THE INTERBEHAVIORIST

A Quarterly Newsletter of Interbehavioral Psychology

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TABLE OF CONTENTS

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The Interbehaviorist is a quarterly publication of news, information, discussion, journal and book notes, book reviews, comments, and brief articles pertaining to interbehavioral psychology -- a contextualistic, integrated-field approach to the natural science of behavior.

The newsletter publishes professional communications that fall between informal correspondence and colloquia, and formal archival publication. As such, the newsletter supplements contemporary journals dedicated to basic and applied research, to the history and philosophy of the behavioral sciences, and to professional issues in the field. The newsletter strongly encourages submission of notes about current professional activities of its subscribers, news and observations about interbehavioral psychology and related perspectives, comments on journal articles and books of interest, more extended book reviews, and brief articles. All submissions should be sent in triplicate to the editor and should conform to the style described in the Publication Manual of the American Psychological Association (3rd edition).

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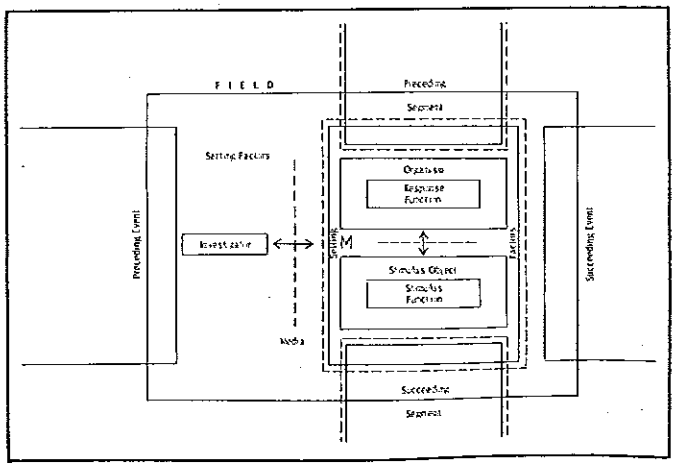

This issue of the newsletter celebrates the beginning of its 16th volume -- and more than a decade and a half of interbehavioral publication. Also cause for celebration is that Volume 15 contained more pages of substantive material than published in any previous year, reflecting the steady, though modest growth we hope to maintain. The latter, though, will require a higher submission rate of articles, book reviews, book notes, and comments, as well as an increase in subscription rates. For the moment, we will maintain our current rates for another year, though our ability to remain solvent throughout the remainder of the volume depends on renewals and new subscribers. We would appreciate both your subscription renewal, if you have not sent it in alreadt, and your assistance in recruiting new subscribers.

In addition to expanding the newsletter, our major project for this year is to prepare a subject and author index for the first 15 volumes. Because the newsletter should remain devoted to substantive issues, however, and because our income will not cover publishing the index as a separate issue within this volume year, the index will be sold separately. When it is completed, we will announce its availability in this column.

Finally, let us turn to the current issue: It contains comments by Dennis J. Delprato on control systems theory and by Harry C. Mahan on cognitive psychology and clear writing; a book note by Susan M. Schneider on Herbert Wendt's In Search of Adam; and an article by Edward K. Morris. The last is a revision of a paper presented in Philip N. Hineline (Chair), Behavior Analysis: A Loyal Apposition to Mainstream Psychology, a symposium conducted at the 1987 meeting of the American Psychological Association in New York City. Other readers should consider submitting appropriately revised versions of their pertinent convention presentations as well.

Renewals

Please renew your subscription at your earliest convenience -- if you have not already done so. If your renewal is not received by March 1, we will have to discontinue your subscription so that we can keep costs in hand.

The ABA Convention

The 1988 meeting of the Association for Behavior Analysis will be held in Philadelphia, May 26 through 30. Of particular note, this year's conference will have a mini-theme on the history of behavior analysis. Although the theme does not focus on interbehavioral psychology, one of the mini-theme's symposia will contain a paper by Linda J. Hayes, "The Psychological Present" that addresses interbehavioral issues directly.

Moreover, two interbehavioral symposia have been planned on other topics. The first will be chaired by Sidney W. Bijou and is entitled, "Psychological Linguistics: Examples of Research and Theory." It will contain papers by P. N. Williamson, Patrick M. Ghezzi, Morris, and Bijou, with discussant comments by Hayes. The second symposium -- "An Introduction to Interbehavioral Psychology" -- will be chaired by Bryan D. Midgley and contain papers by Paul T. Mountjoy, Morris, and Hayes, as well as discussant comments by Bijou. When we know more about other interbehavioral presentations at the convention, we will publish the information in the newsletter.

