to Laudan’s view (as summarized on pp. 147–148), the criterion for scientific progress is, in Capaldi and Proctor’s words, “the ability of a theory to solve *more* [italics added] problems than its predecessor” (p. 147). Perhaps this criterion is appropriate for some sciences, but it is not appropriate for psychology: During the 20th century, psychology has had many changes in metatheoretical paradigms, theories, and

empirical issues, and the changes have often been cyclic waxings and wanings reflecting cyclic waxings and wanings of interests (Reese, in press). Therefore, in psychology the criterion for scientific progress has typically been not the ability of a theory to solve *more* problems than its predecessor but the ability to solve *more important* problems than its predecessor, with relative importance determined by the interests of the advocates of the new theory. This was, in fact, Kuhn’s criterion, which Capaldi and Proctor quote on page 146. Both criteria are relativistic, but Laudan’s is based on quantity and Kuhn’s is based on quality (epistemic value).

Another criterion that has been used is Pepper’s (1942) concept of “adequacy,” based on scope and precision. Regardless of whether the criterion used is Laudan’s, Kuhn’s, or Pepper’s, I do not see how anyone could deny that psychology has been progressive. Examples are that by all of the criteria, neurocognitive science is an advance over the ancient theory that the heart is the seat of the major senses (Aristotle, *On Sense and the Sensible*, chap. 2), scientific psychology is an advance over the earlier armchair psychology of the British Associationists and 19th century structural psychology, and behavior therapy is an advance over Freudian psychoanalysis. Of course, all of the criteria could be rejected, but they have principled bases, and therefore rejecting them would need to be principled because otherwise the rejection would be dogmatic and therefore cognitively noncompelling.

### MISUNDERSTANDING OF ACADEMIC MAINSTREAM PSYCHOLOGY

#### *Role of Experiment*

According to Capaldi and Proctor, academic mainstream psychology is basically an experimental science. Behavior analysis is also an experimental science, but Capaldi

and Proctor do not include it in their characterization of academic mainstream psychology. In any case, although all academic mainstream psychologies are empirical, some are at best quasi-experimental. Examples are clinical psychology, developmental psychology, and the relatively recent branch of psychology called behavioral genetics. These psychologies are not truly experimental because persons are not assigned at random to diagnostic categories, sexes, ages, genotypes, environments, and so forth. Thus, contrary to Capaldi and Proctor, use of experimental methods is not a necessary part of the definition of academic mainstream psychology.

### Goals

Capaldi and Proctor repeatedly say that the goals of academic mainstream psychology are the same as the goals of behavior analysis: prediction and control of behavior (e.g., p. 124; see *control* and *prediction* in their subject index). Actually, except possibly in clinical psychology, the goals of both academic mainstream psychology and behavior analysis are usually understanding and explanation rather than prediction and control. However, although these goals refer directly to behavior in behavior analysis, in academic mainstream psychology they refer not to behavior but to inferred or hypothesized mechanisms. That is, the goals are to understand the mechanisms that theoretically underlie behavior and to explain theoretically how these mechanisms influence behavior.

### Theory

Capaldi and Proctor say that "Skinnerians are radical empiricists who eschew theorizing (see, e.g., Skinner, 1950)" (p. 12) and "Any attempt to provide an explanation for the functional relationship is regarded [by Skinner and Skinnerians] as unscientific and metaphysical" (p. 27). These statements are incorrect, as can readily be seen in the article

by Skinner (1950) that they cited and in Skinner's (1957) book *Verbal Behavior* and the Discussion sections of many research reports by "Skinnerians." Furthermore, many mainstream academic psychologists have also been skeptical about the value of theory (e.g., Clark L. Who?, 1993; Cofer, 1968; Dallenbach, 1953), as have some philosophers (e.g., Hacking, 1983, chap. 9, 10; Popper, 1966, p. 261).

