THE INTERBEHAVIORIST

A Quarterly Newsletter of Interbehavioral Psychology

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TABLE OF CONTENTS

QUOTATION

Why has the part-whole problem not been solved before? Simply because the human mind has not been relativistic enough. It has been too absolutistic: atomistic, mechanistic, and vitalistic.

- Wheeler, 1935

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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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NOTES FROM THE FIELD

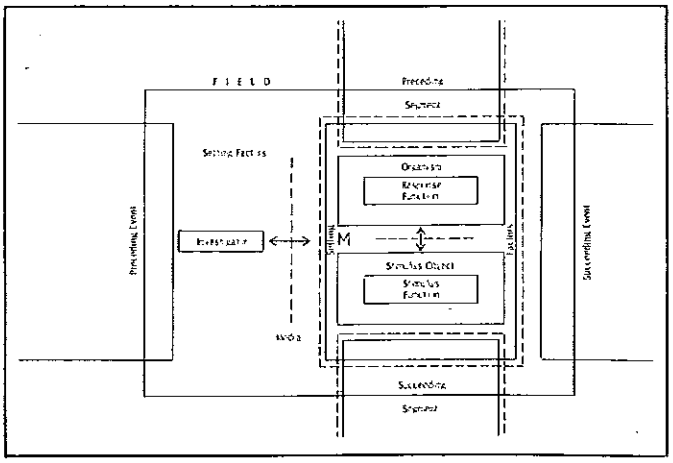
The Spring, 1986 issue of The Psychological Record contained articles by DAVID CORNWELL and SANDY HOBBS on "Spontaneous Play in Childhood" and by IVER H. IVERSEN on "Time Allocation, Sequential, and Kinematic Analyses of Behaviors Controlled by an Aperiodic Reinforcement Schedule." Also in that issue, PAUL T. MOUNTJOY and DONNA M. CONE contributed book reviews of Matarazzo, Weiss, Herd, Miller, and Weiss's Behavioral Health and of Trower's Radical Approaches to Social Skill Training, respectively.

IVERSEN also published an article with Sidman and Carrigan in the Journal of the Experimental Analysis of Behavior entitled "Stimulus Definition in Conditional Discriminations," 1986, 45, 297-304.

R. J. Corsini's (Ed.) (1984) Encyclopedia of Psychology (Wiley) contains a contribution by N. H. PRONKO on interbehavioral psychology. A brief biographical sketch of Professor Kantor also appears in that volume.

DOUGLAS H. RUBEN published "The 'Interbehavioral' Approach to Treatment" in the Journal of Contemporary Psychotherapy, 1986, 16, 62-71 and, with Marilyn Ruben, "Is Applied Behavior Analysis Ready for Parapsychology? A Return to Natural Events" in Psychology: A Quarterly Journal of Human Development, 1985, 22, 1-11.

NOEL W. SMITH's "Belief Systems: A Psychological Analysis" appeared in The Mankind Quarterly, 1985, 25, 195-225.



THE AGORA

We received some kind words on the contents and length of the last issue. Thank you. The compliments, though, go to those who contributed the material. As for your urging us to publish lengthier issues, we are delighted to do so, but that will take your continued and perhaps increased support. As you will see from the current issue, though, the support is out there.

The response to Dennis Delprato's (Eastern Michigan University) guest-edited issue of the newsletter was extremely positive, and hence we will continue the editorial policy of having occasional guest editors. In fact, we are pleased to announce that this year's fourth issue (October) will be guest-edited by Linda J. Parrott (Saint Mary's University). Linda will have a selection of papers she has co-authored with her students and will bring us up to date on interbehavioral activities at the Association for Behavior Analysis.

On other matters, Sandy Hobbs (Paisley College of Technology) sent us abstracts and papers from the April, 1986 conference of the European Experimental Analysis of Behavior Group. Of most interest was a symposium chaired by Edward Blewitt entitled "Kantor and the Behaviorist Tradition." We will present more information on that in a subsequent issue.

