Complex Behavior: A Systematic Reformulation of Radical Behavioral Analyses

Linda J. Parrott
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COMPLEX BEHAVIOR: A SYSTEMATIC REFORMULATION

OF RADICAL BEHAVIORAL ANALYSES

by

Linda J. Parrott

A Dissertation
Submitted to the
Faculty of The Graduate College
in partial fulfillment of the
requirements for the
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Department of Psychology

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Mentalistic psychology thrives in the absence of a satisfactory, naturalistic account of complex human behavior. Inadequacies in the radical behavioral position with respect to complex events are of two sorts. First, the complexity of such events is undermined, either by reducing complex phenomena to simpler ones and then proceeding to analyze the latter; or, by providing only a very superficial treatment of events at their own level of complexity. The former procedure applies to Skinner's treatment of verbal behavior; the latter to his distinction between rule governed and contingency shaped behavior. Second, the analyses made of some classes of complex phenomena indicate a commerce with metaphysical philosophy, and they thereby fail to meet the criterion of naturalism. The dichotomy of public and private events illustrates this inadequacy. For these reasons the radical behavioral account of complex human behavior does not constitute a viable alternative to the interpretation advanced by the mentalist.

These problems in the radical behavioral position may be traced to a single source: The assumptions upon which the position is founded have not been articulated and formalized as propositions. Consequently, a postulational system has not evolved. As a result, hypotheses and theories may be formulated without restraint, and findings and investigations lack systematic organization. A coherent
and thoroughly naturalistic account of complex phenomena cannot be expected to arise under such conditions. What is needed is an acknowledgement of the logical character of science, and with that, the construction of a satisfactory postulational system from which legitimate conceptual and methodological extensions of the science may emanate.
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CHAPTER I

THWARTING THE COGNITIVE REVIVAL IN PSYCHOLOGY

The place of Behaviorism in Psychology is being usurped by what has been called the "New Cognitivism," a revival that must be viewed with alarm by psychologists whose aim it has been to earn for their discipline the status of a natural science (Observer, 1971, 1979; Kantor, 1979). After all, the "New Cognitivism" is essentially, the same old mentalism that has always obstructed the scientific development of psychology. The only novelty in the current formulation is the terminology with which mythical processes held to underlie complex psychological events are described—-a circumstance owing largely to recent developments in computer technology.

While many circumstances may have contributed to a cognitive revival at the present time, among them is the absence of a satisfactory radical behavioral account of complex human phenomena such as imaging, thinking, remembering, and the like. The radical behaviorists' failure to provide such an account can not be attributed to an unwillingness to consider phenomena of this type. At least Skinner cannot be accused of such neglect. Indeed, it is his willingness to address himself to complex phenomena that distinguishes radical from methodological behaviorism. Nonetheless, Skinner's account proves to be unsatisfactory from the standpoint of a thoroughgoing natural science of psychology.

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Inadequacies in the radical behavioral position with respect to complex events are of two sorts. First, the complexity of such events is undermined, either by reducing complex phenomena to simpler ones and then proceeding to analyze the latter; or, by providing only a very superficial treatment of events at their own level of complexity. The former procedure applies to Skinner's treatment of verbal behavior; the latter to his distinction between rule governed and contingency shaped behavior, both of which are discussed in detail below. Second, the analyses made of some classes of complex phenomena indicate a commerce with metaphysical philosophy, and they thereby fail to meet the criterion of naturalism. The dichotomy of public and private events illustrates this inadequacy, as discussed in Chapter 4.

The lack of consistency and rigor in the behavioral account of complex phenomena may be traced to a single source: The assumptions upon which the account depends have not been articulated and formalized as propositions. Consequently a postulation system has not evolved. As a result, hypothesis and theories may be formulated without restraint, and findings and investigations lack systematic organization.

Moreover, a failure to develop a new set of postulates may be taken as evidence for the adoption of a more traditional set because scientists always operate on the basis of assumptions, whether or not they are aware of this fact or of the specific
assumptions adopted (Kantor, 1953, p. 28). Paradoxically, it has always been the argument of radical behaviorists that progress will be registered in psychology only when a natural science approach to psychological events is taken and this, in turn, will be accomplished only when traditional assumptions about such events are abandoned (Skinner, 1953, pp. 8-10).

In summary, the radical behavioral account of complex human activity is either superficial or shows corruption from traditional metaphysical sources and, for these reasons, does not constitute a viable alternative to the interpretation advanced by the "new cognitivist." What is needed is an acknowledgement of the logical character of science, and a commitment on the part of radical behaviorists to construct a satisfactory postulational system, for it is only with the development of such a system that a coherent and naturalistic account of complex human behavior may be formulated, and a cognitive revival in psychology forestalled.

The purpose of this paper, then, is to evaluate the adequacy of the radical behavioral account of several classes of complex human behavior, with the intention of substituting better hypotheses for poorer ones wherever possible. The classes of complex phenomena to be addressed are: "private" events, rule governance, and verbal behavior. The adequacy of a particular account is assessed in accordance with one or more of several criteria. Included among them are, first, the origin, source, and
operation of the assumptions underlying the account. Assumptions not derived, however remotely, from actual confrontations with things and events are regarded as disserviceable to the advancement of science. Hence interpretations of complex behavior based on such assumptions are rejected in favor of analyses based on assumptions derived from confrontational sources. Second, the consistency of assumptions across classes of complex events is assessed. Inconsistencies and contradictions are regarded as sources of analytic error calling for reevaluation of assumptions as well as a reformulation of accounts based on them. Third, the extent to which a given account serves to clarify the nature and operation of even more complex phenomena is examined. The more serviceable an account in this regard, the higher its relative value is taken to be. Underlying these criteria—needless to say—are certain presuppositions concerning the nature and conduct of science. These assumptions may be generally characterized as naturalistic in kind and pragmatic in operation. They are drawn from the writings of J.R. Kantor, primarily; and will be articulated in greater detail as the work proceeds.

In undertaking this study, I have selected B.F. Skinner as a spokesman for the mainstream radical behavioral position in psychology. While many who would identify themselves as radical behaviorists—even Skinnerians—would not find themselves in total agreement with Skinner, all appear to have adopted similar, if
not identical, philosophical presuppositions. Since it is
with respect to these foundations that the radical behavioral
position is contested, I feel that Skinner's views may be regarded
as representative.

The principle aim of the investigations is to improve our
conceptual understanding of complex human behavior. This aim
is taken in recognition of the fact that changes in any aspect
of a scientific system have a definite impact on other aspects
of that system. It is implied, therefore, that changes in our
conceptual understanding of complex behavior will be accompanied
by changes in our investigative operations and procedures with
respect to such behavior.
That behavior science has not reached the postulation stage of systemic development may not be a serious problem. It is possible that what appears to be a problem is only a sign of immaturity. Because the systemic aspects of science are derived primarily from previous investigations and findings, these aspects may not emerge until relatively late in the development of a science. Skinner (1953, p.14) is well aware of this fact. More importantly, he does not appear to be opposed to developments of this sort, arguing that "...experimental psychology is properly and inevitably committed to the construction of a theory of behavior", because "(a) theory is essential to the scientific understanding of behavior as a subject matter, (1947, p. 302, emphasis mine). Nonetheless, he questions the readiness of the science of behavior for even very localized system-building efforts, (1950, p. 99-100). We might assume, then, that while behavior science will enter the postulational stage eventually and without resistance, it is not likely to happen in the near future.

On the other hand, it is possible that the fragments of a system already developed are serving to thwart further system making efforts. If the products of earlier system building work are having this effect, a problem does exist, the seriousness of which depends on whether or not anything can be done about it, regardless of the intention. Assuming that a
problem of this sort does exist, we may begin the process of eliminating it by examining the possibility that the effect is unintentional.

Misunderstandings of Skinner

Skinner's objections to particular types of theory-building (1950, p. 69-71; 1957, p. 12; 1969, Preface P. vii-xii) may have been misinterpreted as an objection to theory building per se, having the unintended effect of delaying systemic development. In keeping with this argument, I have encountered many students of behavior science—particularly those professing an interest in the experimental analysis of behavior—who will defend an anti-theoretical orientation to their work by rather vague appeals to Skinner's authority. Skinner is by no means ambiguous on this issue, however. He explicitly states that he is opposed only to certain types of theories, specifically, those appealing "...to events taking place somewhere else, at some other level of observation, described in different terms, and measured, if at all, in different dimensions" (1950, p. 69; 1969, Preface, p. vii).

Misconceptions seem to have arisen nonetheless, and since they can survive only by neglect, it is important to examine their sources. There are two possibilities to consider. First, erroneous information concerning Skinner's regard for theory
building may be just an elaborated version of precious little information of any sort. A principal source of Skinner's views on this issue is the article, "Are theories of learning necessary?" (Skinner, 1950), which is long and rather tedious to read, particularly when the answer appears to be provided in a relatively brief conclusion. (This is especially true for readers inclined to supply a negative answer from the outset). A hasty reply to this question may create problems, however, because Skinner's conclusion is not strictly, or solely, a conclusion. While he does conclude—on the basis of the experimental material he has presented—that some types of theorizing are unnecessary; he also introduces the notion that there exists another type of theory which is potentially useful for behavior science, though at some future time. This latter type of theory, however, is not of the same sort as that which served, in the beginning of the article, as a standard against which the necessity and utility of theories could be evaluated. Consequently, an original premise of this article—that scientific work always involves a set of underlying assumptions and presuppositions called theories (p. 69)—is lost. What remains, as a consequence of concentrating on only the concluding remarks, is the suggestion that most theory-building is useless and, for what little may be of value, the science of behavior is not yet ready. An antitheoretical stance thereby seems defensible.
Misconceptions concerning Skinner's position may stem from another source, as well. Numerous passages in Skinner's writings, while presumably addressing only objectionable varieties of theory-building, have a certain indiscriminate quality to them which may have contributed to a misinterpretation of his position. The following excerpt from Skinner's Notebooks may serve as an example:

(models) evoke contemplation rather than action. The theoretical physicist wants to represent reality; the laboratory physicist wants to do something about it. One changes a model to produce a different picture; the other manipulates independent variables to change a dependent variable. A model is what something is to be done about; it is not what is to be done. Model is little more than another word for idea—something known by acquaintance.

I look forward to greater recognition of the importance of laboratory scientists. The theorists have been sponging on them for decades and getting most of the credit (1981, p. 173-174, emphasis his).

To characterize the activities of theoretical scientists, or system-builders, as "sponging" on their experimental colleagues certainly does little to promote theoretical work. Moreover, because this passage appears without a context, whereby the types of theory typically and appropriately criticized by Skinner in this manner may be identified, the passage is damaging to theory building efforts of any sort.
Skinner's Misunderstandings

Skinner can not, of course, be faulted for the failure of others to become sufficiently familiar with his position to avoid misinterpreting it. He may, however, be guilty of discouraging system building efforts intentionally. Skinner is opposed to more than just theories of behavior appealing to inaccessible events. He is opposed to hypothetico-deductive methods of theory construction (1938, p. 437; 1950, p. 71; 1969, Preface, p. vii-xii), and while he is not alone among positivistic philosophers or empirical scientists in this regard, the reasons for his opposition are somewhat idiosyncratic. For instance, the most commonly cited objection to deductive logic is that it is not productive of new knowledge (Creighton and Smart, 1932, pp. 26-32; Kantor, 1945, pp. 182-183; Russell, 1960, pp. 82-83). In Creighton and Smart's words:

*Its main purpose...was to furnish a method by means of which the knowledge we already possess may be so arranged as to be absolutely convincing.* (1932, p. 31)

Skinner does not object to the hypothetico-deductive method for this reason. On the contrary, he (1969, Preface, p. ix) believes that under circumstances warranting its use (i.e., when a subject matter is large, very small, or otherwise inaccessible), the hypothetico-deductive method has been productive of brilliant achievements. In the context of behavior science, however, where direct observation is possible, deductive inference is not considered to be necessary or appropriate, (Skinner, 1969, Preface, p. xi). In essence, Skinner pits observation and
induction against hypothesizing and deduction; arguing that the latter are suitable only for the investigation of not directly observable phenomena. As a result, Skinner believes that if hypotheses are constructed in the course of psychological study, we may assume that observable behavior and the equally observable variables of which it is a function are no longer the focus of investigation. Instead, attention has been turned to not directly observable processes of a neural or mental sort, neither of which Skinner finds serviceable to the advancement of an independent, natural science of psychology.

