

THE INTERBEHAVIORIST

A Newsletter of Interbehavioral Psychology

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THE INTERBEHAVIORIST publishes 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 also 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 duplicate hard copy and a single computer disk copy (any major word processor; any Mac or IBM disk format) 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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J. R. Kantor Books

The estate of Helene J. Kantor (1919-1993) has given The Archives of the History of American Psychology the inventory and copyrights of The Principia Press, long the publisher of the works of J. R. Kantor (1888-1984). The Archives is now prepared to respond, as The Principia Press, to orders from the list of books in print and in stock written by J. R. Kantor.

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Call for News

THE INTERBEHAVIORIST publishes news about subscribers' activities and information about others' activities that may be of interest to readers. If you have published an article, chapter, or book with an interbehavioral orientation, or have read one published by someone else, particularly if the source is obscure, please let us know about it.

The Agora

Editorial

We have not been able to move into the English-Spanish mode as fully as we had intended with this issue. Emilio and I will make a much more concerted effort to do so in the final issue of this volume due out in December.

I wish to thank Debra Fredericks for her sustained and considerable contribution to the production of *The Interbehaviorist*. This issue is almost entirely her doing. Thanks, Debbie.

Linda J. Hayes, English Editor
Emilio Ribes, Spanish Editor

Interbehaviorists in ABA SIG Meeting Minutes May, 1995 Washington D.C.

Co-chairs: Linda J. Hayes and Debra W. Fredericks

I. *The Interbehaviorist* Report:

A. Substantive changes: It was announced that *The Interbehaviorist* will be available in both English and Spanish. Spanish papers will include an English abstract and English papers, a Spanish abstract. The new Spanish language Co-Editor is Emilio Ribes. Spanish language submissions should be sent directly to Emilio at:

Emilio Ribes
Center for Psychological Research
12 de Diciembre 204 Chapalita
Guadalajara, JAL. 45030, Mexico

B. Editorial Board: Consequent to the discussion at the 1994 SIG meeting regarding editorial board members' participation, a letter was mailed to all current board members suggesting a re-organization. Responses to this letter encouraged: (1) a reorganization of the board, (2) each board member to identify, on a yearly basis, what their contribution to the newsletter would include, and (3) limited terms with the opportunity to renew. Additional feedback included support for continued publication of student papers, branching out to kindred literatures, and establishing an interbehavioral e-mail network. Nominations for new members to the advisory board

should be sent to Linda J. Hayes, Dept. of Psychology/296, University of Nevada, Reno, NV, 89557-0062.

C. Financial: Subscription fees have been increased to \$10.00 student, \$13.00 regular, \$15.00 foreign, \$20.00 institutional, and \$15.00 per back volume. Income from subscriptions is approximately \$800.00 whereas production costs have been approximately \$2,500 yearly. Given the deficits in publishing the newsletter, the paid position of managing editor has been eliminated. Context press will continue to underwrite production costs.

D. Sections: Submissions are always needed for all sections of the newsletter.

E. Increasing Circulation & Submissions: It is anticipated that the new Advisory Board will facilitate an increase in the number of submissions. Substantive papers will be subject to peer review. Greater effort must be attempted in marketing to universities, in addition to maintaining a higher profile at ABA. An Interbehavioral symposium at next year's ABA was suggested. The Interbehavioral Expo poster was well received at this year's meeting.

II. Interbehaviorists in ABA Special Interest Group:

The meeting was concluded with a general discussion about the role of the special interest group. It was suggested that we target groups in mainstream psychology and applied behavior analysis for participation in our activities. For example, popular constructs, such as "behavioral momentum", might be addressed from an interbehavioral perspective in a symposium at ABA. Symposium topics might include applied issues or interbehavioral implications for different levels of analysis and practice.

Notice

We are looking for a volunteer to organize an interbehavioral symposium for ABA'96. If you are interested in chairing such a symposium contact Linda Hayes or Debra Fredericks: Dept. of Psychology, University of Nevada, Reno, NV, 89557-0062 or e-mail dwf@pogonip.scs.unr.edu.

Special Thanks

A special thanks to James Herrick and Sid Bijou for their generous donations to *The Interbehaviorist*.

Cheiron XXVII 1995 Edward K. Morris University of Kansas

Interbehaviorists were well represented at this year's meeting of Cheiron (The International Society for the History of the Behavioral and Social Sciences), held June 22-25 at Bowdoin College (Brunswick, ME). Paul Mountjoy (Western Michigan University) was the most visible, with a paper, "Memories of Alfred Kinsey" (Bowdoin, 1916), and a poster co-authored with Donna Cone (New England Gerontology Academy), "The Functional Nature of the Philosophical Categories: J. Robert Kantor's Doctoral Dissertation" (see this issue of *The Interbehaviorist*). In addition, Debra Fredericks (University of Nevada) presented a poster, "Historiography and J. R. Kantor's *The Scientific Evolution of Psychology*." In attendance were Noel W. Smith (SUNY-Plattsburgh), founder of this newsletter (and a founding member of Cheiron), and Marc Rilling (Michigan State University), a sympathizer from the Midwest.

Papers were also presented by the nonsectarian, Edward K. Morris (University of Kansas) – "B. F. Skinner, Third Variables, and the Problem of the Province of Knowledge" – and by a Sid Bijou protegee from the 1970s, Andrew Winston (University of Guelph, ONT) – "As his name indicates: R. S. Woodworth's Letters of Reference and Employment for Jewish Psychologists in the 1930's." Among other titles of interest were Donald Dewsbury's (University of Florida), "The Psychonomic Society: A Society in the Image of Its Founders," and Gary Hughes' (St. Thomas University, NB), "Ten Good Reasons to Read the Word of B. F. Skinner."

Next year's meeting will be held on June 27-30 at Earlham College (Richmond, IN). For information, please write Donald Dewsbury (Program Chair), Department of Psychology, University of Florida, Gainesville, FL 32611 (904-392-0596) or Katherine Milar (Conference Host), Department of Psychology, Earlham College, Richmond, IN (317-983-1556):

[Editor's note: Cheiron maintains a fund for student travel support, also, students who present papers or posters receive a free 3 year membership.]

For E-Mail Aficionados

Steven Brown (Kent University) has organized a chapter-by-chapter read of William Stephenson's *The Study of Behavior* over Q-method internet. If you are interested in participating, contact Steve at: sbrown@kentvm.bitnet or sbrown@kentvm.kent.edu.

