

THE ORIGINS OF ENVIRONMENT-BASED PSYCHOLOGICAL THEORY

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One of the most impressive aspects of B. F. Skinner's *The Behavior of Organisms*, as a 50-year-old book, is its introducing features of theory that continue to be among the most subtle to be found in psychology. Although they seem not to be widely understood among contemporary psychologists, some of these subtleties have been appreciated throughout the years by those working within the tradition that developed from Skinner's work. A fundamental example is seen in Skinner's characterization of the reflex—the presumed simplest of functional units—as a relation between classes of events rather than as bundled connections between individual stimuli and responses. This conception construes the reflex as intrinsically abstract while directly observable; the more usual conceptions of the reflex treat it as tangible mechanism, although that mechanism is usually inferred rather than observed. In amplifying his conception, Skinner asserts: "In general, the notion of a reflex is to be emptied of any connotation of the active 'push' of the stimulus. The terms refer here to correlated entities, and to nothing more" (Skinner, 1938, hereafter designated as *B of O*, p. 21). Thus, in its very beginnings behavior-analytic theory moved beyond the primitive metaphors of billiard-ball causation or of organism as purely reactive machine. Similarly, Skinner's version of the reflex is at least as abstract as the mental connections that other psychologies have taken as basic.

Even though they were clearly stated at this beginning, other subtleties have been less appreciated even by Skinnerian theorists. For

example, in introducing the operant, Skinner included a new way of incorporating time into descriptions of psychological process. He provided this matter-of-fact introduction:

One important independent variable is time. In making use of it, I am simply recognizing that the observed datum is the appearance of a given, identifiable sample of behavior at some more-or-less orderly rate. (p. 20)

Although Skinner explicitly identified the use of rate as distinctive of his approach, most of us have only slowly come to understand its implications. Among these:

1. Behavior analysis is essentially the study, definition, and characterization of effective environments as arrayed over time, with "effective" defined by the dynamics of behavior. That is, in behavior-analytic theory the world is characterized through categories of transaction with behavior. Those categories do not consist of punctate, individual events; rather, they are sets of contingent relations or correlations between events or patterns of events over time.

2. Psychological process is construed as behavior-environment interaction. It does not consist in phenomena that underlie that interaction. At the same time, this is not a psychology of empty organisms. As will be described below, Skinner explicitly acknowledges the legitimacy of physiologically based explanations of behavior, although he requires that physiological interpretive terms be supported by relatively direct experimental observations; a purely metaphorical physiology is not essentially different from mentalistic metaphor.

3. Rate, although comprised of tangible, observable events, is an abstraction. You can look right through a rate; that is, it can be going on right now, even though none of the events that comprise the rate is occurring at this moment. Thus, interpreters of human action commonly ignore diffuse but directly

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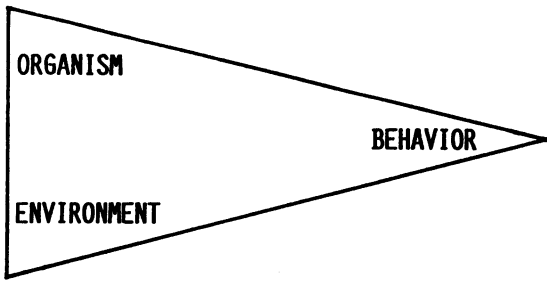


Fig. 1. Organism-based versus environment-based theorizing in simplistic form.

observable events and instead appeal to impalpable entities, such as mental or presumed physiological processes, as causing overt behavior. Apparently the fact of seeming to be localized in time and space (right now; behind the eyes) makes those presumed events seem to provide for more satisfactory explanation. Therefore, Skinner's persistent criticisms of mentalistic explanation have continued to inspire controversy.

