THE INTERBEHAVIORIST

A Quarterly Newsletter of Interbehavioral Psychology

ISSN 8755-612X

Published at the University of Kansas

Volume 15 1987 Number 3

TABLE OF CONTENTS

QUOTATION

Psychology is the scientific study of the way living organisms, particularly human individuals, organize, develop, retain, and perform behavior patterns or acts, all of which are adaptive interactions with stimulating objects, persons, situations, and events.

- Harry C. Mahan

THE INTERBEHAVIORIST

A Quarterly Newsletter of
Interbehavioral Psychology

ISSN 8755-612X

Edward K. Morris, Editor
Department of Human Development
2035D Haworth Hall
University of Kansas
Lawrence, Kansas 66045, U.S.A.
913-864-4840

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).

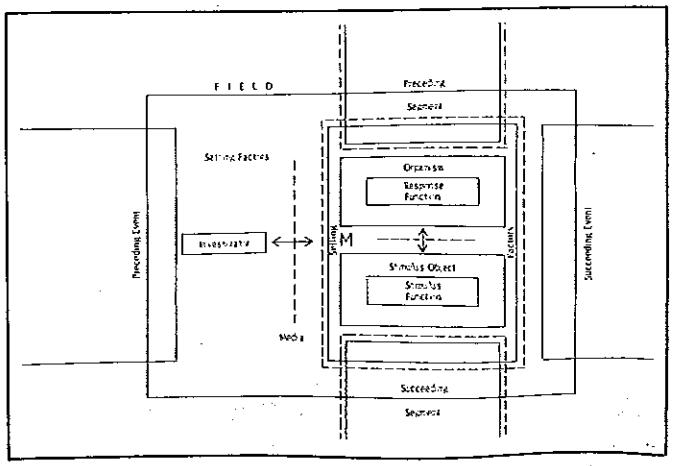
Subscription Information

Student Subscriptions (US).....	$ 4.00
Regular Subscriptions (US).....	6.00
Foreign Subscriptions.....	8.00
Institutional Subscriptions.....	12.00
Back Volumes 1-14.....	6.00
Back Volume Complete Sets...	Please Write

THE PRINCIPIA PRESS

Principia Press's list of currently available titles in interbehavioral psychology is presented below. Check your bookshelves, and those of your library and bookstore, for possible oversights. In addition, the books make excellent gifts for colleagues and for students, especially for the latter in honor of their completed degree requirements. The books may be purchased directly from Principia Press, 5743 Kimbark Avenue, Chicago, IL 60637. Handling charges are $.75 per title; prepaid orders are postpaid. Any queries should also be directed to the address above.

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 AGORA

Once again, we have the pleasure of having an issue of the newsletter prepared by an invited editor, this time by Harry C. Mahan (Oceanside, CA). Except for the material appearing in The Agora, the remainder is his -- the Comments and the Book and Journal notes. His writing is enthusiastic, and his analysis hard-hitting. We welcome both.

The ABA Interbehavioral SIG

The Interbehavioral Psychology ABA Special Interest Group met again this year at the convention, and a few notes bear mentioning.

More than 25 people attended the meeting and discussed such issues as the pros and cons of calling their views "interbehavioral," convention scheduling problems produced by overlapping interbehavioral papers, and the need for an interbehavioral graduate training directory. Among the actions taken were the forming of a student committee and a start on putting together a symposium at next year's convention tentatively entitled "Interbehavioral Psychology for Students."

Linda J. Parrott will prepare a lengthier report for the next issue of the newsletter, but if any of this interests you, please write to Linda, Department of Psychology, University of Nevada-Reno, Reno, NV 89557-0062.

As for the convention itself, one wit quipped that, although hotel food can be rather tasteless, at least the restaurants did not serve transcendental sole.

The Kantor Letters

Louise Kent-Udolf asks that persons interested in sharing their personal letters from J. R. Kantor write her at 715 S. Broadway, Suite 602, Corpus Christi, TX 78401-2599. Depending on the response, she would undertake to edit a compilation of the letters along with commentary prepared by the correspondents.

Mahan Reprints Available

Harry C. Mahan would be happy to make reprints available of his recent article on act psychology and the psychological nouns in The Psychological Record, 1987, 37, 131-134. Please write him at 811 Leonard Street, Oceanside, CA 92054.

Cognition and Computers

Arthur Kahn wrote to encourage newsletter readers to take their interbehavioral orientation into computer programming and computing systems, where a natural science orientation is sorely needed. He commented:

> Recently, in a discussion of the errors made by air controllers, an FAA administrator said that he could not get into the minds of the controllers to determine why they made errors. For a life and death issue, folk psychology is no substitute for science. To further emphasize this issue, in a discussion of persistent software problems, an author said that what was needed is a human mind extender, one which makes it possible for the human mind to conceive problems and solutions beyond its current capacity (Glass, R. L. IEEE Transactions on Software Engineering, Vol. SE-7, No. 2, March 1981). None of this material is in quotation marks...Incidentally, Kantor discusses many of the relevant issues in an article entitled, "Man and Machines in Psychology: Cybernetics and Artificial Intelligence" (The Psychological Record, 1978, 28, 573-583).

On the related issue, Art alerts readers to Gineste's analysis of Fodor's Modularity of Mind and Gineste's conclusion that cognitive psychology does not exist. Gineste's article is entitled, "Jerry A. Fodor: La Modularitie de l'espirit ou de la non existence de la psychologic cognitive" (Bulletin de Psychologic Lome, 38(372).

Notes from the Field

Louise Kent-Udolf has recently been elected president of the Communication Disorders Division of the American Association on Mental Deficiency. She welcomes inquiries from interbehavioral psychologists about participating in the organization.

