THE INTERBEHAVIORIST

A Quarterly Newsletter of Interbehavioral Psychology

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QUOTATION

The organism cannot exist without its supporting environment. Hence, a scientific definition of the organism should also include the environment which influences it.

- I. M. Sechenov (1861)

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Interbehavioral Psychology

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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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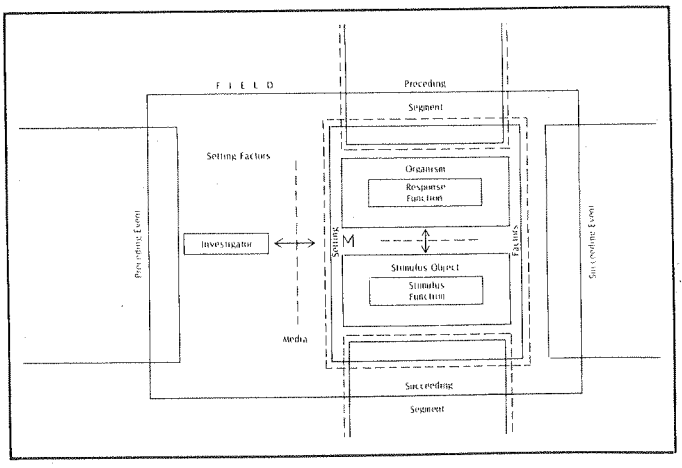
NOTES FROM THE FIELD

Several subscribers contributed comments to the recent special issue of Behavioral and Brain Sciences, which published six canonical papers of B. F. Skinner. Among those offering comments were PHILIP N. HINELINE and JAY MOORE on "The Operational Analysis of Psychological Terms" and WILLIAM S. VERPLANCK on "An Operant Analysis of Problem Solving."

The Spring, 1985 (Vol. 8, No. 1) issue of The Behavior Analyst contained articles by subscribers SIGRID S. GLENN, "Some Reciprocal Roles between Behavior Analysis and Institutional Economics in Post-Darwinian Science"; JAY MOORE, "Some Historical and Conceptual Relations among Logical Positivism, Operationism, and Behaviorism"; and EDWARD K. MORRIS, "Public Information, Dissemination, and Behavior Analysis."

SIDNEY W. BIJOU gave a five-session seminar on behaviorism and interbehaviorism at the Department of Psychology, University of Granada, Spain, May 13-16. He also delivered an address entitled, "A Realistic View of Mental Retardation: Implications for Education and Training," in the first Lecture Series Program on Behavior Analysis and Intervention in Developmental Retardation.

JESUS GIL ROSALES-NIETO and Carmen Luciano of the Department of Psychology, University of Granada, Spain, will be visiting scholars at the Cambridge Center for Behavioral Studies in the Fall, 1985.



THE AGORA

As you will undoubtedly notice, we have enclosed a number of advertising-flyer-and-subscription-forms for the newsletter with your current issue. A pad of these forms was recently sent to the members of the newsletter's advisory board for posting. If anyone else would care for a pad, please let us know.

As for the individual forms, we ask that you pass them around among your colleagues and institutional libraries. More important, though, we ask that you include one with each interbehavioral publication reprint you send out. Those who request your interbehavioral papers are a likely source of new and interested subscribers. When your supply of these forms is depleted, please keep some photocopied extras on hand for mailing. If these are too expensive for you to reproduce, however, we will be pleased to send you an additional supply. By the way, you will probably notice that the form has an eye-catching typo; this will be corrected in the next printing. (No doubt Kansas has many truths, but then who can know the truth of that?)

The newsletter seems to have lost contact with the activities of the Cherion Society -- the International Society for the History of the Behavioral Sciences. If any readers would like to serve as a liaison to that group, and keep us informed of their activities, the general readership would be appreciative.