Interpretive Directionality in Psychological Theories

But Skinner's focus on mentalistic versus antimentalistic explanation as a key issue of contention may have obscured a more fundamental, yet more tractable, point of disagreement between his and more conventional viewpoints. The essential differences of interpretation may concern not the status of hypothetical terms for explaining behavior, but rather a dimension of interpretive prose that, just like rate of occurrence, is right before our eyes but is seldom noticed. The key issue may concern not mentalism, but rather the directionality of interpretive talk.

Interpretive or explanatory prose seems to be intrinsically bipolar, whether it be cast in terms of cause-effect, of noun-verb, of independent variable-dependent variable, or of agent-action. This bipolarity, then, constrains psychological theory to one of two general types: In one, interpretive statements take the form of organism-behavior locutions; in the other, the form is that of environment-behavior locutions. Heated disagreement or misunderstanding occurs when proponents of one theoretical type address interpretive contributions of the other type, as illustrated by the familiar and venerable "nature-nurture"

controversies in which both organism and environment are construed in the most general terms, as illustrated in Figure 1. Of course, it was recognized long ago that the controversy of "heredity or environment" arose substantially from a badly posed question: A more valid and productive form of the question would address the nature of interaction between phylogenetic and ontogenic contributions to behavior. Even though we presumably understand this, questions of nature versus nurture occasionally reappear in simple form, and when they do, they are argued with vehemence—as in some of the controversies surrounding sociobiology or the origins of linguistic functioning.

A constructive consideration of the two modes of theorizing requires that each be presented in greater detail. A first step is illustrated in Figure 2, which shows that both organism-based and environment-based accounts acknowledge a current environmental context as relevant to behavior. Organism-based accounts attribute behavior to the characteristics of (or processes within) the organism acting in the context of that situation. An environment-based account, such as that introduced by Skinner, gives a more salient role to immediately eliciting or occasioning stimuli; however, the primary environments of environment-based theory are *past* environments, for the roles of the present stimuli are seen as dependent upon the organism's prior history. Even an insensitivity of behavior to immediately attendant stimuli is attributed to past history. Some organism-based theorists have either ignored or misunderstood this fact in asserting that behavior lacking immediate environmental causes constitutes an embarrassment to environment-based accounts.

Still, Figure 2 does not give sufficient detail to enable much comparison of the two viewpoints. Figure 3 indicates the types of elaboration that are needed to handle specific cases. As shown, organism-based theory posits inferred processes within or characteristics of the organism, typically in the form of representations, expectations, physiological states, or personality characteristics. Perhaps less obviously, the "environmental history" of an environment-based account is not simply a history of past environments, but is instead a history of behavior-environment interactions and stimulus-stimulus relations with respect