In summary, Skinner objects to hypothetico-deductive methods in psychology because he equates their use with a disserviceable interest in inaccessible events, (1969, Preface p. vii-xii). He objects to hypothetico-deductive methods because they are capable of perpetuating old ways of thinking; not because they are incapable of producing new knowledge, as is more commonly argued.

The difference of opinion on this issue is subtle, but not insignificant. Skinner's objection is formulated with respect to a particular application of deductive logic, and pertains only to this application. In fact, Skinner's objection actually applies to the use made of deduction in the psychological domain, not to deduction per se. Further still, his objection
applies to a particular usage of the deductive method, since it is by no means a requirement of the method that inaccessible events be the subject of hypotheses. That such has been the case in the psychological domain is a fact about the psychological domain, not about deductive logic. In short, Skinner may be credited with the identification of certain examples of improper hypothesis construction, not with the abstraction of a genuine limitation of hypothetico-deductive methods. The latter is expressed in the statement that such methods are instruments of confirmation, not discovery.

Confirmation of hypotheses or assumptions by means of logical operations is not without value, however. Particularly is this the case when the internal consistency of a scientific system is at issue. Under these circumstances, the adequacy of a particular conceptual analysis may be evaluated through the implementation of deductive inferential operations; and should the analysis be found wanting, similar operations may prove useful in the formulation of a new analysis.

An example may help to illustrate this point. Suppose an instance of aggressive behavior is conceptualized as having been caused by an internal state of frustration, and the adequacy of this analysis—from a systematic standpoint—is in question. Under these circumstances, "adequacy" is a matter of consistency among the specific conceptualizations of aggressive behavior.
and more general assumptions underlying the study of behavior of any sort. If one such assumption were that "Psychological Events Occur without any Internal or External Determiners", as is the case in interbehavioral psychological studies (Kantor and Smith, 1975, p. 416), then this conception of aggressive behavior would be regarded as inconsistent, and thereby, inadequate. A reformulation of the account would be accomplished by further scrutiny of basic assumptions of the system, among which are included: "Psychology Studies Interbehavioral Fields," and "Psychological Fields are Multiplex," (Kantor and Smith, 1975, pp. 415-416). Having done so, causal knowledge with respect to a particular instance of aggressive behavior would amount to considerably more than an appeal to an internal state of frustration.

Skinner's misconception of deductive methods—because it results in his abandoning them altogether—prevents him from taking advantage of their regulatory and evaluative functions. Moreover, by the wholesale rejection of deductive methods, system building is left to proceed solely on the basis of inductive operations which Skinner regards as fitting (1938, p. 437; 1947, pp. 301-302; 1950, p. 100), but fails to discuss in sufficient detail to provide for their implementation. With regard to system building by deduction, Skinner (1947) explains that the first two steps of theory building consist of identifying the basic data (p. 305)
and expressing relations among them (p. 307), respectively. The third step, which he regards as "theory making in the best sense" (p. 307), is explained by way of an example from the science of mechanics. In this example, however, "theory making" is not discussed. Instead, the products of this activity are simply said to have "emerged" in one instance, and to have "appeared" in another. Other descriptions (1938, p. 45; 1950, p. 100; 1953, p. 14; 1969, Preface p. vii-xii) are similarly vague. It is as though Skinner believed, as did Francis Bacon (as cited in Russell, 1960, p. 82; Kantor, 1969a, p. 320; and Watson, 1979, p. 5) that a scientific system would automatically emerge from the collection of facts. Under these circumstances, the path of least resistance for the orthodox behaviorist--is needless to say--fact collection, though a more severe condemnation of the "mere collector of facts," than appears in the following passage, is hard to imagine:

Most of the facts entered in our scientific handbooks are virtually hack work. Some were collected in the course of more rewarding scientific pursuits, but the tables were filled out only by the type of man who might otherwise be found collecting stamps or old coins. There is no more pathetic figure in psychology today than the mere collector of facts, who operates, or thinks he operates, with no basis for selecting one fact as against another. In the end he is usually to be found doing something else, or perhaps nothing at all. (Skinner, 1947, p. 301).
Skinner goes on to argue that we must go beyond the collection of facts to the development of a theory; whereupon he hastens to add that not just any theory will do. It must be a theory based upon facts. "Theories", he argues (1947, p. 302), "are statements about organizations of facts." While he does not say how facts are to be organized, one may assume that organization will be achieved on the basis of certain underlying postulates which "most psychologists respect" and which "constitute the beginning of an implicit theory," (p. 305). However, he does not explain the nature of these underlying postulates, or the manner in which they are derived. The end result is confusion: Theory building is both praised and disparaged with no clear indication as to when, how, and to what end valid system making efforts are to proceed.

Furthermore, it is questionable whether or not the final stage of scientific system-building envisaged by Skinner is, in fact, a postulational system. He seems to regard the ability to predict and control events as the hallmark of a mature science (1953, p. 14). By this criterion the science of behavior is devoid of responsibility for further systemic development. Consequently, the future of behavior science promises to be one of more accurate prediction and more precise control. In this regard, Skinner (1957, p. 28) looks forward to the time
when behavior science may "...move on from the study of frequencies
to a consideration of the probability of a single event."

Prediction and control are not indicative of the maturity
of a science, however. They are characteristic outcomes of
less advanced systemic arrangements concerned with clarifying
and formalizing investigative procedures. The science of
behavior, therefore, is destined for the stage of operations,
not postulates. The latter stage is achieved with the articula-
tion of postulates derived from confrontation with events. With
this addition is made possible a harmony of practices and
postulates; and, further, of postulates and protopostulates,
(Kantor, 1953, pp. 72-75). Without harmonious relations of
these sorts there is no possibility of a thorough going natural
science of psychology; nor is there any potential for satis-
factory interdisciplinary cooperation of psychology with other
natural sciences.

Conclusion

There is reason to doubt that the science of behavior will
enter the postulational stage as a matter of course. Quite the
opposite is true: the systemic development of the science is
obstructed by a lack of clarity and vision, by leaders as well
as followers. To clear the way for behavior science, two things
must be done. First, the nature of postulation as scientific
activity, and the role of postulates in scientific enterprises,
must be elucidated further. Second, the problems encountered in
the absence of a postulational system and how they may be
corrected by the institution of such a system must be examined.
The first objective will be addressed in the upcoming chapter.
The second objective will be addressed in the chapters to follow.
CHAPTER III

SCIENCE AND PHILOSOPHY IN PSYCHOLOGICAL PERSPECTIVE

The more extensive is our prior experience with things and events presently encountered, the better we are oriented in the current circumstance and the more likely it is that we will behave effectively in it. Nonetheless, all events are, to some degree, unique; hence our knowledge of the world is always incomplete. As a result, we must operate with some uncertainty as to the consequences of our actions, and of the actions of other organisms, things, and events. Incomplete knowledge is of course better than no knowledge at all. It is the evidence upon which we form assumptions and make predictions about our future encounters with the world. These, in turn, serve to guide us in the performance of successful action. In short, we are prepared to make assumptions about the world and to operate on the basis of them because knowledge of the world—even tentative knowledge—is crucial to effective interactions with the world.

As a critical study of fundamental assumptions and the grounds for them, philosophy is an inevitable foundation for all intellectual enterprises, not the least of which are the science. Indeed, an understanding of assumptions is essential to science. As Kantor (1981a, p. 109) has pointed out:
Assumptions initiate and shape hypotheses and theories and make possible the systematic organization of investigations and findings. Assumptions known, understood, and formalized in propositions constitute the postulates and postulate systems of all the sciences...

There is no abstract philosophy, however, only particular kinds and some kinds are more useful to the sciences than others. Specifically, a philosophy is suitable for the sciences when the assumptions upon which it is based are derived from interactions of philosophers with actual things and events. A philosophy is not suitable for science when its axioms are derived from cultural tradition, (Kantor, 1981b).

To substantiate these claims and to further clarify the relations of philosophy to science, we must examine both science and philosophy in psychological perspective. Having done so we will be better prepared to appreciate the role of postulates in more specific analyses of complex psychological phenomena.

The Nature of Science

To understand the nature of science we must first distinguish science from nonscience, a task complicated by the absence of any obvious differences between these two enterprises. Scientific activity is simply a more meticulous, methodological, and expertly performed version of our ordinary contacts with
things and events, (Kantor, 1963, p. 17). Still, however, the sciences and nonsciences are organized around different purposes or goals and a distinction may be achieved by way of this comparison. According to Kantor and Smith (1975, p. 490), the principle aim of science is to make known what is still unknown, and in so doing accelerate the progress of civilization. Similarly, Skinner argues that the goal of science is the "destruction of mystery" (1971, p. 54), having as its outcome the survival of the individual, as well as the larger culture. The sciences, then, are serious enterprises, contributing to the performance of successful practical action, (Skinner, 1957, p. 418; Kantor, 1953, p. 6). The nonsciences, on the other hand, serve purposes of entertainment, for the most part, (Skinner, 1974, p. 187). The fine arts, for example, are primarily concerned with the arousal of feelings and other ineffectual reactions.

The goals of an enterprise are determined after the fact, however. They are little more than elaborate descriptions of the previously accumulated products of investigative and constructional practices. Hence, if the sciences and nonsciences may be differentiated on the basis of their goals, a distinction may also be made with respect to their products (i.e., findings and hypotheses.) The products of scientific work, unlike those

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characteristics of the nonsciences, involve a factor of novelty or originality. As Kantor (1953, p. 6) describes it:

If the scientific enterprise is successful, something new emerges, something, moreover, frequently incompatible with previous conditions.

Skinner (1971) contrasts the basic sciences with the humanities to make a similar point, namely, that the sciences imply discovery and development, while other endeavors do not. In Skinner's words:

Aristotle could not have understood a page of modern physics or biology, but Socrates and his friends would have little trouble in following most current discussions of human affairs. (1971, p.3)

Products are just phases of process, however. Without the process of science there would be no products of the sort just mentioned, nor would goals of the kind articulated by scientists be possible to accomplish. It is not possible to accomplish scientific objectives in nonscientific ways. Hence, to fully appreciate the difference between the sciences and the nonsciences, a final comparison of these enterprises on the issue of process or procedure must be undertaken.

Scientific activities, as previously mentioned, are constituted of expertly performed contacts with things and events. Such contacts are usually regarded as exemplifying one or the other of the following procedures: observation,
manipulation, transformation, or construction. Despite the
diversity of procedure implied by these categories, manipulation
and transformation are simply means by which trends in observed
events may be intensified to facilitate further observation; and
constructional processes are really only remote or indirect
forms of observation. Science, therefore, is largely an
exercise in meticulous and highly stylized observation.