Author query: Susan M. Schneider is searching for the origins of the term "radical behaviorism." Skinner's first use of it was apparently in his 1945 paper on the operational analysis of psychological terms, but he has since said that the phrase was "in the air" before then. Indeed, in The Behavior of Organisms, Skinner (1938) cited a 1933 Psychological Review paper in which Kantor used the phrase in referring to Warren (1921). Warren, in another paper published in 1921, used the phrase to describe Watsonian behaviorism. If anyone has further leads as to the origins of the phrase, especially in regard to Skinnerian behaviorism, Susan would appreciate hearing from you. Please write to her, c/o the Department of Human Development, University of Kansas, Lawrence, KS 66045.

The quotation on the front cover of this issue was sent to us by Warren K. Bickel (Frances Scott Key Medical Center, Baltimore). We would be pleased to accept other such submissions.

Just before we went to press, Shery Chamberlain, the assistant business manager of the Society for the Advancement of Behavior Analysis (SABA) and Managing Editor of The Behavior Analyst, sent us a cassette tape recording and transcript of Professor Kantor's question-and-answer conversation hour at the 1977 convention of the Association for Behavior Analysis. We are delighted by this kind gesture from SABA, and with Shery's thoughtfulness in preparing the transcript and sending the materials on to us. We will reprint the transcript (along with Paul Mountjoy's introduction) in the Fall issue of the newsletter.

An Interbehavioral Organization

A meeting was held in Columbus, Ohio, on May 27 to discuss the possible development of an independent interbehavioral organization and other ways to assure the continued publication of interbehavioral materials. In attendance were Dennis Delprato, Robert Epstein, Gail Gardner, William Gardner, Craig Knapp, Lisa Johnson, Parker Lichtenstein, Jay Moore, Ed Morris, Linda Parrott, Roger Ray, Charles Rice, and Jon Williams.

As to the first item of business -- an interbehavioral organization -- the consensus of the group was that an interbehavioral organization may be premature at this time, given the easy access interbehavioral psychologists have had to the annual meetings of the Association for Behavior Analysis (ABA). An annual gathering of interested interbehavioral psychologists, though, was seen as having great value for those pursuing interbehavioral research; hence, annual brain-storming meetings may be an important opportunity worth creating. Roger Ray will chair a group looking into the possibility of gathering together interbehavioral psychologists for such meetings. Future plans will be announced in the newsletter.

The second item of business concerned the interbehavioral publications -- The Principia Press, The Psychological Record, and The Interbehaviorist. Robert Epstein, Director of the Cambridge Center for

Behavioral Studies, expressed an interest in the Center's publishing the newsletter, with all editorial responsibilities continuing to rest with the current editor. The Society for the Advancement of Behavior Analysis (SABA), the legal overseer of ABA, also expressed an interest in assuring the continued publication of all forms of interbehavioral material. Although flattered by this interest, the group decided that no decision about either offer was necessary at this time. Moreover, some of the decisions were actually outside the purview of the group. For the moment, then, the interbehavioral publications will maintain their independent status.

The Association for Behavior Analysis Convention presentations. As mentioned in the last issue of the newsletter, the May 1985 ABA meeting included numerous presentations by our subscribers. Those presentations we found to be particularly interbehaviorial in orientation are listed below. If we missed any, we will be pleased to make corrections.

Bijou, S. W., Umbreit, J., Ghezzi, P. M., & Chao, C. (University of Arizona), "Who Said What to Whom: Identifying Language Interactions."

Bijou, S. W., & Umbreit, J. (University of Arizona), "Application of Psychological Linguistics to Research on Normal and Handicapped Children."

Fox, J. J., Rogers-Warren, A., Daurelle, L. A., Alpert, C. L., & Hancock, T. B. (Vanderbilt University), "Analyzing Complex Parent-Child Interactions: Alternate Units of Analysis."

Gardner, W., & Fraser, M. E. A. (Jacksonville State University), "Referent-Oriented Interbehavioral Approach to Language Analysis."

Iversen, I. H. (Southwest Foundation), "Contiguity: A Fundamental Principle in Limbo."

Parrott, L. J. (Saint Mary's University), "An Interbehavioral Perspective on Ethics and Values."