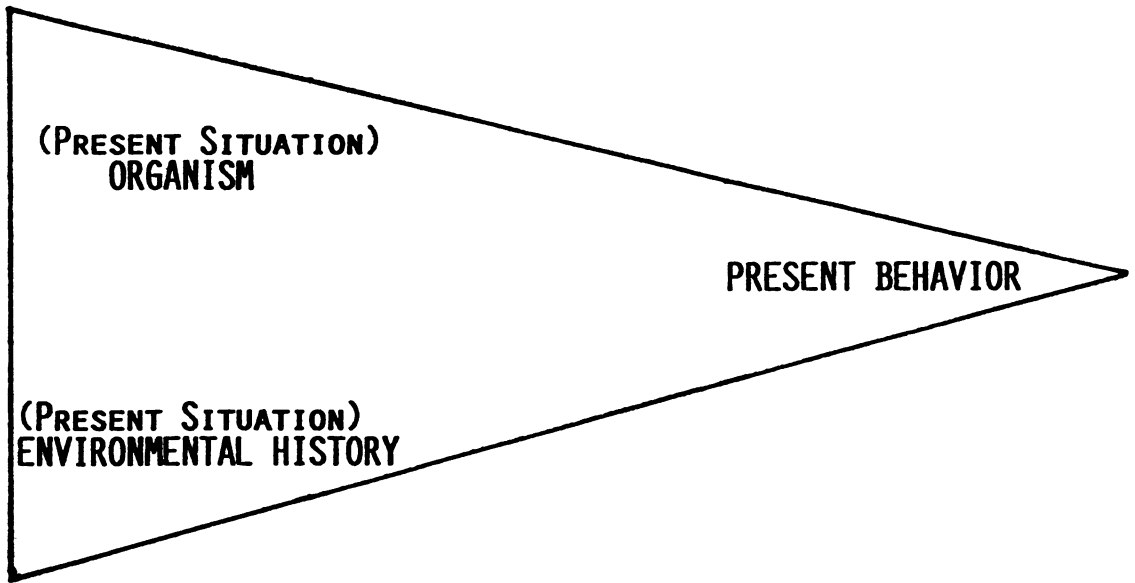


Fig. 2. The place of "present situation" in two types of theorizing.

to behavior. A history of behavior–environment interactions is what is meant by Skinner's often-used phrase "contingencies of reinforcement," although it should be emphasized that the environment-based account invokes additional principles besides reinforcement.

Presenting the two viewpoints as complementary may obscure their potential for inducing the vehement interpretive disagreements that we experience. Nevertheless, the differing directionalities, although simple or subtle in themselves, play out in drastically differing research styles and strategies, accompanied by different assumptions regarding what constitutes a sound experiment or a legitimate explanation. First—and not to be underestimated when theorists of the two traditions evaluate each other's work—is the virtual inevitability of each slighting the other's favored causes. That is, in an environment-based account the organism is treated as the locus where environment and behavior interact. To the extent that particular characteristics of the organism are acknowledged, they have the status of background parameters.² Particular environment–behavior re-

lations may hold for particular species or, within species, for individuals of particular gender or age. In contrast, organism-based theory acknowledges past environments only indirectly, as the implicit sources of encoded representations or as the origins of expectations that are said to produce the immediate behavior. Thus, to an environment-based theorist, the expectations and representations of an organism-based account are merely redescrptions of history. Other environmental variables also are converted to components of the organism, as in deprivations or other more enduring conditions typically characterized as motivational states. Although each type of account also includes an evolutionary component, one construes it as genetic code, and the other construes it as environments on a vastly expanded time scale.

Second, the two types of interpretation are bound to disagree regarding the status of behavior. For an organism-based theorist, behavior is primarily an index of processes within the organism, whether those processes are construed as inferred physiological events

² This also takes the form of omitting agency of person, and thus is interpreted as denying personal responsibility and the like. For an example of an interpreter applying

this to himself with respect to his own behavior, see Skinner (1972). At the same time, when we speak of responsibility, we usually are concerned with appropriate consequences for action, which is a primary focus of environment-based accounts.