### Openness

Capaldi and Proctor sometimes look at academic mainstream psychology through rose-colored glasses. One example is: "Within mainstream psychology even wildly different ontological and epistemological approaches get a hearing" (p. 171). As behavior analysts know well, behavior analysis gets short shrift, at best, in academic mainstream psychology; as Piagetians know, academic mainstream psychology largely ignores their kind of cognitivism except when criticizing it; and as contextualists who read Capaldi and Proctor's book will come to know, the conclusion in the last chapter is in effect that contextualists can have an influence in academic mainstream psychology, but only if they give up contextualism.

## MISUNDERSTANDING OF CONTEXTUALISM

Capaldi and Proctor characterize contextualism as a postmodern version of pragmatism (pp. xii, 9), but they get it backwards. Contextualism is actually a *modern* version of pragmatism and postmodernism is a postmodern version of contextualism. According to Pepper (1942), contextualism emerged from the pragmatic epistemological rules advanced by Peirce, James, and Dewey (in the late 19th and early 20th centuries). These rules "thickened" (Pepper, p. 268) into the worldview that Pepper called "contextualism" and that Pepper was already

writing about in the 1930s (e.g., Pepper, 1932, 1934), well before the emergence of “postmodernism” as a contextualistic worldview.

Other misunderstandings are about the basic metaphor of contextualism, the nature of “variants” of contextualism, and the contextualistic concepts of causality, analysis and synthesis, relativism, truth, reduction, dispersiveness, and novelty. Except for causality, which was discussed earlier, these misunderstandings are discussed in the following subsections.

#### *The Basic Metaphor*

As Capaldi and Proctor note (p. 42), Pepper (1942, p. 232) said that the basic metaphor of contextualism is “the historic event.” He was wrong, as he explained immediately: It is an “event alive in its present,” not “a past event” that is “dead and has to be exhumed” (p. 232). Thus, it is not an historic event at all but an “active present event” (Pepper, p. 253), an event that is currently ongoing.

#### *Variants of Contextualism*

Capaldi and Proctor say that “philosophic” contextualism differs fundamentally from the two “modified” contextualisms they discuss. Actually, as I understand these variants, they are in fundamental agreement regarding the underlying worldview, and they differ only because of differences in the categorical contextualistic concepts and principles they choose to emphasize and the ones they choose to ignore.

For example, Gergen and postmodern philosophers such as Rorty emphasize dispersiveness and ignore Pepper’s stricture that contextualism accepts any degree of order that is found. I find these particular variants interesting primarily as explorations of the consequences of taking dispersiveness seriously. I can understand why Capaldi and Proctor and, earlier, Overton (1984) would

reject such variants as possible bases for science. Nevertheless, I must add that these rejections make sense only if—in agreement with Capaldi and Proctor, Overton, and many philosophers of science—science is defined mechanistically. This definition has already been discussed.

#### *Analysis and Synthesis*

Capaldi and Proctor assert that their understanding of Pepper’s contextualism is “better and more appropriate” than contextualists’ understanding (p. 47). Although I disagree with the understanding of some contextualists, I disagree even more with Capaldi and Proctor’s understanding. An example is Capaldi and Proctor’s understanding of analysis and synthesis.

*Analysis.* Capaldi and Proctor say incorrectly that contextualism is nonanalytic, as noted earlier, but they also contradict themselves and say correctly that it is analytic—in contextualism “an event may be analyzed from many points of view, depending on one’s purposes” (p. 44). However, in the next sentence they misinterpret the nature of analysis in contextualism: “For a contextualist, there is no correct or incorrect analysis; analysis always proceeds from some point of view and serves some practical purpose.” This characterization is wrong because according to the contextualistic definition of truth, an analysis is *correct* if it attains the intended goal, whether practical or theoretical, and is *incorrect* if it does not (Reese, 1993).

Capaldi and Proctor say that in contextualism, “all analyses are tentative, in the sense that no event can be completely analyzed” (p. 44). This statement is correct if “completely analyzed” means going back to the First Cause, because such an analysis would be an impossible infinite regress. However, under this interpretation the statement is correct not only for contextualism but also for the other relatively adequate

worldviews. The statement is also correct—and only for contextualism—if “completely analyzed” means that although the presently obtained analysis is correct, it will not necessarily be correct forever. The reason is that in contextualism, but not the other worldviews, the laws of nature can change.