Conference Announcements

The Eleventh International Conference on Subjectivity, "Advancing the Study of Subjectivity", will be held October 12-14, 1995 at the University of Illinois, Chicago. This year's scheduled presenters include Steven Brown (Kent State University), "Uses of Q-Methodology as the Foundation of Scientific Study of Subjectivity." In addition, a scheduled panel discussion on "Introducing studies on subjectivity into proposals for research, theses, and dissertation" should be of interest to anyone considering a Q-Method research project. For more information, e-mail Robert Mrtek at: MRTEK@UIC.EDU.

The Nevada Conference on Psychological Philosophy is scheduled for December 28-30, 1995 in Reno. Anyone interested should contact: Linda Hayes or Pat Ghezzi at the Psychology Department, University of Nevada, Reno, 89557.

Noel Smith Retires?

After 32 years of teaching here (and 2 elsewhere), I decided it was time for a change. My department apparently decided to keep the change minimal and to that end has allotted me an office, telephone, and computer; and so I am doing what I have always been doing but without the burden of teaching.

Beginning later this year, I anticipate spending a few months of the year in the warmer climate of Gainesville Florida. The University of Florida which is located there has an excellent library that should facilitate my work on the textbook I am now writing.

The first time I noticed any difference in being retired was this fall when I didn't have to do all the scurrying around that marks the beginning of every semester. And now the choice of being at work or being at leisure is a pleasant change. Before retirement I couldn't have taken advantage of a beautiful day and gone into the mountains in the middle of the week to view the fall foliage as I can now do. Even so, it seems that I am keeping incredibly busy and wondering where all the time is going. Much of it, I hope will help promote interbehaviorism. The textbook in progress, a symposium I am organizing for Cheiron next year, correspondence with colleagues, reviewing of manuscripts, and so on, are aimed in that direction.

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Department of Psychology
S.U.N.Y.

Article

The Functional Nature of the Philosophical Categories: Jacob Robert Kantor's Doctoral Dissertation

Paul T. Mountjoy

Western Michigan University

Donna M. Cone

New England Gerontology Academy

Aunque la disertación de J. R. Kantor era en la esfera de filosofía, existe extendido equivocación de las contribuciones de la disertación de Kantor porque es mirado principalmente como un psicólogo quien ofreció otra escuela de psicología. En esta, los autores examinan la disertación de Kantor y su relación a su carrera posterior para disipar las malas interpretaciones y concluir que cuando pensando en modo general, Kantor primero era un filósofo de ciencia quien aplicó su filosofía principalmente a psicología.

John Mills (1994) has published a draft of the section on Kantor (1888-1984) from his forthcoming book on the history of American psychology. In it he characterized the dissertation as a treatment of psychological issues in the history of philosophy (Kantor, 1917). We believe this to be an incorrect characterization, and in view of the widespread misinterpretations of Kantor's writings (as well as the foundational role of the dissertation in Kantor's career) therefore believe that this document is of sufficient general interest that a proper description of it should be made available.

Perhaps a major reason for the widespread misunderstanding of Kantor's contributions lies in the fact that he is regarded primarily as a psychologist who proposed another "school" of psychology. Examination of his dissertation, and its relationships to his later career, should help to dispel this misapprehension. Viewed broadly, Kantor was first and foremost a philosopher of science who applied his philosophy primarily to psychology, and in this sense he indeed did develop a specific approach to the data of psychology. That is, Interbehavioral Psychology (Kantor, 1959), as a model system, illustrates how implementation of Interbehavioral Philosophy (Kantor, 1981) results in a completely naturalistic system of psychology which allows the investigation and understanding of all types of psychological events which are, of course, subject to revision as empirical investigation

proceeds; however, the basic philosophical assumptions are expected to be verified by these research activities.

Kantor's scholarly career spanned some 68 years from his first publications in 1915 (which predated the dissertation by two years) to his final book and articles in 1983. During these productive years he contributed thoughts concerning every area of scientific psychology as well as the history and philosophy of science. Functioning both as a critic and as a creative thinker his activities deserve careful scrutiny by modern critics and scholars. In an ideal world the Ph.D. dissertation serves as a foundation upon which a scholarly career is constructed. Kantor achieved this ideal, as he returned repeatedly in his later writings to themes from the dissertation which he then proceeded to elaborate in detail (e.g., Kantor, 1963; 1969). Thus, his analysis of the functional nature of the philosophical categories did serve as a broad philosophical basis for his subsequent publications.

The context of time and place within which the dissertation was produced serve as an instructive starting place. The University of Chicago in 1917 certainly was a center of intellectual activity. The cornerstones of the Department of Philosophy and Psychology were Angell and Carr. Kantor has spoken of a visit by Watson. This was a time of incredible intellectual ferment with far reaching changes in the intellectual climate. Such was the case not only in science and philosophy, but in art, literature, architecture, music and drama as well. The very foundations of many human intellectual enterprises were undergoing a process of revision. It is evident that the faculty at Chicago carried out well the dual tasks of a University by teaching both the current states of affairs and the changes which were occurring within the culture. As a graduate student, Kantor would have been taught to be both critical and creative by his mentors. The dissertation shows that he learned

his lessons well.

The dissertation consists of 236 typewritten double-spaced pages, and the Table of Contents indicates a division into five parts, each containing between two and four chapters. Since no page numbers are given in the Table of Contents, *and* each chapter is individually numbered in either typescript or pen at the top of its pages, and some pages are inserted without numeration, we shall not reference page numbers in this paper. However, the major portions of the dissertation may be briefly characterized by this summary of the Table of Contents:

Part One - The Realistic Attitude toward Experience. (The pre-Athenians, *and* Plato and Aristotle.)

Part two - The Transition Attitude toward Experience. (Hellenistic and Alexandrian.)

Part three - The Romantic Attitude toward Experience. (Roman-Christian and Early Scholastic.)

Part Four - The Naturalistic Attitude toward Experience. (Scholastic, Nationalist, and Experience Periods.)

Part Five - The Humanistic Attitude toward Experience. (Personalistic, British, Kantian, and Current Attitudes.)

Even a passing familiarity with the history of philosophy allows the conclusion that Kantor has dutifully demonstrated his familiarity with the broad sweep of this aspect of history to his mentors. That is, he knows the conventional wisdom. Where is the creativity? That is to be found within certain of the details. Within the body of the dissertation Kantor has inserted five small sections which are not listed in the Table of Contents, and these contain the creative aspects of this unusual graduate student. Kantor titled each of these interpolated sections "Analytical Table of Contents." It is clear to us that those few pages contain the concepts which are dear to his heart, indeed, in a sense to his very soul, which he is disclosing for all the world to see. What, we wonder, were the reactions of his mentors to these bold thoughts?