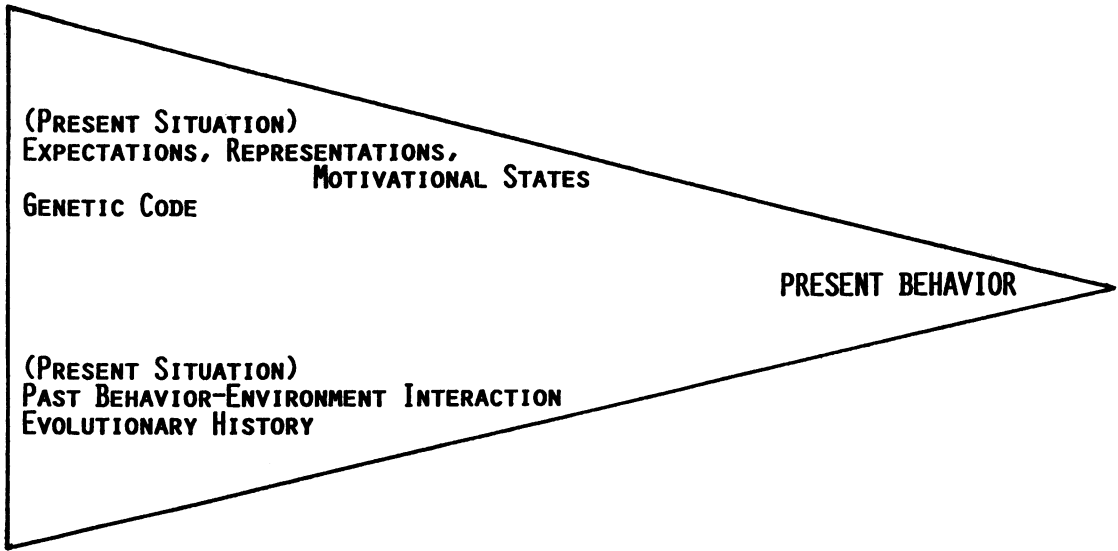


Fig. 3. Elaborations of organism-based and environment-based interpretation.

or as metaphorical mental processes. In contrast, for environment-based theory, behavior is the main focus rather than an index of something else: Descriptions of behavior-environment interaction comprise the theory. This is the meaning of Skinner's phrase, "A science of behavior." It follows that the two types of theory also disagree regarding what constitutes process. For organism-based theorists, process is said to underlie behavior and to be located within the organism. For environment-based theorists, process consists *in and of* the interplay between behavior and environmental events. Behavioral process, then, is extended over time; by implication, there is no one, instantaneous "psychological now" within which the causation of behavior must occur.

This last point identifies a third characteristic difference, although it may not be a logically necessary one, between the two types of interpretation. Organism-based theorists tend to assume that contiguity is a necessary feature of causal relationships—specifically, that the causes of an action must be immediately attendant to that action. Environment-based theorists accept functional relations as sufficient to an explanatory account even though the functionally related events are separated in time.

Finally, some organism-based accounts dif-

fer from environment-based accounts in their appeals to underlying physiology. In this context it should be noted that, from *B of O* onward, Skinner has consistently allowed a physiological and thus a reductionistic/mechanistic account of behavior. But he points out that the reductionistic sequence does not stop with physiology:

I am not overlooking the advance that is made in the unification of knowledge when terms at one level of analysis are defined ('explained') at a lower level. Eventually a synthesis of the laws of behavior and of the nervous system may be achieved, although the reduction to lower terms will not, of course, stop at the level of neurology. The final description will be in terms of whatever quasi-ultimate physical units are then in fashion. (*B of O*, p. 428)

To be sure, in Skinner's view the neurological, organism-based account is a distinct explanatory domain—properly construed more as part of biology than of psychology. In addition, in the chapter that addresses physiology and behavior, Skinner explicitly denies any primacy of physiological accounts, and by implication, of organism-based theory:

What is generally not understood by those interested in establishing neurological bases is that a rigorous description at the level of behavior is necessary for the demonstration

of a neurological correlate. The discovery of neurological facts may proceed independently of a science of behavior if the facts are directly observed as structural and functional changes in tissue, but before such a fact may be shown to account for a fact of behavior, both must be quantitatively described and shown to correspond in all their properties. (*B of O*, p. 422)

Several contemporary theorists (e.g., Rosnow & Georgoudi, 1986; Sarbin, 1977) have found it useful to characterize psychological viewpoints in terms of their "root metaphors," as delineated by Pepper (1942). According to that rubric, organism-based theories are either mechanistic or organismic in nature, whereas environment-based theories may be mechanistic or contextualistic. The interpretive tradition that originated in Skinner's work is well characterized as a thoroughgoing contextualist account (Hayes, 1986; Hayes, Hayes, & Reese, 1988; Morris, 1988). Indeed, in its purest form, behavior-analytic theory is contextualistic to the extent of virtually excluding mechanistic metaphors. Hence, although one can identify complementarities between the concepts of behavior analysis and those of competing theories (e.g., Catania, 1973; Segal, 1977), any attempt at rigorous integrative theory based upon these complementarities risks foundering on the incompatibility of fundamentally conflicting metaphors. As pointed out by Hayes et al., Pepper's analysis implies that eclectic combinations of elements from distinct root metaphors will be inherently confusing. Hence in the present essay the two interpretive modes will be treated as distinct.