Observation is not peculiar to the sciences, however. It
is also a significant feature of most nonscientific endeavors,
particularly in their early phases. For example, literary fiction
usually begins with observations of actual events, as does even
the most abstract of modern art.

Nonetheless the process of science does differ from that
of other enterprises in one important respect. The sciences
are postulational, (Kantor, 1963, p. 15.) That is, scientific
work always involves a set of underlying presuppositions.
This aspect of scientific work is an inevitable outcome of the
originality of scientific products. Original products accumulate.
When they become too numerous to be useful in the absence of
some form of organization, generalizations about particular
collections of observations, known as theories and laws, begin
to multiply and become formal attributes of the enterprise.
The articulation of theories and laws draw them into question,
however, and as concern over their adequacy and compatibility arises, postulational systems are instituted (Kantor, 1953, p. 59.)

In summary, the sciences and nonsciences may be differentiated primarily on the basis of what each becomes. The nonsciences, lacking as they are in discovery and progress, do not eventuate in postulational systems.

The Nature of Philosophy

What philosophy is varies according to the philosophy one adopts. Basic to all, however, is the construction of assumptions, beliefs, and other intellectual attitudes (Kantor, 1981a, p. 3). Technical philosophies, in turn, constitute systems of formal assumptions or propositions which have, for various reasons, become codified and institutionalized. The serviceability of a particular technical philosophy—such as that of Plato, Descartes, Hume, Russell, Kantor, etc.—to the development of a scientific psychology, depends on the assumptions underlying the process of system building adopted by that particular philosophical enterprise. For a philosophy to be useful to the sciences it must be "rooted in scientific soil", as Kantor (1969b) has described it. What this means, essentially, is that a scientific philosophy must itself be regarded as a science—a science of systems (Kantor, 1953, p. 26). So conceived, philosophy may
viewed as having a distinctive, natural subject matter. Philo-
sophizing may be viewed as the activity of individual philosophers
in contact with concrete, localized, things and events, resulting
in specific products called postulates and postulate systems.
Moreover, because these activities occur on the part of individ-
uals under particular cultural auspices, we may assume that the
resulting postulates are influenced not only by the type of
materials worked with but also by the personal histories of
the individual workers and the cultural conditions under which
the work is done. The fundamental implication of this
characterization of the philosophical enterprise is that there
is no place in scientific philosophy for ultimate reality,
absolutes, eternal truths, universals, or infinites of any
description. Philosophy, as the science of systems, is subject
to the same restrictions of specificity and privileges of
tentative assertion as is afforded any of the special sciences--
and nothing more.

We are now prepared to discuss the role of a postulational
system in the continuous development of a scientific psychology.

Postulational Systems

Definition

A postulational system constitutes a collection of formal
assumptions concerning such issues as the kinds of events worthy
of study and how knowledge of them might be obtained. The assumptions or postulates making up such a system are organized, hierarchically, in accordance with the generality or scope of the issues addressed. This organization makes for levels or classes of assumptions called protopostulates, metapostulates, postulates, and specific hypotheses. Assumptions of the protopostulate class concern issues pertinent to science and the relation of science to nonscience. Metapostulates concern issues pertinent to a particular science and its relation to other sciences. Postulates and specific hypotheses pertain only to the subject matter of a given science; the latter constituting the numerous and changing assumptions made during the investigative phases of scientific work (Kantor, 1958, pp. 63-82).

Role

From the above description of a postulational system, it is obvious that specific hypotheses are necessarily influenced by more general postulates which, in turn, are unavoidably influenced by even more general metapostulates, and so on. More important than the control exerted by one level of assumptions upon another is the influence of assumptions per se on all other aspects of the scientific enterprise, including event selection, investigating operations, and theory construction. An example may be helpful here. Complex human behavior is widely held to have a biological basis, despite the fact that there are no
data to support this doctrine (Kantor, 1947, 1981). Consequently, considerable scientific resources and energies are spent in the fruitless search for the engram. In this case, an attempt is made to solve a psychological problem through a study of biological events—a strategy legitimized by reductionistic assumptions concerning the relation of psychology to biology.

In addition to these influences on scientific enterprises is the effect of prevailing civilizational circumstances. A culture may or may not be favorable to scientific progress. As a result, the nature of a particular science, as well as its potential for growth and development at a particular time, is largely a matter of its philosophical and cultural foundations.

Scientific Progress

The fact that postulational systems influence specific findings and investigations is not intended to suggest that the latter do not also influence postulational systems. After all, the assumptions making up such systems have their origins in specific observations of things and events. The control is reciprocal, and because this is so, there are two principal ways in which the sciences can progress. First, progress may be made through the discovery of previously unknown things and events. In this manner is the investigative domain of the science is enlarged. Secondly, progress may be registered in science when already known events are evaluated on the basis of improved criteria (Kantor, 1958, p. 3).
Which of these approaches is adopted by a particular science depends, to a large extent, on the nature of its subject matter. The critical characteristic is scope. When a particular subject matter has sufficient scope to allow for the discovery of new types of phenomena, the former approach may be more profitably adopted. Microbiology is a good example of such a science, as is any science whose discoveries have multiplied in conjunction with technological developments. The subject matter of psychological science, namely, interactions of organisms with stimulus objects, is relatively limited in scope. There are, of course, innumerable interactions of organisms with stimulus objects to consider, but all are instances of the same general type of phenomenon. Consequently, if psychology is to show significant progress it is likely to be accomplished through a reevaluation of the assumptions underlying the description and interpretation of its subject matter. Moreover, the fact that psychological thinking has been dominated by theological and metaphysical assumptions for centuries (Lundin, 1979; Kantor, 1963), is a good indication that ample room for improvement exists.

The task of reevaluating the assumptive bases of the sciences is the occupation of technical philosophers. Their task has three principal components, categorized by Kantor (1981, p. 116) as monitorial, coordinative, and semantic. The first
of these entails a critical examination of the origin and validity of established premises. To examine a premise with respect to its origin is a fairly straightforward procedure. To evaluate the validity of a premise is somewhat more controversial: It involves setting up evaluative criteria which are themselves valid. Consequently the question of what is valid--be it a premise or a criterion--remains unanswered. Kantor (1950, p. 10) insists that valid criteria for an evaluation of premises may be derived from unbiased and objective observations. However, on the basis of what we have already described about the philosophical and cultural influences on the scientific worker, there are no such things as unbiased and objective observations--at least not from a logical standpoint. From a practical perspective we may regard as valid whatever has been helpful to us in dealing effectively with the world. For Kantor (1981a, p. 116) that amounts to the adoption of two rules. Premises are valid if: 1) their constituent constructs are derived from contact with events; and 2) entangling cultural institutions have been suppressed. These same rules have been adopted in the evaluation of premises undertaken in the upcoming chapters.

The second task of a technical philosophy involves coordinating the findings and interpretations of the various sciences so as to eliminate contradictions among them, as well
as to guard against other kinds of problems. What constitutes a problem in this context is determined by the assumptions underlying the technical philosophy. For present purposes, we have assumed, with Kantor (Kantor & Smith, 1975, pp. 412-417; Kantor, 1981a, p. 116), that all sciences are natural sciences in that all study natural phenomena. Hence, no science is any more basic than any other. Therefore, all attempts to reduce one set of events to another, or to interpret the data of one science in accordance with theories developed in another must be thwarted.

Finally, a technical philosophy concerns itself with the way terms are used in the sciences. The task here is to guard against misinterpretations of events through the inappropriate use of terms. Technical terms are established in the course of investigating particular phenomena, after which they may be borrowed by another science to describe similar or related phenomena. A number of problems may result, the most serious of which is that aspects of the original phenomenon become improperly attached to the new set of events. As a result the new set of events are misinterpreted. For example, the term "response" originated in biology. In biological usage the term refers to exclusively organismic performance or movement. A related phenomenon in the psychological domain is the organism's participation in the interaction of organism and environment,
to which the term was applied. In psychological context, a "response" is not exclusively an organismic performance. It is a phase of a larger, unitary, adjustmental event which includes as well the performance of stimulus objects (Kantor, 1970). Because of the use of the term "response" to describe the psychological event its distinctive character is overshadowed with biological interpretations. It is the task of a technical philosophy to discover and repair the damage done by semantic confusions.

Summary

Philosophy, like science, is an investigative enterprise concerned with the same sorts of things and events as are the individual sciences. In fact, it is itself a science but with a somewhat broader scope.

The principal task of a technical philosophy is system building, through which scientific practices of event selection, investigation and theory construction may be harmonized. The articulation of postulational systems also allows for a harmony of specific hypotheses with more general philosophical assumptions and cultural conditions. When a postulational system has been constructed and acknowledged by scientific workers, it may serve to illustrate, regulate, and evaluate scientific enterprises (Kantor & Smith, 1975, pp. 407-412). For these reasons it is critical to the advancement of science for scientists to be aware of the postulates upon which they knowingly or unknowingly operate.
CHAPTER IV

SYSTEMIC FOUNDATIONS FOR THE CONCEPT OF PRIVATE EVENTS

Complex Activities

Before embarking on a critical examination of the behavioral interpretation of various classes of complex activity, we must specify the activities to be included in this category and it is not without some difficulty that we do so. The difficulties arise from the fact that all systems of classification are arbitrarily produced. In other words, there are no fundamental differences between simple and more complex activities. Nonetheless, a relatively standard set of categories has been adopted by psychologists, and it is on the basis of these conventions that we may distinguish the simple from the complex. Simple psychological phenomena are those in which a readily apparent response is coordinated with a readily apparent stimulus. Complex activities, on the other hand, are those in which one or the other of the coordinated factors is inapparent to an observer.

For example, when presented with an arithmetic problem (stimulus) a solution is eventually achieved, as signified by the production of an answer (response). However because the production of the answer does not occur immediately upon
presentation of the problem, and some form of interaction between the problem solver and the problem appears to be taking place during this delay, there is reason to believe that the overt production of an answer constitutes an incomplete description of the response factor. The activities occurring during the delay constitute an example of complex behavior in which the response factor is inapparent.

The inapparent stimulus factor case is illustrated by instances of remembering. In such cases a response occurs which appears to be coordinated with stimulus events that are no longer present, and are hence inapparent to a present observer. Complexities are multiplied when both stimulus and response factors are inapparent. Such is the case with imagining and daydreaming activities.

Activities in which either the response factor or both the response and stimulus factors are inapparent to an external observer are discussed collectively under the heading "Private Events" by radical behaviorists. The analysis which follows will address these events in particular. (Activities characterized by inapparent stimulus factors will be discussed in subsequent chapters).

Private Events Distinguished from Public Events

Skinner (1953, p. 257) argues that a psychological event is classified as private on the basis of its limited accessibility
to an observer, not because it has any special structure or nature. The observer in this context includes both the person in whom the event is assumed to be occurring as well as an external observer. With regard to the latter, limited accessibility is not intended to include circumstances under which an event is not observed simply because an observer is not present when it occurs. Rather, it refers to an impossibility of unaided observation resulting from the fact that the events to be observed are taking place inside the skin of another person (Skinner, 1953, p. 258; 1974, p. 21). The impossibility of observing such events must also be qualified as a temporary state of affairs. Eventually, Skinner (1974, p. 215) argues, a complete account of private events will be provided by anatomists and physiologists.