The first analytical table of contents proceeds Part One, and consists only of one page. It bears the appellation of Introduction, and the first paragraph consists of this single sentence:

"The futility and bareness of philosophy is owing to a failure to appreciate its aim and purpose."

Who among us today would have dared to begin our dissertation by calling into question the validity of the very discipline in which we were hoping to receive our Ph.D.? We confess that neither of us would, and we do regard ourselves to be appropriately assertive individuals. For make no mistake about it, this is a dissertation submitted to the Department of Philosophy, in partial fulfillment, etc.

The first Analytical Table of Contents then continues to state that this study of the categories will make it evident that the categories are evaluations of experience. This realization leads to a rejection of the view that the history of philosophy is a record of abstract logical systems. Current philosophical attitudes are stated to be moving in this direction, and his study will serve to distinguish the categories of philosophy from those of science and religion by considering their significant features.

All in all, a very ambitious introduction. What follows? We shall ignore the conventional materials and concentrate upon the four remaining Analytical Tables of Contents, and the Abstract.

The second (one and one-half pages) is inserted just preceding the Athenian Period, and, in pen, Kantor has added "The Platonic Phase" as an additional heading. These pages argue that the entire philosophical attitude changed from a study of external nature to an interest in human conduct. Plato's viewpoint is limited to a fixed metaphysical reality, while Aristotle is the first to set up a conscious table of categories. It is concluded that Greek philosophy remained far removed from actual experiential conditions.

The third (a single page) precedes the Hellenistic Period and indicates a shift toward philosophy highly conscious of its own attitudes toward experience. In the Alexandrian-Roman period the categories reflect the influence of a fusion of western and eastern civilizations. The individual feels helpless and dependent upon God.

The fourth Analytical Table of Contents is the longest, extending onto a third page, and precedes The Humanistic period, which "marks the beginnings of the ascendancy of the human experiences as a basis for the interpretation of reality." That is, in the naturalistic period the study of nature is pursued, while the later rise of Personalistic attitudes begins the emphasis upon the capacity of the human mind to know reality. Locke is presented as concerned with what can and cannot be the object of human inves-

tigation, while monadism is the central theme for Leibnitz. In British thought reality is found in conscious states of the individual, while Kant emphasized the judging processes of a thinker.

Fifth and last is the one and one-half page Analytical Table of Contents which precedes the fourth and final chapter of Part Five, and of the dissertation, *The Current Attitudes*. It begins by stating "that reality is to be sought for only in experience which consists of the actual conditions and events of everyday life." A reduction of philosophy to "a mechanical science" (e.g., the Kantian machine shop for grinding out knowledge, and later, Spencer) was reacted to by "a metaphysics which sought reality outside of the domain of science" (Bergson). Current philosophical movements "may be termed Neo-Realism and Pragmatism." Neo-Realism resembles "the rationalistic philosophy of the seventeenth century." Since Pragmatists see the categories of philosophy as evaluations of experience which the practical sciences produce, they omit "all of that experience which falls without the domain of the special sciences."

It should be obvious at this point that Kantor was concerned with the basic problem of the nature of reality, and how it was to be known, rather than with the psychological aspects of the history of philosophy. It is equally obvious that the Analytical Tables of Contents refer variously to the entire dissertation (the first), one chapter (the second), two chapters (the third), six chapters (the fourth), and again to one chapter (the fifth), while the three chapters of Part Four of the dissertation proper are essentially ignored. Thus, by concentrating upon those Analytical Tables of Contents we have neglected a significant portion of the dissertation. However, we shall justify this treatment at the conclusion of our paper.

The Abstract consists of a cover sheet plus seven pages of text, and these pages neither merely summarize the dissertation nor the Analytical Tables of Contents. Instead, Kantor introduces six new "rubrics" under which he reorganized the materials. In a footnote he states that these "actual terms do not appear in the original dissertation; they are employed here for convenience in summarizing the material." We argue that these rubrics organize the materials in a creative manner, and herewith list them with descriptive comments paraphrased from his discussion of each, and some brief quotations.

Rubric one *Attributive Categorization*. Governing principles are the source of facts. Thales (water),

Anaximines (air), Anaximander (the boundless).

Rubric two *Predicative Categorization*. Categories constitute particular predications of things. "With Plato the Pythagorean forms. . . become predicates of reality."

Rubric three *Projective Categorization*. Departure from objective conditions to personal activities. For example, Plotinus projected the interests and desires of mankind into a mystic interpretation of things.

Rubric four *Interpretive Categorization*. A return to concrete phenomena under changing social and political conditions, culminating in the methodology of Bacon and Descartes.

Rubric five *Evaluative Categorization*. In the 17th and 18th centuries philosophy becomes Humanistic and Personalistic, with a final development in Kant whose formal categories are purely mechanical and hence "not generally serviceable in the description of any scientific phenomena."

Rubric six *Instrumental Categorization*. "Here we have a clear and complete acceptance of the idea that philosophical work is primarily an attempt to develop an attitude toward the actual world in which the thinker finds himself."

Now, we ask you to compare the above quoted sentence to the first sentence of the dissertation quoted earlier concerning the futility and bareness of philosophy! Kantor began in despair and worked out a solution satisfactory to himself within the confines of 236 pages. Would that we all could do so well at least once in our life in so brief a compass.

Kantor's dissertation contains no discussion of any of the traditional topics of psychology. For example, there is neither a mention of the traditional faculties of knowing, willing, and feeling, nor of the more modern topics such as sensation, perception, thinking, memory, motivation, etc. The body of the text defines the philosophical categories as intellectual tools which are used in the human intellectual enterprise known as philosophy. Philosophy itself is defined as orientation toward things and events, and the dissertation discusses manifold shifts in these orientations as a function of time and specific cultural conditions. That is, the human experience is expressed in categories, and these categories have changed as a function of various temporal changes in the human condition. The references that support the arguments are philosophical works, histories of philosophy, and political and social histories. To be sure, there are citations to the books of figures in the

history of philosophy who also are treated of in histories of psychology (Aristotle, Leibnitz, and Kant, to name a few) but their psychological doctrines are basically ignored. Actually, the one citation to James is to his *Pluralistic Universe*. There is no citation of his *Principles of Psychology* which clearly was the dominant psychological treatise of the period.

The dissertation, then, constitutes an establishment of a philosophical foundation, a general orientation toward the world of things and events, which will serve to guide any scientist in carrying out the scientific job. Science is, in Kantor's view, applied philosophy.

