Some Reflections on Kantor's (1970) "An Analysis of the Experimental Analysis of Behavior (TEAB)"¹

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General theories of behavior formulated by individual psychologists virtually disappeared from the scientific scene by the 1960's. Two notable exceptions are those of J. R. Kantor and B. F. Skinner. The general approach of these two investigators has been very different. Kantor developed his views primarily through both broad and deep historical and philosophical scholarship. Skinner, on the other hand, was first and foremost a laboratory worker steeped in the traditions of the classical physiology of Claude Bernard, and, more generally, the empiricistic approaches of classical physics from Galileo to Faraday to Mach. A "library vs. laboratory," or "thinker vs. doer" dichotomy is, of course, highly simplistic; nevertheless, something of that kind lies close to the heart of Kantor's treatment of the experimental analysis of behavior (TEAB) published in the *Journal of the Experimental Analysis of Behavior* in 1970. This article, based upon an address to Division 25 of The American Psychological Association in 1969, has already received some critical attention in the literature (e.g., Morris, Higgins & Bickel, 1982; Morris, 1982; Salzinger, 1973; and Winokur, 1971). What is attempted in the present paper is a somewhat more detailed analysis of Kantor's remarks from the standpoint of his own theory of behavior and in light of the special characteristics of TEAB.

Kantor begins his discussion by asserting his high regard for the TEAB as a scientific movement: "Without doubt it stands out prominently in the great scientific revolution that has been devel-

oping in psychology since the early decades of the century . . . I admire TEAB because its policy is to turn away completely from every form of animistic entity or process whether called mind, 'consciousness,' 'drive,' 'sensation,' 'emotion,' or the like" (p. 101).

His admiration is not unrestrained, however, and like Erda, gives a dark warning: "Granting that TEAB is well guarded against both overt and covert animistic influences, there still harks the danger of a constrained scientific horizon limiting observation and analysis to non-human and reflex-derived behaviors" (p. 101). This is the principal recurrent theme in his essay.

Kantor's article is organized into three main sections which he heads: "TEAB Experimentation," "TEAB Analysis," and "TEAB Behavior." Each of these sections will be discussed in turn with brief commentary.

TEAB EXPERIMENTATION

The focus here is upon method and scope of study. Fundamental to method, Kantor notes, are "adequate and correct hypotheses" (p. 101). He is apparently not referring here to the exercise of the hypothetico-deductive method, but rather to basic assumptions about the subject matter to be studied: "What elevates TEAB to a lofty scientific position is that it operates with the exemplary assumption that psychological events consist of behavior, and nothing else" (p. 101). He apparently eschews any heuristic or other value that might ensue from alternative, i.e., transcendental, postulation, or the experimental methods or data derived therefrom. The experimental work of Fechner and Ebbinghaus and, by extrapolation, all of modern cognitive psychology is described as "scientific fumbling" because of the gap between

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¹ Special thanks to E. K. Morris for kindly providing some very helpful reprints of his and his colleagues' writings relevant to Kantor.

mentalistic postulation and behavioral operations. This is a bold, if not reckless assessment, based upon the general view that "... it is only when operations comport with proper postulations that we can have authentic science. Proper postulations, of course, are those derived from prior investigation of similar events" (p. 102). The principal message here is that for psychology to be a science behavior cannot be considered a *symptom* of any other process—transcendental, neural or otherwise. This is of course a major criticism leveled by radical behaviorists at cognitive psychology and TEAB is seen as "one of the first adequate scientific formulations of experimental psychology" (p. 102).

Kantor then treats the implications of what he considers the two primary intellectual sources of TEAB experimentation, namely Pavlov's work on conditioned reflexes and "... the effects of rewards on the more elaborate performances of various kinds of non-human animals originally inspired by the hypotheses derived from Darwinian evolution" (p. 102). In these two sources are found for Kantor the limitations of TEAB experimentation—a scope "symbolized by persistent preoccupation with conditions of animal learning behavior ... [and] the elaborate analysis of how reward conditions influence the development and performance of sundry kinds of action" (p. 100). The faults derived from these activities are: "(1) the simplification of all behavior, and (2) the inclination toward specialized research ... most of the conditions of behavior are neglected in favor of one kind, that is reinforcement" (p. 102). These violate or potentially violate what Kantor deems "two outstanding values that are especially relevant for psychology: the placing of limits upon investigations and the transformation of original events into something simpler or just different to accommodate them to available apparatus, professional conventions, or other similar circumstances" (pp. 103–104).