Information on convention registration and housing should be directed to Shery Chamberlain, ABA Offices, Department of Psychology, Western Michigan University, Kalamazoo, MI 49008.

Notes from the Field

The editor will be on sabbatical in the Department of Psychology at Harvard University for the 1988 spring semester, and at The May Institute (Chatham, MA) for the 1988 summer session. Newsletter production, however, will not be interrupted; indeed, the production deadlines may be even more expeditiously met. Submissions and correspondence should still be sent to the University of Kansas address.

Sandy Hobbs sent us a reprint of his "The Social Psychology of a 'Good' Story," in which he argues interbehaviorally that the study of contemporary legend is hampered by poor definitions. Also, Henry Pronko sent us an abstract for a book he is preparing for Greenwood Press; the book is entitled From AI to Zeitgeist: A Philosophical Guide for the Skeptical Psychologist.

COMMENTS

Control Systems Theory: Report on a Conference

Dennis J. Delprato

Eastern Michigan University

The 1987 meeting of the Control System Group was held October 7-11 near Kenosha, WI. Those interested in an authentically scientific behavioral science might be advised to examine the control systems literature and perhaps attend next year's meeting. When I addressed possible directions for behavioral science of the future (Delprato, 1986), included was a cybernetic (or systems) approach as exhibited in the work of K. U. Smith (see, e.g., Smith & Smith, 1966). Control systems theory (CST) and research is a vigorous contemporary area of activity that applies cybernetic thinking to human and nonhuman behavior. CST maintains ties to the contemporary general systems and the cybernetic movements; however, work by William T. Powers (e.g., Powers, 1973, 1978) and by those directly influenced by Powers' work (e.g., Marken, 1985, 1986) has served to give CST a distinctive flavor.

The main emphasis of CST is that it offers an alternative to conventional open-loop, one-way, lineal, external control. Far be it for a control system theorist to suggest that the answer to understanding behavior is going to be found in the environment. Far be it for a control system theorist to suggest that so-called reinforcers either select or strengthen responses. As I see it, readers already responsive to Kantor's approach might be intrigued with the possibility that the closed-loop control model is a way to do something with the double-headed interactional arrow of R§----¶S. Actually, CST does away with response and stimulus as formal constructs, but I am confident that Kantor's descriptive and transactional uses of these constructs can be shown to fit into the CST framework.

The above provides at least some context for a few remarks on observations I made at the 1987 conference itself. First, it was a conference for those who take their intellectual pursuits seriously. Forget bus tours, athletic events, and other activities best handled under other aegis. Something of value was going on from 8 a.m. to 1 a.m. Furthermore, the facility was over 40 miles from Chicago and at least 5 miles from Kenosha -- and what's in Kenosha? All sessions were plenary. The attendance was only about 25 in number, but given the intensity of participation this was more than sufficient. On the first morning, a schedule was arranged in which those who wished to present were identified. No one read papers, but most made available copies of papers related to their presentations as well as other relevant papers.

Second, the group was interdisciplinary, showing a great integrative potential of CST. I am sure I'll leave out certain specialties and misrepresent others, but the attendees apparently included experimental psychologists, a human factors specialist, clinical psychologists, counselors, sociologists, a piano teacher, a special education teacher, an economist, a cyberneticist-management scientist, a professor of clinical social work, a cyberneticist-mechanical engineer-publisher, and a systems engineer.

What about my reaction to more substantive issues that came up at the conference? Well, I put forth two papers, copies of which are available from me upon request. One paper suggested that cybernetics and CST offer a third alternative to how we approach control. Basically, the point was that closed-loop control can replace one-way, lineal, external control (determinism) and free will (which usually comes down to nonspatiotemporal, one-way control). The thesis of the other paper was that CST is a participant in the movement of behavioral science to the third stage of scientific thinking, that is, the field/system approach. I was disappointed, but not surprised, by the reaction of the group to these two theses. I think the crux of the matter comes down to the fact that both theses place great weight upon the radical idea of approaching history scientifically (e.g., Kantor, 1963, 1969). Included in such an approach is the recognition that science is not separable from other cultural

influences and the importance of distinguishing between valid scientific constructs based on events and invalid ones derived from cultural traditions. I fear that the CST movement could be held back unless participants more carefully take into account how it is that we come to make the statements we do. Thus, although CST has so much to offer regarding the pervasive control/cause issue, one reaction was that we should not too hastily disregard free will! Tied into this suggestion, I think, was a tendency to want to keep the organism autonomous in a traditional way instead of following through consistently with CST.