In his dissertation, Kantor laid the foundation for his later arguments that humankind had a propensity to elevate concepts to a status of reality which transcended events. He attempted to reverse this error and argued that reality was actually those events within which humankind's experience is embedded. In this way he established a firm foundation for the empirical enterprises of all the sciences, not just for psychology alone.

We have concentrated upon the Analytical Tables of Contents and the Rubrics of the Abstract because these pages contain the essence of his original contributions to philosophy. We feel that our failure to discuss the more conventional aspects of the dissertation in detail is justified by our evaluation of those materials as data from which Kantor generated his conclusions concerning the function of the philosophical categories. To do justice to those data would require far more space than is available to us here. And, the conclusion that the proper aim and purpose of philosophy is the categorization of people's experiences with environing things and events rather than with transcendent mental states or sterile mathematical abstractions is indeed the essence of the dissertation since it was to serve as the guide to Kantor's scholarly works throughout his life. This, then, was his response to his opening statement that:

"The futility and bareness of philosophy is owing to a failure to appreciate its aim and purpose."

A few words concerning the physical aspects of the dissertation manuscript, as well as to its role in his life, seem to be in order. The typescript was developed on a manual typewriter long before the advent of word processing. Anyone who has directed doctoral dissertations in recent years is familiar with the

fact that deans of graduate schools currently demand a perfect manuscript. In 1917 standards were different. On almost every page of his dissertation there are multiple corrections and changes in the purple fountain pen ink that was Kantor's trademark until the end of his life. We assume that examination of other dissertations from the period would reveal that today's standards of absolute typescript perfection were not enforced at that time. And this very imperfection of the finished product seems to indicate that to Kantor it was not actually a finished product; instead it was a work in progress. The presence of a copy of his dissertation in his personal papers at the time of his death also appears to indicate that he regarded the dissertation as a work in progress; one to which he could return from time to time to evaluate the progress of his life work. To him, the development of naturalistic constructs was an unending task to which he wholeheartedly devoted his energies. He felt that his own works were not the final stage in the process of philosophical and scientific analyses of the event continuum. In lectures and conversations during the early 1950s (and later) he often reiterated that his life would be fulfilled only when his students progressed beyond his own achievements.

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[Author's note: This paper is a revision of the poster presented at Cheiron 1995 Conference. It is a conceptual draft we are working on for Kantor's biography. As such, it should be considered work in progress, suggestions and comments are welcome.]

Interview

An Interbehavioral Approach to The Study of Psychopathology: An Interview with Rue Cromwell

This interview begins our series of interviews with prominent Interbehavioral psychologists, in addition to some who may not be recognized as such. Inspired by Verplanck's (1995) description of four groups of "Interbehavioral" psychologists published in *The Interbehaviorist*, 23(1), these interviews are part of a larger project that will attempt to identify divergences and convergences in the scientific work of scholars who have interacted with Kantor (either directly or indirectly) at some point in their training or career. Excerpts from interviews will be published periodically in **THE INTERBEHAVIORIST**.

Rue L. Cromwell received his Ph.D. in Clinical Psychology from Ohio State University in 1955. After holding a number of academic positions in both psychiatry and psychology, Dr. Cromwell accepted the M. Erik Wright Distinguished Professor of Clinical Psychology position at the University of Kansas in 1986. He currently supervises an active research lab studying information processing and genetic factors in schizophrenia. Dr. Cromwell has been a long-time subscriber to THE INTERBEHAVIORIST, studied under Kantor as an undergraduate, and professes to have carried Kantor's influence throughout his career. Dr. Cromwell was interviewed by THE INTERBEHAVIORIST in November of 1994.

TI: *When were you introduced to Kantor's work?*

Cromwell: I was an undergraduate at Indiana University 1946 to 1950 and, during my senior year, took a seminar from Kantor. At that time, an interbehavioral course was required for psychology majors. The class was quite full, about 50 people. This course was not an entirely pleasant experience; Kantor was a very stern, tough individual for his students who did not have a very clear or coherent monistic framework. The bottom line was that the students, almost without exception, were frightened by him. I, like the other students, coming out of rural middle Midwestern, lower middle class America, with backgrounds in dualistic religious thinking, did not have a clear conceptual monistic view, therefore, I was one of a good number of people who made straight F's on the quizzes. We struggled with this throughout the year, often getting together to study and finally, like a light bulb, it just suddenly dawned

on me what a monistic framework of thinking was all about and why it was so important to psychology. All of a sudden, from then on and for the rest of the semester and the next semester I was making straight A's. The shift was as extreme as that. And while a lot of students went away grumbling about being mistreated with these bad grades and so forth I became very enthusiastic. Kantor was very difficult for an undergraduate to get close to, yet, I admired him. Still, it was only later, at psychology conventions that I got a chance to really approach him and talk to him about psychology.

TI: *Did you continue to study interbehaviorism in graduate school?*

Cromwell: The Korean war broke out that summer in 1950. I received a deferment because at that time clinical psychology was known as a critical occupational specialty since they needed thousands more psychologists for the Veterans Administration so I entered a Clinical psychology program at Ohio State. Two of the people there I worked with were George Kelly and Julian Ritter. George Kelly had published some stuff in *The Psychological Record* (Kantor's journal) but Julian Ritter was the one who had really been influenced by Kantor, in fact, crediting Kantor for *Social Learning Theory* (Ritter, 1954). Ritter and I had a common bond in that we both knew Kantor and had a great respect for him. Ritter's personality theory was pretty much continuous with the basic philosophy of science framework laid out by Kantor in regard to his views about dualism, reductionism, and so forth.

TI: *As a graduate student in Clinical psychology, did your interest in interbehaviorism lead you in a different direction from accepted schools of thought?*

Cromwell: I think the major way of conceptualizing the influence of Kantor had to do with a keen awareness or sensitivity to dualistic thinking which was rampant at that time in learning clinical psychology and in my view, still is. I was intrigued that one can approach clinical psychology, which a lot of people would still say is actually dealing with the mind, from a strictly monistic point of view and so became aware of how important it is for constructs in psychology to be reducible to events in time and space. If you cannot reduce a construct to its denotable or point-to-able events in time and space you are in trouble. Another issue that is closely related is the distinction between an event as opposed to the construction of events; a lot of people even today think of the construct as the fundamental data of psychology. For example, I just read a textbook manuscript that said cognitive schema are the basic determinants of behavior. This would cause Kantor to turn over in his grave, it is so blatantly a dualistic type of construction where you have the construction of the event composed as causing the event. That is a little bit different than dualism although closely related but it was that framework where I learned not only to think about psychology, but life in general; within a monistic framework and, like I said, shifting from F's to A's it was kind of like a religious conversion because all of sudden I had a new framework of thinking about things. It was not only a new way of going about science but also a new way of going about existence and activities outside my role as a scientist.