Ray, R. D. (Rollins College), "Social and Individual Patterning of Multiple Behaviors: The Concept of Coupling."

Ray, R. D. (Rollins College), "Of Bahamian Children, American Whales, and Soviet Scientists with Puppy Dog Tales: A Decade of Empirical Interbehavioral Systems Analysis Revisited."

Wruble, M., Delprato, D. J., Whitney, B., Holmes, P. A., & Gola, T. J. (Eastern Michigan University), "Response Pattern Analysis of Schedules that Differentially Reinforce Pauses in Behavior."

Special Interest Group. The Interbehavioral Psychology Special Interest Group (SIG) of ABA held its first meeting to elect officers and plan for future ABA activities. Fifteen people attended the meeting: Sid Bijou, Jim Fox, Pat Ghezzi, Tom Gola, William Gardner, Peter Holmes, Craig Knapp, Jan LeFrancois, Ed Morris, Linda Parrott, Ann Rogers-Warren, Doug Ruben, Roger Ray, John Umbreit, and Marc Wruble.

Linda Parrott was elected as the SIG Chair; Rick Amado will assume the duties of the secretary-treasurer; Jim Fox and Ann Rogers-Warren were elected as ABA Program Chairs; and Ed Morris volunteered to serve as the newsletter editor, which will mean that The Interbehaviorist will publish a column as appropriate on ABA Interbehavioral Psychology SIG news.

Plans for next year include the development of a conversation hour/workshop period by Jim Fox for those pursuing interbehavioral research, and the organization of a symposium on family interactions by Ed Morris, which Ann Rogers-Warren will chair. In addition, Roger Ray is planning a workshop on general systems theory and methods. If you are interested in contributing in any of these areas, please contact the respective organizers.

Kantor Memorials

We want to thank those who have contributed to the Kantor Memorial Fund of the newsletter. The fund serves as the basis for our long-term financial stability and special publication and publicity events. The current fund stands at $768, reflecting the generous contributions of Richard Amado, Don Bloomquist, Dennis Delprato, William Gardner, Helene Kantor, Louise Kent-Udolf, Harry Mahan, Henry Pronko, and Doug Ruben.

Principia Press

For a complete list of Professor Kantor's books, and the prices thereof, please write: Principia Press, 5743 N. Kimbark, Chicago, IL 60637.

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 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) (prepaid).

Subscriptions

Any efforts current subscribers can make to promote new subscriptions, especially from university, college, and institutional libraries, would be greatly appreciated. Subscription information is listed inside the front cover of the newsletter and on the enclosed flyers.

The new subscribers since the last issue are listed below. For those interested, a mailing list is available on request.

New Subscribers

Marybeth Fraser (Jacksonville State University)
Gatling Residential Center, NC
Patrick Ghezzi (University of Arizona)
Marino Perez (Spain)
William Pierce (Clinical Diagnostic, Golden, CO)
University of Arizona Library
Robert D. Zettle (Wichita State University)
Gerald Zuriff (Wheaton College)

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BOOK AND JOURNAL NOTES

Boakes, R. (1984). From Darwin to behaviorism: Psychology and the minds of animals. New York: Cambridge University Press.

As Boakes says in his preface, "My aim in this book is to provide an account of the study of animal behaviour and of various ideas during the period from around 1870 to 1930 as to what kind of minds animals possess" (p. xiii). Boakes achieves his aim in more detail and at more length than in any current text. New texts on the history of behaviorism, though, seem to be cropping up. The Boakes book contains a reference to an "in press" book edited by C. Buxton, entitled Points of View in the Modern History of Behaviorism (Academic Press). In addition, an advertisement has recently appeared for a text by John M. O'Donnell, The Origins of Behaviorism: American Psychology, 1870-1920 (Columbia University Press).

Getting back to Boakes, his perspective is neither interbehavioral nor even radical behavioral, but rather reflects the eclecticism of the study of animal behavior. He begins the book with Darwin in a chapter on mental evolution, and ends the book just before the ascendance of radical behaviorism. In between, Boakes devotes chapters to intelligence and instinct; experimental psychology and habits; reflex action and the nervous system; conditioned reflexes; comparative psychology and the beginning of behaviorism; apes, problem-solving, and purpose; and nature and nurture.