With regard to the former observer, that is, the person within whom the private events are occurring, limited accessibility to these events is a consequence of two sets of circumstances. The first of these has to do with the physical structure of the human organism and its evolutionary history. Skinner (1974) claims that observation of events taking place inside one's body is limited by the fact that certain areas of the body are not enervated such as to allow reactions to events taking place in those areas. One may react to conditions of the body produced by some malfunction or injury to the spleen, for example,
but the spleen itself is not enervated sufficiently to allow the organism to identify the place or origin of these conditions. This situation is owing to the fact that the conditions under which the species evolved biologically were not such as to afford greater chances of survival to members capable of observing events taking place within their own skins. As Skinner (1974) puts it:

(The nervous) systems arose through natural selection because of the role played in the internal economy of the organism. But self-knowledge arose much later in the history of the species, as the product of the social contingencies arranged by the verbal community, and those contingencies have not been active long enough to permit the evolution of an appropriate nervous system. (p. 216)

The second set of circumstances, responsible for the limited accessibility of persons to events taking place within their own skins, has to do with the fact that self-knowledge is of social origin. Skinner (1974) explains:

We might expect that because a person is in such intimate contact with his own body he should be able to describe its conditions and processes particularly well, but the very privacy which seems to confer a special privilege on the individual makes it difficult for the community to teach him to make distinctions. The community can teach a child to name colors in various ways. For example, it can show him colored objects, ask him to respond with color words, and commend or correct him when his responses correspond or fail to correspond with the colors of the objects. If the child has a normal color vision, we expect him to learn to identify colors accurately. The community cannot, however, follow the
same practice in teaching him to describe the states of his own body because it lacks the information it needs to commend or correct him. (p. 22-23)

Despite these difficulties, the community can and does teach its members to identify or otherwise react to events taking place within their own skins, primarily on the basis of correlated public events. (Skinner, 1957, 1974). The fact remains, however, that self-knowledge of this sort is limited even for events taking place in appropriately innervated areas, and is lacking altogether with respect to areas inadequately innervated. For these reasons, private events must be characterized as having limited accessibility even to the person in whom the event is taking place.

Varieties of Private Events

Private events may constitute stimuli or responses but are not restricted to either since events taking place at a neurological level or which exist in unclear dimensions may also be included in this category.

Private Stimuli

Private stimuli are distinguished from public stimuli in two ways. The first concerns their place of origin. The second concerns the nature of their relations with response events.
With regard to the first criterion, stimulation arising from the surrounding environment are regarded as public, while private stimuli arise from internal sources. The two principal sources of private stimulation are: the digestive, respiratory, and circulatory systems; and "the position and movement of the body in space and...the position and movement of parts of the body with respect to other parts," (Skinner, 1953). Other internal sources of stimulation mentioned by Skinner include conditions associated with behavior but not necessarily produced by it (1957); and irritation or inflammation of tissues, as in the case of an itch or pain arising from a decayed tooth, respectively (1953, p. 258).

Classification of events as stimuli, and further as private stimuli on the basis of their place of origin is not accomplished without some difficulty. Particularly is this the case when proprioceptive sources are at issue. In as much as "the position and movement of the body or of its parts" constitutes a definition of behavior (Skinner, 1938, p. 6), we may assume that this source of private stimulation is one's own behavior, be it public or private. Consequently, from the standpoint of the person being stimulated from this source, private behavior, as well as at least some aspects of public behavior, and private stimuli amount to the same thing. This deduction finds support in the following passage:
The covert behavior evokes the same response as
the overt behavior because it is essentially the same
stimulus except for magnitude. (Skinner, 1957,
p. 142).11

Feelings and emotions are analyzed in a similar manner, as
Skinner (1969) points out: "In a sense a feeling is both the
thing felt and the act of feeling it" (p. 255), and our own
behaviors are among the things felt.

Private stimuli are also distinguished from public stimuli
on the basis of the kinds of relations with response events they
may have. They may enter into controlling relations, particularly
in a discriminative or eliciting capacity (Skinner, 1953, p.
275), although also in the capacity of reinforcers of an
automatic sort (Skinner, 1953; 1957; 1974), but they are not
regarded as having any causal status (Skinner, 1957, p.
437; 1969, p. 257). Causal properties are ascribed to stimuli
arising from the surrounding environment.

Private Responses

Private responses are distinguished from public responses
primarily on the basis of their magnitude (Skinner, 1953,
p. 282; 1957, p. 141; 1969, p. 242; 1974, p. 27). As such,
the public-private dichotomy is essentially a continuum:
some private responses are executed with the same musculature
as public responses but on such a small scale as to be
invisible to an external observer—or even to the person in whom
the event is occurring.
Not all private responses are of this sort, however. In some cases the private response is assumed to constitute only a fractional component of its overt counterpart, not a minature version of it (Skinner, 1974, p. 82). When a person sees or hears something in its absence, for example, the person is said to be doing in the absence of the thing some part of what he/she does in its presence.

Whatever the precise nature of private responses, all are held to operate in the same manner as public responses, and to be subject to the same laws. In fact, there are no important distinctions to be made between the two levels or forms of activity (Skinner, 1957, p. 437).

**Unclassified Private Events**

Private events which are not readily classified as responses or stimuli include those which do not appear to be executed by the muscular apparatus and which exist in unclear dimensions. Feelings, as acts of feeling, must be included in this category. In an emotional or feeling episode one may react to various conditions of the body and its movement, however, the nature of such a reaction is not specified (Skinner, 1969, p. 255). Presumably, however, such reactions do not involve muscular action.

For the most part, events of unclear dimensions are regarded as response events as the following passage indicates:
The range of verbal behavior is roughly suggested in descending order of energy, by shouting, loud talking, quiet talking, whispering, muttering 'under one's breath', subaudible speech with detectable muscular action, subaudible speech of unclear dimensions, and perhaps even the 'unconscious thinking' sometimes inferred in instances of problem solving. (Skinner, 1957, p. 438, emphasis mine).  

Skinner argues that "(t)here is no point at which it is profitable to draw a line distinguishing thinking from acting on this continuum," however, the point at which it would seem reasonable to do so is the point at which muscular action is no longer involved. By his own declaration (1938, p. 6), events which do not involve movements of the body or its parts are not properly regarded as response events.

Events of unclear dimensions are also discussed in the context of rapid self-editing of ongoing speech. In such instances, "changes are made on the spur of the moment and so rapidly that we cannot reasonably attribute them to an actual review of covert forms" (Skinner, 1957, p. 371). It appears, instead, that we are unable to react to and reject responses before they have occurred (1957, p. 435) or before they have reached their final form (1957, p. 371).

We must assume that a 'response which has not yet occurred' is intended to imply a 'response which has not yet occurred on a muscular level', since a nonexisting event cannot be reacted to. While Skinner is reluctant to specify the dimensions of
such events, the only plausible naturalistic dimensions of a premuscular response are neurological. Hence, from Skinner's perspective, neural activity corresponding to a particular muscular event is believed to precede this event in time, and is further regarded as itself a response event.

**Private Events Summary**

The behavioral position with respect to complex inapparent activity may be summarized as follows: Some of an organism's behavior, as well as the stimulus conditions responsible for the occurrence of behavior, take place within the organism's own skin. Events occurring within the skin are not distinguished by any special physical status. In fact, a more complete understanding of their nature is anticipated with advances in the biological sciences.\(^{14}\)

With respect to their operation, private responses are not held to differ in any fundamental way from their public counterparts. Private stimuli, on the other hand, do not appear to have all of the properties of public stimuli. Specifically, private stimuli lack causal properties.\(^{15}\)

**A Critique of the Private Events Analysis**

Because events of the present sort have been traditionally assumed to exist in other than spatio-temporal dimensions, Skinner's
contention that such events are not distinguished by any special physical status is widely held to mark a break with tradition. And, of course, to some extent it does. However, the emancipation from transcendental institutions is incomplete, resulting in numerous inconsistencies in the behavioral position. In fact, Skinner's analysis of complex activity reveals the influence of traditional philosophy at every turn, namely, in the conceptualization of stimuli, responses, and the relations between them.

Further, it is not only with respect to complex phenomena that these influences may be detected. Quite the contrary, being founded on such traditions, Behaviorism is rife with difficulties on even the most fundamental of issues. Among them must be included the essential character of the psychological datum. So critical in fact is this issue to the unfolding of the behavioral argument that it constitutes a perfect starting place for the critique that follows.

Functional relations versus causal relations

Skinner (1953, 1969, 1974) argues repeatedly that functional relations between stimulus and response events constitute the subject matter of behavioral psychology. By this argument it might be assumed that functions would constitute units of analysis in Skinner's system. This does not appear to be the case, however.
Functions are not regarded as crude data for which explanations are to be found in the context of their occurrence. Rather, functions are regarded as explanations for the facts of their own occurrence. That is, behavioral events and their occurrence with respect to stimulus events are regarded as effects of prior stimulus causes. This distortion of the concept of function is owing to the fact that functional and causal relations are totally confused in Skinner's system as the following passage indicates:

The external variables of which behavior is a function provide for what may be called a causal or functional analysis. We undertake to predict and control the behavior of the individual organism. This is our "dependent variable" the effect for which we are to find the cause. Our "independent variable"—the causes of behavior—are the external conditions of which behavior is a function. Relations between the two—the "cause-and-effect relationships" in behavior—are the laws of a science. A synthesis of these laws expressed in quantitative terms yields a comprehensive picture of the organism as a behaving system. (1953, p. 35)

Skinner does not acknowledge a confusion in his thinking on this point. He frankly asserts that functional and causal relations amount to the same thing (Skinner, 1953, p. 23):

The terms "cause" and "effect" are no longer widely used in science. They have been associated with so many theories of the structure and operation of the universe that they mean more than scientists want to say. The terms which replace them, however, refer to the same factual core. A "cause" becomes a "change in an independent variable" and an "effect" a "change in a dependent variable". The old "cause-and-effect connection" becomes a "functional relation".

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According to Skinner, the scientific community adopted the newer terms simply to avoid the suggestion carried by the older terms as to how a cause causes its effect (1953, p. 23). According to other members of that same community (Kantor, 1970; Zimmerman, 1979), however, the adoption of the newer terms represents a revolution in scientific thinking. As Kantor (1970) has pointed out, the newer functional vocabulary originated in other sciences—specifically mathematics—and when mathematicians speak of functional relations there is no implication whatsoever that traditional causal philosophy is reflected in their usage. "Mathematical functions simply imply absolute equivalence of variants and no existential or causal dependence or independence," (Kantor, 1970, pp. 106-107).

Moreover, given Skinner's understanding of the differences implied by the two sets of terms, it is not clear why he should prefer the functional vocabulary since he himself addresses the issue of how a cause causes its effect. In Skinner's system, stimuli cause behaviors to occur with greater frequency and they do so by virtue of the fact that behaving organisms have inherited the capacity to be affected by the consequences of their actions (Skinner, 1969; 1971, p. 114). Obviously, then, the additional meaning carried by the older terms—to which contemporary scientists have taken issue—is precisely the meaning Skinner has improperly attached to the newer terms.
From these arguments, we may summarize the behavioral position with respect to the character of psychological data as follows: psychological events are relations between stimuli and responses which require for their understanding some form of causative determination. Accordingly, stimulus changes—conceived as independent entities taking place at a particular point in time—are assumed to bring about behavioral effects at some later point. Furthermore, the causal power of stimuli to bring about such effects may be attributed to the biological structure of behaving organisms.