TI: *Did you continue to keep in contact with Kantor after graduate school and/or continue to read his writings?*

Cromwell: After I got out of graduate school I took a faculty job at Peabody college. In the clinical training program there was a lot of research on mental retardation and except for applying Ritter's social learning theory to research on mental retardation in a way consistent with the Kantorian point of view, I can't remember focusing much on Interbehaviorism per se. The next thing I recall is taking a sojourn as a professor in medical schools: Vanderbilt University Medical School, Wayne State University, and Uni-

versity of Rochester Medical School. Medical students and psychiatric residents were so far removed from philosophy of science and the type of good conceptual thinking that provides a base for psychology I really didn't have a chance to communicate with any one at all. Then, when I came here to Kansas, my first effort (with large auditorium undergraduate abnormal psychology courses) was to do an introduction to the nature of concepts and different ways to explain why events occur. I reviewed concepts of dualistic explanations, reductionistic explanations, and constructionistic fallacies with circular ontologies. Forth would be the monistic mode of explanation in order to get across the notion that there is a lot of garbage that we are still dealing with in abnormal psychology in terms of faulty constructions. I spent a lot of time covering these issues when I taught so that brought me back in touch with Kantor. It seems to be so important when dealing with abnormal psychology and schizophrenia (where a lot of concepts are mentalistic) to get across the notion that there are very real, very valid scientific problems there and it would be much more clear and useful to approach those problems from a monistic framework. This lead me to do that first chapter in the schizophrenia book where I again bring in Kantor here as part of this evolutionary process. *[Editor's note: see book review this issue.]*

TI: *It is interesting that you have brought Kantor into an area that is traditionally very biodeterministic. How do you integrate these two seemingly incompatible approaches?*

Cromwell: Kantor was not one to discard biological, physical, psychological, or sociologic constructs but basically, I learned from him that there are different modes or levels of construction of the same event and the goal of science is to find which mode of construction has the greater utility in predicting these events - all of which involve interaction. In the particular area of psychology there is an arbitrary division of labor. Psychologists are interested in those events that involve the interaction of the individual with stimulus objects in the environment. You record the data, you analyze it, and build your constructs on that basis and if it should turn out that a biological construction buys something in terms of predicting human behavior better, then that's a viable construction until at some time it may be superseded with some other construction.

TI: *Do you view biological events as setting factors?*

Cromwell: I view genetics and biology as one alternative group or set of constructs and propositions which are constructions for events that we first have to describe in behavioral terms. I do genetic research on monozygotic and dizygotic twins and the whole point to the game for me is that you've got to depict the subject matter, e.g. the basic data of psychology, clearly within a time-space framework in terms of what you are observing and what you are measuring. Then, if one can enhance one's predictability by making predictions about monozygotic twins behaving with more similarity than dizygotic twins then fine - go with that construction, as long as it includes predictability beyond any other framework. An example would be a study I did recently on visual iconic integration. The visual system resolves visual images within about 100 milliseconds - this construction is derived from the fact that you can present a pattern of dots, remove them within a few milliseconds and after a short stimulus asynchrony (offset with nothing), you present another pattern of dots - if the first pattern is still "being retained" it will integrate with the second pattern and be seen as, for example, the number "68" when the number "68" is a combination of the two sets of dots. However, if these two anomalous patterns of dots get separated from each other in time to 100 milliseconds apart, the person becomes less and less able to detect the number or the visual pattern which these two sets of dots integrate into. We did a very nice study to show

that tricyclic antidepressant drugs actually lengthen the decay line of the image of the integrated pattern of dots. As it turns out, people not on any antidepressant drugs have a complete decay at about 100 milliseconds. In other words, if the two dot patterns are 100 milliseconds apart there is a probability close to zero accuracy. Persons on tricyclic antidepressants still do pretty well out about 80 and 100 milliseconds apart. As a result, we have had to develop a set of not just biological but neurochemical constructs, at least in order to predict the results to begin with. Understanding neurotransmitters, we predicted the results ahead of time and hit it right on the nose - it was exactly as we predicted. To me that is "good Kantor" in that you are using the construct system to describe the event which increases your ability to predict. If it should happen that you've got to move to some other level such as sociologic or political scientific to formulate constructs to describe the event and enhance predictability so be it. The flaws in thinking are the ones of assuming that the construct system you are using causes the event. That would be dualistic in the biological realm and is also reductionism. So often the fundamental flaws in construction in psychological propositions have to do with the, in my opinion, unbelievably fallacious assumption that once you have a set of constructs which improve predictability, that is causing the event. My perspective may not be "orthodox" interbehaviorism, especially my focus on predictability, however, I believe it is consistent with what I learned from Kantor.

Bookreview

**In Search of a Comprehensive Behavioral Account of Schizophrenia:
A Review of Cromwell and Snyder's
*Schizophrenia – Origins, Processes, Treatment, and Outcome***

Glenn M. Callaghan
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The following discussion of Cromwell and Snyder's *Schizophrenia - Origins, Processes, Treatment, and Outcome* (1993) represents a mixed review of the authors' approach to compiling information on this topic. While Cromwell and Snyder's edited text provides the reader with a thorough up to date collection of research on a broad range of topics, the text fails to capitalize on the authors' philosophical paradigm based in contemporary behaviorism. This review highlights the obvious contributions of Cromwell and Snyder's text and calls attention to what is noticeably absent.

Cromwell and Snyder have gathered information on a variety of issues relevant to research in the field of schizophrenia and have assembled chapters by notable specialists. Most chapters emphasize the role of biology and purported psychophysiological aspects related to the cause and condition of schizophrenia. Several chapters emphasize the necessarily multifactorial model of inheritance of this particular psychopathology (e.g., Chapter 6, "Smooth Pursuit Oculomotor Dysfunction as an Index of Schizotaxia", by McGuffin & O'Donovan). Many of the other chapters summarize research that has been published elsewhere on topics such as research on reaction time (Steffy & Waldman) and hemispheric and structural differences (i.e., globus pallidus [Early]; thalamus, [Oke, Carver, & Adams]). Chapter 13, "Social Self and the Schizophrenic Process: Theory and Research", discusses subject matter based less in biological models and describes research on the self and schizophrenia. Chapter 19, "Mapping the Intrafamilial Environment of the Schizophrenic Patient", continues this break from a medical model explicating research results and theoretical issues surrounding family related issues.