Capaldi and Proctor say next that in contextualism, “In dealing with particular events, we may go from one event to another, and there is no stopping place.” I see only two possible meanings of this statement, both implying that Capaldi and Proctor misinterpret their own verb phrase “may go” to mean “must go.” One possibility is that the statement refers to a search for prior causes (prior events) and “no stopping place” means infinite regress. The other possibility is that when one event ends, we must proceed to deal with the next one, *ad infinitum*. These meanings are neither required nor encouraged in contextualism; the analysis stops when its purpose has been served, that is, when the problem that was set has been solved. Similarly, in mechanism one can explain a particular chemical reaction, for example, without tracing it back to the Big Bang or tracking it to the cataclysmic end of the universe; and in behavior analysis one can explain a particular learned behavior by referring to the organism’s personal history without invoking the evolutionary history of the species and without considering all the ways the learned behavior influences other behaviors.

*Synthesis.* Capaldi and Proctor misinterpret synthesis in contextualism, at least with respect to context effects. They cite chapters in a 1985 book, *Context and Learning*, edited by Balsam and Tomie, as evidence that mechanists recognize context effects (pp. 111–112), but I would say three things about this point. First, although Capaldi and Proctor do not give any reason to believe that the authors they cite are mechanists, mechanists indeed can recognize cases in

which context effects occur. Second, mechanists do not expect context effects—they expect universal, context-free laws—and therefore they might well devote a book to cases in which context effects are found. In contrast, contextualists expect context effects and therefore are more likely to devote a book to cases in which context effects are not found.

Third, and most important, when context effects are found, thoroughgoing mechanists should interpret context as a causal variable that *moderates* the effect of a basic causal variable. An example is Hull’s (1943, pp. 372–374) explanation of the “arpeggio paradox”: Following training that makes a particular tone a conditioned stimulus, the conditioned response does not occur if the tone is presented in an arpeggio. According to Hull, the sequence of tones in the arpeggio provides a context that inhibits the conditioned stimulus function of the conditioned tone. In contrast, thoroughgoing contextualists should interpret context as an indispensable element in the whole event. For example, Dewey said “The ‘from which’ and ‘to which’ qualify the event and make it, concretely, the distinctive event which it is”; for example, if the event is sliding down a hill, “it makes considerable difference to the slide as an event whether it terminates on rocks, water, or a pile of grass” (Dewey, 1931, p. 211; see also his pp. 205, 212, 214, 219–220, 223).

### *Relativism*

*Arguments against relativism.* Capaldi and Proctor argue that relativism is wrong because many philosophers reject it (e.g., p. 5) and because several universal (nonrelative) laws have been found in cross-cultural research (p. 168). For example, Capaldi and Proctor say, “The relativism of Kuhn and Feyerabend and thus, by implication, the forms of relativism accepted by various contextualists have met serious challenges,

which they appear to be unable to refute. In consequence, to use the terminology of Lakatos (1970, 1978), they have become non-progressive programs” (p. 36). Actually, the challenges regarding relativism in its various senses have been adequately refuted, at least from the point of view of contextualists and other relativists. I would add here that relativism as a worldview is, like any worldview, irrefutable on the basis of evidence and on the basis of philosophical standards borrowed from other worldviews.

As Kuhn said, in a passage that Capaldi and Proctor quote,

Though each may hope to convert the other to his way of seeing his science and its problems, neither may hope to prove his case. The competition between paradigms is not the sort of battle that can be resolved by proofs. (Kuhn, 1962, p. 147; Capaldi & Proctor, p. 31)

That is, a theory within one paradigm cannot be falsified by rules from a different paradigm because “the proposed construction of alternate tests and theories must proceed from within one or another paradigm-based tradition” (Kuhn, 1962, p. 145, as quoted by Capaldi & Proctor, pp. 30–31). Contrary to Capaldi and Proctor (p. 31), Kuhn did not mean that “the standards for theory evaluation are mere conventions or matters of taste.” Adoption of a particular paradigm entails use of particular standards of evaluation; therefore, although adoption of a paradigm is to some extent a matter of taste, the evaluation standards come with the paradigm that is adopted and are not themselves matters of taste.