New Subscribers

R. Grauben Assis (Brasil)
Leo Baker (Ireland)
Michael Byck (MI)
Andrew Hawkins (WV)
S. Thios (OH)
Michael Ward (CA)

COMMENTS

Before commencing with Professor Mahan's issue of the newsletter, we thought a brief biographical listing might be of interest:

Mahan, Harry C., psychologist; b. Ashtabula, OH, March 14, 1909. Address: 811 Leonard, Oceanside, CA 92054. Phone: 619-722-9341.

A.B., Ohio University, 1931; M.A., Ohio State University, 1932; Indiana University, 1935-1936; Ph.D., Ohio State University, 1940.

Psychologist, Warren Pa. State Hospital, 1932-1935, and Indiana Division of Corrections, 1938-1941; private practice in industrial psychology, 1946-1950; associate professor, psychology and business administration, University of Wichita, 1947-1950; head, department of psychology, Palomar College, 1957-1976; chairman, California State Psychological Examination Committee, 1957-1960.

Subspecialties: Neuropsychology, statistics, systematic psychology, and newsletter guest editor.

Slowly Changing Conceptions of Mind

Never, in human history, has the development of technology proceeded at as rapid a pace as during the past few decades. This is true in psychology, as well as in other fields, as attested to by Trotter (1987): "After years of work, psychology has finally developed research tools, methods, and theories sophisticated enough to examine the subtle abilities of infants and to interpret their complex behaviors" (p. 36). Trotter goes on to mention the use of computers, videotape equipment, and high-tech methods for measuring respiration, heart rate, body movement, visual fixation, and sucking, all of which, he says, give clues as to what is going on inside the infant. Two paragraphs later, he quotes child psychologist Lewis P. Lipsitt as saying that we now know that even a newborn child has a mind and is a cognitive being capable of mental operations.

The point to be made here is that although advances in the basic theory of mind have been available for over 50 years (Kantor, 1935), they have been almost entirely ignored by the many psychologists who demand merely to have the very latest models in technological equipment. This is indeed ironic, as the objective results of painstaking and highly commendable observations are then translated into obsolescent constructs.

Although psychologists like Lipsitt may use the term "mind" when discussing their work with journalists and others outside of the profession of psychology, they probably use it less frequently in lectures to their students or in presentations to their colleagues. They well know that the dictionary definition of the mind as "the element or complex of elements in an individual that feels, thinks, perceives, wills, and esp. reasons" (Webster's, 1983, p. 755) is obscure at best, and hearkens back to a spiritism that has long been avoided, although perhaps not rejected, by psychology. They refuse to accept the behavioristic position that mind does not exist, but they have not become aware or refuse to become aware that another definition exists that will fit their needs precisely. Unfortunately, there has been very little, if any, discussion of this latter definition in the literature during recent years. However, since the weaknesses of the older definition must be made apparent before the need for a revision can become convincing, a review of Ryle's famous criticism of it (see Donnelin, 1986).

In order to defeat the radical dualism of mind and body that has characterized much of philosophical thinking, Ryle pointed out the mistake of regarding the mind as "a ghost in a machine" -- a phrase which is associated with his name. In investigating such psychological concepts as memory, perception, and imagination, which are ordinarily considered mental, he showed that the basis for the properties of the dualistic model in which mind is considered "a ghost" is actually human action. Ryle did not, however, develop an approach that provides psychology with a viable alternative.

Many people have the idea that J. R. Kantor eschewed mind altogether, but this is not the case. Although undoubtedly the most outspoken and persistent critic that spiritistic mentality has ever had, Kantor actually accepted the term -- provided that it could be retained within his

radical modification of the concept. Unfortunately, this was not made as clear as it should have been during his many years of writing and, as a result, psychology can hardly be blamed for its unfamiliarity with this important change in concept. Indeed, his espousal of the term to cover his approach to the emergence of interbehavioral repertoires on the part of individuals comes as a surprise even to many of his students.

Because neither Kantor's (1935) original paper nor its later incorporation in a volume of his writings (Kantor, 1971) is readily available, I quote from this material at some length in the paragraphs that follow. The first paragraph describes Kantor's concept of mind, and the second the mind's development. Following the quotations, attention will be returned to Trotter's paper (1987) in order to show how infant research has followed Kantor's recommendations and how the presentation of such research can be improved by using Kantor's concepts.

> Mind is individual. It is a phenomenon pertaining to particular organisms or persons. Mind is not a substance or quality, but action -- the ways in which an individual adapts himself or herself to the things or conditions in his or her milieu. Now, psychological action is always interaction. This means that if I take one or two things offered me, I do so because of the effect that thing has upon me, and I am attracted to it. This interactional process has evolved during the course of my psychological life. To trace out the evolution of all of the myriads of such interactions summed up by the term my mind, means to study as many as possible of the billions of specific conditions which are unique and indispensible features of that evolution. (p. 267)

> The primary stage of psychological evolution undoubtedly takes place before birth, so that the earliest psychological evolution closely parallels late uterine maturation. As soon as the organism is born and is thus enabled to come into contact with the complicated world of things on its own account, psychological evolution proceeds with a tremendous velocity. (p. 270)

Trotter (1987) cites numerous research studies by psychologists such as Lipsitt, Meltzoff, Lewis, Kagan, Izard, Huebner, and others that verify Kantor's principles of 50 years ago and that show that the development of meaningful conscious interactions begins much earlier than was previously recognized. As a matter of fact, in a companion article, Roberts (1987) unknowingly verified that Kantor was correct in stating that meaningful communication can take place even prior to birth. She quotes psychologist Lee Salk: "Prenatal experience plays an important role in the continuity of life" (p. 41).

If we now turn back to Trotter's (1987) early quotation from child psychologist Lipsitt on the psychological interactions of newborns, that quotation can now be changed to read: "We know now that even a newborn child can participate in meaningful relationships with his or her surroundings, and that these relationships may be cognitive as well as affective in their nature."

In my opinion, these recommended changes in the definition of mind in no way detract from the value of the term nor should they per se result in a diminution of its use. On the contrary, they should make the term more meaningful to both lay persons and psychologists and, when combined with recent advances in research technology, they should result in more rapid progress and clearer thinking in the future.