As far as the suggestion that CST is truly on to something and is part of a growing movement toward the latest way of approaching the world, it would not be surprising if "true believers" would respond something like: "Wait a minute, we are unique, we are ahead of everyone else, we have found the answer and are waiting for other enlightened ones to beat a path to our door. What do you mean, there are others who have come to our insights in other ways? No, they are but cheap imitations. There is no one else like us." Most innovative thinkers have fallen to this sort of thinking -- even Kantor at times. Yet sound historical analysis reveals that new ideas, like all other natural events, evolve and relate to other natural events. It does not appear that most CST advocates are fully aware of the devolution and evolution of the naturalistic cultural tradition. They tend to not appreciate that behavioral science (nee psychology) was given nonspatiotemporal soul as its subject matter and that the field is still struggling with all of the implications of naturalizing soul. In brief, most of my discomfort with observations made at the conference seems to come down to what I saw as carryovers from the transcendental phase of our cultural tradition. To use the vernacular, I felt frustrated because I knew that if anyone is prepared to definitively break with hoary ideas about the world, it is those who have been attracted to CST. Yet, even here they were making some very ancient mistakes.

For example, the perception construct is a source of much confusion. I heard statements of how we construct our world in our brain (reductionism was another problem), and that we only know our nerves (Newton-Locke-Kant, J. Muller). Nonetheless, I also heard one of the most naturalistic statements I ever heard concerning perceiving interactions.

The self construct was another source of confusion. Discussion of this for the most part was very much like that found in circles still under the influence of the older cultural tradition. Some speakers seemed to come quite close to suggesting that it is useful to think of a transcendental self. Yet, signs of truly new thinking were evident, as when some of G. H. Mead's ideas were inserted into the discussion.

There also was some talk of images in the head (shudder). Certainly, such theorizing is found in some of the highest circles of mainstream, that's mainstream, psychological science (which Mary Powers has suggested is oxymoronic), but this is no excuse for careless theorizing.

In brief, for purposes of "truth in advertising," I point out that those who have given careful consideration to the tortuous career of behavioral science and who thus are very sensitive to statements having their origin in the venerable transcendental, dualistic cultural tradition would have had some uncomfortable moments at the Control System Group conference. But we must be careful not to let such moments turn us away from an idea of extraordinary importance for the future of a naturalistic approach to behavior -- that of closed-loop control. I recommend study of Bill Powers' seminal Behavior: The Control of Perception (1973a) (note the emphasis in the title of this work), as well as the other references cited above. Furthermore, an overview of some current thinking in CST, as well as a basic bibliography, is available in the Winter, 1986 (No. 7) issue of Continuing the Conversation. This issue can be obtained for $1.00 from Continuing the Conversation, c/o HortIdeas, Rt. 1, Box 302, Gravel Switch, KY 40328. A one-year subscription to the CST newsletter is available for $10 from Control System Group, 10209 N. 56th St., Scottsdale, AZ 85253.

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Cognitive Psychology, Interbehavioral Psychology, and Clear Writing

Harry C. Mahan

Oceanside, CA

On numerous recent occasions, this commentator has seen references to what has become known as "cognitive psychology." These references have indicated that these areas of interest and emphasis have arisen due to the limitations of behaviorism in treating cognitive behavior. Instead of adopting interbehavioral principles, however, cognitive psychology has reverted to a mentalistic perspective with which an interbehavioral view cannot be reconciled. It was with considerable interest and curiosity, therefore, that I recently accepted a request to review an edited volume (Glaser, 1987) which contains several papers by people who are identified as cognitive psychologists.

Although my review has not yet been completed, reading the volume has demonstrated that my preconceived notion of cognitive psychology was somewhat off the mark. I have found the technology, about which I had no previous ideas, to be commendable and, within my limited ability to pass judgment, quite worthwhile. The theory underlying the cognitive approach has, in this case at least, been free from the brain dogma I had anticipated. Moreover, in spite of the overuse of such ambiguous terms as "process" and "representation" (which I have also found in J. R. Kantor's writings), clearcut mentalism is not readily apparent.