Arthur Kahn (Baltimore, MD) offered us some interesting observations and a query on a new book by John M. O'Donnell, The Origins of Behaviorism: American Psychology, 1870-1920 (see the Journal and Book Notes section for a brief review). Let me excerpt from his letter:

> There is an unusual reference to J. R. Kantor on p. 219. In a discussion of the University of Minnesota, the author states: "It is no surprise that many of the leading behaviorists including G. M. Guthrie, Joseph Peterson, J. R. Kantor, Karl Lashley, and B. F. Skinner spent portions of their careers at Minnesota." It is amazing how the author put two and three together and got five to make his point. He tries to equate applied psychology to behaviorism in its early development. Frankly, I never considered Lashley a behaviorist. It would be interesting if someone...could be encouraged to determine from what is available why Kantor went to Minnesota.

Please let us know if any sleuthing on this last point by the newsletter's readers turns up an answer to Art's query.

Not all of popular psychology in print is terrible. Noel W. Smith (SUNY-Plattsburgh) sent us a clipping from a health column witten by Lester Coleman, M.D. in which the columnist answered an inquiry about whether fish was "brain food" and whether eating it would make the reader smart. Coleman responded with a "No," and commented further: "The brain is an organ that flourishes and grows on good nutrition -- as does every other organ of the body....Intellectual development is the result of reading, studying, and exposure to the sciences and humanities. These are really the 'brain foods' that are so vital to that development." Well said!

That is all from the editor's desk. Good reading, to you.

Reassessment in Psychology

Noel W. Smith (SUNY-Plattsburgh) informs us that University Press of America has recently reprinted more copies of his Festschrift for Professor Kantor (with Paul T. Mountjoy and Douglas H. Ruben), Reassessment in Psychology: The Interbehavioral Alternative. The publisher's address is P.O. Box 19101, Washington, DC 20036.

New Ideas in Therapy

The chapters for Douglas H. Ruben and Dennis J. Delprato's (Eastern Michigan University) New Ideas in Therapy have been submitted to Greenwood Press. We look forward to the book's publication. As the book now stands, the chapter authors and chapter titles are as follows:

Section I: Foundations of the New Therapy

1. J. R. Kantor: What Qualifies Interbehavioral Psychology as an Approach to Treatment?
2. N. H. Pronko: Theory versus Practice
3. Paul R. Fuller: From the Classroom to the Field and Back

Section II: Methods

4. Robert W. Lundin: The Interbehavioral Approach to Psychopathology
5. Robert G. Wahler and Della M. Hann:

Interbehavioral Approach to Clinical Child Psychology: Toward an Understanding of Troubled Families

6. F. Dudley McGlynn, Edwin W. Cook III, and Paul E. Greenbaum: Toward an Interbehavioral Medicine
7. D. J. Delprato: Clinical Implications of Response Patterns
8. William Stephenson: Q-Methodology: Interbehavioral and Quantum Theoretical Connections in Clinical Psychology
9. Douglas H. Ruben and Marilyn J. Ruben: Assumptions about Teaching Assertiveness: Training the Person or Behavior?
10. Dallas W. Stevenson and Michael Hemingway: Multidisciplinary Approach to Obesity and Risk Factor Management
11. Edward K. Morris, Lisa M. Johnson, Steven E. Larsen, Lynda K. Powell, James T. Todd, and Jane B. Atwater: Interbehavioral Perspectives on Legal Deviance: Some Considerations of Context
12. Lynne A. Daurelle, Ann P. Kaiser, and James C. Fox: An Interbehavioral Perspective on Parent Training for Families of Developmentally Delayed Children
13. Mary Ann Scafasci: Community-Based Psychological Services for Developmentally Retarded Persons
14. Donna M. Cone: Public Policy Research from a Field-Theory Perspective

The Principia Press

Listed below are the currently available of Professor Kantor's books. Please check your bookshelves, and those of your institutional library and bookstore, for possible oversights. The books may be purchased directly from Principia Press, 5743 Kimbark Avenue, Chicago, IL 60637. Handling charges are $.75 per title; prepaid orders are post free.