It is this section of Kantor's article that has received the greatest critical attention. Winokur (1971), Salzinger (1973),

and, more recently, Morris, Higgins and Bickel (1982), have taken Kantor to task by pointing out the expansion of TEAB into regions of complex behavior presently dominated by cognitive psychology. (However, one is still not able to describe this as "creeping behaviorism.") There seem, contrary to Kantor's implication, no inherent limitations of TEAB method that would prevent the analysis of behaviors such as remembering, perceiving, thinking, and the like. However, the issues here are very complex and as Kantor states explicitly or implicitly, have to do with history, philosophical assumptions, social control in science, and even aesthetics.

Recently, I have commented on how three features of TEAB have dictated its relative nontreatment of complex performances: radical behaviorist philosophy, the methods and results embodied by TEAB, and B. F. Skinner himself (Marr, in press). Briefly, radical behaviorist philosophy eschews non-behavioral mediating processes, i.e., those "events" which are the *sine qua non* of a cognitive account. Performances such as remembering, seem to have much hidden in them and an account of the odyssey of information flow from input to output, while comforting and exciting to a cognitive psychologist is for the radical behaviorist simply bad epic poetry. Such verbal behavior has not in general reinforced a functional analysis of remembering, which would be limited to relating conditions at "input" to characteristics and conditions of "output"—mnemonic causality, as Russell puts it.

The methods of TEAB have tended to emphasize steady-state conditions and attendant "reversible" conditions. Perhaps, as Kantor points out, this is the heritage of Pavlov, but the fundamental source is the methodology of classical physiology developed by Claude Bernard (1865/1957), a teacher of Pavlov. The single-subject ABA design is the standard method in traditional physiology and pharmacology. This methodology seems ill-suited to the analysis of transient and irreversible phenomena, particularly

when the effects of present conditions may not be detected for some time and then only in particular contexts. These are, of course, the kinds of events Kantor was urging TEAB to explore and it is unlikely that the traditional methodology is really suited to the task of examining "all psychological behavior" (p. 103). TEAB tends to place inappropriate emphasis on its traditional methods at the expense of aggressive analysis of complex phenomena. Applied behavior analysis has taught us that methods have to be suited to the subject at hand. As Kantor emphasized, the significant aspect of our science is found not in method, but in our conceptualizations of behavior.

The growing assault by behavior analysts on topics that hitherto lay in the domain of cognitive psychology is, in my view, a product of two processes. First, cognitive psychologists, whatever their theoretical stance, have not been intimidated by complexity and have, in fact, provided the basic functional relationships that are likely to guide the tardy application of TEAB to the same phenomena. Kantor might have called all the work of the cognitivist "scientific fumbling," but nevertheless, the influence of this activity has been a principal attractant not only for potential students of TEAB, but for large numbers of established workers in the field. On a more pragmatic level, cognitive research is where the jobs and the financial support are—the principal reinforcers controlling choice of academic careers.

A second source of our increasing attention to "cognitive processes" is a growing disillusionment with or waning interest in certain traditional areas of research within TEAB, particularly schedules of reinforcement. A cursory look at TEAB over the last decade or so since Kantor's article reveals this. One of the most powerful early attractions of TEAB was the experimental demonstration of behavioral control via schedules of reinforcement. These arrangements epitomized Skinner's assertion "control your conditions and you will see order" (1972, p. 104). The patterns of responding engendered by schedules seemed remark-

ably free of species and reinforcer variables. The patterns themselves as traced out on a cumulative record are objects of beauty—and of continuing mystery. Beauty and mystery make an irresistible combination and Kantor, like a Biblical prophet, warned of our being seduced by the "elaborate analysis of how reward conditions influence the development and performance of sundry kinds of actions" (p. 102). We are still awaiting anything like a general account of schedule performance. The complexities in performance are totally belied by the ease with which we describe the contingencies that produce them. Thus, if one is to deal with complexities, why not pursue behaviors that *everyone* is interested in—the kind Kantor encouraged us to pursue.

Analysis of particular features of schedule performance has given way to the study of complex stimulus control, quantitative descriptions of choice, theoretical and experimental integration of respondent and operant contingencies, and the experimental analysis of verbal and social behaviors. As it has turned out, Kantor need not have worried; the dynamics of scientific practice are such that either a field languishes in provincial interests, or it expands in its attempts to deal with problems of more general appeal. Moreover, what he had to say probably could not have influenced the practices of TEAB one way or the other.