References

Donnelin, K. S. (1986). Gilbert Ryles's The concept of mind. Encyclopeadia Britannica (15th ed.) (Vol. 25, p. 616). Chicago: Encyclopeadia Britannica.

Kantor, J. R. (1935). The evolution of mind. Psychological Review, 42, 455-465.

Kantor, J. R. (Ed.). (1971). The aim and progress of psychology and other sciences. Chicago: Principia Press.

Roberts, M. (1987, May). Class before birth. Psychology Today, p. 41.

Trotter, R. J. (1987, May). You've come a long way, baby. Psychology Today, pp. 34-45.

Webster's ninth new colleagiate dictionary. (1983). Springfield, MA: Merriam.

Interbehavioral Psychology in Application

During the past few years much has been said about the cognitive revolution in psychology, but we have heard very little about another revolution based on the principles of contextual interactionism (e.g., interbehavioral psychology), although the results of its research have been of great significance. We have also been told that interbehavioral psychology does not lend itself to research and that it is merely theoretical and philosophical.

Recent articles covering different domains of criminal psychology, however, offer clear evidence that research projects in which the context of behavior is considered are much more productive than those conducted under laboratory conditions (see Wilkes, 1987). The authors of these studies probably gave little thought to any underlying principles, for these principles are beginning to permeate the scientific atmosphere without being recognized. Even though unrecognized, interbehavioral principles are there just the same, and their importance cannot be overestimated.

Perhaps the first area of psychology in which the contextual aspects of behavior were emphasized was social psychology, where Dane Archer made an emphasis on context the key to his research (Wilkes, 1987). He departed from psychology as the study of an individual within a single social context to psychology as the study of different contexts and the variability of an individual's behavior within them. Never before has this been done on as wide a scale nor with the research skill and technology that Archer has had at his command. Like other contextualists, Archer has succeeded in dispelling many myths and has obtained results that provide insight into violent criminal behavior. Archer's work is such that it will be examined closely not only by psychologists and sociologists who are professionally interested in crime, but also by policy and lawmakers at all levels of society. Although interbehavioral principles may not be recognized as the theoretical foundation upon which this work is based, the success of this research program should provide considerable encouragement to those who have long been convinced that such principles are essential to the integration of psychology into society at large.

The paper by Wener, Frazier, and Farbstein (1987) is of particular interest to this commentator, who spent three years (I was about to say "miserable" years) as a senior psychologist and classification supervisor in prisons of the type classified in the article as "first generation." That was 50 years ago, but recent riots have demonstrated that conditions at the Indiana Reformatory are (due in part to overcrowding) probably even worse than they were back in those dark ages. In any event, this is one of the most gratifying pieces that I have ever read in my many years as a psychologist, for it demonstrates that members of our profession have not only been instrumental in reconstructing the jails and prisons described, but also in changing the entire philosophy of administering detention and incarceration. Those of us who broke ground in the 1930s had little hope for much improvement, let alone any as marked as this article describes. Granted, such changes have occurred only in rare instances, but the trend has now begun in earnest. With the resulting economies in detention and correctional systems now so important, further progress is inevitable. The day may not have arrived when psychologists consider prisons as offering congenial employment opportunities, but that may not be far away. I only wish that my classification staff of the 1930s which, in its entirety, went into military service when World War II approached, could be around to see these changes of which we never dreamed. We subscribed to the principles of interbehavioral psychology in those days; their application has fulfilled our most optimistic expectations.

References

Kantor, J. R. (1935). The evolution of mind. Psychological Review, 42, 455-465.

Trotter, R. J. (1987, May). You've come a long way, baby. Psychology Today, pp. 34-35.

Wener, R., Frazier, W., & Farbstein, J. (1987, June). Psychology Today, pp. 40-49.

Wilkes, J. (1987, June). Profile: Dane Archer: Murder in mind. Psychology Today, pp. 26-32.

BOOK AND JOURNAL NOTES

Benton, A. (1985). Focal brain damage and the concept of localization of function. In L. Costa & O. Spreen (Eds.), Studies in neuropsychology: Selected papers of Arthur Benton (pp. 85-93). New York: Oxford University Press.

One of the most important aspects of interbehavioral psychology is its approach to the role of the brain in studies of behavior, and to brain localization in particular. There seems to be no lack of books and articles that perpetuate old myths about the brain; indeed, that such myths still find wide acceptance in so-called higher scientific circles is surprising. It is, therefore, extremely refreshing to have access to a paper that not only traces these myths to their historical sources, but presents the history of their rejection by outstanding writers in clinical neurology. This rejection goes well back into the 19th century and, although it has been incorporated into interbehavioral theory, it did not originate therein.

Two important words which Benton uses to differentiate between sound and fallacious thinking with respect to brain localization are "symptom" and "function." The former refers to the impairment of some aspect of behavior resulting from damage to a particular area of the brain, and the latter to a mentalistic power that resides in the particular brain location. His approach is entirely historical and, in refuting the concept of localization of function, he cites such authorities as Lange, Jackson, Loeb, Head, Goldstein, and Luria, with pertinent quotations from each. These writers realized the difference between (a) the necessity of an intact brain for action and (b) the brain's being the seat of action -- and they pointed this out in no uncertain terms. This paper is, therefore, an important reference for writers of an interbehavioral perspective involved in discussions of brain function or brain localization.

In addition to refuting the localization fallacy, Benton also points out that defining the particular aspect of behavior that is impaired in localized brain damage is tricky business. Not only does every act have a number of aspects, any one of which may be impaired, but the impairment may vary over time or it may change in amount. In this regard, Benton's paper is particularly important, as some of his original sources are becoming hard to find. It was no doubt with this awareness that the editors of the volume in which this paper is published brought Benton's contributions together for today's students and for those yet to come. Benton's paper should be on the required reading list of every graduate student and psychology instructor as a way to overcome brain dogma.