In general, the book is good, though it does not have the vision of Boring's text. Nonetheless, it is certainly to be recommended. The pictures, alone, are worth the proverbial thousands of words. Of particularly refreshing note is the greater role Boakes gives to the contributions of women to the history of behavioral psychology than is typical of history texts. In particular, he notes the contributions of Washburn, Kohts, and Rayner, as well as those of the wives of outstanding psychologists (e.g., Sara Pavlov and Rosalie Rayner). The lot of the latter was not an easy one. Indeed, Thorndike's comment to his fiancee is perhaps too indicative of the tenor of those times. Speaking of their impending marriage, Thorndike remarked, "I can go ahead and do something in the world now and you will find looking after me and the world of science lots more worthwhile than anything else you could do" (p. 72). Enough said.

The only mention of Professor Kantor by Boakes was contained in the following footnote:

> M. Meyer, A. P. Weiss, E. A. Singer, and J. R. Kantor are among the people frequently classed as strict behaviourists at this time. However, since they made no direct contribution to animal psychology and do not appear

to have influenced its development, they are not discussed here. (p. 258)

Boakes then gives a reference to W. Harrell and R. Harrison's, The rise and fall of behaviorism. Journal of General Psychology, 1938, 18, 367-421.

For further commentary on the Boakes book, see reviews by William Baum in The Behavior Analyst, 1985, 8 (in press) and by Robert J. Richards in Science, 1985, 228, 862-863. (Edward K. Morris, University of Kansas)

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Boring, E. G. (1950). A history of experimental psychology (2nd ed.). Englewood Cliffs, NJ: Prentice-Hall, Inc.

Although admittedly not a proper history by interbehavioral standards, Boring's history of experimental psychology is a classic. Moreover, it does provide some interesting comments that may serve as a basis for interpreting some of Professor Kantor's approach to the subject matter.

First, Boring makes occasional comments about national character. For instance, he defines the phenomenology of German psychology (pp. 18-21) as unadorned description, which is propaedeutic to science -- science in the experimental sense. In addition, he speaks of phenomenology as belonging "with the descriptive, the classificatory, and the inductive approaches; it contrasts with the mathematical and deductive [English] attacks. It represents, moreover, an attitude that suited the painstaking and methodical Germans" (p. 18). Boring goes on about phenomenology: It was "the careful collection of observational fact, that was sound, keen-sighted as to detail, conscientious and thorough, but not as a rule brilliant, and seldom concerned with large generalizations" (p. 19). Boring's comments on brilliantness aside, some of this seems to describe Professor Kantor's work. We must not forget that Professor Kantor's behavior, as that of all others, was the product of historical and cultural context. An understanding of that context can offer us an understanding of his interbehavioral program.

Second, in a discussion of Descartes's dualism, Boring comments: "Much confusion has resulted from the fact that both soul and mind are l'ame in French and Seele in German. It is much easier in English to keep psychology separate from theology" (p. 162). Professor Kantor's German heritage, then, perhaps made it easier for him to see the relationship of theology to mentalism, and not to be misled in viewing mentalism as somehow scientific, while theology is not. Thus, whereas other psychologists could blithely, and with some relief, discard the non-science of theology in favor of the science of the mind, Professor Kantor saw no real change, but rather only the continued evolution of what remained an unscientific account.

The characteristics of the German and English languages might also perhaps explain why Professor Kantor's criticisms of non-scientific psychology were often couched as much in terms of theology and the soul, while B. F. Skinner's criticisms were predominantly about mentalism and the mind. (Edward K. Morris, University of Kansas)

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Dannefer, D. (1984). Adult development and social theory: A paradigmatic reappraisal. American Sociological Review, 49, 100-116.