Kantor's criticism of TEAB in its "simplification of all behavior" (p. 102) has been echoed many times by critics of radical behaviorism and TEAB. It is a litany in Chomsky's (1959) assessment of *Verbal Behavior* (1957). Yet the issue of simplification is not itself simple. The reduction of possible influencing and irrelevant variables is the essence of experimental control and the origin of scientific enlightenment. Galileo exemplified the process. For example, he substituted the inclined plane for observation of free fall to study the motion of bodies under gravity, because it provided for more careful measurement. He was able to show a simple mathematical relationship to free fall and, in the process, neglected such factors as friction and air

resistance. He controlled his conditions and saw order. This order in due time became part of a larger order of Newtonian mechanics, which, in turn, became part of a more general treatment known as Hamiltonian dynamics. This all took about 200 years, and no one held his breath. No one warned Galileo, Newton, Euler or Lagrange that they had better be dealing with more complex problems, though such problems were (and are) around to be solved. So-called classical mechanics is still alive and growing. The point to be emphasized here is that psychologists are in the unenviable position of studying phenomena that not only the lay public (including those who directly or indirectly provide financial support) is in some sense intimately familiar with, but demands to know more about—and *now*. One of the great appeals of cognitive psychology is that not only has it confronted questions like “What is memory?, What is language?, How do we solve problems?” etc., but has provided the intelligent layman and specialist alike with accounts—simplistic and metaphorical and misguided and even illogical one might say, but accounts none the less—the sort of “hush-puppy” explanations of the kind Kantor was most contemptuous of. The problem is that dealing with complex, i.e., multivariate and interactive phenomena, in the absence of a foundational science often invites those “transcendental” accounts Kantor abhorred.

Skinner attempted to develop an experimental science of behavior with contingencies of reinforcement as the fundamental explanatory concept. It is only reasonable that early research should be devoted to the study of “simple” contingencies in the hopes of deriving general principles to apply to more complex cases. In the meantime, Skinner himself saw the relevance of this preliminary work to complex behavior—thus works like *Science and Human Behavior* (1953), *Verbal Behavior* (1957), and more recently, *About Behaviorism* (1974) are largely *interpretations* of laboratory data derived from relatively simple conditions. Some of the more fruitful concepts were orig-

inally not founded on real experimental data at all—contingency-controlled vs. rule-governed behavior, for example. Skinner has been so clever and creative at this kind of extrapolation and generalization, that many in TEAB may have been implicitly assuming that proper analysis of certain complex behaviors had already been done—the “nothing but” syndrome, a condition not likely to endear one to colleagues actually investigating the phenomena at hand whatever their theoretical persuasion.

All this is not to say that complex performances should have been ignored until basic laboratory work with relatively simple conditions was complete. That has not been the case with other sciences. Chemistry had determined the atomic structure and reaction properties of numerous substances long before quantum mechanics could provide a general account of such phenomena. Selective breeding of domestic and farm animals had been conducted for millenia before genetic mechanisms were elucidated. Thus both basic and applied scientific practice proceed at various levels of knowledge and understanding—a condition embodied by the old expression “I don’t understand everything I know.”

TEAB ANALYSIS

Kantor is concerned with two levels of analysis, what he terms (1) General Postulation Analysis and (2) Specific Operational Analysis. The first refers to the general assumptions of a science in approaching and organizing the description of the events of interest. As noted earlier, TEAB is, in Kantor’s view, on solid ground here. Specific Operational Analysis “pertains to the local, technological and manipulative aspects of scientific work” (p. 104). Kantor emphasizes that in order for a science to function effectively the two forms of analysis must “fit together in a harmonious system” (Kantor, 1969, p. 39). Again Kantor sees great limitations in TEAB in the second category of analysis. “TEAB analysis appears to be primarily a specialized isolation of the variables or factors involved in animal conditioning and learning. Accord-

ingly, the outstanding factors referred to in the analysis are performance and non-performance, that is extinction. So far as performance is concerned, the analyses turn about such variables as frequency, ratio, latency and rate. *There is thus the suggestion of a mechanistic type of operation, or at best something analogous to the action of a purely physiological organism*" (p. 104, italics mine).