- - - - -

Cone, D. M. (1983). An objective analysis of species-typical and other behaviors. In N. W. Smith, P. T. Mountjoy, & D. H. Ruben (Eds.), Reassessment in psychology: The interbehavioral alternative (pp. 347-360). Washington, DC: University Press of America.

Although all of the papers in Smith, Mountjoy, and Ruben's (1983) Reassessment in Psychology are very much worth reading and are worthy of comment, Donna Cone's chapter contains several very important points which should be incorporated into the thinking of all interbehavioral psychologists. Cone not only illustrates the important influence J. R. Kantor (1924) and Zing-Yang Kuo (1976) would have had, if read, on comparative psychology, but she brings into relief and clarifies the difference between biological and psychological interbehavior, which neither Kantor and Smith (1975) nor Kuo (1976) succeed in doing quite as clearly.

In establishing a completely naturalistic approach to the behavior of various species of animals, from fish to mammals, Cone reemphasizes the necessity of limiting data to that which falls within the framework of Kantor's stimulus function, behavior history, setting factors, media of contact, and response function. This framework eliminates the possibility of introducing such extraneous constructs as instincts, drives, and motives into the discussion. Although Cone is obviously quite familiar with Kuo, she does not mention that his framework of

investigation -- which consists of morphological factors, biophysical and biochemical factors, stimulating objects, developmental history, and environmental context -- is almost the same as Kantor's, though developed independently from it. Kantor's framework establishes a foundation for Kuo's more scientific comparative psychology than is to be found elsewhere in psychology or ethology -- and Cone has followed Kantor's example. This permits a completely interbehavioral approach which takes the place of stimuli as "triggers" for behavior and which establishes a field concept requiring observed rather than constructed explanations.

Perhaps Cone's most original contribution is her clarification of the difference between biological and psychological interbehavior, with the former depending upon the evolution of the species and the latter depending upon the interbehavioral history (see Kantor) or developmental history (see Kuo) of the individual organism. Kuo stresses developmental history in his analysis of both biological and psychological interbehavior, but there is little doubt that some biological behavior is based on structure-function mechanisms, even though the entire organism in its ecological niche is involved. The lower the species on the evolutionary scale, the more its behavior will be biological in nature. This should not be a problem as long as one remembers that evolutionary development has taken place not simply within the organism, but in the relationship of the members of a species to that species' environmental context.

It is indeed encouraging to note that the principles developed from a philosophical position many years ago by J. R. Kantor (1924), and that have long been ignored by the psychological establishment for not having been based on actual research, have now been demonstrated to accomplish exactly what they were supposed to do: To provide a set of sound postulations upon which research in a science of behavior can be based. Cone has performed a valuable service in emphasizing this and it is to be hoped that other comparative psychologists, as well as students of human behavior, will follow in her footsteps.

References

Kantor, J. R. (1924). Principles of psychology (Vol. I). Chicago: Principia Press.

Kantor, J. R., & Smith, N. W. (1975). The science of psychology: An interbehavioral survey. Chicago: Principia Press.

Kuo, Z-Y. (1976). The dynamics of behavior development: An epigenetic view. New York: Plenum Press. (Original work published 1967)

- - - - -

Gergen, K. M. (1985). Social psychology and the phoenix of unreality. In S. Koch & D. E. Leary (Eds.), A century of psychology as science (pp. 528-557). New York: McGraw-Hill.

A subtitle for this paper might very well be "An epistemological approach," for Gergen starts by stating that "it will prove useful to center our attention on the critical conflict between environmental and person-centered theories of knowledge" (p. 529). The former characterizes Wundt's Principles of Physiological Psychology and the latter his Volkerpsychologie and "elements of this antagonism now insinuate themselves into all aspects of psychological inquiry" (p. 530). It is the history of this conflict between exogenic and endogenic thinking to which Gergen's paper is devoted.

As an introduction to the "lines of battle," Gergen devotes a page to describing six differences between exogenic and endogenic thinking in order that the reader may "understand the present complexion of the discipline by examining its historical development" (p. 532). As far as I am concerned, this approach is novel and extremely interesting. Only a writer thoroughly familiar with epistemology would tackle such a venture.

Gergen's development of the history of this dichotomous approach to psychology in general, and to social psychology in particular, is astonishingly thorough in spite of its being telescoped into 23 elucidating pages. In some instances, however, this condensation detracts from the smoothness of the writing, for Gergen is a compulsive bibliographer with a total

of 181 listed references and as many as 44 on a single page. He also includes 23 footnotes, some having considerable length with additional references included. This condensation of what might have been an entire volume into a single chapter sometimes gives the false impression that explanation based on total mastery has given way to an exercise in erudition. The excess references and explanatory notes, however, is a minor distraction and can be overlooked, for the story Gergen unfolds with considerable skill is vital to the future of psychology.

The justification for presenting a commentary on Gergen's paper in The Interbehaviorist is that, without Gergen's knowing it, he provides the historical foundation for the need for the approach to both general and social psychology that J. R. Kantor (1924, 1926, 1929, 1982) has already provided. Never before has a psychological historian delineated the theoretical shortcomings of psychology's past 100 years with such telling accuracy, nor has anyone previously pointed out that psychology's immediate future lies in theory construction rather than in empirical research. Gergen, However, is fully aware that, professional politics and economics being what they are, "as the availability of academic positions continues to recede, many institutions may prefer to select a traditional candidate over one searching, but uncertain. Those who wish to fly on new wings may continue to dream of future weather" (p. 540). Whether this note of pessimism is justified or not, only time will tell, but when the present wave of mentalistic cognitivism recedes, as it inevitably will, the spark of interbehavioral psychology, which is glowing ever brighter, may at last burst into flame.