*All theories are equal.* In one sense, cited by Capaldi and Proctor (p. 144), relativism means that all theories are equally acceptable. This seems to be a favored sense outside relativism, presumably because it is easy to demolish. However, contrary to Capaldi

and Proctor (e.g., p. 137), it is not a necessary feature of contextualism or any other worldview.

One argument for the principle that all theories are equally acceptable begins, paradoxically, with the premise that they are not. The argument is that if a theory is inconsistent with evidence (i.e., is not totally acceptable), it can be made consistent by ad hoc patching (pp. 143–144). Capaldi and Proctor challenge the legitimacy of ad hoc patching because it provides “no guarantee . . . that the modified theory would be as empirically adequate as the original one,” “no guarantee that the newly added auxiliary assumption will explain or predict very much more than the specific phenomenon that led to its incorporation,” and “no guarantee that the revised assumption will fit as coherently into the system of statements as the original assumption” (p. 144). True; but the lack of guarantees is not a valid criticism of ad hoc patching. After all, as Hume pointed out, we have no guarantee that the sun will rise tomorrow (Hume, 1777/1902, Sect. IV, Pt. I). Ad hoc patching is what scientists do, presumably because it is a conservative approach, it has a respected history, and it has often proved to be useful (James, 1907, pp. 59–64; Kuhn, 1970a; Laudan, 1977, pp. 114–118; Quine, as summarized by Capaldi & Proctor, p. 143). Of course, well-trained scientists know that circular patchwork is bad because it merely gives a theoretical name to the troublesome phenomenon and has no implications outside that phenomenon.

*All worldviews are equal.* In another sense, relativism means that all worldviews are equally acceptable. This is the postmodernist sense, but it seems silly to me and to virtually all philosophers other than postmodernists. It was not Pepper’s (1942) sense, however, because he specified that all worldviews that are *relatively adequate to about the same degree* are equally acceptable. He evaluated

several of the then-current worldviews and found that animism, for example, is inadequate because of low precision and therefore is less acceptable than the four worldviews that met the adequacy criteria (contextualism, formism, mechanism, and organicism). Furthermore, he concluded that the latter four worldviews met the adequacy criteria about equally well and therefore were equally acceptable. The view that Pepper endorsed is consistent with contextualism, and it has been rejected only by philosophers (e.g., Suppe, 1977) and others (e.g., Spiker, 1986) who evidently did not understand the nature and roles of worldviews.

*Truth is relative.* In still another sense, relativism means that truth is relative rather than absolute, eternal, or universal. Variations of this sense are discussed in the next subsection.

### *Truth*

Truth is relative in several senses, six of which are discussed in the following paragraphs.

*Truth varies across worldviews.* In one sense, the relativity of truth means that truth has no single, absolute criterion. Capaldi and Proctor say “we insist that there are rational criteria for evaluating knowledge claims, and whatever one’s approach to science, knowledge claims are subject to the same set of rational criteria” (p. 14). This insistence is crucial to Capaldi and Proctor’s method of evaluating the factual claims of contextualists: “The contributions of contextualism can be evaluated and judged only by employing some of the various criteria employed within academic psychology to evaluate its methods and procedures” (p. 13). However, their insistence reflects a failure to recognize that truth criteria vary across worldviews, and it indicates no understanding of why they vary. Pepper (1942) discussed these topics at length.

*Absolute truth is real but unknowable.* In a

second sense, the relativity of truth means that although absolute truth is real, it is not completely knowable and therefore the best we can do is to approximate it relatively closely. Knowledge is limited by epistemological rules such as the indeterminacy principle in physics and the logical principle that the truth of an empirical generalization is not certain until all possible instances of the phenomenon have been observed—for example, the truth of the generalization that all swans are white is uncertain because the next swan observed may be black (Capaldi & Proctor, p. 24). This sense is consistent with both mechanism and contextualism.