Principles of Psychology (2 vols.) $20.00
Psychology and Logic (2 vols.) $25.00
Interbehavioral Psychology $15.00
The Logic of Modern Science $15.00
An Objective Psychology of Grammar $13.00
The Scientific Evolution of Psychology (2 vols.) $40.00
The Science of Psychology: An Interbehavioral Survey $20.00
Psychological Linguistics $15.00
Interbehavioral Philosophy $27.50
Cultural Psychology $16.00
Tragedy and the Event Continuum $15.00
Selected Writings, 1929-1983 $20.00
Psychological Comments and Queries $20.00

The Mahan Book

As mentioned in previous issues of the newsletter, Harry Mahan (Project Socrates) has generously donated the remaining copies of his text, The Interactional Psychology of J. R. Kantor: An Introduction (Mahan, 1968), to us for resale to help finance the newsletter. The book contains a full-page photoportrait of Professor Kantor, chapters on the interbehavioral approach to major topics in psychology, and a bibliography of Professor Kantor's publications through 1963. The book is available through us for $5.00 (U.S.) or $7.50 (foreign).

New Subscribers

We appreciate all efforts made to promote the newsletter, especially in university, college, and institutional libraries. Subscription information is provided inside the front cover. The new subscribers since the last issue are:

Julie Castle (Plattsburgh, NY)
Paul R. Fuller (Grand Rapids, MI)
Andrew D. Hill (Ypsilanti, MI)
Donald E. Jackson (Eastern Michigan U.)
Carolyn Koblin (Eastern Michigan U.)
Reginald Marsack (Eastern Michigan U.)
My Book Service (Japan)

* * *

The quotation on the front cover of this issue was submitted by Susan M. Schneider (University of Kansas). The reference is: Wheeler, R. H. (1935). Organismic vs. mechanistic logic. Psychological Review, 42, 335-353.

BOOK AND JOURNAL NOTES

Ceci, S. J. Memory development [reviews of Cognitive learning and memory in children and Basic processes in memory development]. Science, 1986, 231 (March), 231.

John M. Grossberg (San Diego State University) sent us Ceci's review of some recent books on memory, which contains Ceci's comments on how current research might be revitalized. Some of these views have a distinctly interbehavioral flavor, as in the following:

> The evidence presented in several chapters suggests that memory processes cannot be adequately studied in the disembedded laboratory contexts that have characterized the majority of work on the subject. Contexts vary in the effectiveness with which they recruit mnemonic strategies, foster motivation, and shape one's perception of the particular memory task at hand. One of the many important messages of these volumes is that the exclusive reliance upon laboratory contexts is likely to result in misleading models of memory development. (p. 231)

* * *

Lewontin, R. C., Rose, S., & Kamin, L. J. (1984). Not in our genes: Biology, ideology, and human nature. New York: Pantheon.

In commenting on this book in Volume 12 (number 4), James T. Todd was favorable about what the authors' interbehavioral orientation, but was critical of misunderstandings the authors had about contemporary behaviorism. There is still much that is good in the book, which John M. Grossberg (San Diego State University) reminded us of with the following quotation from it.

> We describe [our position] as dialectical, in contrast to reductionist. Reductionist explanation attempts to derive properties of wholes from intrinsic properties of parts, properties that exist apart from and before the parts are assembled into complex structures. It is characteristic of reductionism that it assigns relative weights to different partial causes and attempts to assess the importance of each cause by holding all others constant while varying a single factor. Dialectical explanations, on the contrary, do not abstract properties of parts in isolation from their associations in wholes but see the properties of parts as arising out of their associations. That is, according to the dialectical view, the properties of parts and wholes codetermine each other. The properties of individual human beings do not exist in isolation but arise as a consequence of social life, yet the nature of that social life is a consequence of our being human and not, say, plants. It follows, then, that dialectical explanation contrasts with cultural or dualistic modes of explanation that separate the world into different types of phenomena -- culture and biology, mind and body -- which are to be explained in quite different and nonoverlapping ways. (p. 11)

* * *

Lundin, R. W. (1985). An objective psychology of music. Malabar, FL: Robert E. Krieger.