There is something rather quaint in this description and while much was left out, the last sentence manifests the real complaint and indeed, it is here that TEAB and Kantor begin to part company in a fundamental way. While limitations of topic and method in TEAB may have been overcome in the last 15 years, the essentially mechanistic perspective of behavioral analysis has in fact been enlarged. Kantor's concerns about mechanistic approaches to behavior are multifaceted, ranging from philosophical problems of materialism to rejection of mechanical or computer models of organisms to issues of cause and effect (Kantor, 1969). I will ignore the first of these and comment on cause and effect presently. For now I will focus briefly on the problem of mechanistic models. By and large TEAB has indeed rejected mechanical or computer *models* of organisms. It has not rejected behavior as, in part, a manifestation of a biological system. Recently I commented: "No one committed to the experimental analysis of behavior should be dismayed by the methodological bond with physiology. It is but one expression of our fundamental conviction that behavior is a biological property of organisms. Our biological perspective has received too little emphasis. Cognitive psychology has attracted many researchers away from the consideration of genetic, adaptive, historical and contextual analyses into the domain of the machine, to focus on *conceptual* internal processes that are more appropriate to a computing device than to a biological system" (Marr, 1984). Our biological perspective has broadened over the last decade and a half to embrace species-specific behavior, behavioral genetics, ecological processes, "behavioral

regulation," "set points," etc. In addition, quantitative models of behavior, which have come to represent a substantial and rapidly growing area within TEAB are fundamentally based upon a mechanistic, dynamic perspective of behavior. It is important to emphasize that these models do not depend upon espousing some hypothetical internal mechanism. Like classical dynamics, the focus is upon predicting or controlling characteristics of performance given certain initial conditions and the actions of external "forces," e.g., contingencies of reinforcement. This all remains in the context of a functional account. Mechanism in the sense of the development of quantitative relations describing a dynamic system is on the rise (see, e.g., McDowell and Kessel, 1979; Nevin, 1979).

Kantor further characterizes TEAB limitations in this way: "... conditions of learning depend a great deal upon the reinforcement schedules arranged by the investigator and in consequence the actions of the manipulator assume a large place in psychological data. The behavior of the manipulator becomes part of the learning event. To say the least, the total psychological situation becomes highly selective and specialized . . . TEAB analysis . . . still reflects clearly that control, in sciences, or at least in psychology, consists of partially substituting contrived situations for *original events*. Accordingly the question arises whether despite the great ingenuity and capable resources of individual TEAB workers in dealing with particular problems, TEAB analyses carry far enough to reveal essential factors in all psychological events" (p. 104, italics mine). Here we see in starkest contrast the "library vs. laboratory" dichotomy referred to earlier. *An experimental analysis of any phenomenon necessarily consists of "substituting contrived situations for original events."* Controlled manipulation of operationally defined variables is the essential definition of experimentation. In situ observation of "original events" provides the questions to be asked and suggestions of the relevant variables, the proper manipulation of

which might, in turn, provide answers to the questions posed. Limitations arise not from experimentation per se, but in the restrictions of observations, of the questions, and of the attendant variables. Since experimenting is a human activity, the experimenter necessarily plays all the roles of observer, question poser, variable manipulator, experimental designer, and interpreter of the outcomes. This is as true in physics as it is in psychology. Indeed, scientific behavior in all its guises is "highly selective and specialized" activity. Moreover, there are no a priori rules about such conduct, not even about what constitutes an "original event." Kantor's (1969) reasonable concern about the distinction between events and their description conceals the problem of distinguishing an event from a nonevent or one event from another. To describe is to select.

TEAB BEHAVIOR

In this final section Kantor discusses five alternative approaches to the study of behavior. In this discussion he is contrasting his interbehavioral perspective with his view of TEAB. An excellent assessment of the following contrasts is found in Morris (1982). My commentary therefore will be brief and highly selective.

The first issue discussed by Kantor is "Organismic Responses" vs. "Behavioral Fields." The principal dimension of contrast is the relative emphasis on responses or response characteristics as opposed to contextual factors. Thus "Organismic activities are only phases of larger adjustmental events. When analyzing reflexes, account must be taken of what is done by the stimulus object on connection with organismic acts, and still further of many setting factors, that is, enabling and impeding conditions" (p. 105).

To the extent to which Kantor emphasizes TEAB treatment of responses as *independent entities*, he commits an error (with the possible exception of primary "emitted" behavior). TEAB has not, in-

deed, could not treat responses independently from stimulus conditions any more than force and acceleration be independent entities in physics (see, e.g., Schick, 1971). Kantor was, however, on the mark when it came to the role of "setting factors," what I prefer to call initial and boundary conditions. Only relatively recently has TEAB given proper emphasis to these factors (e.g., Michael, 1982). It is not simply that TEAB took them to be unimportant, but merely outside its concern, except insofar as levels of those kinds of variables specified control conditions. But, it is only by understanding and specifying initial conditions as well as the prevailing constraints (i.e., boundary conditions) that regularities from which laws may be derived can be effectively discerned. For example, the role of contextual and historical variables in the analysis of verbal behavior and remembering is clearly fundamental.