Dannefer's abstract begins, and interbehavioral psychologists would agree: "The study of adult development is in need of theoretical reformulation" (p. 100). This paper is a step in an interbehavioral direction from a sociological perspective. Dannefer's emphasis on context is refreshing; however, he is not a behaviorist, and his alternative "sociogenic" approach is couched in terms such as "human intentionality." Still, the article is worth consideration.

Dannefer warns sociologists that the interdisciplinary nature of the study of development across the life-span has resulted in the unwarranted acceptance of certain implicit assumptions from developmental theory. The "ontogenetic fallacy," for example, largely consists of downplaying the role of the environment, including the social environment. Dannefer criticizes several approaches for using this paradigm, but reserves his strongest attack for "ontogenetic stage theories, [which] conceive of the developmental process as uniform and constant. Since the sequence is invariant, the causal factor must be

invariant as well; the enormous range of environmental variability is thus logically required to be causally unimportant" (p. 104).

Dannefer points out, with unintentional irony, that developmental stage theories are actually more restrictive of human autonomy than theories that give the environment a larger role. (Susan M. Schneider, University of Kansas)

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Duncan, R., & Weston-Smith, M. (Eds.). (1985). The encyclopaedia of medical ignorance: Exploring the frontiers of medical knowledge. Elmsford, NY: Pergamon Press. Reviewed by James Le Fanu in the New York Times Book Review (May 12, 1985, p. 33).

Le Fanu's book review is sprinkled with the to-be-expected brain dogma, but also with interesting insights, opinions, and quotations. He argues, for instance, that the effectiveness of early medicine was not based on knowledge of basic mechanisms, but rather that many major breakthroughs have been due to "relatively crude screening techniques for new therapeutic compounds plus serendipity and a little science." Future success, though, he argues, will have to depend increasingly on understanding basic mechanisms.

Among the chapters singled out for praise was one by Philip Gell (University of Cambridge), entitled "Destiny and the Genes: Genetic Pathology and the Individual." Mr. Gell argues that specific gene-disease linkages are the great exception, not the rule. In place of the search for simple, mechanical causation, Gell argues for an interbehaviorally-sounding perspective:

> We are dealing not with a chain of causation, but with a network, that is, a system like a spider's web in which a perterbation at any point of the web changes the tension of every fiber right back to its anchorage in the blackberry bush...If the gap in knowledge of man between the operations of the genotype and the behaviour of the person is not merely unbridged, but in principle unbridgeable then our ignorance will remain ineluctable.

Le Fanu concludes his review with a timely quotation from the late Norman Geschwind of the Harvard Medical School about the current state of ignorance in some areas:

> There may be neglect of correct existing fact and theories, either through deliberate suppression or through widespread acceptance of incorrect data or erroneous criticisms. Furthermore...there are cases in which research is not carried out either because of the erroneous belief that there are no suitable investigative methods or because well established scientists and administrators are unwilling to allocate resources because of prejudice, timidity, or simple desire to support their own fields at the expense of newer less influential ones.

(Edward K. Morris, University of Kansas)

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Hursh, S. R. (1984). Behavioral economics. Journal of the Experimental Analysis of Behavior, 42, 435-452.

Although psychology and economics are both sciences of behavior, they have historically had little influence on each other. Psychology is a more broadly defined science; thus its principles and analyses might be viewed as subsuming economics. In this regard, both Kantor (1981, pp. 194-231) and Skinner (1953, pp. 384-401) have provided behavioral or psychological analyses of economic events. Clearly, economics could benefit from taking a behavioral perspective, but the potential benefits of economic perspectives for psychology have only recently been suggested.

One such economic perspective is provided by Steven Hursh in "Behavioral Economics." In this article, Hursh describes a number of implications of economic principles for research in the experimental analysis of behavior, most of which are quite compatible with an interbehavioral perspective. Perhaps the most important implication of research influenced by economic concepts is its emphasis on contextual variables. For example, in research on reinforcement schedules, "closed economies," in which subjects receive their total allotment of food during experimental sessions, often engender response rates quite different from those found in the more frequently studied "open economies," in which the

subjects' food allotment is constant and independent of responding during experimental sessions. Such findings imply that much of our knowledge of response rates under various schedules of reinforcement may need to be qualified to take into account the economic context of "closed" and "open" systems.