The strongest chapters of the text are those writ-

ten by Cromwell (Chapters 1, "Heritage of the Schizophrenia Concept"; 4, "Schizophrenia Research: Things To Do Before the Geneticist Arrives"; and 20, "A Summary View of Schizophrenia"). These remain the most thoughtful and useful to the reader. The first chapter clearly outlines some of the significant issues relevant to the history of schizophrenia, emphasizing that the concept of schizophrenia developed in a cultural context in the same way that the paradigm of Skinnerian behaviorism developed. Cromwell dares describe the tenets of radical behaviorism and J. R. Kantor's interbehaviorism in a field that rarely sees these topics mentioned. The author thoughtfully acknowledges the importance of each position and highlights the same basic tenets of interbehaviorism for the reader using readily accessible language. Another noteworthy aspect of Chapter 1 lies in Cromwell's poignant discussion of problems with dualism, reification, and reductionism. Few researchers and theoreticians in the field of schizophrenia bother to announce their theoretical position and approach to understanding the multitude of data that come before them. Cromwell has eloquently denounced the difficulties inherent with these three challenges faced by investigators.

Cromwell's fourth chapter presents a well-stated set of questions researchers and readers alike should pose before concluding that the seemingly unexplainable determinants of schizophrenia lie in genetics. This is one of this book's stronger chapters because it offers the reader a set of practical guidelines to approach a difficult topic area in psychopathology. The chapter highlights the importance of considering base rates when determining the heritability probability of schizophrenia. Cromwell also points out that not all shared characteristics of schizophrenia are negative, calling into question a "flawed gene"

hypothesis (p. 58). This chapter is required reading for any contemporary schizophrenia researcher.

Chapter 20 is likewise useful to the reader as it summarizes the preceding chapters and highlights important considerations that follow. In addition, Cromwell emphasizes the oligogenic nature of schizophrenia stating that "not one, nor many, but a small number of factors" is responsible of causing this disorder. (p. 335) This is a conceptually useful conclusion for readers as it begins to limit variables to what are more causal. For readers unsure whether the text is appropriate for their needs, a reading of this closing chapter would provide sufficient information to make that decision.

While Cromwell argues against reductionism in Chapter 1, much of what follows in the text, albeit consistent from within a biological model, remains reductionistic. The structural reductionism seen in Chapter 2, "Left Globus Pallidus Hyperactivity and Right-Sided Hemineglect in Schizophrenia" (Early), and Chapter 3, "Dopamine-Initiated Disturbances of Thalamic Information Processing in Schizophrenia" (Oke, et al.) reduces a set of observable behaviors and those that are more subtle (i.e., thoughts, feelings) to a level of analysis that is entirely unobservable. This is not to argue that neurological and structural functioning is not an appropriate level of research. However, as Cromwell makes clear in Chapter 1, reducing human behavior to the smallest possible level of analysis does not necessarily advance knowledge in the field of psychology.

The critical statement made above illustrates the shortcomings found in this text. These criticisms do not lie in the editors' selection of authors or their respective contributions, as the chapters remain academically sound. The primary criticism lies with the fact that Cromwell does not provide more room for an interbehavioral nor radical behavioral analysis of this disorder. What the reader gets is a set of chapters on schizophrenia summarizing research from several different perspectives, but primarily a biological one. Psychology lacks, especially in the area of schizophrenia, a coherent text written from a paradigm that is not rooted in a medical model of illness. A contemporary behavioral approach is one obvious missing and much needed perspective on schizophrenia. Noticeably missing in this book is an explicit interbehavioral analysis. Readers will recognize Cromwell's continued emphasis on the multiple determinants of schizophrenia as it is repeated through

his chapters. However, without making explicit why multiple determinants matter, the reader is left wondering why it is important to emphasize this point.

There exist at least three ways to remedy this lack of representation of contemporary behavioral views. The first would be to write a text that summarizes the behavioral research conducted thus far and to point out areas needing further exploration. This approach is the simplest and does not add a great deal to our current understanding of this phenomenon; however, even this strategy might fill a few gaps in this book. Clearly absent in Cromwell and Snyder's text is any discussion of current behavioral strategies to treat schizophrenia such as those by Bellack and colleagues (1986, 1989) or Mueser and colleagues (1991). With the absence of this information, the text is not as thorough as it might otherwise be. Apart from this fact, this approach has already been taken by thoughtful authors such as Bellack (1986). A second strategy might be to summarize relevant research in contemporary behaviorism and related theoretical papers and combine them to provide a more conceptually cohesive account of how this set of behaviors continues to be maintained. The strategy of integrating behavioral views would prove the most challenging as it would require a substantial amount of conceptual thinking. A third technique would add considerably to our understanding and offer a great deal to readers with philosophical interest outside a medical model. This approach could present the current research findings in the same manner as Cromwell and Snyder, but would provide a behavioral interpretation or explanation of findings in various fields.

To illustrate this third approach, Cromwell could provide the reader with a discussion of Rosenberg's chapter on the social self (written from a personal construct and psychodynamic perspective) with a contrasting analysis of the self by one of several contemporary behaviorists (e.g., Hayes, 1995; Kohlenberg & Tsai, 1991, 1995) or a behavioral interpretation of why Rosenberg's chapter is important. One problem with books on schizophrenia is they often combine multiple paradigms by experts in each area, but do little to offer any cohesive set of explanations about how these areas might relate. This suggested analysis could afford the reader with an opportunity to notice differences between paradigms. It could also convey to the reader the conceptual importance of this information in a manner

consistent with the his or her perspective. As with the second solution, this is not a simple task. However, because the reader is missing out on Cromwell's interbehavioral perspective as the text exists in its current form, it appears a challenge worth facing. Additionally, this latter strategy would help clarify to the reader why a chapter on reaction time in schizophrenia (Steffy & Waldman; Chapter 9) is important to consider and assists in understanding this disorder. Further, chapters on cognition and perception could be supplemented with an analysis of why these particular behaviors are important to contemplate.

Of considerable interest would also be an inclusion of a set of chapters on the behavior of the researcher and providers of treatment. An analysis of the behavior of the clinician who provides medications and how changed behavior by the patient impacts the clinician would be of great importance. This strategy would capitalize on one of behaviorism's finest achievements, the ability to turn its analytical tools on the behavior of the scientist.

These criticisms are not meant to dissuade readers from pursuing this text. Again, it is a noteworthy text summarizing valuable research topics and providing up to date empirical results. In addition it is clearly written and provides the reader with access to a variety of topical discussions relevant to the field. However, interbehavioral and radical behavioral approaches offer a valuable perspective to the field of psychology. It would be tremendously advantageous to capitalize on the philosophical knowledge base of Cromwell and address prominent disorders such as schizophrenia from these unique, environmentally

rooted paradigms. It is this reviewer's hope that a reader will rise to this challenge.