One of the most interesting sections of Gergen's chapter, as far as this commentator is concerned, is that on Kurt Lewin. Gergen considers Lewin primarily as a mentalist (as did J. R. Kantor) who showed some vacillation from one position to the other. Although Lewin attempted to think in terms of a field approach, his field did not depart enough from the mentalism in which he had been trained and, although he left a mark on American psychology, he did not solve the exogenic-endogenic problem.

Gergen also presents an excellent picture of the trends responsible for the recent cognitive movement in psychology. He correctly describes this movement as a return to an earlier mentalistic position and, in presenting the reasons for that return, does so in a somewhat sympathetic fashion. He does not, however, try to give the impression that cognitivism is capable of meeting psychology's future theoretical or linguistic needs even though he does give it credit for the development of medical psychology.

Upon concluding the historical sections of his paper, Gergen cites a large number of writers who have expressed dissatisfaction with psychology as they see it at the present time, or at least when their papers were published in the 1970s. He states that "all are indicative of a major evolution in thinking" and that "such generalized ferment has not taken place in psychology since the advent of behaviorism in the 1920's" (p. 545). In attempting to discover where this ferment and dissatisfaction are likely to lead, however, Gergen raises many questions, the answers to which are anything but clear. This leaves the reader with a sense of having been "let down" with nothing definite to tie onto after having been led to believe that the present, or at least the future, would show some hope of bringing order out of chaos. However, it is under such conditions that opportunities arise and, if anything is certain in psychology, it is that some important changes will be occurring in the future.

As I concluded my reading of this theoretically and historically important paper, I could not help but wonder how the final sections might have been written had the author been familiar with the work of J. R. Kantor. An inclusion of Dewey and Bentley's Knowing the Known (1976) in the reference list would also have permitted inclusion of an epistemological perspective that was conspicuously absent.

Interbehavioral psychologists may be disappointed (but not surprised) at these omissions, but the omissions may be for the best. Authors of surveys have neither the time nor the inclination to give interbehavioral principles the attention they deserve and, as a result, they even quote adverse criticisms by people who are not psycholgists at all (e.g., Carroll, 1985, pp. 835-836). As a matter of fact, Gergen did include one interbehavioral paper (Sarbin, 1977) among his references,

but it was only one of 11 cited in a single sentence of his text (pp. 544-545) which, if it were read at all (which is doubtful), was probably not understood.

In the final analysis, Gergen's paper becomes one of an increasing number which, without his awareness, is making the case for an interbehavioral approach stronger all the time. Even the entrenched psychological establishment which has gone out of its way to ignore and tacitly suppress the work of J. R. Kantor for so many years cannot stop this trend.

References

Carroll, J. B. (1985). Psychology and linguistics: Detachment and affiliation in the second half-century. In S. Koch & D. E. Leary (Eds.), A century of psychology as science (pp. 825-855). New York: McGraw-Hill.

Dewey, J., & Bentley, A. F. (1976). Knowing and the known. Westport, CT: Greenwood Press. (Original work published 1949).

Kantor, J. R. (1924). Principles of psychology (Vol. 1) Chicago: Principia Press.

Kantor, J. R. (1926). Principles of psychology (Vol. 2). Chicago: Principia Press.

Kantor, J. R. (1929). An outline of social psychology. Chicago: Follett.

Kantor, J. R. (1982). Cultural psychology. Chicago: Principia Press.

Sarbin, T. R. (1977). Contextualism: A world view for modern psychology. In A. W. Langfield (Ed.), Nebraska Symposium on Motivation: Personal construct psychology. Lincoln: University of Nebraska Press.

- - - - -

Giorgi, A. (1985). Toward the articulation of psychology as a coherent discipline. In S. Koch & D. E. Leary (Eds.). A century of psychology as science (pp. 46-59). New York: McGraw-Hill.

Let me begin these comments by saying that it is always a pleasure to read a chapter by a first class scholar who appreciates the importance of theory in psychology, even when an understanding of that chapter requires a review of literature which has long grown dim in memory. The excuse for such reviews is appreciated, especially when they lead to unfamiliar areas of psychology that one hardly knew existed. The present paper has both historical and cultural significance which, upon first reading, is not readily apparent.

Giorgi begins by quoting George Henry Lewes (1817-1878) and J. R. Kantor (1888-1984) who observed, 100 years apart, that psychology is in a state of chaos. Giorgi does not agree with Sigmond Koch that this chaos is beyond remedy, for he considers the current state of affairs to be a function of an inadequate theoretical foundation on the part of both science and psychology. Giorgi states:

> The very conception of science entertained by psychology is at odds with the subject matter it seeks to comprehend. Some people commit themselves to science first and then try to make psychology fit their concept of science without questioning whether existing science is appropriate for the study of human psychological phenomena [italics mine]. (p. 49)

This last word is the key to Giorgi's position, for his thesis is that only phenomenology can include that which is truly psychological. The fact that some types of behavioral psychology cannot deal with mind is no proof that what is referred to as mind does not exist, but only their concept of science cannot deal with it.

The main body of Giorgi's chapter is devoted to developing a reconceptualization of psychology within the framework of that school of philosophical psychology which, since the days of Edmund Husserl (1859-1938), has been known as phenomenology. That this is obviously Giorgi's position is indicated by his use of the terms "phenomena" and "phenomenon" 18 times on 8 different pages. Giorgi's major interest is also listed as "phenomenology" in the directory of the American Psychological Association. That he is a leading United States authority in this area is indicated by his publications as listed in the chapter's reference section.

Due to my lack of previous familiarity with phenomenology, my understanding and appreciation of this chapter have been made possible only by Brett's, Boring's, Kantor's, and Sahakian's histories of

psychology, all of which present an excellent coverage of the topic. This coverage is greatly enhanced by that on phenomenology under "Schools of Philosophy" in the 1986 revision of the 15th Edition of the Encyclopaedia Britannica. This reference is highly recommended as it describes phenomenology's 20th century development in several different countries. Although phenomenology has many branches and has apparently influenced the thinking of many psychologists, including Rollo May and Carl Rogers, Giorgi appears to be a follower of the French phenomenologist, Maurice Merleau-Ponty (1908-1961), whose volume on behavior was translated into English not many years ago.