Science and music are often regarded as lying on opposite ends of a continuum, the former being characterized by objective demonstration, the latter by subjective expression. Psychologists interested in the study of music must overcome such obstacles in arriving at an adequate understanding of musical phenomena. In An Objective Psychology of Music, Lundin (1985) demonstrates that what is ostensibly subjective can be examined scientifically, and to good effect.

Lundin treats musical behavior as any other behavior, analyzable into of stimulus and response functions. On the stimulus side, we find chapters on the various dimensions of tone (e.g., pitch and timbre) and their combinations (e.g., melody and harmony). On the response side, we find chapters on affective and aesthetic responses to music and the nature of musical ability. Lundin also provides material of special interest to

applied psychologists, with a discussion of the uses of music in industry and therapy. Regardless of the topic, though, Lundin consistently stands on firm scientific ground by relying on empirical research to derive his conclusions.

Although various theoretical perspectives are given due consideration, Lundin maintains an interbehavioral view throughout. For instance, instead of conceptualizing musical ability as a single trait, he states that "it consists of a number of acquired interrelated behaviors built up through a process of interaction of individual organisms with musical stimuli throughout the life history" (p. 214).

In all, the book is comprehensive, well organized, and serves as a useful introduction to a scientific approach to the study of music. (Steven E. Larsen, University of Kansas)

* * *

O'Donnell, J. M. (1985). The origins of behaviorism: American psychology, 1870-1920. New York: New York University Press.

The Origins of Behaviorism is meant to be an alternative to traditional histories of psychology which emphasize the intellectual and scientific foundations of the discipline. Thus, according to O'Donnell, "The object of this inquiry is not to trace the intellectual sources of Watson's formulations but to provide a history of the emergence of the disciplinary pattern that supported those fomulations" (p. x). That is, the book is an analysis of the evolution of the contexts within the university system that allowed the development and existence of early behaviorism.

In this regard, the book is true to its purpose. The author provides a great deal of information about the development of the university system throughout the nineteenth century and shows how this development supported the rise of professional academic psychology. In addition, he describes how the formation of psychology departments separate from philosophy was based as much on institutional and practical considerations as on intellectual ones. In illustrating his analysis, O'Donnell quotes numerous original sources of the era, including much personal correspondence between early psychologists, and thereby provides a sometimes intimate view of the personal, political, and academic bases for many aspects of early of behaviorism.

Because the emphasis of the book is on the institutional contexts in which behaviorism developed only a sketchy description of its intellectual and scientific development is provided. Occasionally, this underemphasis of the scientific behavior of the early psychologists makes parts of the book seem incomplete and, therefore, difficult to follow. The author's purpose was not, however, to include background for a history of the scientific development of behaviorism. So, in order to fill some gaps, readers might want to have handy a history of psychology's scientific development during these years, such as Boakes' (1984) From Darwin to Behaviorism or Boring's (1950) A History of Experimental Psychology. (For a generally earlier scientific analysis, see Kantor, 1963, 1969).

An important consideration for potential readers is that the author occasionally clouds his narrative with a bewildering amount of detail. In the first half of the book especially, the sheer number of people, dates, and places mentioned can be overwhelming. In some cases, the people and events will seem obscure even to psychologists who are familiar with the history of their discipline. As the book progresses, however, the focus of the text narrows from a wide-ranging analysis of the evolution of academic psychology within the larger university system to a description of the development of behaviorism itself in relation to the social and institutional pressures on academic psychology. The narrower focus in the last half of the book is accompanied by less cluttered and more readable prose.