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Discussion

An Internet Discussion of Constructs

At every Interbehaviorists in ABA SIG meeting over the past few years, Interbehaviorists have spoken of their wish to have more time set aside at ABA for socializing among the group, as it is in these informal gatherings that many stimulating discussions have been held. In this age of communications technology, e-mail has surfaced as an alternative to in-person informal gatherings. We invite readers to submit their comments in response to the following interactions on e-mail. One such interaction is reprinted below. We thank those who participated in this interaction for allowing us to publish their remarks without an opportunity for editing.

In response to a statement by Dennis Delprato about relationships being abstractions, the following dialogue developed:

Noel Smith: I am not convinced that a relationship is abstract.

Dennis Delprato: Did you ever teach anything like experimental psychology to undergraduates? $y=f(x)$ is abstract to them. Or think of the operant, taken as either a class of R instances having common environmental consequence or as class of R instances functionally related to environmental consequences ("under control of" in Skinner's terminology). In either case, the operant is not directly point-at-able. This is what I mean by abstract. I believe some of Kantor's comments somewhere (perhaps on mathematics) contributed to my view on abstractions – not that he is to be blamed for any possible misrepresentations on my part.

Noel: It seems to me that the relation of bodies and space and of people and environments is quite concrete.

Dennis: I agree with this. But does not the relation here have different referents from the functional relationship in $y=f(x)$ or in the expression relationship between variables X & Y as in correlation? The latter, along with Stephenson-inspired emphasis on subjectivity and other considerations, lead me to question the value of continuing with Kantor's stress on "objective" science. Such an emphasis may mislead us to draw a firm line of distinction between objective (e.g., overt, observable) and subjective (private).

Noel: I too have difficulty with these terms. Perhaps a useful direction is to construe science as the systematic study of observable events that can be categorized, even if rather artificially or arbitrarily, for further study.

Dennis: What you have just said leads me to think of the "Reno" approach to the field construct. Think about Linda's (Hayes) paper that you replied to. Her student, Mark Swain, has also made similar comments. The Reno school certainly doesn't deny that observable events are fundamental. They just ask where do we go from them? To objectivity or to observer-constructions? Which is why postulates are so important. Recall Linda's private events chapter in the book she co-edited with H. Reese. Bryan Midgley and I, I believe, came to the conclusion that we resolved apparent differences in your and Linda's TI field papers. Did we? I'll pass this on to Bryan and to Mark for their reference.

Bryan Midgley: I don't have Noel's or Linda's paper in front of me, but the way in which I recall resolving their apparent conflict went something like this. Noel seemed to stress the participants that coexist in the interbehavioral field while, of course, not denying their interrelations. Linda, in contrast, seemed to stress the interrelations while, of course, not denying that which interrelates – the participants. Perhaps my resolution is too simplistic. Dennis mentions Noel's use of "relations" as having different referents than "functional relation" (e.g., $x=f(y)$). I wonder.

Noel: Are these examples not constructs – scientific constructs? That is, the operant (and possibly

$y=f(x)$, depending on its referent if any) are constructs derived from concrete events. One could also call them abstractions, but abstractions with a very important character. I maintain that a relation is an event, not a construct, and therefore not an abstraction. But the mathematical description of an event is not the event itself and therefore is a construct or a type of abstraction. I hold that it is extremely important to be clear about constructs and events and to keep them distinct in order to avoid miscommunication as well as muddled thinking. The word "abstraction", it seems to me, is too broad and leads to confusion. The observer-constructions you mention are absolutely essential, but they must be distinguished from the events to which they refer. The components of the field and their relationships are all events, as I see it. So is the field itself which is comprised of those events, thus being an event itself – just a more encompassing one than the components. But the description of that field whether verbal or diagrammatic or mathematical is a construct (an "observer-construction", if you wish).

Bryan: I found Noel's latest response very informative and very useful. The most significant points seem to revolve around the question of whether we should use the word "abstraction" to describe anything. There are events and there are constructs. Where do abstractions fit in (I ask rhetorically)? As for relations as events, Kantor did indeed describe them as such. The (or at least a) problem seems to be that relations are not immediately apparent (this, I believe, being one of Dennis' points). Indeed, we have probably all been involved in research meetings, looking at graphs (constructions), wondering if a "real" relationship actually exists. Relations as events are sometimes hard to get at. I feel like the old time behaviorist involved in a debate with a Gestalt psychologist. The Gestalt psychologist has taught a rat to discriminate a relationship – respond to "larger than" when presented with two differently sized stimuli (e.g., two triangles) – which the rat does perfectly well. The relation was an event to the rat and to the Gestalter.

Noel: Did Stephenson ever address this issue [constructs & events]? His viewpoint would be of considerable interest.

Steve Brown: I see nothing about "events" as such in the Index to *The Study Of Behavior*, and

nothing else comes to mind... about events, relations, abstractions, constructs. My guess is – and it's only a guess – that he would consider any and all of these as substantive (each has a dictionary definition and means something specific), about which there could be transitive thought galore. Consider the example, offered by Bryan, of two psychologists looking at a graph and wondering if a relationship exists. They ponder aloud:

- The t-test is almost significant. I'll bet there's something there.
- If we took intelligence out as a covariate, I'm sure the effect would show itself.
- Is there some statistic with more power than t or F?
- Maybe we should add more cases.
- I told everybody that this was a wild-goose chase.
- Let's see if we can replicate this with different Ss.
- Noel was right. We should have used Q methodology instead!

And so forth. Even if "abstraction" cannot be used to describe anything substantive, this wouldn't preclude measurement. It might even be an interesting real study. I thought of doing this 20 or so years ago when I sat in on a graduate course on experimental personality. The instructor thought he was being so objective, and I was dying to take some "objective" findings of his and get him and some of his colleagues to talk about them. I'm sure all the talk was hiding objectivities of another sort – i.e., subjectivities! The one possibly significant story of Stephenson's that might be pertinent is found on p. 182 of his "Quantum Theory of Subjectivity," in *Integrative Psychiatry* (1989), in which he reminisced about one of Spearman's experiments. Spearman placed a subject in front of a matchbox and the subject was to imagine what was on the far side and the bottom (which couldn't be seen). He was trying to distinguish between sensorial cognition (based on what you could see) from notional cognition. He thought that cognition had a notional component that could be accessed via introspection. Stephenson saw that what was really involved was not introspection, but the talk that clothed the so-called introspections – i.e., the concourse of communicability about the matchbox. He provides no concrete study, but it wouldn't be hard to provide one. I suspect you could

do the same thing with all of Bryan et al.'s talk about abstractions, i.e., with the subject-matter of "abstraction" replacing the matchbox. The email notes that you sent me already have the beginnings of a Q sample.