Although interbehavioral psychologists cannot subscribe to Giorgi's theoretical position, they can agree with the following statement in his concluding section:

> I attempted to show that psychology could be a coherent science by indicating that the factual absence of such a status is not intrinsic, but, rather, is due to poor theorizing concerning psychological phenomena as well as the meaning of science. Whoever can describe the scope of psychology and specify its proper object can, on that basis, discover its principle of coherence. (pp. 57-58)

Interbehavioral psychologists are convinced that they have found, or can find, this coherence in the work of J. R. Kantor. It is good to know, however, that there is another all-encompassing system that is also based on the highest level of philosophical thought, though with entirely different postulates developed from a different point of view.

\- - - - -

Kantor, J. R. (1959). Psychology and biology. In J. R. Kantor, Interbehavioral psychology (pp. 203-210). Chicago: Principia Press.

It is ironic that in dozens of textbooks in general psychology, each consisting of several hundred pages covering many aspects of the subject matter, students will not find a description of the relationship of psychology to biology which compares to that which is presented in this brief chapter. Although Kantor has, at times, been accused of being difficult to comprehend, such is certainly not the case here. The chapter does deserve careful reading and rereading, however, because the material is well worth mastering; moreover, a number of statements are worth the effort to commit to memory. There is certainly nothing difficult about Kantor's writing here, and his basic points are covered with a clarity which is seldom found in such discussions.

It is tempting, even in a brief commentary, to quote some of Kantor's statements, but this would dilute the pleasure of reading the entire chapter and appreciating the relationships that tie its parts together. On the very first page is a description of biological events which, it should be emphasized, include not just the structure and functions of cells, organs, and organ systems, but also "movements and actions which relate organisms directly and immediately to environmental objects and conditions" (pp. 203-204). Psychological events likewise consist of such adjustments, but at a different level.

Psychological actions are not limited to structure-function mechanism, nor to the natural properties of stimulating objects. Just where biological adaptations leave off and psychological interbehavior begins Kantor does not tell us, but ecological adjustments are limited to the preservation of the organism. The psychological characteristics of variability and modifiability are certainly not limited to mammals but, if an act is truly psychological, it can be understood only in relationship to other actions that are also psychological. Biological structures and functions provide only possibilities and limitations.

Some of the topics covered in the chapter include "things and events unique to biology," "the historical nature of biological and psychological events," "influence of biology on psychology," "influence of psychology on biology," and "cooperation between psychology and biology." One of the characteristics of this presentation that makes it superior to others is that it does not go into either an over-emphasis on the brain, which characterizes much of present day

psychology, nor does it concern itself with behavioral genetics. Both of these bring forth a negative reaction on the part of interbehavioral psychologists, but there is nothing negative about the present presentation.

In his final section, Kantor points out that biology has as much to gain from an understanding of psychological principles as psychology has to gain from biology. This is particularly true of ethology, which could profit from including an analysis of interbehavioral events rather than falling back on such constructs an instinct, drives, motives, and triggering mechanisms to release behavior. Kantor does not dwell on these or other points where biology has accepted the constructs of an obsolescent psychology, and he makes it clear how these weak points can be easily alleviated.

The contents of this chapter should be emphasized in all courses in introductory psychology and all psychologists should be able to write at least a paragraph on psychological-biological relationships.

- - - - -

Kuo, Z-Y. (1976). The dynamics of behavior development: An epigenetic view. New York: Plenum Press. (Original work published in 1967)

Back in the 1960s, I purchased a copy of Zing-Yang Kuo's (1967) The Dynamics of Behavior Development, which was about to go out of print and which I attempted to keep in print by writing to psychologists who were influential with the publisher, Random House. Dr. Kuo died shortly after the book came out and Dr. Kantor (1971) wrote an obituary which appeared in The Psychological Record and which is included in Kantor's selected writings (Kantor, 1984). As far as I know, Dr. Kuo was the only psychologist for whom Kantor had unconditional praise, and my opinion of the book at the time was that it is really basic to the experimental substantiation of Kantor's position on the so-called heredity-environment controversy. I was very disappointed when it went out of print and I had forgotten it entirely until I reread it recently. Just out of curiosity, I looked in Books in Print to see if it was listed and to my pleasant surprise I found that Plenum Press has it listed with a 1976 date; the edition of Books in Print in which I was looking was for 1986-1987, so the book is still available today.

It is a very small book, and hence the listed price of $29.50 seems steep, but it has more in it than many books several times its length. It can be read in a few hours and it should be read over and over by anyone who is interested in the interbehavioral point of view.

One distinctive thing about the book is that it describes Kuo's nonhuman animal experimentation in China which involved such a disregard for animal suffering and animal life that it is in a class by itself. Such experiments could not even be dreamed of in this country, let alone performed. I am an animal lover and I do not condone such experiments, but since they were performed, we should note the results. After all, they were not any more barbaric than the great Chinese cultural revolution, a social and political experiment that cost about ten million human lives and which the Chinese now consider a failure. One does not have to condone cannibalism to study it; one does not have to condone animal experiments of that type to study their results. In any event, some of Dr. Kuo's most significant results did not result from the sacrifice of his animal subjects. It is difficult for me to imagine how Kuo performed his experiments, even in China; and his patience was as remarkable as the extent of his experiments, some of which involved hundreds of animals.