One aspect of the author's attention to detail should be mentioned separately. The author supports some of his points by citing numerical data, such as the number of hours of class time offered by universities in various subject areas or the number of publications in a given area in a particular year. Although most of these statistics are probably accurate enough to support specific points, some of them seem too accurate to be true. For

example, the number of hours offered in various subject areas is extrapolated to the nearest hour out of thousands of hours (an accuracy much better than one percent). In a similar manner, the author reported that in 1908, "only six animal experiments were published, representing 4.1 percent of the total output of American laboratories and a mere six-tenths of 1 percent increase over the previous decade" (p. 181). An examination of the 1908 Journal of Comparative Neurology and Psychology, however, shows that it alone contained at least seven articles describing animal experiments; John B. Watson himself published two articles in other outlets describing numerous separate experiments on monkeys and birds (Watson, 1908a, 1908b), and several other psychological and physiological journals of 1908 reported experiments on the behavior of animals. Furthermore, the cited 0.6% increase in the number of animal experiments published cannot be correct based on arithmetic considerations alone. Although most of the arguments in the text probably do not depend on 100% numerical accuracy, that such unlikely numbers were cited and not verified could detract slightly from readers' confidence in the accuracy of the book in general. (James T. Todd, University of Kansas)

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Watson, J. B. (1908a). Imitation in monkeys. Psychological Bulletin, 5, 169-179.

Watson, J. B. (1908b). The behavior of noddy and sooty terns. In Papers from the Tortugas Laboratory of the Carnegie Institution of Washington (Vol. 2, pp. 187-255). Washington DC: Carnegie Institution.

Rucker, D. (1969). The Chicago pragmatists. Minneapolis: University of Minnesota Press.

This well-written survey provides a historical and substantive exposition of the Chicago School of pragmatism (1890s to 1920s; John Dewey, most famous member), including chapters on the early years of the University of Chicago (with interesting political details), and on education, religion, and other applied aspects of pragmatism. The book ends with an analysis of why the School produced fruitful interdisciplinary cooperation, and notes its later influence.

The chapters on the philosophy of pragmatism, itself, and on functionalism (psychology's version of pragmatism) and behaviorism, would probably be of most interest to interbehaviorists. Be forewarned, though, that the philosophy chapter was rather heavy going for this novice. Nonetheless, perhaps the following will whet the appetite. From the preface,

> Because the Chicago philosophy reflected an awareness of the interconnections among the advances being made in biology, psychology, and sociology, it was able to provide a method and a perspective for an array of disciplines. (p. vi)

And, from the chapter "Psychology: Functionalism and Behaviorism,"

> Titchener was quick to seize upon what appeared to be two vulnerable spots in functionalism: its teleological leanings and its connection with philosophy. To him, both aspects were unscientific. (p. 62)

Kantor is mentioned once, as "one of several [Chicago students in psychology] who became well known, but not as functional psychologists" (p. 70). (Susan M. Schneider, University of Kansas)

* * *

Sameroff, A. J. (1983). Developmental systems: Contexts and evolution. In P. H. Mussen (Ed.), Handbook of child psychology (Vol. 1, pp. 238-294) (4th ed.). New York: John Wiley and Sons.

Sameroff's chapter is full of distinctions between "systems" thinking and more mechanistic modes of action. Kuo, Schneirla, Piaget, Reese, Overton, Bertalanffy, and Laszlo, to name a few, are presented as expressing the view that an understanding of developmental psychology requires that it be viewed in terms of organized "transactions" between active participants. The author concludes that general systems theory (GST) offers the rubric under which such scientific phenomena can be best described.

Sameroff, at times, however, strays too far from pristine events in pursuing particular analogies. For instance, in pursuing the extension of Laszlo's GST principle of "adaptive self-stabilization" (p. 266) to ontological biological development, he states:

> When a deviation occurs during embryological development...the self-righting tendencies must be directed at reaching the developmental level where the fetus should be at the current point in time rather than the developmental level at the point in time at which the deviation occurred. One can imagine the complexity of genetic coding that is required to control the interaction of the developing infant with a temporal series of environmental conditions so that the resulting newborn is within minimal range of variability. (p. 267)

Samaroff appears to be bestowing too much responsibility on the organism's genetic make-up as a controlling agent. This is quite different from treating different levels of organization as "potentiality-providing entities" (which Sameroff does elsewhere on other matters).