Needless to say, this aspect of Skinner's system must be regarded as deviant from the standpoint of the larger scientific community. Physicists have long abandoned simple causal doctrines of this sort. The same may be said of interbehavioral psychologists (Kantor, 1950). Moreover, equating functionality with causality is the source of numerous inadequacies in the Skinnerian account of complex activities. Because causes are conceptualized as autonomous entities, stimulus and response events may be described and discussed independently of one another. The outcome of this procedure is to regard stimulus events as physical objects and response events as organismic phenomena (Skinner, 1957, p. 14-15). It is to these problems that we may now turn.
Stimulus objects versus stimulus functions

Difficulties arise when objects or situations are regarded as stimuli. Because any number of responses may occur with respect to the same object it is difficult, if not impossible, to determine which stimulus is coordinated with which response for a particular organism. This problem is solved if objects are taken to be sources of stimulation only. The problem then becomes which stimulus function resides in a specific object for a particular organism (Kantor, 1933).

The power of the stimulus-as-function formulation is illustrated by Skinner's solution to the problem of identifying the physical dimensions of private stimulus objects. The stimulus-as-object conception appears to create no particular problems when these objects are observable events, although even here some difficulties are encountered (Skinner, 1969, p. 79). When stimuli are private, however, the problem of identifying their properties, and thereby distinguishing one from another, appears to be insurmountable. Consequently, a classification in terms of sources of stimulation is adopted (Skinner, 1953). The irony of this situation is that while Skinner regards this solution as less than satisfactory, a functional interpretation of stimulus events calls for just such an analysis. The point is that adequate descriptions emerge from actual confrontations with the events to be
described, despite postulational improprieties. In the case of feelings, for example, the thing felt and the act of feeling it seem to constitute a unity. Similarly, proprioceptive stimuli and reactions to them appear to be one and the same thing. These mergers present problems when stimulus events are regarded as prior and independent causes of response effects. However, from a functional standpoint, no problem exists. In each case what is observed is an interaction of the organism with conditions of his or her own body—a functional relation in which stimulus and response events constitute simultaneous phases of a single coordinated event.

Were Skinner's conception of stimuli derived from actual observations of events, observations would not draw it into question. The fact that they do argues for reconsideration, however no such action is taken. Instead, the causal interpretation is vigorously defended, although not without an additional problem. The problem concerns the causal status of private stimuli.

The causal status of private stimuli

Despite the fact that private stimuli may have eliciting, discriminative, and reinforcing effectiveness, they are not regarded as having any causal status. Only public stimuli may serve in this capacity. By this restriction we must assume that
causality refers to something other than elicitation, discrimination, and reinforcement operations. However, in all other contexts, the stimuli involved in these operations are regarded as the causes of behavior. That is, these operations and their derivatives are taken to be synonymous with the concept of causality. Why this situation should change when stimuli become inapparent to an external observer is unclear.

It might be argued that the restriction of causality to public stimuli is imposed on practical grounds. Events inside the skin of another organism are not only inapparent to an external observer; they are also not directly manipulable. Because causality is believed to be demonstrated when events are manipulated and the effects reliably produced, the causal properties of private stimuli are not demonstrable. Hence, while private stimuli may have causal properties, it serves no useful purpose to make this inference since whatever may be their nature no practical action may be taken with respect to them. In actual fact, however, Skinner makes this argument only with respect to private events of alleged nonphysical dimensions (1974, p. 210). By contrast, the noncausal status of private stimuli is simply asserted.

It would be reassuring to think that the restriction of causality to public stimuli were made on the basis of observation. This argument cannot be made however as causality is not among the events observed, be they public or private.
From this standpoint, it is not the noncausal status of private stimuli that must be questioned but rather the causal status of their public counterparts. Skinner does not question the causal role of public stimuli. In fact, the attribution of causality to public stimuli is fundamental to the behavioral position: apart from an explicit denial of the psychic nature of private events, the principal difference between Behaviorism and Mentalism is the location of the alleged causes of behavior.

Organismic responses or behavioral fields

The causal interpretation of functional relations also results in an inadequate conception of response events. When stimuli are regarded as causes of behavioral effects, behaviors come to be understood as exclusively organismic performances or movements (Kantor, 1970, p. 105). This, in turn, leads to attempts to localize response events in particular muscle groups or other biological structures. Skinner's own position on response events varies with the occasion, however most often biological phenomena are described as psychological events. The outcome of this confusion is to assume, as Skinner does, that the observational problems posed by private events of psychological significance will eventually be solved by technologies designed to amplify biological occurrences (Skinner, 1953, p. 282). However, when psychological
events are properly conceived as *interactions* between behaving organisms and stimulating environments, it becomes obvious that the problem of privacy will never be resolved in the manner anticipated by the radical behaviorist.

Assuming that a more complete description of psychological occurrences will emerge eventually from microscopic examinations of organismic structures is not the most serious consequence of failing to properly differentiate the psychological from the biological domain. A more serious problem is that when attempts are made to distinguish one type of event from another, improper relations between these events are constructed. In essence, psychological events are assumed to be explained by way of biological phenomena. It is in keeping with such reasoning that Skinner (1974) makes the following statement:

The physiologist of the future will tell us all that can be known about what is happening inside the behaving organism. This account will be an important advance over a behavioral analysis, because the latter is necessarily 'historical'—that is to say, it is confined to functional relations showing temporal gaps. Something is done today which affects the behavior of an organism tomorrow. No matter how clearly that fact can be established, a step is missing, and we must wait for the physiologist to supply it. He will be able to show how an organism is changed when exposed to contingencies of reinforcement and why the changed organism then behaves in a different way, possibly at a much later date. (p. 215, emphasis mine).
The point is that the manner in which response events are conceptualized determines how biological factors figure into their analysis. For example, when responses are conceived as organismic performances, they appear to demand a biological substratum or basis. Accordingly, changes in organismic performance—normally described as "learning"—appear to demand changes in an underlying biological structure. Hence the latter are simply asserted, despite the absence of any concrete evidence. Moreover, the two sets of changes are not regarded as simultaneous occurrences: the psychological changes are regarded as results of prior biological changes.

Alternatively, when responses are conceived as phases of functional relations, they exist as events without any substantive structure themselves and, consequently, are not dependent upon such a structure at any other level of analysis. Given this view of response events, "learning" is something that happens to functional relations, not to organisms.

This is not to say that biological factors are not involved in psychological events, only that their involvement is not profitably described as causal. Kantor (1982) explains:
When we think in terms of biological functions as comprised in psychological behavior situations, we are on our way to an accurate description of events, while in considering the two as separable and causally related, valid analysis of what actually happens in human behavior is completely forestalled. In other words, when we regard the organism's anatomical features (which have a place in every action) as parts (actually indivisible of course and therefore only logical parts) of an act going on, then we can account for the specific variations of action because of size, weight, and other biological factors of a person. On the other hand, when we regard such anatomical features, whether definite organs, structures, or hypothetical biological factors, as determiners or foundations of conduct, then our psychological data are inevitable misinterpreted. By all means must we avoid here the logical error of confusing a necessary condition with a cause. (p. 72)

In summary, because the revolutionary concept of functionality is confused with traditional causality in behavior theory, psychological stimuli and responses are regarded as separate and independent entities. Consequently, each is assumed to have, or to have a basis in, substantive structure. That is, stimulus-causes are regarded as physical objects, not phases of functional relations. Likewise, response-effects are conceptualized as strictly organismic phenomena. In other words, psychological events are reduced to the things and events of the physical and biological sciences.

Despite the fact that psychology—as conceived by behaviorists—is the study of such causes and their effects, the causes appear to have less intellectual appeal than their effects. Psychology is rarely reduced to physics, in whole or even in part.
On the other hand, the biological basis of psychological phenomena is heralded as revealed truth. This emphasis on response-effects and their biological bases may also be attributed to the influence of conventional doctrine concerning the causes of behavior, to which we may now turn.

**Internal causes**

A review of psychological history reveals that for the largest portion of that history, responses were believed to be caused from within. Originally, internal causes were of a spiritual sort; they were the directives of the transcendental soul. In more modern times, the mind has occupied this position; and still more recently, the nervous system, and specifically the brain has emerged as the most important source and origin of an organism's complex activity, (Kantor, 1963, 1969a). Needless to say, when the causes of behavior as well as the behavior itself are localized in the organism, interest in the stimulating environment wanes.  

That behavioral psychologists have adopted the doctrine of internal causes is indicated in Skinner's discussions of self editing. As previously mentioned, Skinner's contention that a person can react to and reject responses before they have actually occurred, or before they have reached their final form, must be taken to mean that responses occurring on a muscular level have
their origin in the brain. However, there is no reason but adherence to the doctrine of internal causes to assume that neural events at one point in time become muscular events at a later point.

To deny neural events a causal role in the occurrence of psychological events is not to deny their involvement altogether. On the contrary, they may reasonably be assumed to participate in the psychological event. However, the neural activities pertinent to and participating in the occurrence of a particular response are those occurring at the same time as that response, not prior to it.

The doctrine of internal causes is also responsible for two other aspects of the behavioral interpretation of complex events: The public-private dichotomy; and the inadequate differentiation of classes of so-called private events.

**Public-private dichotomy**

Conventional doctrine holds that the brain, as well as its terminological predecessors, is not only the source and origin of behavior, but also the behaving entity. That is, throughout psychological history, complex acts have been held to be enacted by particular parts of the organism, namely the soul, mind, or brain (Kantor, 1963, 1969a). Hence, thinking, reasoning, and the like were conceptualized as activities occurring inside the organism.
The behaviorist, in keeping with this tradition, takes the position that some of the psychological activity of an organism occurs inside the organism's skin. By this means is established the public-private dichotomy: Events occurring "within the skin" are private, while events occurring at the "surface of the skin" are public.

In what sense, however, does behavior occur at the "surface of the skin"? Furthermore, is it not obvious that the whole organism—skin and all—is involved in every instance of psychological activity? Clearly it is only the behaviorists' unwitting commitment to conventional philosophy that leads them to regard obvious or apparent events as activities of the whole organism; and subtle or inapparent events as activities of its parts.

The behaviorists' dichotomization of psychological events into public and private classes is not of theoretical significance only. More important are the consequences of this analysis for the identification, differentiation, and investigation of events in the private class. Essentially, by classifying events as private on the basis of their inaccessibility to external observers, such events are rendered inscrutable—at least for the present time. Moreover, by reducing psychological functions to biological occurrences, the responsibility for the analysis of private events is transferred to the biological scientist. Consequently, private events are not adequately
differentiated because they are not currently possible to identify. Further, in the event that private events do become possible to identify (by means of technological innovations designed to amplify biological activities), and are thereby able to be differentiated one from the other, it will be the job of the biological scientist—not the psychologist—to investigate their nature and operation.

In short, some behaviorists are willing to consider the issue of private events, in large part to avoid being accused of neglecting them (Skinner, 1974); but, in order to avoid being identified as cognitivists, they are unwilling to elaborate as to the nature and operation of such events. As a result, the behavioral position with respect to complex human activity is fundamentally identical to that of the contemporary cognitivist, minus the detail of the latter's position. Little wonder it is, then, that Behaviorism has not been successful in preventing a cognitive revival in psychology.

Summary and Conclusion

The behavioral position with respect to complex human activity is clearly influenced by conventional causal philosophy. This influence may be observed in the confusion of functional relations with causal relations, resulting in a causal interpretation of stimuli. So conceptualized, stimuli and responses may
be regarded as separate and independent entities, localized in physical objects and biological organisms, respectively.

At this point in the analysis may be seen the influence of additional philosophical assumptions concerning the relationship between biological and psychological events. Under this influence, psychological events lose their distinctive character altogether. They are interpreted as organismic happenings, explained in terms of their underlying biological structures.