Kantor comments further: "Of extreme importance for the appreciation of behavior fields is their uniqueness and individuality. There is no fixed or universal type. Implied in the field construct is the principle that each class of behavior events must be analyzed according to its intrinsic factors" (p. 105). As Morris (1982) has pointed out, it is possible to see relationships between Kantor's behavior field and Skinner's response class. Unconstrained, however, these constructs suffer from unwieldiness. Morris puts it this way ". . . it is difficult to sort out (a) the almost infinite number of content-free response functions or classes that may be created and examined through laboratory investigation, as well as (b) the almost infinite number of substantive content-related response functions or classes that can exist in the everyday world" (p. 201). Complexity and uniqueness is inherent in all phenomena of scientific interest, but Science cannot deal effectively with unique events and must search for and be based upon general principles. Kantor's view may properly characterize the reality of the behavioral world, but it is difficult to see how an *experimental analysis* can be conducted

in the midst of such chaos. TEAB has, as Morris stresses, the advantage of defining functional classes of responses, e.g., respondents vs. operants, thus providing some organization of complex behavioral functions.

The second topic of contrast is "Stimulus Objects" vs. "Interbehavioral Functions." "I regret to say that TEAB does not probe deeply enough in determining the nature of stimuli. It stops short at the surface notion of a stimulus as simply an object or condition that determines a response" (p. 106). Kantor's treatment of stimulus objects and functions parallel his treatment of behavioral fields and essentially the same points could be made about the infinite variations possible on a behavioral theme, but in a scientific composition, it is the basic theme that commands the primary attention.

TEAB's shallow dealing with stimuli is said by Kantor to be based upon three weak "strata": (1) that stimuli are taken to be prior independent entities, (2) the reflex model (elicited responses), and (3) the laboratory tactics of TEAB. The first of these deserves special comment, because critical treatment of causality is a major aspect of Kantorian theory. Kantor is critical of such expressions as $R = f(S)$ (i.e., some feature of behavior can be expressed as a function of some property of a stimulus) not only because of the lack of other pertinent variables in the expression, but also because it gives a false notion of causality, and ignores interactions and possible inverse relations. In addition, he comments: "In the interest of effective behavioral analysis I also question the familiar convention of dependent and independent variables. There seems to be here a confusion of events and constructs. The dependence and independence are not inherent in the events, but only in the manipulative procedures of investigators" (p. 106).

Causality as such has not been an issue in TEAB, because of the adoption of a Machian functional account, which eschews any separate doctrine of, or place for, causality (Skinner, 1953). It is certainly possible, as Kantor suggests, to be

led astray by uncritical treatment of functional relations as is the case with other varieties of scientific verbal behavior (Morris et al., 1982).

Independent and dependent variables are conventions specifying what is manipulated and what is measured when the manipulation occurs. Nevin (in press) has pointed out that some quantitative relations in behavioral analysis may be usefully expressed with exchanges of dependent and independent variables. For example, response rate may be expressed as a function of reinforcement frequency, or reinforcement frequency may be expressed as a function of response rate. This rearrangement emphasizes the functional as opposed to a primary causal role of variables, in accordance with Kantor's views.

Kantor's other contrasts are (3) "Situational Media" vs. "Causal Powers" where he proposes that TEAB study perceptual phenomena more extensively; (4) "Setting Components" vs. "Reinforcements" where he again emphasizes the narrow approach of TEAB; and, finally, (5) "Organismic Determiners" vs. "Organic Components" where his organic factors are "... participants in the response phase of behavioral fields that give shape and support to the behavior patterns" as opposed to "... determiners of psychological behavior" (p. 107). This last issue has remained of little direct interest to TEAB.

By way of summary, Kantor's concerns with the directions of research and the relatively narrow perspective of TEAB in 1970 were reasonable. The breadth of application of method and theory in TEAB has certainly increased in the last decade and a half as it must if the field is to survive.

More problematical for TEAB is Kantor's conceptual framework. His world view reflects a "form of life" not easily integrated into experimental method. Kantor is foremost a realist, but his real world is seemingly beyond the powers of a coherent, analytical, principled account. Perhaps via such modern methods as quasi-experimental design, and con-

firmary analysis (e.g., James, Mulaik, & Brett, 1982) details of interactional processes of the kind Kantor espouses might be obtained. But such methods are remote from both TEAB and the unique aspects of behavioral events emphasized by Kantor. Despite the necessary vagueness and complexity of Kantor's system, he keeps TEAB mindful of the multivariate and dynamic character of behavioral processes. As TEAB has grown in philosophical concern and sophistication, Kantor's star has risen. Perhaps its light will help prevent TEAB from straying into the cul de sac of "scientific fumbling."

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