Considering the other valuable assets of Sameroff's chapter, however, such criticism seems nit-picky. Certainly, his support of the following assertion is more representative of his work:

> ...some biologists see the functional environment as so tightly organized and specific to each organism that it is no longer seen as an environment, but rather as part of the organism. (p. 242)

Sameroff would profit from an appreciation of Kantor's "interbehavioral continuum" and we would profit from a reading of Sameroff. (Bryan D. Midgley, University of Kansas)

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Snyder, A. W., Bossomaier, T. R. J., & Hughes, A. (1986). Optical image quality and cone mosaic. Science, 1986, 231, 499-500.

This paper is interesting because it shows that we don't have to shape our behavioral observations to fit an invariant physiology. Instead, the authors conclude that the cone mosaic "in real eyes" varies according to the organism's "habitat and life-style," a mutually interactive system if there ever was one. (John M. Grossberg, San Diego State University)

* * *

Thompson, T., & Zeiler, M. D. (Eds.). (1986). Analysis and integration of behavioral units. Hillsdale, NJ: Lawrence Erlbaum.

This volume is based on a conference in honor of Kenneth MacCorquodale's 65th birthday. The contributors were Zeiler, Thompson, Meazzini, Ricci, Harzem, Hineline, Baer, Dews, Marr, Catania, Cerutti, Sidman, Schnaitter, Lubinski, Thompson, Meehl, and Falk.

The general topic of the conference -- the nature of units of analysis in behavioral science and the mechanisms responsible for their integration -- has been an important issue throughout the history of psychology. Although the implied purpose of the conference was to attempt to discover the basic unit of analysis in a science of behavior, many of the contributors correctly noted that in selecting units of analysis, no rigid and formal specifications should be developed, suitable only for specific situations (cf. Kantor, 1950, p. 285). Moreover, many of the authors questioned the appropriateness of attempting complete analysis of behavioral situations in terms of only two basic units -- operants and respondents -- and, in addition, some authors explicitly emphasized the relational nature of behavioral units. These are steps in the right direction.

Throughout the book, the historically

characteristic overemphasis on the response factor within the experimental analysis of behavior (TEAB) is still evident. The issues addressed in many of the book's chapters, however, suggest that progress is being made in terms of the tendency within the TEAB to transform events into simpler or perhaps different events. Whether this tendency has been due to scientific conventions (e.g., rate of responding is the appropriate measure of response strength), available apparatus (e.g., an operant chamber), or available species (e.g., rats and pigeons), it is refreshing to see behavior analysts attempt to enlarge their domain of analysis. For instance, over half of the book explicitly addresses issues related to complex human behavior, in accordance with a recommendation Kantor (1970) put forth to TEAB a number of years ago.

Kantor himself is not referenced in the book, although a quote from Schoenfeld (1969) describing Kantor's views on the relationship between language and logic appears in Marr's chapter. Those familiar with Kantor's work, however, may recognize a trend in many of the chapters towards a more field-theoretic approach than has traditionally been explicit in TEAB -- a trend that is likely to lead to more complete analyses. (Lisa M. Johnson, University of Kansas)

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Kantor, J. R. (1970). An analysis of the experimental analysis of behavior (TEAB). Journal of the Experimental Analysis of Behavior, 13, 101-108.

Schoenfeld, W. N. (1969). J. R. Kantor's Objective psychology of grammar and Psychology and logic: A retrospective appreciation. Journal of the Experimental Analysis of Behavior, 12, 329-347.

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Ribes, E. (1985). Human behaviour as operant behaviour: An empirical or conceptual issue? In C. F. Lowe, M. Richelle, D. E. Blackman, & C. M. Bradshaw (Eds.), Behaviour analysis and contemporary psychology (pp. 117-133). Hillsdale, NJ: Lawrence Erlbaum.

Ribes's chapter is one of sixteen that were published from the First European Meeting on the Experimental Analysis of Behavior. Unfortunately is the only chapter to cite the work of Professor Kantor, though other chapters touch on contextualism and integrated-field thinking.