Contextual effects are also observed in Hursh's research on what he calls "demand elasticity" and "inelasticity," that is, on the relationship between response rate, reinforcer availability, and response requirements (i.e., price). "Inelastic" and "elastic" demands are influenced by a variety of factors, including the nature of the reinforcer, the availability of other reinforcers, the type of species studied, and again the economic context of closed and open systems.

Hursh's economic analysis leads to a view of causation similar to that of interbehavioral psychology. Hursh rejects the perspective of unidirectional causality of dependent and independent variables. Instead, he advocates a view of "dynamic behavioral adaptation whereby both performance and obtained rate of reinforcement are viewed as outcomes of adjustment to environmental constraints" (p. 415).

A potential disadvantage of an economic analysis is that the terms used to describe behavioral events are metaphorical. Although metaphors and analogies have some place in science, they can also be misleading. Thus, caution should be urged when applying economic concepts to psychological events. Nevertheless, any disadvantage these economic concepts accrue may well be offset by their utility in suggesting new areas of conceptual and experimental analysis. (Steven E. Larsen, University of Kansas)

References

Kantor, J. R. (1981). Interbehavioral philosophy. Chicago: Principia Press.

Skinner, B. F. (1953). Science and human behavior. New York: Macmillan.

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Lundin, R. W. (1985). Theories and systems of psychology (3rd ed.). Lexington, MA: D. C. Heath.

The third edition of Robert Lundin's history and systems text is now available. In it, he includes J. R. Kantor's interbehaviorism in a chapter on "later behaviorism" that also includes Guthrie, Hull, Skinner, and Bandura. Lundin provides references to 10 of Kantor's works, as well as to the work of Lichtenstein and of Pronko. Mention is also made of this newslstter, albeit albeit under an earlier title.

The 10-page section (pp. 203-212) on Kantor covers such topics as basic considerations, psychological interactions, learning, implicit interactions, feeling, emotions, remembering, biological participation, and criticisms and contributions of interbehaviorism.

Lundin's history and systems book is one of the few to present interbehavioral psychology accurately and sensitively. (Edward K. Morris, University of Kansas)

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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 original authors so that we may send them a copy of the newsletter; we will automatically send book comments and reviews to the publishers.

* * *

COMMENT

Interaction or Transaction?

Rollo Handy

Behavioral Research Council

I was quite interested in the book note on Zuriff in The Interbehaviorist, Vol. 13, No. 2, 1985, particularly in the statement: "Zuriff seems to regard 'interaction' as no more than mechanical give-and-take, which is not at all the meaning of the interbehavioral perspective on strong reciprocal interaction within a contextualistic framework."

John Dewey and Arthur Bentley (Knowing and the Known, Boston: Beacon Press, 1949) argued that "interaction" probably most often would be understood in the "mechanical give-and-take" sense mentioned

in the book note, and they therefore adopted the word "transaction" to refer to mutually reciprocal connections occurring in a specific field. If you have not read their admittedly difficult discussion of those matters, you might be interested in doing so...In general, although there are differences, the agreement between Dewey and Bentley, and Kantor on such matters is striking.

Editor's Note: We concur with Professor Handy, and recommend as an introduction to Dewey and Bentley's analysis his article, "The Dewey-Bentley Transactional Procedures of Inquiry," The Psychological Record, 1973, 23, 305-317.

* * *

COMMENT

Overt-Covert: Encore

N. H. Pronko

Wichita State University

Smith's (1985) comment -- "'Covert' Defended" -- certainly points to the need for refining the meaning of terms adopted from ordinary language for more precise scientific usage (e.g., stimulus, response, implicit, overt, and covert). We share this problem with the physicists and their use of such terms as particle, force, attraction, and repulsion.