In accordance with these assumptions is the public-private dichotomy asserted: Public events are interpreted as activities of the whole organism, occurring at the surface of the skin; while private events are seen as activities of parts of the organism, occurring within the skin.

Events within the skin are not readily investigated, however. Moreover, should such investigation be undertaken it would not be undertaken by psychological workers, since the events of interest are biological not psychological in nature. Hence, the behavioral position with respect to complex human behavior lacks detail and offers no promise of elaboration. As such, the behavioral position does not provide a viable alternative to traditional interpretations of this subject matter. It is, in fact, just an impoverished version of the traditional viewpoint.
CHAPTER V

THE PARADOX OF RULE GOVERNANCE

The rule governed - contingency shaped dichotomy is an unavoidable embarrassment to the field of behavior analysis. A category of rule governed behavior has seemed necessary to account for innumerable instances of behavior that do not appear to have been contingency shaped. The dichotomy is embarrassing however, because, despite the enormous utility of the rule governance concept, there is no basis in behavior analytic theory on which to propose a distinction between rule governed and contingency shaped behavior. From a behavior analytic standpoint, the dichotomy is false.

On the other hand, observations of behavioral events appear to demand just such a dichotomous account. Certain observations do seem to warrant special analytic treatment, specifically, those involving verbal behavior. It is not a coincidence that rule governed behavior depends on a verbal repertoire. Quite the contrary, the paradox of rule governance may be traced to Skinner's (1975) analysis of verbal behavior: The rule governed - contingency shaped dichotomy is an outgrowth of more fundamental problems with this analysis.
The problems encountered in Skinner's analysis of verbal behavior are not of the same variety as those identified in the previous discussion of "private events". Skinner's analysis of verbal behavior is not mentalistic. It is superficial. Solutions to these problems are not unattainable, however, they are not likely to be appreciated by those who fail to see that any problems exist. Indeed, failing to see that any problems exist is by far the more difficult problem to solve. For this reason, a considerable portion of the present chapter has been devoted to the identification and explication of what I believe are inadequacies in Skinner's analysis of verbal behavior. In doing so, it is not my intention to imply that Skinner's analysis has not been useful. Rather, it is my intention to substitute better hypotheses for poorer ones when contacts with events permit me to do so. Therefore, I shall also provide an alternative hypothesis of verbal behavior, along with the implications of this alternative for the dilemma posed by the concept of rule governance.

Rule Governed Behavior

As mentioned above, certain observations of behavior appear to require a rule governed interpretation. We may begin our critique by examining the circumstances under which a rule governed interpretation has seemed necessary and has been
proven useful. Generally speaking, behavior is interpreted as rule governed when it appears to be maintained despite "defective" contingencies of reinforcement. Defects may be found among antecedent or consequent conditions, although more often among the latter. Specifically, consequences may be unlikely, negligible, or long deferred (Skinner, 1969, pp. 166-171; 1974, pp. 125-128). With respect to unlikely consequences, Skinner (1969, p. 168) cites perseverent behavior which is, by definition, a continuation of behavior in the absence of reinforcement. Similarly, people wear seat belts even though the likelihood of any one person making contact with the consequences of doing so is very low. By far the most often cited defect in contingencies, however, is the lack of immediate consequences of behavior. For example, people quit smoking cigarettes even though the negative consequences of continuing to smoke are long deferred. Likewise, people behavior virtuously even though the ultimate consequences of virtuous conduct are remote -- possibly even non-existent.

In addition to the fact that these behaviors appear to be maintained in the absence of effective contingencies, all seem to occur in the presence of a particular type of antecedent condition. Specifically, the antecedent condition constitutes a description of a contingency of reinforcement, that is, a
description of behavior to be executed, the conditions under which it is to be executed, and its likely consequences.

Because these behaviors did not seem to fit the pattern of more commonly observed operant behaviors, a separate category was established to accommodate them. And, as is the nature of the behavior analytic system, a separate category of behaviors implies a separate explanation for the occurrence of behaviors in that category. The explanation that emerged under these circumstances amounted to shifting to antecedent conditions the control ordinarily exerted by consequences. Rule governed behavior is, as the name suggests, behavior under the control of antecedent stimulation supplied by a contingency-specifying statement or rule.

Once formulated, the concept of rule governance proved to be extremely useful. In so enormously complex a culture as we live, our survival depends in large part upon our ability to acquire knowledge indirectly through the experiences of others. Rules permit us to do so. To paraphrase Skinner (1974, p. 124), rules carry us beyond personal experience and beyond the defective sampling of nature inevitable in a single lifetime. They also bring us under the control of conditions which could play no part in shaping and maintaining our behavior.

Rules, however, are simply stimulus objects. How is it that behavior comes under their control remains to be addressed.
How Do Rules Govern Behavior?

The temporal relation of rules to the behaviors they control suggests that rules may operate as discriminative stimuli. However, the discriminative functions of stimuli are established when behaviors are reinforced in the presence of those stimuli, and it was the apparent absence of immediate reinforcement for such behaviors that gave rise to the concept of rule governance in the first place. Hence, if rules are to be conceptualized as discriminative stimuli, the reinforcers for rule governed behavior must be identified. In other words, in order to account for the control exerted by rules, rule governed behavior must be conceptualized as contingency-shaped behavior. In so doing, it becomes obvious that the consequences for behavior specified in a rule are typically not the consequences responsible for the control exerted by that rule. That is, while a contingency may be specified in a rule, the behavior under the control of that rule is subject to another contingency. For example, the eventuality of lung cancer—a specified consequence of continuing to smoke cigarettes—does not account for the cessation of smoking. Nor, even, do more immediate social consequences of smoking cessation account for it, since the effect of these consequences may only be seen in subsequent occurrences of this behavior. Instead, the consequences responsible for the occurrence of rule following behavior are those that have
attended rule following behavior in the past (Skinner, 1969, p. 148). We follow rules because we have been reinforced for following rules in the past.

The solution, then, to the problem of how rules govern behavior, is achieved by considering rule governance to be an operant, of which individual instances of rule governed behavior may be considered members. By this procedure, the conceptual formulations and empirical evidence originally acquired with respect to contingency shaped behavior—and by virtue of which such behavior is explained—become relevant to the analysis and understanding of rule governed behavior.

While we may applaud the parsimony of this solution, rule governed and contingency shaped behavior are not entirely analogous. Rule governed behavior is—at least—a special subdivision of the contingency shaped category; and to overlook the differences between behaviors of this subdivision and those of the larger category may result in further difficulties. Skinner cannot be accused of overlooking these differences; however, the differences identified by him are relatively trivial. Furthermore, the logic of his comparison of rule governed and contingency shaped behavior is not at all clear. It seems as though Skinner is willing to acknowledge the existence of an important difference between these two classes of behavior, but he does not seem to know what it is. We may
substantiate these claims by reviewing Skinner's position as to the differences between rule governed and contingency shaped behavior.

Differences Between Rule Governed and Contingency Shaped Behavior

Skinner (1969, 1974) contends that while a given instance of rule governed behavior may appear topographically similar to its contingency shaped counterpart, the two episodes of behavior actually differ in several important respects. Specifically, they occur under the control of different antecedent conditions (1969, p. 166), may be maintained by different reinforcers (1969, p. 169), and may be subject to different motivational operations (1969, p. 169). In other words, they are instances of different operants. Moreover, despite the apparent similarity between the two response forms, closer examination reveals that the rule governed response may be less precise, less complete, and less variable than the corresponding contingency shaped behavior (1969, p. 167-170).

In summary, Skinner's argument amounts to an assertion that an instance of one operant differs from an instance of another. That is, an operant involving a rule differs from one which does not involve a rule; and, not surprisingly, it differs along those dimensions that define an operant, namely,
the antecedent, consequent, and motivational conditions attending
the response in question. He might as well have argued that
an operant involving a red key light differs from one involving
a green key light, with the main difference between them being
the color of the key light. However, few would have found
this analysis of any value whatsoever, and I submit that a
comparable analysis of the difference between rule governed
and contingency shaped behavior has similar worth. Also
objectionable in Skinner's argument is the bias implicit in
comparative statements about the adequacy of the response
topography in each case. Response topographies are not
incomplete, imprecise, or otherwise defective. They are what
they are. What is defective is the assumption that they ought
to be something other than what they are.

To this point, Skinner's contribution to our understanding
of the difference between contingency shaped and rule governed
behavior is very small indeed, despite the fact that he is
obviously motivated otherwise. This contradiction is owing to
two sets of circumstances. First, in Skinner's analysis, contin-
gency shaped operants are compared with particular instances of
rule governed behavior, rather than with the operant, rule
governance. As a result, the nature and operation of rule
governed behavior become ambiguous: On one hand, rule governed
behavior must be conceptualized as contingency shaped behavior
if the control exerted by rules is to be explained. On the other hand, rule governed behavior is said to occur under conditions which would not support contingency shaped behavior (Skinner, 1974, p. 123-128), implying that it is not, itself, contingency shaped.

To reiterate the point being made here: Skinner's error is to compare contingency shaped response classes with individual instances of rule governed behavior, the result being that rule governed behavior may or may not be interpreted as operant behavior. Were an appropriate comparison of response classes undertaken, what would have emerged as the only consistent difference between rule governance and any other operant is the fact that the antecedent stimuli in the former case are always verbal.20

The second set of circumstances responsible for Skinner's failure to clarify the distinction between rule governed and contingency shaped behavior have to do with his analysis of verbal behavior. Before dealing with this analysis, however, we must review, briefly, the strategy adopted by Skinner for the comparison of rule governed and contingency shaped behavior, as well as the reasons for his adoption of this strategy.

Skinner compares behavior shaped by exposure to a particular contingency with topographically similar behavior.
under the control of a contingency-specifying rule. The rule in
the latter case, is said to "specify" or "describe" the
contingency responsible for the shaping of behavior in the former
case. Because two different episodes of behavior are compared,
we must ask: According to what criteria are episodes selected
for comparison? The topographical similarity of the responses
does not appear to serve in this capacity, at least not
in isolation, because it is only when the contingency maintaining
one response is described by a rule in the second episode that
episodes are, in fact, compared. When the latter condition prevails,
corresponding pairs of contingency shaped and rule governed response
episodes may be identified for comparison. In conclusion, it appears
that Skinner's comparison of rule governed and contingency shaped
behavior is based on the assumption that a special relationship
exists between patterns of verbal stimuli (rules) and contingent
relations among stimuli and responses (contingency shaped behavior);
and, further, that this relation is one of "description" or
"specification" or "reference".