Ribes raises the fundamental issue of whether current research within the experimental analysis of behavior is experimental research on human behavior or only operant research using human subjects. Sadly, he concludes, it is the latter, and he is doubly sad because this promotes the view that all human behavior is operant behavior. One result of the restriction to the operant paradigm is that researchers have been led to posit the covert verbal responses to account for nonhuman-human differences, whereas an integrated-field perspective would handle the issue more naturalistically. In his words,

> Looking at differential complexity in the mediation of contingencies allows for the recognition of hierarchical levels in the organization of behaviour, without the need to postulate internal entities or autonomous emergent principles. (p. 130)

Ribes presents a scholarly overview of the reflexological tradition in philosophy and biology, and its inadequacies for human (and nonhuman) behavior, especially as this tradition relates to (a) causality relations, (b) atomistic discontinuous-state categories, and (c) contextual and historical conditions. As to the first, Ribes is especially praising of Ernst Mach's work, though such movement within behavior analysts will have to be accompanied by shifts in paradigmatic assumptions.

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We encourage readers to submit brief book and journal notes for this section of the newsletter. Many readers have spoken of their value. When you do submit material, please include the address of the authors so that we may send them a copy of the newsletter; we will automatically send book comments and reviews to the publishers.

COMMENTS

A Matter of Interpretation?

A Reply to Fox

N. H. Pronko

Wichita State University

In his "A Reply to Pronko," Fox (1986) raises a number of points, none of which are at issue except the one pertaining to the role of setting factors. What bothers me is the apparent lifting out of a single aspect of a prior, integrated field event (i.e. the setting factors, stimulus functions, etc.) and endowing that single feature with a special force or status influencing succeeding events. Such a procedure would amount to fragmenting the prior event in violation of Kantor's ingenious formulation of an integrated field, a formulation which treats all the relevant aspects of a field evenhandedly.

At this point in a personal conversation along these lines, a colleague, Grant Kenyon, protests: "But a construct can't include every single feature of relevant, prior connected events." This is a proper criticism. The resolution? An "awareness of abstracting," so that, when certain features of antecedent events need emphasizing, we do so without fragmenting them and with the full realization that such features do their job only within the behavioral context in which they are embedded, without which they, by themselves, are psychologically meaningless. That was my only concern.

Fox, J. (1986). On setting factors and interbehavioral research: A reply to Pronko. The Interbehaviorist, 14, 16-18.

* * *

On Some Controversial Terms

Harry C. Mahan, Oceanside, CA

Some of the most exasperating problems which psychologists have to face are in developing a terminology which they can use to communicate meaningfully with their students, with their colleagues, and with the public. This is especially true when their work requires translation into another language by a translator, as in the case of Luria (1966).

In some instances, as in the translator's use of the words "mental" and "reflex" in Luria's introductory chapter, other more appropriate terms may be available and their use may solve the problem. In some cases, however, where English may be the native language of both the writer and the reader, substitute terms may not be available, and in such instances a mutually agreed upon definition is required. Examples of terms which require this solution are "interactional" and "covert," both of which have been the subject of adverse comments on these pages and in other publications (Dewey & Bentley, 1949; Handy, 1973, 1985; Pronko, 1983-1984, 1985; Smith, 1985). As one who has probably used these two terms more frequently than any other writer (Mahan, 1970), I would like to add my commentary.

Within a longer discussion regarding the Dewey-Bentley position (1949) in general, and their terminology in particular, I made the following statement in an appendix to my little volume (1970):

> Where I have departed from them, as in using the term "interactional" rather than "transactional," and in givingbnames to the parties to the interaction, I have done so to facilitate communication and the development of basic concepts on the part of students who are unfamiliar with the system. With these minor changes, the approach of my own discussions and that of Dexey and Bentley is essentially the same.

It may not be widely known that Kantor (1924) used the term "interaction" to describe his psychology, but his use certainly did not fall under the objectionable Dewey-Bentley definition. I quote from him as follows:

> Once more we are reminded that the indispensable methodological minimum for any form of scientific investigation is the isolation and observation of the interactions of at least two actually observable things.

> In the psychological domain we can definitely conclude that these interacting things are, on the one hand, a person who performs reactions (or those reactions themselves) and, on the other, the objects, conditions, and events constituting the stimuli for these reactions.