With respect to "overt" and "covert," I reread relevant portions of Skinner's (1974) About Behaviorism because of his frequent use of those terms there, as well as the terms "public" and "private." Chapter 2, "The World Within the Skin," and Chapter 13, "What Is Inside the Skin," alone reveal Skinner's underlying organism-centered postulate. On page 215, his reliance on the physiologist to supply an answer to the question of "how the organism is changed when exposed to contingencies of reinforcement" support my contention that the overt-covert distinction is derived from and is most congenial with an organism-centered approach.

One must agree heartily with Smith's (1983, p. 28) definition of the psychological field, which puts the organism in its proper place in the entire event. My question is: How can the terms "overt" and "covert" be applied to an integrated field event when they were derived from, and are applicable to, the isolated organism? As to Smith's proposal of the term "subtle," it might serve if applied to the field. Its obvious antonyms are "gross" and "crude." Or, perhaps all events are observable under interbehavioral postulation, no such distinction needs to be made. The biologist does not set apart subtle neural activity, osmosis, or secretion from cardiac activity or the knee jerk. What would be served by making such a distinction in behavioral inquiry?

One final point: Noel Smith and I may have started something. We have a forum now for testing and clarifying viewpoints and specific formulations. Let others get into the fray.

Pronko, N. H. (1983-1984). A vote toward the obsolescence of the term "covert." The Interbehaviorist, 12(1), 11.

Skinner, B. F. (1974). About behaviorism. New York: A. A. Knopf.

Smith, N. W. (1983). An imperative for revolutionary alternatives to recurring problems in psychology. In N. W. Smith, P. T. Mountjoy, & D. H. Ruben (Eds.), Reassessment in psychology: The interbehavioral alternative (pp. 21-50). Washington, DC: University Press of America.

Smith, N. W. (1985). "Covert" defended. The Interbehaviorist, 13, 6-7.

* * *

ARTICLE

The Information Processing Metaphor in Cognitive Psychology Examined

Jeff Hilt

SUNY-Plattsburgh

Metaphor use and metaphorical understanding in cognitive psychology raise important questions that warrant examination. Because cognitive psychology is clearly the dominating paradigm in psychology today, a discussion of its premises that rest on metaphors bears scrutiny. The cognitive framework, itself, places little emphasis on discussion of its theoretical assumptions and does not critically examine the conceptual practices imbedded in its

dubious metaphorical models, although it is heavily involved in research that relies on such models.

A metaphor may be defined as a word or phrase literally denoting one kind of thing or idea used in place of another to suggest a likeness or analogy between them. Cognitive psychology employs the metaphor of the mind as an information processing system or computer. For metaphorical understanding to take place, some similarity or analogy must exist between the things to which the terms originally applied and the phenomena to which the metaphor is extended. If someone said that a cat was a garden hose, little would be communicated because no obvious similarity is immediately evident. This makes the point that metaphors for which metaphorical understanding is lacking because no similarity exists might more accurately be called myths. Thus, if at most levels, the brain and computers are not similar, or are only superficially similar, then cognitive psychology must be called mythology.

In a discussion of Max Black's interactionist view of metaphor, Mary Hesse (1980) states, "For a conjunction of terms to be taken as the primary systems and secondary systems to constitute metaphor, it is necessary that there should be patent falsehood in taking the conjunction literally" (p. 113). For our purposes, the primary system is the mind/brain, and the secondary system is the computer. Could cognitive psycholopy be taking the metaphor literally? Hoffman and Nead (1983) sound a warning: "In the analysis of scientists' rhetoric, one must not let abstractions or thematic generalizations slip by as literal foundational claims...(p. 513). By aligning themselves with the scientifically recognized field of computer science, cognitive psychology may be engaging in yet another misdirected attempt to scientize psychology. The history of science points out psychology's fascination with mechanistic analogies and the lack of understanding that results. Indeed, in the present case, no evidence exists to support the metaphor -- only overwhelming popularity supports it.