When Dr. Kuo was in this country during the 1920s, he and Kantor, apparently without knowing about each other, attacked McDougall's instinct theory, which had been very influential, and they did so with a thoroughness that completely demolished McDougall's theory for good. It was one of Kantor's real successes (see Kantor, 1923), but Kuo received much of the credit for it. Unfortunately, instinct theory slipped back into psychology through Freudian theory, but the criticisms of McDougall apply to Freud as well. I spent a lot of time in the 1930s with Kantor's lengthy 1923 article, but I never read Kuo's attack on the instinct theory. Some of the basis of his attack is included in his little book and one can get his point of view very clearly from his discussion of those early days. I can assure you that there is nothing else like it. It is

highly recommended for very careful reading.

References

Kantor, J. R. (1923). The problem of instincts and its relation to social psychology. Journal of Abnormal and Social Psychology, 28, 50-77.

Kantor, J. R. (1971). In memoriam: Zing-Yang Kuo. The Psychological Record, 21, 381-383.

Kantor, J. R. (1984). Selected writings in philosophy, psychology, and other sciences, 1929-1983. Chicago: Principia Press.

Kuo, Z-Y. (1967) The dynamics of behavior development: An epigenetic view. New York: Random House

Kuo, Z-Y. (1976). The dynamics of behavior development: An epigenetic view. New York: Plenum Press.

- - - - -

Kuo, Z-Y. (1967). Stimulus and environmental context. In Z-Y. Kuo, The dynamics of behavioral development: An epigenetic view (pp. 149-179). New York: Random House.

Every year when baseball or football teams assemble at their training camps prior to the beginning of a new season, the cry is "back to fundamentals." Psychology could well profit from this example. There is no place where some of the "fundamentals" of the contextual interactionist (interbehavioral) perspective are presented more succinctly than in Kuo's boiled down chapter. Here, in 30 small size pages, he not only states, but also illustrates by numerous examples, how action is dependent upon a multiplicity of factors, some external and some within the organism.

These principles have, at some time or other, been familiar to all interbehavioral psychologists, but they should be recalled from the recesses of memory and made part of daily thinking. Here is the type of psychology that we actually apply to the understanding of behavior in day-to-day situations but which, when it reaches the classroom and is presented to students, is either oversimplified or distorted beyond recognition. In Kuo's material, though, we have psychology as science at its best, with an emphasis on natural events whether these be physical, chemical, physiological, or social. Ruled out are not only transcendental constructs such as instincts, drives, and motives, but the oversimplification of organism-stimulus relationships such as those of Pavlov, Watson, Hull, and Skinner. If one is to be scientific, intervening variables and hypothetical constructs can never take the place of actual events. Although critical of others, Kuo does not hesitate to point out the errors of his own past and how his thinking changed over a period of years.

In covering his topic, Kuo gets to work at once under such headings as "definition of stimulus," "complexity of stimulation," "the oversimplification of the conditioned reflex concept and the S-R formula," "quantitative aspects of stimulation," "the nature of the environmental context," and "combined effects of developmental history and environmental context." These are all familiar topics, but here they are presented in such a way as to take on new vitality and a convincingness that only Kuo's decades of careful observation and experimentation could provide. Gone are the days when a description of physiological stimulation was relevant for students of psychology, because even the simplest stimulation of a total organism (in contrast to a nerve-muscle preparation) involves much more than a single sense organ and a reflex arc. Kuo considers the physiological aspects part of the behavior itself, with both involving the organism in its environmental context.

For Kuo, context includes not only factors that are extraneous to the reacting organism, but those that are intraorganismic as well. These include chemical factors (many of which have come under careful investigation since Kuo wrote), as well as matters of health, fatigue, etc. He points out that to make the determiners of action investigatable within the realm of science, research must be much more painstaking and inclusive than has been characteristic of the past.

It would seem that science is beginning to follow the principles that Kuo enunciated, especially in the refinement of bio-psychological methodology, although much of this is being done without any understanding of the basic principles involved. It is only when these principles are stated and are familiar to

teachers, students, and research workers that research and general knowledge of human and nonhuman actions can be clearly understood. Not until psychologists and animal behavior specialists (ethologists) recognize that the solutions to problems posed by the interactions between organisms and environmental contexts must be sought in the realm of natural events (no matter how challenging this may be to research workers) that their efforts will be worthy of the respect to which science is entitled.

- - - - -

Sanford, N. (1985). What have we learned about personality? In S. Koch & D. E. Leary (Eds.), A century of psychology as science (pp. 490-514). New York: McGraw-Hill.

Of the 42 papers included in the volume, A Century of Psychology as Science, the one by Professor Nevitt Sanford on personality is in a class by itself. This is true not only because Professor Sanford has lived the history of personality study as it has developed since the 1930s, but also because he writes in a style that is personal and warm and that makes his readers wish that they might have studied or been associated with him. By coincidence, Professor Sanford and this commentator were born within three months of each other 78 years ago; however, although each of us has had a career-long interest in personality, our backgrounds and the literature that had an early influence on us are quite different.

Very early in his career, Professor Sanford found himself at Harvard where he worked in Henry Murray's clinic, not only as a student, but also as an associate. Early on, he also became interested in psychoanalysis and was one of its early American practitioners outside of the medical profession. From that time, until the very recent past when he retired as President of the Wright Institute in Berkeley, California, Professor Sanford authored and co-authored many well known books and articles, taught at leading universities, and held positions of responsibility in professional associations. His knowledge of the literature is profound, not only in psychology, but in sociology and anthropology as well. His writing style indicates not only a thorough mastery of all he has read, but an ability to glean the important points, forgetting those that are irrelevant.

One point made evident throughout Professor Sanford's chapter is that personality psychology and academic psychology are two quite different subjects, not only with respect to content, but with respect to departmental sociology as well. He is one writer who does not hesitate to mention grantsmanship as playing a role in the specialty identification of psychology department members. In this respect, personality psychology has very little status. The reasons he gives for this neglect apply to interbehavioral psychology as well, these reasons focusing around lack of quantification and laboratory studies.