*The permanence of absolute truth is a fiction.* In a third sense, the relativity of truth means that although absolute truth exists here and now, the laws of nature can change and therefore absolute truth is not necessarily permanent. When this sense is adopted, the goal of science is to identify the currently true laws, which also requires recognizing cases in which a law that had been true has become false. This sense is consistent with contextualism but not mechanism.

*Absolute truth is culturally relative.* In a fourth sense, truth is culturally defined and therefore is relative to the culture in which it is evaluated. This sense is consistent with contextualism, but Quine evidently considered it indefensible:

Truth, says the cultural relativist, is culture-bound. But if it were, then he, within his own culture, ought to see his own culture-bound truth as absolute. He cannot proclaim cultural relativism without rising above it, and he cannot rise above it without giving it up. (Quoted by Capaldi & Proctor, p. 153)

I am reminded of the old saw about determining the truth value of a person’s saying “I always lie,” which is a problem some philosophers are interested in but need not concern scientists because it is only a logical,

linguistic problem. Anthropologists, for example, can recognize that a calendar date is culture-bound without giving up the calendar used in their own society, and they can now do retrospective research on the hubbub about “Y2K” even though recognizing that the end of the second millennium occurred millennia ago in, for example, the Chinese, Jewish, and Masonic calendars.

*Combination of senses.* A fifth sense combines the preceding two senses: Truth is culturally defined, but cultures change over time and therefore current truths are relative to the culture as it exists here and now. Current truths can be differentiated from historical truths by defining the latter as truths relative to the culture as it existed there and then (knowledge about the culture as it existed there and then is often interpreted to be refracted through the lens of the current culture, but this issue is not considered herein). This sense is consistent with contextualism.

*Truth depends on purpose.* In a sixth sense, which is the essential one in contextualism, relativism means that truth depends on purpose. That is, truth and goodness, which are synonymous in contextualism, are attributed to an activity that leads to attainment of the antecedent purpose of the activity. This is the “successful working” truth criterion. As Capaldi and Proctor note, Pepper found it troublesome and discussed alternative versions, which Capaldi and Proctor say are “more adequate contextualistic truth criteria” (pp. 44, 114). However, Capaldi and Proctor do not mention that Pepper also found the alternative versions troublesome and, like other contextualists, in the end he went back to the successful working criterion (Pepper, 1942, p. 278).

A final point about successful working is that success is always defined relative to the originally intended goal and the physical, psychological, and temporal context in which the projected activity actually oc-

curred. The working will not necessarily be successful in other contexts, and therefore the truth of an activity is always understood to be possibly conditional and provisional.

### *Reduction*

Capaldi and Proctor say “The view that so-called mechanists are necessarily committed to reductionism is fallacious for at least two reasons” (p. 97). One reason is that “even within mechanism, a major alternative approach to reductionism—interfield theory—has been proposed”:

Bechtel (1988) indicates that interfield theories “do not attempt to derive one theory from another but rather seek to identify relationships between phenomena studied by the two different fields of inquiry. . . . It allows both analyses to inform each other in the attempt to develop an interfield theory, but does not require subsuming one explanation under another.” (p. 97)

I agree that this approach is nonreductionistic and yet consistent with mechanism. It is also consistent with contextualism.

The other reason is that some academic mainstream psychologists doubt that reduction is possible. Capaldi and Proctor cite only a 1974 paper by Fodor “contending that it is not possible to construct bridge laws equating terms in psychology with those of lower-level disciplines” (p. 97). One problem here is that (a) the issue of whether bridging laws can be constructed is empirical and (b) what could not be done more than a quarter century ago, when Fodor made the quoted statement, does not necessarily pre-empt what can be done now or later. A second problem is that the empirical success of reduction is not relevant to whether or not the mechanistic worldview requires in-principle reduction.