The most conspicuous characteristic of much of the writing in this volume, although absent in two of the four research papers, is a style of English composition that gives the definite impression of a lack of careful editing or even rereading on the part of, not just one, but several of the authors, including the book's editor. The technical descriptions of the research conducted were, without exception, written in a style that was natural and clear, but one cannot help concluding that the authors did not feel comfortable with strictly psychological materials and, in their self-consciousness, fell far below their writing capabilities. The writing, then, is an example of what may happen when experts in technical fields attempt to write theoretical descriptions. Such descriptions are difficult at best and require a degree of familiarity with a basic psychological perspective not taught in colleges and universities today and emphasized in the writing of only a few contributors to the current literature in psychology.

The examples given below are taken from a very few pages in the editor's introduction to the volume, but they should suffice to demonstrate my point. It is my contention that vast improvement in the clarity of much of this volume could have been accomplished with very little effort; moreover, since all of the papers, including the introduction, were written with the aid of United States government grant funding, the composition should have been reviewed with considerably more care. Here are sentences or partial sentences as they appeared and as they have been clarified. All of these, it will be noted, were found on just three pages.

In the phrase, "basic decoding skills of word recognition" (p. xi), "decoding"

is superfluous. In the phrase, "higher level skills of comprehension that integrate sentence ideas into memory" (p. xi), "ideas into memory" should be omitted and "sentence" should be plural. The phrase, "the cognitive analysis of human performance" (p. xi), should be "the analysis of human cognitive performance."

The sentence "[Naive students] have little principled understanding" (p. x), should read "[Naive students] have little understanding of the principles involved." "Thus, theories of knowledge should become targets for instruction" (p. x), should read, "Thus, theories of the principles involved should become targets for instruction."

Finally, the sentence, "As proficiency develops, these items of information become structured, are integrated with past organizations of knowledge, so that they are retrieved from memory rapidly in larger units; structuredness and accessability to interrelated chunks of knowledge become targets for instruction" (pp. ix-x), should read, "As proficiency develops, these items of cognitive reaction become integrated with past organizations of knowledge, so that they take place rapidly when needed; such patterns and their combination with additional cognitive patterns become targets for instruction."

This type of writing does not characterize most of the writing of the contributors, but, given their reputations and affiliations, such writing should not appear at all. When this commentator was assigned to report writing at the Education Center of the Marine Corps Schools, Quantico, Virginia in the early 1960s, every report was reviewed mercilessly by an editorial "murder board" before it was accepted for forwarding on "up the line." Review boards are good for all concerned and, especially where grant funding is involved, should be standard practice in psychology. Moreover, academic departments of psychology should understand, and incorporate within their teaching, the view that good theory gives rise to clear thinking, and clear thinking to clear writing.

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BOOK AND JOURNAL NOTES

Wendt, H. (1955). In search of Adam (J. Cleugh, Trans.). New York: Collier.

In describing the history of paleontology and the search for the origins of mankind, this work offers many insights and anecdotes on the history of science in general. Moreover, Wendt does an excellent job of covering the numerous personalities, theories, and in-fights along the way to modern views, using biographical sketches and imaginary dialogue to advantage. The second half of the book, on human paleontology since Darwin, is progressively less profitable, though, being now over 30 years out of date.

In the first half of the book, which is likely to be of most interest, Wendt commends the pre-Socratic Greeks and their "pure natural philosophy" (p. 153; see also pp. 154-157 on the ancient Greeks) -- though his enthusiasm for Aristotle is definitely muted in comparison to Kantor's. Other possibly surprising inclusions in the cast of characters are Sir Walter Raleigh, La Mettrie, Erasmus Darwin, Goethe, and Spinoza. Although these individuals had little to do with paleontology directly, they were involved in the gradual move from theology and authority to naturalism and the theory of evolution, which has a symbiotic relationship with modern paleontology. Considerable, but entirely suitable space, then, is devoted to Lamarck and Charles Darwin. Spinoza's cited contribution is also worth mentioning here: He supported the "natural reaction against the theory that had now been taught for two thousand years and had hardened into dogma, tending to separate body from soul and matter from spirit" (p. 152).