Debra Fredericks: One thing that Linda has focused on here in Reno is realism and the truth criterion of maintaining that a construction is a direct or indirect product of interaction - isn't everything actually a construction except the event itself? - as soon as we engage in deriving or abstracting as a product of direct interaction with the "real world" we have taken an observer perspective. How do we determine, (as Kantor seems to do too easily at times), then, the validity of our product (validity in terms of how far removed from naturalistic interaction the product is) - correspondence? But, correspondence with what? Coherence? But coherence with what? - our own constructions? Pragmatic utility - but mentalists can claim utility also. It depends on one's goals. It seems that stating that a relation is an event is neither defensible or indefensible. From a one-world event perspective there are no relations because there are no two things to relate (automatically a dualism). So, even if relations are derived from direct interaction with the world it is still necessarily an observer perspective.

Dennis: Could the Reno School be applying criteria of truth to constructs? Thus, the "correspondence" and "coherence" questions. Then the nondualistic one-world view is that correspondence and coherence do not apply to relationships between constructs because this is a unified construction in itself.

Bryan: Actually you and Debra both lost me here. Care to clarify?

Dennis: To go back to abstractions, as I recall, I claimed that relationships were abstractions. You maintained they were events. I just took a quick look at Psychology & Logic. On page 163 Kantor blasts Dewey for giving the "impression of confusing non-existence with abstraction products as materials." Indeed, Kantor here is referring to Dewey's reference to "abstracted relations." It seems that, following Kantor, you were pointing out that abstracted relations do not destroy the events upon which they were derived and create something outside of *rerum natura*. Furthermore, the abstracted relations function as

events, as any linguistic reference to events serve as events. I was merely focusing on the abstractive aspect of relationships. Certainly, as Kantor says on pp. 254 of Psychology & Logic (vol. 1), RELATIONS ARE STIMULUS OBJECTS TO INTERACT WITH.

Bryan: When you read this last sentence, Dennis, do you take "relations" to mean the pristine interbehavioral events or the linguistic references (themselves pristine interbehavioral events) to those events? Although you and Noel would certainly agree that constructs are derived from events, I'm not sure that you both would necessarily agree on what psychologists study. Given your exchange with Noel so far, I imagine that Noel might say that psychologists study the events of interbehaving (i.e., they study behavior). In contrast, you seem to be implying that psychologists study constructs (somehow derived from events). That is, scientists usually describe what they study in terms of events (e.g., behavior, molecular genetics, the rings of Saturn). Perhaps what they should say is that they study graphs, video tapes, and/or computer printouts somehow related to behavior, molecular genetics, the rings of Saturn, etc. These ideas came about because you have presented the idea of behavior as an abstraction. If I understand your comments above, behavior is an abstraction when we are focusing on constructs. Behavior is a stimulus object when we are focusing on events. True? You have said that: "The most striking feature of psychological behavior is that it is never directly seen. Behavior of concern to the psychologist must always be inferred; it is an abstraction. What the person is doing psychologically is never available to direct observation" (The Interbehaviorist, 1994, p. 8, emphasis added). Does this support my notion that you are treating constructs as the subject matter of psychology (or is this more a reference to issues related to subjectivity and Q)? Perhaps I can put this differently: Ed [Morris] distinguishes between what psychologists study and what their subject matter is. He argues that for behavior analysis, behavior is both what is studied and the subject matter. Would you, Dennis, disagree? (i.e., psychologists study graphs, charts, etc. Their subject matter is interbehavior.) So, have I completely misunderstood these issues?

Noel: I think you have handled them very well. The distinction between "what psychologists study and what their subject matter is" is another reference

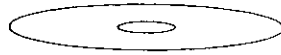
to that all too common confusion between constructs and events. Most psychologists claim to be studying sensations, information processing, consciousness, and the like when what they are actually studying and what they can only study is what organisms do (or do in relationship to other things). As Kantor sometimes put it, what psychologists say they are studying and what they are actually studying often do not correspond.

I would say that "psychologists study the events of interbehaving but in order to do so they often do it indirectly through such constructs as numbers, graphs, Q-sorts, etc. after making the direct observations. As long as the referent is closely tied to the event, that poses no problem and in fact is a necessary part of the scientific enterprise. What has led psychology and other behavior sciences so astray is what Kantor sometimes called "autistic construct" and what I like to call imposed constructs as opposed to derived constructs. This gets to what the Reno School apparently means by "truth criteria". I would argue that those derived constructs that are most useful (or are most legitimate or scientific in Dennis's words or have the highest truth value in Reno terms) are those that refer to relationships. It seems to me, that is what interbehaviorism is all about. I think Q handles this very well. While t's and F's are also about relationships they are often closely tied to organocentrism and mechanistic assumptions. Individuality is an "error term" to F. Subjectivity hardly exists in conventional statistics. In another message Debbie stated that "stating a relation is an event is neither defensible or indefensible. From a one-world event perspective there are no relations because there are no two things to relate (automatically a dualism)." This is an issue on which Bentley and Kantor tangled

and eventually parted ways. The Dewey and Bentley transactionalism absorbed everything into an amalgam from which, for example, stimulus and object could only be differentiated in abstraction and subsequent to the transaction. They criticized Kantor for having separate end points in the interaction from the outset. Kantor argued that their undifferentiated amalgam was nothing less than Hegelian idealism – more spookology – with which he would have no traffic. For him, the organism, object, setting, and other components of the field are events of nature that are known from observation (unlike Dewey and Bentley, he refused to fuse the knower with the known). The way they are related is studied by further observation. One makes such observations by whatever means are available and expedient and then categorizes those observations in some way that seem to give some systematic understanding of the events. I found Dennis's two citations of Kantor's Psychology & Logic to be very useful. Others might want to check them. It seems that some points are still hanging, and I will leave them for others.

[Editorial Comment: If I can speak for the "Reno School", I would say that the issue of truth is irrelevant to our position. The "confusion" of the knower with the known, which Kantor was careful to avoid, is not so much a confusion as an assertion on our part that the focus of psychological study is function. This, along with an appreciation of the observer's participation in any act of description – any act of knowing – is what has led us to the Reno position. If our position is of some continuing interest, perhaps my students and I can put together a longer, more thoughtful response to these very interesting remarks in the next issue of The Interbehaviorist.]

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