Estes (1980) has already examined the comparison of human memory and computer memory, including capacity and the retention of information. The capacity of human memory, for instance, depends on experience, while that of computers is independent of experience. In human memory, retention is graded, while in computer memory, retention is all-or-none. Estes concludes, "Human short term memory is oriented toward events rather than retention of units" (p. 65). The distinction made between short-term and long-term memory is based on observed recall performance, rather than on any observable structures of the brain. When discussing human forgetting, Estes does not make any distinction between human short-term and long-term memory, but notes that

> In the human, the forgetting takes the form of a progressive loss of precision or completeness of information about the original experience; even after considerable forgetting has occurred, the individual may remember something about the events or items making up the original experience. (p. 67)

It seems that a basis for an analogy between computers and people exists at the observable behavioral level and at the cellular-bit level. This is assuming that what computers can do is called behavior. At some level, be it higher or lower, one may always find something on which to establish a comparison or to base an analogy. Some psychologists, for example, are fond of comparing the firing of neurons to the two-state nature of a computer bit. They feel justified in using the computer analogy because the neuron either fires or it does not, and a bit is either off or on. It should then follow that an understanding of snail behavior could result from the comparison of a snail and many interconnected light bulbs. Kantor (1978) points out, however, that "To categorize the neural structures of the nervous systems as wires, and synaptic junctions as electrical connections, is to play games with things and events by means of metaphor" (p. 581).

This point is further elucidated by Waltz (1984, p. 118), who implied that at the behavioral level, the processes underlying computer learning could not provide an explanation of the processes underlying human learning. Waltz goes on to say, though, that the computer is flexible enough to emulate the essential functions of human thinking. Although Waltz alludes to the fact that humans and

computers are totally different entities, he reverts to using terms employed in describing human behavior for describing what computers do. This may be where confusion arises.

Blewitt (1983) provides criticism for those who believe that at the behavioral level a similarity exists:

> That the computer analogy predicts certain findings does not mean that the same processes are involved in the prediction as in the original event. Just because the computer can engage in action which has a topographical similarity to that of human memorial behavior does not mean that the controlling relationships are the same in the computer as in the human. (p. 393)

Later, Blewitt concludes that the information processing analogy teaches people about how computers work, but fails to provide any insight into how people remember.

Swartz (1958) asks rhetorical questions concerning the history of psychology relative to the other sciences:

> Where are the analogies of yesteryear, the borrowings from Newtonian physics, from chemistry, from early biology, etc.? How many have survived the inevitable sifting process of genuine psychological study? How many have been discarded barriers to a scientific conception of behavior? (p. 55)

Swartz concludes by implying that history has shown that the evolution of psychology has been suppressed by the use of analogy.

Kantor (1978) sums up the computer analogy problem this way:

> The greatest scientific fault of the cult of imitation and artificiality remains, of course, the departure from the analysis of actual event fields. Supporters of Artificial Intelligence avail themselves of the power of words, and the manipulations they make possible, to apply names to events in order to convert them to substances, powers, and so on, which are then applied to things. Overlooked are the basic components of psychological fields, the interbehavior of organisms with the units of the environment including other organisms, as well as the invariable setting factors. (p. 583)

The errors in the use of the computer analogy in cognitive psychology should be apparent. Psychology does not need analogies to become more scientific. Psychologists need to observe events, and then formulate constructs based on their observations. In cognitive psychology, all events will undoubtedly be interpreted into information processing frameworks. The computer is a product of hundreds of years of thought in many fields, including mathematics, mechanics, electronics, and engineering. The behavior of the human being is the result of the complex interactions of many things, including setting factors, biological factors, cultural factors, and the interactional history of the human and the stimulus event with which the individual is behaving. Comparison of a machine composed of electronic components (e.g., diodes, resistors, and transistors), on the one hand, with an organism of the complexity of humans, on the other hand, is a simplifier's solution, and a failure to recognize that human behavior is the result of complex interactions. The journey to understanding human behavior begins with one indispensable step, that of observing events. Cognitive psychology has not taken this step. Its use of metaphor remains in the realm of mythology.

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