Despite mentalistic leanings which show up toward the end in a discussion of differences between humans and nonhumans, Wendt applauds each step towards naturalism and closer confrontation with events. A possible truism in the field of scientific historiography suggests itself here: The progress of science has been so clearly bound up with rejection of religious and other types of authority over science, that histories of science inevitably have a naturalistic ring. (Susan M. Schneider, University of Kansas)

ARTICLE

Not So Worlds Apart: Contextualism, Radical Behaviorism, and Developmental Psychology

Edward K. Morris

University of Kansas

In 1942, Stephen C. Pepper (1942/1960) published a book entitled World Hypotheses. In that enlightened, philosophical, but sometimes opaque work, Pepper presented what he took to be the four relatively adequate world views -- formism, mechanism, contextualism, and organicism -- formulated somewhat commonsensically according to their underlying root metaphors. Pepper's work, however, seemed to be of little consequence in psychology until the early 1970s. At that time, Hayne Reese and Willis Overton adapted Pepper's distinctions between the mechanistic and organismic world views as a basis for analyzing and explaining various tensions they observed in developmental psychology. Those tensions surrounded, and still surround, such issues as what are and are not (a) meaningful research questions, (b) appropriate research strategies, (c) acceptable explanations for empirical findings, and (c) adequate theories of development in general (see Overton & Reese, 1973; Reese & Overton, 1970).

The initial and continuing exemplars of the mechanistic and organismic world views established by Reese and Overton were, respectively, Sid Bijou and Don Baer's behavior analysis of child development (see Baer, 1970, 1973, 1976; Bijou, 1976; Bijou & Baer, 1961, 1965, 1978) and Jean Piaget's cognitive-developmental theory (see Piaget, 1952, 1970). The root metaphor for the former was said to be the machine and, for the latter, the biological organism. Although Reese and Overton presented the two views equitably, the general reception within developmental psychology has been to champion organicism and to dismiss mechanism. Indeed, mechanism has taken on quite pejorative connotations, and to be cast as a mechanist is among the gravest of aspersions (but see Zuriff, 1985, pp. 186-192, about the varieties of what it means to be mechanistic).

But mechanism and organicism are only two of Pepper's world hypotheses. In the past two decades, contextualism has emerged in the developmental literature as a third alternative, promoted initially in the 1970s by Klaus Riegel (1976, 1978) and now, in the 1980s, most notably by Richard Lerner (see, e.g., Lerner, 1984; Lerner, Hultsch, & Dixon, 1983). Moreover, psychology as a whole seems to be taking greater notice of contextualism, as evidenced recently by Rosnow and Georgoudi's 1986 text, Contextualism and Understanding in Behavioral Science (see also Sarbin, 1977).

The rather bold thesis of this paper is that, in contrast to traditional caricaturizations, the contemporary behavior-analytic view of development adheres to a contextualistic, not a mechanistic, world view (see also Reese, 1982; Ringen, 1976). This is not a dogmatic assertion because some characteristics of behavior analysis clearly suggest mechanism. The argument in full, of course, requires more space and intellectual acumen than is available, but the present analysis offers at least the patina of the rich quality and deep texture of this material.

A Little History

A discussion of contextualism seems appropriately begun with a little historical background, because contextualism's root metaphor is the "historic event." In the standard account, behaviorism and mechanism are taken to be linked through the legacy of Democritus's atomism, the material side of Cartesian dualism, and John Locke's espousal of an epistemological tabula rasa, on down through the empiricist and associationist movements in philosophy, from Berkeley and Hume, through James and John Stuart Mill, emerging together in psychology as John B. Watson's (1913) classical behaviorism. Although this might be an accurate account of what E. G. Boring (1950) called "behavioristics" (pp. 620-663), the historical antecedents to

contemporary behavior analysis lie less in the history of psychological science, and more in developments within biology.

A more accurate history of behavior analysis would trace it back (a) through Charles Darwin's (1859, 1871, 1872) theory of evolution and its effect on American functionalism (Herrnstein, 1969); (b) through Ernst Mach's (1883/1960) positivistic epistemology and its basis in biological economy (see Marr, 1985; cf. Skinner, 1945); and (c) through philosophical pragmatism as a theory of truth, as espoused by Charles Pierce (1940) and William James (1907) (see Zuriff, 1985, pp. 257-261).

Although a presentation of this revisionist history is beyond the scope of my more focused comments, the three lines of descent will emerge occasionally in what follows. In particular, Mach's positivism has a phenomenological character to it that belies the mechanistic distinction between the knower and the known (Day, 1969a, 1969b; see Giorgi, 1975). And, contextualism was literally born of the pragmatism of Pierce, James, John Dewey (1896) and George Herbert Mead (1934). Mach's positivism and American pragmatism stand in important contrast to the logical positivism and operationism that are typically mis-ascribed to behavior analysis today (Smith, 1986, see especially, pp. 259-297).