Actually, as Marr (1993) pointed out, mechanism does not require this kind of re-

duction (Capaldi and Proctor quote the relevant statement on p. 98). The reason is that the way a machine works depends on the relations among its parts and not on reducing these relations to, for example, chemical reactions. For the same reason, however, mechanism requires another kind of reduction—reduction of the way a machine works to the interrelated parts and the ways they work. This kind of reduction is usually called analysis, and it is a fundamental feature of mechanism, as Pepper (1942) said and as Capaldi and Proctor acknowledge. Reduction in this sense also has a major role in contextualism.

#### *Dispersiveness*

Pepper (1942) identified dispersiveness as one of the categorical principles of contextualism. The principle is that facts tend to be isolated from one another; one of its major implications is that facts may be disorganized rather than ordered into relatively permanent structures. However, as indicated in the next subsection, dispersiveness is a derived rather than basic category, and it is limited.

#### *Novelty*

*Definition of novelty.* Most critics of contextualism and many of its advocates (see Capaldi & Proctor, pp. 56–57) have misunderstood the meaning of *novelty* in contextualism. Capaldi and Proctor fault the contextualists, commenting that “no contextualist appears to have defined a novel event or a novelty in a clear, unambiguous way” (p. 42). I would disagree; I think that definitions given by at least Pepper and me are clear and unambiguous.

In my 1991 article, which Capaldi and Proctor cite, I pointed out that dispersiveness is not a *basic* concept in contextualism but rather is derived from the concept of novelty, which in turn is derived from the concept of change, which is basic in contex-

tualism. I quoted Pepper’s statements, (a) “Pure cosmic chance, or unpredictability, is . . . a concept consistent with [contextualistic] theories even if not resorted to or emphasized by this or that writer” (Pepper, 1942, p. 143; Reese, 1991, p. 198), but (b) contextualism “*does not deny the possibility of disorder or another order in nature also*. This italicized restriction is the forcible one in contextualism, and amounts to the assertion that change is categorical and not derivative in any degree at all” (Pepper, p. 234; Reese, pp. 198–199).

These statements indicate that chance causality is a basic concept in contextualism; they were the basis for my definition:

[Novelty means that] the universe may change in unexplainable ways. . . . The contextualistic position does not *require* that the universe change in unexplainable ways; rather, it requires the *possibility* of unexplainable change. As Pepper (1942, pp. 258–260) commented, no such change has yet been demonstrated—so far, all unanticipated events that have occurred have been explainable after the fact—but the possibility nevertheless remains open. (1991, p. 216)

The preceding quotations show that Capaldi and Proctor err in saying, “Philosophic contextualists universally accept the idea that novel events will and must occur” (p. 56); “the use of the concept in practice leaves no doubt as to what is intended: It is that prediction of any sort is impossible in principle” (p. 42); and “novelty is categorical within contextualism and is an ineradicable feature of that view” (p. 56). On the contrary, the ineradicable categorical concept is the *possibility* of novelty, not the actual occurrence of novelty here and now (or even there and then).

*Role of evidence.* The foregoing quotations show that Capaldi and Proctor err in saying

“So committed is the contextualist to novelty and change that it is denied that there are unchangeable structures in nature” (p. 43) and “stability of phenomena is eschewed” (p. 86). As I said in my 1991 article, “In contextualism the possibility of true novelty requires that predictability is entirely a matter of practice rather than principle” (p. 222). Both predictability and unpredictability are consistent with contextualism and therefore both are accepted if they occur. Indeed, Harré acknowledged the role of practice—that is, evidence—in the following passage, which Capaldi and Proctor mistakenly quote as support for their view. The italics are added.