As Professor Sanford unfolds the history of his subject, as he personally went through it over a period of 50 years, he brings out the developments from an overall perspective. For example, to him, the development of clinical psychology in the 1940s was an outgrowth of the personality psychology of the 1930s, but the former virtually swallowed up its parent. He also describes his disappointing personal experiences in discovering how humanistic psychology had deteriorated within a few years of its founding. Even with its disappointments, the Wright Institute must have given Professor Sanford considerable gratification during the last years of his career, for here he could concentrate on the area of psychology that he considered most important and where there were no departmental rivalries or outside pressures, such as loyalty oaths which resulted in his leaving the University of California, although he does not mention this.

For a personal and very authoritative coverage of what has transpired in personality psychology during the past 50 years, this chapter is highly recommended both to older psychologists, who will note much that they have missed, and to students, who will gain considerable insight into how personality study can enrich the student much more than running rats.

Early in his chapter, Professor Sanford points out that his career was greatly influenced by psychoanalysis and what he learned from Freud. One of his most

important lessons was that personality development and structure are influenced by experiences during the subject's early years. This principle is, of course, of primary importance in interbehavioral psychology, but without the emphasis on instincts, which were integral to Freud. In my own case, I discarded Freudian theory very early and started reading the works of J. R. Kantor in 1933. My work was with state hospital patients of all types and, during my internship, I was trained in thorough history taking from family members. Our histories did not, however, attempt to include any explanations based on unconscious motivation or instincts of any kind whatever. I agree with Professor Sanford when he says that taking case histories should be included in the first course in psychology. (Not being able to do this, I had my students read biographies.)

Although I am not familiar with many of the writers who are well known to Professor Sanford, I am familiar with the work of Goldstein and of Lewin, for whom he has great respect, as do all who are familiar with them. Professor Sanford goes so far as to mention the possibility of combining psychoanalytic and field theory, but this is not pursued.

To me, it seems ironic that this brilliant clinician, teacher, research worker, and administrator should have been thoroughly familiar with the work of just about anyone whom one could mention in the field of personality with the exception of Professor Kantor. I cannot help but wonder what Professor Sanford's thinking would have been like had he read Kantor early in his career, as I did, and had it shaped his thinking. One will never know, but it is to be hoped that future students of personality will not commit this oversight, for in interbehavioral psychology one has the basic foundation not only of a superior type of approach to personality, but to general psychology as well.

- - - - -

Snyderman, M., & Rothman, S. (1987). Survey of expert opinion on intelligence and aptitude testing. American Psychologist, 42, 137-144.

Several months ago, I wrote a historical paper, which will remain unpublished, entitled, "Obituary for the IQ: Old Faculties Die Hard." I did not realize when I wrote it how premature it was. Now, Snyderman and Rothman have conducted a survey that indicates the IQ's tight grip on life. On the first page of their report, the authors state:

> A comprehensive survey of expert opinion about intelligence testing is necessary because the use of intelligence and aptitude testing represents an important public policy issue. A survey of expert opinion will not settle this issue, but it will allow a clearer picture of informed opinion to enter the public debate. In a way, it is a method of pooling "expert testimony" for the benefit of those charged with policy decisions. (p. 137)

The experts in this case were 1020 psychological specialists and members of related behavioral sciences who are in contact with the testing movement. It is my contention that the questions included in this survey were the wrong questions and that replies favoring pro-intraracial and interracial genetic differences in intelligence were virtually inevitable from the nature of the questions.

A basic assumption of many psychologists and others who have been involved with the testing movement since the days of Henry H. Goddard is that behavior samples, which tests are, indicate the presence of a transcending power or force within the person tested. At first, this force was known only as "intelligence," but during recent years it has also gone under the name of "IQ," which was originally a technical term to refer to children's standing on a test in relation to their actual ages. Looked at any way one will, this construct is a faculty that transcends behavior and that exists only as a vague generalization at best; at worst, it is an animistic power that condemns the individual to a lower socio-economic status or to a special track in school. Perhaps its most vicious application has been in labeling individuals in relation to their fellows. Actually, however, this theoretical interpretation need have no relation to the technology of testing; for instance, placement tests have been very widely used in the armed forces with considerable

effectiveness and with little stigma attached to obtaining a low score. The same thing can be said of licensing examinations and tests of special aptitudes. It is only those tests that are associated with something subjects are said to "have," rather than with what they have accomplished, that become objectionable.

Although Snyderman and Rothman do not include a copy of their survey in their report, they do include one question that indicates their concept of intelligence as a faculty responsible for achievement. This question is, "In your opinion, to what degree is the average American's socio-economic status determined by his or her IQ?" This concept of the IQ as a force that determines a particular level of behavior is one interbehavioral psychologists (Kantor, 1924, p. 97) have been attempting to expunge from the science of psychology since the early 1920s. It is a concept that should be relegated to the past as soon as possible. The refusal of many recipients to respond to the survey may be evidence of their recognition of this fact.

It should have long since been recognized that the usefulness of psychological tests in many situations, although indicative of behavioral differences in similar situations, in no way indicates a mental power that transcends behavior. One way to demonstrate this is by using a vocabulary test, the results of which are never associated with heredity but which, nevertheless, are, in a number of situations, a very good predictor of success or failure. Most so-called intelligence tests depend upon vocabulary, however; so, to say that intelligence is something biological is to say that vocabulary must be biological as well. Since this is patently ridiculous, it can be seen that the evidence that intelligence is a faculty based on a vocabulary test is on shaky ground. It should be recognized that, as surely as children have no vocabulary at birth, they have no intelligence either, for no behavior is present that can be described as intelligent. Intelligence is not, nor can it be, a biological characteristic, no matter how much biological impairment or malfunctioning may affect performance.

References

Kantor, J. R. (1924). Principles of psychology (Vol. 1). Chicago, IL: Principia Press.

Edward K. Morris, Editor
The Interbehaviorist
Department of Human Development
University of Kansas
Lawrence, KS 66045

Bryan D. Midgley
Department of Human Development
University of Kansas
Lawrence, KS 66045