For further introduction and analysis of this history, I recommend as exemplary resources Willard Day's (1980) chapter in Rieber and Salzinger's Psychology: Theoretical-Historical Perspectives and Laurence Smith's (1986) recent Behaviorism and Logical Positivism: A Reassessment of the Alliance. Let me now, however, turn to the substance of my thesis by briefly describing Pepper's views of mechanism and contextualism before illustrating the contextualism of contemporary behavior analysis.

Mechanism and Contextualism

According to Pepper (1942/1960), the mechanistic world view would take behavior and environment to be parsed, respectively, into responses and stimuli existing as fundamental and discrete elements, out of which development in all its complexities and qualities is built. As for causation, the elements are said to act upon one another as do physical forces, the result of which are chain-like mechanical connections between, or sequences of, stimuli and responses. Causation is thereby immediate and contiguous, material, and efficient. In this view, the developing organism is characterized as passive and empty -- inherently at rest. It is just "being," and not very "becoming." Mechanism's locutions are nominal and thing-based.

As for Pepper's treatment of contextualism, a detailed elaboration will not be presented here; rather, the view will be unfolded more inductively in what follows. For the moment, however, the briefest of descriptions is offered. In contextualism, activity occurs in context and thus must be studied in context, for context gives activity its meaning, and does so through historical causation; in historical causation, change is constant; and change that is effective -- that is, "successful working" -- is the pragmatic criterion for truth. In this view, the developing organism is characterized as active and inherently changing. It is never just "being," but rather always "becoming." Contextualism's locutions are verbial and action-based.

With mechanism and contextualism now sketched, some of the specific consequences of these views are presented, both to contrast them and to illustrate the contextual nature of contemporary behavior analysis. The main thesis is not that mechanistic thinking is absent in behavior analysis, for it is not. Rather, my point is that, at a fundamental level, radical behaviorism (Skinner, 1953, 1969, 1971, 1974), especially when seen from a field-theoretic view such as J. R. Kantor's interbehavioral psychology (Kantor, 1959, 1972, 1981, 1982; see Smith, Mountjoy, & Ruben, 1983), yields a strong contextualistic flavor and bouquet for the varietals that are harvested from the contemporary behavior-analytic vineyard (Morris, 1982, 1984; Morris, Higgins, & Bickel, 1982, 1983). I say this at the risk of offending certain of my radical behavioral and interbehavioral colleagues. But, having toured their respective estates and sipped their best wine, I find their blend to be a superior product. To illustrate this, let me speak directly to some consequences of mechanism and contextualism, the first of which relates to elementarism and holism.

Elementarism versus holism. According to Pepper, mechanistic theories of

development would adhere to elementarism, as opposed to holism -- a position that represents the organism as but a collection of materially fundamental, atomic response elements. Complex behavior is then but a compounding of the basic elements, and identical response elements are taken to have identical meanings or functions -- such is the nature of a machine. The environment is similarly characterized and, in both cases, the whole is no more than the sum of its parts.

But this is not behavior analysis -- for at least two reasons. First, behavior is the unit of analysis, not the response (see Thompson & Zeiler, 1986). A response is a formal entity, and the analysis of behavior in terms of responses alone would quite plainly be reductionistic. Behavior, in contrast, is a dynamic relation, not a thing, in which a response is but one component. The molar unit of behavior includes not only responses, but also their correlated stimuli and their current and historical context. The contemporary behavioral view, then, is holistic in that the meaning or significance of a response exists not in itself, but only in relation to the myriad factors with which it co-relates. The function or meaning of a response is not identifiable on the basis of its form alone (see Krechevesky, 1939; Verplanck, 1954). Behavior analysis knows no fundamental, atomic response element, definable a priori. The stream of behavior is fluid and dynamic, and the banks of its lines of fracture slippery and ever-changing.

Second, if responses and stimuli have no inherent function, then physically identical responses and stimuli need not have the same functions or meanings within or across individuals (Baer, 1981). Indeed, it must be that they can never have exactly the same function given that the context of each individual's history is unique and ever-changing. Moreover, physically dissimilar responses and physically dissimilar stimuli can have identical functions or meanings. In general, then, the process-achievement, means-end relation is a highly dynamic one in behavior analysis.