Skinner's Analysis of Verbal Behavior

We may now return to the issue of Skinner's analysis of
verbal behavior. In doing so we are immediately struck with
the fact that, in this context, no such relations between verbal
and nonverbal events are presumed to exist. In fact such
relations are explicitly denied, as the following discussion of the tact indicates:

It may be tempting to say that in a tact the response 'refers to', 'mentions', 'announces', 'talks about', 'names', 'denotes', or 'describes' its stimulus. But the essential relation between response and controlling stimulus is precisely the same as in echoic, textual, and intraverbal behavior. We are not likely to say that the intraverbal stimulus is 'referred to' by all the responses it evokes, or that an echoic or textual response 'mentions' or 'describes' its controlling variable. The only useful functional relation is expressed in the statement that the presence of a given stimulus raises the probability of occurrence of a given form of response. This is also the essence of the tact. (Skinner, 1957, p. 82)

In other words, a verbal stimulus or rule does not describe a contingency; it is merely the stimulus product of a response under the control of stimulation supplied by the contingency. As such, a rule is not unlike any other stimulus produced by any other response under the control of the same conditions. For example, a rule, having the form "if the light is red, stop to avoid an accident", and tire marks on the pavement before a stop light are equivalent forms of stimulation by virtue of the fact that both occur under the control of a contingency in which a failure to stop has resulted in injury (personally, or in an acturial sense). In short, there is nothing special about the relation between the rule and the contingency, since whatever relation exists between them also
exists between the tire marks and the contingency. By this analysis, however, the concept of rules becomes totally superfluous. Consequently, it would seem parsimonious to abandon the concept, along with the special operant of rule governance dependent upon it. But this is not what has occurred. Instead, the implication for the concept of rules and the behavior of stating them, arising from Skinner's nonreferential analysis of verbal behavior, are simply ignored. Apparently, rules are rules and verbal behavior is verbal behavior.

Obviously, there exists an inconsistency in Skinner's analysis on this issue: Rules are not only rules, they are verbal stimuli; and the statement of rules is not only the statement of rules, it is verbal behavior. Hence, whatever analysis is made with respect to one set of terms must apply equally well to the other set. The question is: Which analysis is to made of both?

Referential Versus Nonreferential Analyses

Rule Governance

The rationale for a referential analysis of rules and statements of rules has already been presented. In essence, unless rules are conceptualized as descriptions of contingencies, there is no basis for a comparison of contingency shaped and
rule governed behavior. In fact, there is no basis for a separate category of rule governance. Moreover, unless a referential interpretation of rules is made, the concept of an incomplete or fragmentary rule, that is, one which describes only a part of a complete contingency (Skinner, 1969, p. 158), has no meaning whatsoever. It is only when a rule is believed to describe a contingency in the first place that it can be said to be incomplete for failing to describe all parts of it.

A more important reason than either of these for retaining a referential analysis of rule governance concerns the behavior of the rule follower and how this behavior comes to be evoked at the appropriate time in the appropriate place. I shall return to this issue upon first reviewing Skinner's rationale for adopting a nonreferential analysis of verbal behavior.

**Verbal Behavior**

The traditional interpretation of reference holds that words symbolize mental events called "ideas" and, as such, words are used by speakers to refer to the ideas they symbolize. Accordingly, the meaning of a word is considered to be the "idea" to which it refers. This interpretation is rejected by Skinner for good reason. By presupposition, mental events have no legitimate status in the science of behavior. Hence, if words refer to anything, they refer to actual things and events, not mental representations of them. However, the notion that
words refer to actual things and events is also rejected, on the grounds of its limited applicability. Only the tact may be said to exemplify a relation of reference of this sort. The remaining verbal operants exemplify relations not between words and things but between words and words. In these cases, the only useful relation, Skinner argues, is not one of reference but is, instead, expressed in the statement that "... the presence of a given stimulus raises the probability of occurrence of a given form of response," (Skinner, 1957, p. 82). Furthermore—and this is the crux of the issue—the tact may be described in precisely the same manner, making the issue of reference, from the standpoint of the speaker, entirely superfluous.

Skinner's argument with traditional views of reference from the standpoint of the listener has a logical flavor. The traditional view suggests that one's behavior with respect to a word-symbol is, up to a point, that which the object itself would have occasioned (Russell, 1940, p. 82). Skinner disagrees. He explains his position with a fox hunting example, arguing that one "turns and looks" under the control of the word "fox", whereas one "rides after" an actual fox (1957, p. 87-88).
Certainly this difference prevails, however, such differences are entirely dependent upon the behaviors identified under each set of circumstances. Were one to look at the behavior of seeing, for example, the response to the word and to the animal would be quite similar, except for the fact that in the former case the response involves an imagining as opposed to a perceptive reaction system. Skinner (1957) mentions behaviors of this sort and acknowledges the fact that the word and the animal may induce similar responses of the respondent type (p. 88). However, he wishes to restrict his discussion to the operant behavior of the listener, because his logical refutation of the traditional argument depends on a consideration of operant behavior only. Skinner's argument is as follows:

The heard stimulus 'fox' is the occasion upon which turning and looking about is frequently followed by the reinforcement of seeing a fox. Technically, the behavior of turning and looking is a discriminated operant, rather than a conditioned reflex. The difference is important. The verbal stimulus 'fox' is not a substitute for a fox but an occasion upon which certain responses have been, and probably will be, reinforced by seeing a fox. The behavior which is controlled by the fox itself—looking toward or riding after—cannot be evoked by the verbal stimulus, and there is therefore no possibility of a substitution of stimuli as an analog of a sign or symbol. ...The practical behavior of the listener (the consequences of which are ultimately responsible for the development of the verbal response in the first place) must be formulated as a discriminated operant involving three terms, no two of which provide a parallel for the notion of a symbol. (1957, p. 88)
In other words, because the listener's behavior exemplifies the three term contingency relation, the symbolic interpretation must be wrong in that only two of the three terms are implicated in this interpretation.

Skinner's argument is not convincing however. It is only because Skinner conceptualizes psychological events as three-term contingency relations that three terms seem to be required for an analysis of referential situations. The number of terms involved is entirely a matter of the presuppositions underlying the two approaches to the problem. Moreover, the logic of including only the operant behavior of a listener in this context is unclear. A tendency to see a fox upon hearing the word "fox" also constitutes practical behavior in that this behavior orients the listener with respect to the actual fox.

While it may be true that the traditional interpretation of reference may be abandoned without loss, Skinner's arguments to this effect, as well as his conclusions concerning the necessity of the concept, leave something to be desired. In the first place, the fact that relations of reference between words and things are characteristic only of the tact does not imply that the concept of reference has a limited applicability. It does imply something about the tact, however. The tact is the basic unit of verbal action, without which there would be
no other units and from which all other units are derived and continue to depend. Hence, whatever is true of the tact is also indirectly true of the verbal units based on the tact. Secondly, with regard to Skinner's conclusion concerning the necessity of a concept of reference—even in the limited case of the tact—an important issue seems to have been overlooked. That is, one of the conditions under which a tact occurs is the thing or event to which traditional theorists say it refers.

More important than either of these issues, however, is the fact that Skinner does not actually address the issue of reference. All of his objections concern the traditional interpretation of this concept rather than the concept itself. Still, he claims to find fault with relations of reference. It appears, therefore, that he does not distinguish between the concept of reference and the interpretation of this concept. This lack of clarity has an unfortunate outcome: the concept is abandoned on the grounds that the traditional interpretation of it is objectionable.

At least this is how it might appear at first glance. In actual fact, the concept of reference is so important to the analysis of behavior—particularly complex behavior—that it is not easily abandoned. It tends to reappear in disguised form, as is evident in the statement: "Rules describe contingencies". Furthermore, the alternatives to this concept are equally,
if not more, objectionable. Take, for example, the suggestion that remote consequences may be brought to bear by way of verbal stimuli (Skinner, 1969, p. 169), or that such stimuli are capable of conditioning future behavior by means of remote stimulus control (Skinner, 1957, p. 416). Surely there is no more reason for adopting these sorts of notions than there is for a concept of reference, particularly when a thoroughly naturalistic interpretation of this concept is possible to achieve (Kantor, 1977; 1935). Indeed, it is to this interpretation that we now may turn.

Relations of Reference and Referential Behavior

A relation of reference is established when a completely arbitrary pattern of responding becomes coordinated with a particular stimulus, or class of stimuli; and when the interaction of response and stimulus may be described as ineffectual, which is to say, the response does not in any way alter the stimulus or have any other effect on it. For example, the pattern of responding productive of the sound "pen", coordinated with the stimulus object, pen, constitutes a referential reaction. The form of this response, it may be noted, is completely arbitrary as is obvious from the fact that, in another language, the pattern of responding observed under the same stimulating conditions is completely different.
Moreover, the response has no effect whatsoever upon the object, pen. (This is not to say that the response has no effect upon the response of another person who, in turn, may have an effect upon the object. This is a different issue altogether).

This analysis is not unlike Skinner's in some respects. For example, Skinner mentions the fact that verbal behavior is "...impotent against the physical world." (1957, p. 2). Likewise, the arbitrariness of the response form is implicit in the refined definition of verbal behavior which states that a response may be considered verbal only when its reinforcement is accomplished by a listener who has been explicitly conditioned to reinforce such responses (Skinner, 1957, p. 225).

The arbitrary form of referential responses and the fact that they participate in ineffectual relations with stimuli, make for a number of other peculiarities. First, there are practically no limitations on the extent to which response form may vary. Since an enormous number of response forms are possible, a different form (or combination of forms) may become coordinated with each different stimulus object or event. In short, referential relations are very specific compared to nonreferential relations. For example, while we may sit on a chair, a sofa, a table, or a desk, each of these stimuli is coordinated with a different referential response, its "name".

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Secondly, because referential responses have no effect on the stimuli with which they are coordinated, their occurrence does not interfere with the execution of nonreferential responses coordinated with these same stimuli. As a result, arbitrary referential responses are frequent accompaniments of nonarbitrary nonreferential responses (particularly, of course, when the responding individual is not alone). A wide variety of nonreferential responses may be coordinated with any particular stimulus. However, one type of nonreferential response inevitably occurs on every occasion of contact between the responding individual and the stimulus, namely, perceptual activity. Perceptual activities almost always involve a visual component, but include, as well, all of the other senses. Because referential activities often occur in combination with nonreferential activities and because perceptual activities are inevitable components of all nonreferential behaviors, referential responses are associated with perceptual activities more often than with other kinds of nonreferential behavior. The implications of this association of responses is important and will be discussed in more detail below.

A third characteristic of referential relations, attributable to both the arbitrariness of the response form and the ineffectuality of the relation between stimulus and response, is the fact that the occurrence of a referential response is not
restricted to circumstances in which the stimulus originally coordinated with this response is present. That is, the response "typewriter" may occur in the absence of the object, typewriter. On the contrary, the nonreferential response of typing can only occur in the presence of a typewriter.

Skinner (1974), makes a similar point when he argues that "(a)part from an occasional relevant audience, verbal behavior requires no environmental support," (p. 89). Responses do not occur in the absence of environmental support, however. They occur in conjunction with substitute stimuli. Substitute stimuli are stimuli which have been spatially or temporally associated with the original stimulus (i.e., the stimulus with which the response was originally coordinated), and to which the reaction appropriate to the original stimulus has become associated (Kantor, 1924). The auditory (or visual) stimulus products of referential behavior are particularly likely to serve as substitute stimuli since they are particularly likely to have been produced in the presence of these other stimuli. For example, the sound "pen" is likely to serve as a substitute stimulus for the object, pen, because this auditory stimulus is likely to have occurred in close temporal association with this object in the experience of particular individuals.
Having completed our survey of reference and referential behavior, we may return to the problem of how verbal stimuli, in the form of a rule, bring about behavior with respect to an entirely different stimulus complex, namely, the actual circumstances under which rule-following takes place.

Accounting for the Behavior of Following Rules

In order to account for the behavior of following rules, we must review exactly what it is we are trying to explain. For behavior to be considered rule governed, it must occur in the presence of stimuli having the form of a contingency-specific rule. That is, the rule must be a part of the circumstances under which rule-following takes place. Rule following, however, is something other than listening to or reading a rule—it is the behavior specified in the rule, occurring with respect to the antecedent stimuli described in the rule. What requires explanation, then, is how one pattern of stimuli—the rule—brings about not only the behavior of hearing or seeing this stimulus, but also a totally different response under the control of a totally different stimulus complex.