Gergen (1973) has pointed out in a paper of considerable significance, what little *evidence* exists concerning the temporal stability of social formations and practices, personality-types, and so on, point to their remarkably ephemeral character. We are quite *unjustified* in supposing that the forms of micro-social action, and even *perhaps* the way individuals are related to these forms, that is their social psychology, are constant over time. All the *evidence* we have, slender though it is, *suggests* that social forms and individual cognitions of these forms are highly unstable and in rapid flux. (Harré, quoted by Capaldi & Proctor, p. 86)

#### OTHER PROBLEMS

##### *Mistaken Support*

*Contextualists cited as mechanists.* Capaldi and Proctor offer many citations and quotations in support of their interpretation of mechanism. However, some of the authors they cite or quote were contextualists, and their comments were therefore relevant to contextualism rather than mechanism. One example is Larry Laudan (frequent cita-

tions), whose philosophy of science is essentially contextualistic (see, e.g., Capaldi & Proctor’s quotations of Laudan on pp. 156–157, 164). Other examples are Edward C. Tolman (pp. 109–111), whose version of behaviorism was consistent with contextualism (Hahn, 1942, p. 27; Pepper, 1934), and James J. Jenkins (p. 112), whose approach was implicitly contextualistic in the reference Capaldi and Proctor cite (Jenkins, 1979) and explicitly contextualistic in an earlier paper (Jenkins, 1974).

*Erroneous interpretations.* I did not check the reasonableness of Capaldi and Proctor’s interpretations of the works they cite, except for interpretations of my own work and some other work with which I am thoroughly familiar. I found several errors. For example, Capaldi and Proctor say that in my 1991 article, “science based on mechanism is disparaged, and thus psychology is rejected along with the so-called mechanism in general” (p. 3). Actually, in that article I described a contextualistic approach to developmental psychology and neither disparaged nor rejected any approach.

Capaldi and Proctor’s error supports one of their major criticisms of contextualism—that contextualists reject academic mainstream psychology (e.g., pp. xii, 3, 7, 8, 13). This criticism is not entirely baseless, in that some contextualists have explicitly rejected what Capaldi and Proctor call academic mainstream psychology, but the criticism is nevertheless illegitimate because the rejection of mechanism by contextualists is based on a difference in worldviews. That is, so-called academic mainstream psychology is essentially mechanistic, and contextualists necessarily reject it because it is inconsistent with their worldview. Two corollary reasons for the rejection are that (a) some of the questions that contextualists want answers to are not easily—if at all—addressed in academic mainstream psychology, and (b) contextualists believe that nonexperimental

methods are more likely to provide the answers to these questions than are experimental methods. I would add that many academic mainstream psychologists have also criticized academic mainstream psychology for not being able to answer some important questions.

#### *Noncontextual Quotations*

A thoroughgoing contextualist might be careful to avoid quoting out of context, but Capaldi and Proctor are evidently mechanists who are troubled by context effects. For example,

Reese (1991) has suggested that contextualism is “a legitimate model for the science” (p. 220) and goes on to say that modified contextualists who pursue a scientific approach do so “because they believe science is the best way to get temporarily effective answers (p. 221). (Capaldi & Proctor, p. 64)

Actually, the first comment they quoted was about the premise that prediction is the benchmark of science. I said, “If one accepts the counterpremise that prediction is not a necessary benchmark of science, then contextualism becomes a legitimate model for science (and so does formism, which is also dispersive)” (Reese, 1991, p. 220). The context of the second comment was, “Why do scientists who adopt contextualism continue to do science, knowing that no final answers are possible? Because they believe science is the best way to get temporarily effective answers” (p. 221). In the first comment I rejected the idea that “science” is necessarily mechanistic, and in the second comment I obviously (I thought) used the word “science” to mean “contextualistic science.” Thus, the conclusion Capaldi and Proctor draw—that “philosophic contextualists, but not modified contextualists, reject science as it is conventionally understood” (p. 66)—is mistaken because “modified” as well as

“philosophic” contextualists reject mechanistic science (which is the conventional understanding that Capaldi and Proctor refer to). They necessarily reject it because it is outside the purview of their worldview, not necessarily because of antipathy toward it.

#### *Stated Aim*

Capaldi and Proctor state several times that their aim is to promote a meaningful dialogue between academic mainstream psychology and contextualistic psychology. Given their characterization of contextualism, I wonder whether either camp will be much interested in dialogue—academic mainstream psychologists because according to Capaldi and Proctor’s description, contextualism is a seriously botched work, and contextualists because Capaldi and Proctor would have them give up contextualism as a prerequisite of the dialogue.

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