These latter points may seem trivial and obvious, but they are among the most common misuderstandings about behavior analysis. For instance, a columnist in Psychology Today recently wrote that behavioral psychologists assume that "all people respond similarly to the same stimuli" (Zilbergeld, 1984, p. 9; for analyses of this and other misunderstandings, see Bijou, 1979; Horowitz, 1975; Todd & Morris, 1983; for brief, informed presentations of the behavior-analytic view, see Branch, 1987; Michael, 1985; Reese, 1986). Nothing could be further from the truth -- but, on with the analysis and a presentation of the second consequence of the mechanistic and contextual world views, this one related to the problem of causation.

Causal analysis versus functional analysis. In mechanism, the analytic task is to account for behavior in terms of antecedent-consequent relations, that is, as contingent and contiguous cause-and-effect. As a conceptual and investigative tactic, this antecedent-consequent procedure of parsing behavior into linearly sequenced stimuli and responses is obviously central to many practices within behavior analysis, and it no doubt conditions many behavior analysts into a mechanistic-leaning world view.

In a more broadly based view, though, the analysis of behavior proceeds more contextualistically. It focuses, first, on the general function of behavior as adaptation and, second, on the general structure of behavior as the interrelation of concurrent stimulus and response functions within their current context. The latter is exactly the point B. F. Skinner made in his 1935 analysis of the generic nature of stimuli and responses (Skinner, 1935). Subsequently, however, more specific conceptual structures have been invoked, subclassifying these functional interrelations into at least respondent and operant processes. In these latter units of behavior, behavior is typically analyzed in terms of contiguous antecedent-consequent relations, but again, that is more heuristic than model, though clearly some tension exists (Baltes & Reese, 1977).

Development as behavioral versus structural change. A third consequence of the mechanistic and contextualistic world views has to do with the characterization of development. From the mechanistic perspective, development is little more than change in responding across increasing chronological age. Development occurs as a continuous, linear succession

of cause and effect, where change is reducible to, and predictable from, its prior forms.

In contrast, the contextual character of behavior analysis focuses on change as the development of interrelated stimulus and response functions, not change in the form of a response alone. In this sense, then, development refers to changes in behavioral structure, not responses -- that is, to changes in the interrelations among stimulus and response functions and their contexts. In contextualism, change is not a derived category, but rather is categorical. Historical causation means that stimulus and response functions are inherently in transaction.

The passive versus the active organism. A final consequence of the mechanistic and contextualistic world views to be discussed is the passive versus the active nature of the organism. Within the mechanistic account, causation fits the Newtonian model of the universe wherein efficient causes affect material -- material that is otherwise at rest. More specifically, stimuli and responses are cause and effect, the former operating in a unidirectional, linear fashion. This is not to say that mechanists do not talk about "interactions" between stimuli and responses, but such talk is still usually about underlying unidirectional causality -- causality that bounces back and forth between the effects of stimuli on responses and of responses on stimuli. Research on "child effects" on adult behavior (see, e.g., Bell & Harper, 1977), for instance, is not inherently contextualistic; it can be construed mechanistically as well.

In contrast, even though the unit of behavior in behavior analysis is parsed into stimuli and responses, the focus is on their mutually defining functions. More importantly, those functions stand in a strong reciprocal or, better -- in Dewey and Bentley's (1949, p. 108) terminology -- in a transactional relation with one another. They are mutually defining and implicative. From this perspective, the behavior-analytic view is that people are "active," not "passive," participants in their development. In saying that people are active participants, however, behavior analysts are not implying that people are self-actional, autonomous agents. Likewise, though, neither should they assert that the environment is the ultimate autonomous cause, the practical utility of this perspective notwithstanding. The tension here between these views of causality is at the heart of the trait-situation debate, which shares some logical characteristics and flaws with the nature-nurture issue, and hence I would like to elaborate further in describing the active agent in behavior analysis.

Within the behavior-analytic view, stimulus functions and response functions develop historically and simultaneously, and are defined with respect to one another. As such, stimulus functions have no more control over behavior than do response functions -- the two are interdependently and mutually defining. Hence, a situation does not compel a response to occur except through a person's historically derived response functions for that situation. But, neither does a person compel a response to occur except through the situation's historically derived stimulus functions for the response. Thus, situations do not possess independent or inherent power to control behavior any more than persons possess independent power for such control. Both are products of unique, ever-changing interactional histories.