Skinner's solution to this problem is to fail to recognize it as a problem. According to Skinner, rule following is simply a matter of doing what a rule tells one to do. What this means or how it comes about is not even discussed, at least not in the context of rule governance. Fortunately,
a referential interpretation of the listener's or rule-follower's behavior may provide a solution to this problem.

A spoken rule, such as "If it rains, take an umbrella to stay dry," is a pattern of auditory stimuli with which hearing responses of a particular listener are coordinated. For rule-following behavior to occur, the listener must have had an extensive verbal history, as well as a history of reinforcement for following rules. The verbal history required is one in which the stimuli making up the rule, including "rain", "umbrella "staying dry", etc., have been associated with particular objects or events in the behavior life of the listener. This association is usually accomplished by the fact that the listener has produced these same auditory stimuli in the presence of these objects or events at some previous time. A history of verbal interactions with objects implies that associative relations between verbal and perceptual activities coordinated with the same stimulus objects have been established. This is the case because any sort of interaction with an object implies some form of perceptual contact with that object. As a result, one of the listener's reactions to the auditory stimuli making up the rule is perceptual in nature. That is, upon hearing the name of an object, the listener tends to "see" it, "hear" it, or otherwise react to it in an implicit perceptual manner, (Kantor, 1924.)
It is this activity that has been the subject of controversy. A misconception of it has given rise to the traditional view that the meaning of a word is the psychic idea to which it refers. And it is this misconception that has led Skinner to ignore implicit perceptual activities altogether. It is not necessary to interpret such events mentalistically, however. The present analysis assumes only that one's history of interactions with stimuli is present in one's current interactions. Neither is it possible to ignore such activities since it is only by means of implicit perceptual behavior that the transfer of control, from the verbal stimuli of the rule to the nonverbal stimuli of the actual circumstances under which rule governed behavior is to be executed, can take place. It is in this sense—and this sense only—that remote consequences are brought to bear on current behavior, and future behaviors are conditioned in the present.

Conclusion

In conclusion, a concept of rule governance has seemed necessary to account for behavior which does not appear to be contingency shaped. However, in addition to the fact that there is no other explanation for its occurrence, this concept assumes a referential analysis of verbal behavior which has been rejected for other reasons. As a result, one analysis is made of rule governed behavior, and quite another analysis
is made of verbal behavior, despite the fact that these two topics clearly overlap.

Upon review of Skinner's rationale for a nonreferential analysis of verbal behavior it was concluded that his objections to the concept of reference were not actually directed at this concept, but rather at traditional, mentalistic interpretations of the concept. A naturalistic interpretation of reference was therefore provided wherein the distinguishing characteristics of referential behavior were: 1) the topography of a referential response is arbitrary; and 2) the response participates in a noneffectual relation with a stimulus object. A consequence of these two characteristics of referential behavior was the fact that referential responses tended to be associated with nonreferential behaviors, particularly of a perceptual sort. This association, in turn, allowed for the occurrence of implicit perceptual activity through the substitute stimulation provided by a rule. The significance of implicit perceptual activity is the fact that without such a concept, the occurrence of rule following behavior is simple asserted without explanation.
CHAPTER VI

PSYCHOLOGICAL HISTORY AND FUTURE

History

The unenviable succession of so-called solutions to the problems of complex behavior, constituting a central theme throughout the history of psychology, is an outcome of the failure of psychology to abandon a set of underlying assumptions characteristic of the non-sciences, theology in particular. Psychology persists in assuming that the organism is the primary factor in psychological events and, further, that the organism has a dual nature. The historical dichotomy of soul and flesh, introduced by the Church Fathers during the Hellenistic period of our cultural history (Kantor, 1963, 1969a; Lundin, 1979) survives today in the duality of mind and body -- or more recently -- brain and behavior. 25 In other words, psychology has been and is still assumed to be a study of nonnatural as well as natural things and events. No other science assumes a nonnatural subject matter. In fact, as the history of the natural sciences shows (Kantor, 1938), significant advances in knowledge correspond temporally with the rejection of just such hypotheses.

The things and events isolated as the domain of a particular science not only serve to distinguish that science from all others,
but also have a profound influence on all aspects of scientific work. Investigative practices, for example, are tailored to meet the needs of particular subject matters. Because knowledge about nonnatural things cannot be obtained by the methods developed for the investigation of natural things, new methods of obtaining knowledge had to be invented. As Kantor (1979b) has pointed out:

(I)t was no accident that Wundt made introspection the primary method of psychological study and investigation. How else could one penetrate into the mysteries of the soul?

It was not a fruitful effort, however, for while Wundt may have contributed greatly to conventional psychology, his soul-searching technique of introspection could not and did not contribute anything to the development of a natural science of psychology.

The theory building practices of conventional psychologists also differ from those of the natural scientist. For the natural scientist, the articulation of a law or principle amounts to making a general statement about many observations of similar phenomena. Principles derived in this manner constitute descriptions of events. For example, the physical law of gravity does not explain why bodies fall; it is simply a statement of the fact that they do. Conventional psychologists, on the other hand, are free to infer the involvement of non-observables in their theoretical constructions. After all, non-observables are an acknowledged aspect of the conventional psychologists'
subject matter. The involvement of such events, moreover, is usually taken to be one of explanation. For example, remembering events are explained by way of the "memory", and imagining is attributed to the "imagination."

The difference between conventional psychology and the natural sciences, with regard to the above described fundamental features of the scientific enterprise, earns for conventional psychology the status of a nonscience. Why psychology should aspire to the status of a natural science is another issue. Essentially, it is an issue of purpose. The question arises: What is the goal of psychology and can it accomplish this objective as a nonscientific enterprise? In as much as psychology is the study of interactions between responding organisms and stimulating environments, presumably the goal of this study is to make known what is still unknown about such interactions. Increasing our knowledge of the world is clearly a scientific goal, however, and to assume that such a goal may be attained in total disregard for the descriptive, investigative, and constructional processes of the scientific community implies a gross misunderstanding of the relation between process and product. Products are simply phases of process. In short, conventional psychology has set for itself a task impossible to accomplish. And with a cognitive revival in psychology underway, we may look forward with alarm and disappointment to the continuation of an inconsequential enterprise.
Future

It is possible to imagine another future for psychology. It may begin to take notice of the events with which it actually deals. It may begin to describe these events rather than the way psychologists operate upon them. In so doing may be abandoned the causal constructions of dependent and independent variables in favor of the total field in which all constituents may be recognized and acknowledged as participating factors. It may abandon its age-old commerce with a spurious metaphysical philosophy, and therein discover one of genuine utility. Psychology may earn the status of a natural science.
1-See Chapters V and VI.

2-When these have not been articulated they may be inferred from other scientific practices.

3-When premises are not stated explicitly, they may be derived from specific analyses of psychological events.


5-For example, see Skinner, 1950, p. 71.

6-Epstein's introduction to Notebooks by B.F. Skinner states that it was their intention to publish notes that "seemed to make some positive contribution—in elaborating or clarifying Skinner's work, in exploring new territory, or in shedding light on Skinner himself," (p. xii). Advertisements for the book also indicate that it is a serious work. Hence, despite the relative informality of this work, it seems legitimate to quote passages from it for the purpose of illustrating Skinner's position on various issues.

7-As well as old ways of thinking about thinking, as discussed in the preface to the 1969 book.

8-Knowledge refers to reinforcement history as interbehavioral history, as is apparent in current behavior. The implication that an organism possesses knowledge as a cognitive entity is not intended by the use of this term. Present usage is consistent with that of Skinner (1953, p. 408-410) and of Kantor (1924, p. 398).

9-These philosophical categories may be conceptualized as rules.

10-I am indebted to Rosalind Burns for her help in clarifying this issue.

11-Unclear by this exposition is the status of auditory stimuli arising from movements of the vocal apparatus. While these stimuli do not technically arise from the surrounding environment—the criterion by which public stimuli are distinguished—it cannot be argued that such events are impossible of observation by an external observer—the criterion by which private events are distinguished.

12-Nonverbal behaviors of unclear dimensions are also discussed (Skinner, 1953, p. 273).

13-An alternative interpretation of what Skinner intends here is that we react not to the forms themselves (in whatever dimensions they exist)
but to the contingencies responsible for them. This interpretation does not solve the problem of what event is, in fact, being reacted to since the contingency is not among the events available for a reaction until a particular form is available to make contact with this contingency.

14 Skinner is not entirely consistent on this issue. For example, in 1969 (p. 226) he argues that the psychological problems of private events will never be resolved on physiological terms. However, the position most often taken is the reverse. (See Skinner, 1953, p. 282 as an example.)

15 Michael (1982) claims that Skinner's position on the causal status of private events is not that these events lack causal properties, but rather that they have no special causal status. The implication of this claim is that causality is not conceptualized by Skinner as being something other than the reinforcing, eliciting, and discriminative properties of stimuli. I think Michael is correct in arguing that Skinner's position could be interpreted in this manner. However, I do not believe that Skinner addresses the more fundamental issue of causality or the difference between causality and functionality. In fact, had he addressed these issues explicitly, it would not have been possible to interpret his position as one in which causality is reserved for public events only. Moreover, that multiple interpretations of Skinner's views on complex issues are possible provides an illustration of one of the major themes of the present paper. Specifically, that harmony among the various aspects of a scientific enterprise is a product of system building efforts. Failure to construct a satisfactory postulational foundation for a science, as is evident in a lack of clarity on the issue of causality, leads to inconsistency in theoretical extensions—as is evident in multiple interpretations of Skinner's views as to the causal status of private events.

16 Behavioral effects are produced by prior stimulus causes even when the stimulus in question follows the response since consequences are not said to have an effect on the response they follow but on future responses of the same class.

17 The newer functional vocabulary is also characteristic of contemporary physics and chemistry.

18 It is in accordance with such understanding that behavioral psychologists describe their discipline as the science of behavior as opposed to behavior-stimulus relations.

19 Considerable attention is paid to the differences between contingency shaped behavior and rule governed behavior is both About Behaviorism and Contingencies of Reinforcement.

20 An exception is discussed in About Behaviorism, p. 126. Skinner
argues that imitation is a version of rule following under the control of an incomplete rule which specifies only the response to be executed.

21 This simultaneity of responses applies only when referential responses are executed by the vocal apparatus. Moreover, simultaneity may be prevented if the nonreferential and referential activities are both executed with the same musculature, i.e., talking and flute playing.

22 It is this fact that has led Skinner to note that the control exerted by rules is more conspicuous that that exerted by contingencies (1969, p. 170).

23 Skinner does address a similar issue in the context of instruction (1957, pp. 357-362). However, an examination of the specific problems encountered in the analysis of rule governance does not appear to have been attempted.

24 Other means of association are also possible. These other means of association actually amount to the relations between parts of objects and auditory stimuli or other more remote referential operations. For instance, a listener could follow the rule about the umbrella even if this person had never had any form of perceptual contact with an umbrella, provided he had had previous perceptual contact with such things as canopies, spokes, and canes, which had been called by name in his experience and these names, in turn, had been associated with the auditory stimulus "umbrella."

25 The psychic powers or processes originally ascribed to the soul are no less evident in contemporary treatments of the brain.

26 Such practices are often defended by what might be called the "useful hypothesis" argument. In essence, mental processes and the like are held to be legitimate constructs if believing in their existence proves useful. This argument is not convincing since it is highly improbable that the objectively non-existent may prove useful in the accumulation of objective knowledge.
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