As for the term "covert," I used this term throughout my "Primer" (1970) with success and no little satisfaction, as the term "implicit" in its predecessor (1968) had been very disappointing. In switching from one term to the other, the dictionary's definition of "covert" as "not openly shown" was all that was intended. I did not then, nor do I now, think that the term has anything to do with the location of the interaction. Actually, the term "covert" as used in psychology has never had a spiritistic or intra-organic connotation, and, in both the classroom and on tape, I have always pronounced it with a weak "o," which seems to be more pleasing and which clearly distinguishes it from the CIA variety (Pronko, 1983-1984).

I consider neither "interaction" nor "overt" to be more than mildly controversial, but Lee (1986) has really opened up a can of worms by arguing that a number of nouns, including some that have been very objectionable, indeed be incorporated into the vocabulary of contextual interactionism (interbehavioral psychology) from mainstream traditional approaches. I shall look forward to future issues of The Interbehaviorist for comments on Lee's paper (and I may make some myself).

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The Brain and Psychological Processes: Three Quotations

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Quotation No. 1: Elliot Stellar (1977)

> Although the term mind is more abstract than is more readily observable overt behavior, it remains a convenient word in ordinary conversation for designating the inner (private, subjective) aspects of human experience. Scientifically, however, mind cannot be used to refer to the nonphysical (since science is limited to phenomena; i.e., to the physical), despite the great emphasis that philosophical and cultural heritage has placed upon the spiritual. Physiological psychology begins, then, with the basic understanding that if the word mind is to be used, it is to be conceived in such terms as the activities of the nervous system or other parts of the living body.

Quotation No. 2: J. R. Kantor (1947)

> The fundamental outcome of transforming psychology into a science by means of the organic was that the nervous system became a medium or means whereby psychic factors could be brought into juxtaposition with natural processes. Through the nervous system, nonspatial psychic qualities, processes or existences were projected into the spatiotemporal domain. The point

to be emphasized is that physiological psychology did not constitute a break in the perennial commerce with mental states or qualities, but simply was employed to legitimize them.

Those who believe that such verbal gymnastics absolve physiological psychology from the taint of metaphysics overlook that the verbal dodge of abstractionist theory is also a form of metaphysics. To occupy oneself with sensations or psychic qualities in any form is to diverge from the investigation of discrimative interbehavior with actual things. There is no question that physiological psychologists have rung all sorts of changes on traditional metaphysics, with the result that whatever events they treated became patterned as foci of the great dualistic tradition which for twenty centuries has dominated the thinking concerning psychological happenings. Like all theorists, the mental-neural identifiers begin with events, but the descriptions of these events and their interpretations are wrought metaphysically, not scientifically.

Quotation No. 3: Alexander Luria (1966)

For many years psychological processes were regarded as direct experiences or as primary "properties" of mental life, incapable of further analysis, and most attempts to discover their material basis were nothing more than attempts to find areas of the brain which might serve as direct carriers or "organs" of these forms of mental life.

These basic concepts were responsible for the attempts to "superimpose nonspatial concepts of modern psychology on to the spatial structure of the brain" and for the parallelistic ideas of "psychomorphology" which remained in existence for centuries despite changes in prevailing ideas of mental function. We must therefore pause for a while to discuss the concept of "function" and the changes which have taken place in the concept in modern biology and psychology.

On the one hand, the term function denotes a particular property of a tissue. In this sense we are justified in speaking of the function of bile secretion characteristic of liver cells, the function of the insulin production of the cells of the pancreas, and the function of perspiration performed by the cell of sweat glands. However, the word function may also be used in a quite different sense. It may denote a complex adaptive activity of a whole system, and sometimes of a whole organism, establishing certain relationships with the external environment, and producing some form of adaptive effort. It is obvious that a wide gulf separates the concepts of "function" as used in these two senses.

Human psychological processes must therefore be understood, not as elementary properties or primary faculties, but as systems with an historical origin and complex functional structure. It will be clear that in place of the obsolete psychomorphological ideas of narrow (brain or other bodily) localization we must introduce new concepts, more in line with the social origin and systematic structure of the complex forms of human psychological activity.

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