Interactions may display qualities attributed to personal or situational control, depending on how they are viewed or investigated, but those attributions are shorthand conventions derived from an overemphasis on organismic and mechanistic thinking, as opposed to a contextual world view. What is really objectionable here is the reification and causal status given to the person as an active agent or to the situation as a stimulating force. Predictions can be made about behavior on the basis of information about persons or situations, but the ability to do so does not thereby bestow causal power on either. Indeed, to assert that such power exists at all moves the analysis of behavior away from the transactional account it requires. For further commentary on the transactional nature of contemporary behavior analysis, Henry Pronko and David Herman's article, "From Dewey's Reflex Arc Concept to Transactionalism and Beyond" is recommended (see the Fall, 1982 issue of Behaviorism; see also Keehn, 1980).

The Context in Contextualism

Before closing, a few comments about context are in order, for "context" is

currently being bandied about with increasing frequency in behavior analysis and mainstream developmental psychology. In behavior analysis, context often refers to contextual determinants of behavior. These, however, are typically no more than broad and sometimes unspecified sets of multiple antecedent causes (cf. Balsam & Tomie, 1984). But likewise, in developmental psychology, context often refers to no more than the complexity of various levels in which behavioral development occurs (see, e.g., Bronfenbrenner, 1983). In both cases, these factors are critically important to consider, but they are not inherently a part of a contextualistic world view. Indeed, talk of context sometimes gives the facade of greater scope, but with the loss of precision. Context, as such, is quite happy in mechanism, and is not categorical in contextualism. To give contextual causes their due is not to embrace contextualism as a world view.

Still, within behavior analysis, current concerns over context illustrate that the field does not reduce behavior to mere stimuli and responses, but rather that it adheres strongly to the view of behavior as having a reciprocal and dynamic nature. Behavior analysis has, from its very beginning, acknowledged the importance of both the historical and current context, though admittedly often only implicitly so. Concurrently, however, we see presentations of behavior analysis that make the roles of phylogenic, ontogenic, and interactional historical causation quite explicit. And, given that the historical context establishes what reciprocal stimulus and response functions may occur at any one time in the first place, the current context establishes what behavior can and will then occur. As for what can occur, the structure of the current context, as manifest in organismic and environmental structure, establishes what formal responses and stimulation can or cannot physically take place. And, as for what will occur, the function of the current context establishes what behavior takes place by giving meaning to stimuli and responses (e.g., establishing operations; see Michael, 1982). As with the nature of historical causation, the structure and function of the current context should also be seen as dynamic and everchanging.

Conclusion

In conclusion, this characterization of behavior analysis as contextualistic remains vastly underdeveloped. Indeed, for the mechanist and the organicist, the argument may seem merely to have translated mechanism into a contextualistic language without really having presented contextualism as a viable alternative. Contextualism, itself, also rests on a slippery slope -- it seems forever about to slide into organicism or into mechanism (see also Overton, 1984).

Nonetheless, if the behavior analysis of development and mainstream developmental psychology are both moving towards contextualism (see Hayes, 1986; Hayes & Brownstein, 1986, pp. 177-178; Morris, 1986), then perhaps they can interact -- even transact -- in a productive manner. For the past 20 years, they have been speaking across paradigms, or across domains, or not speaking at all (Morris, Hursh, Winston, Gelfand, Hartmann, Reese, & Baer, 1982). Developmental psychologists dismissed behavior analysis as mechanistic and, if you will, cogniphobic -- that is, pathologically unwilling to entertain the domain of cognition. And, behavior analysts have dismissed developmental psychology as organismic and cogniphiliac in domain. In fact, the world views and domains often become confused as the basis of these differences. The differences were often seen as incommensurable on the cogniphobic-cogniphiliac dimension, whereas the incommensurability has largely been a function of differences in world view.

With a convergence of behavior analysis and developmental psychology towards contextualism, however, they become commensurable. Moreover, cognition as a domain of development becomes a matter of data not, in Pepper's word, of "danda," for cogniphobia and cogniphilia are orthogonal to the world views. This argument is not that behavior analysis and developmental psychology will become one, but that once we see that both adhere to a common world view, then neither can dismiss the other on the grounds of paradigmatic incommensurability. And, as a consequence, they must confront important issues, such as cognition and intentionality, straight on and perhaps will do so more effectively than before (Day, 1976; Deitz & Arrington, 1984;

Morris, 1985; Wittgenstein, 1953).

In sum, if these opposites become apposites, their attraction may give way to romance and, perchance, to marriage, and then to children of unbounded potential. What better a development could one ask for?

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