Interpretive Directionality in Ordinary Language

The above characterizations have described two interpretive modes as applying to formal psychological theories. However, most psychological theorizing involves ordinary as well as special language. Consequently, it may be important to examine similar interpretive differences as they occur in everyday language in order to understand a major component of the clash between behavior analysis and other viewpoints. The crucial characteristic is still directionality of interpretive talk, and a most important fact is that in ordinary conversation we all speak comfortably in both

directionalities. Furthermore, each directionality seems usually to occur without apparent prejudice to the other, apparently because there are distinct occasions that are conventional for each. Delineating those occasions may reveal some likely origins of the thoroughgoing environment-based account that originated in the laboratory research described in *B of O*. That delineation may also enable a new understanding of the controversies that arose when that account was addressed to issues beyond the operant laboratory.

Social psychologists have taken note of the two interpretive modes in ordinary language, under the rubric of *attribution theory*. For example, Heider (1958) identified the two modes as persons using either situational factors or dispositional factors in accounting for human actions. Kelley (1967) formalized and elaborated the sets of factors contributing to the two modes in terms of a covariance model, asserting that a person executes a sort of intuitive analysis of variance in adopting one or the other type of account. (Note that attribution theory is itself a predominantly organism-based account, whose central points are couched in terms of dispositional factors. For a clear illustration of this point, which will be of special interest later, as well as for a clear and systematic introduction to that theory, see Ross, 1977.) Thus, for example, observers of behavior that counters the situational demands of its immediate context tend to attribute that behavior to internal or dispositional characteristics of the actor (Jones & Davis, 1965). More important for our present concerns are factors that Kelley identified as contributing to external attributions. Prominent among these were (a) *distinctness of entities*, which denotes the strength of correlation between particular environmental events and the action; (b) *consistency over time*, referring to changes in those entities over time, and to whether presence versus absence of the entities corresponds to occurrence versus nonoccurrence of the behavior in question; (c) *consistency over modality*, addressing the degree to which the entity's correlation with behavior is robust over a range of variations in the entity; and (d) *consensus*, whether other people make similar attributions regarding an external entity. In Kelley's model, which seems representative of attribution theory, positive relations of these four types should

result in external attributions—that is, in environment-based interpretations of behavior.

The strategy of experimentation portrayed in *B of O* is strikingly well described by factors (a), (b), and (c) identified above. Skinner's style of research—extensively and systematically manipulating entities through repeated presentations and removals of experimental conditions; evaluating results over time by means of an innovative recording technique, the cumulative record; replicating results in detail with several individual subjects—all of this, according to attribution theory, should lead inexorably to an environment-based account. Furthermore, Skinner's own description of the formative phases of that work (Skinner, 1956) indicates that the research strategies evolved through interplay among data, apparatus, and experimenter's behavior; they did not begin with an ideological stance. If Kelley had been in a position to observe Skinner at work, he would have described Skinner's research and its interpretation as entirely consistent with the predictions of attribution theory.

Behavior-Analytic Theory from the Viewpoint of Attribution Theory

If these aspects of attribution theory are correct, virtually anyone who closely followed such experiments from beginning to end should, as a member of the vernacular culture, tend to give an environment-based interpretation. Why, then, is Skinner's approach so controversial? Can the controversy still be understood in terms of attribution theory and the directionality of interpretive prose? There are several bases whereby it can indeed, for although Skinner's interpretations appear to be culturally typical in the context of the experimentation from which they emerged, they are not culturally typical as applied to other situations.

First, Skinner's experiments were not like the statistically based group designs that have typified most of psychology. Although a co-worker or even naive observer given extended exposure to the same sequences of observations would give causal status to the environmental manipulations of the experiment (thus appealing to situational factors when explaining the subjects' behavior), most psychologists were (and are) accustomed to interpreting

data in very different circumstances. Typical experiments in psychology are based upon brief observations of many subjects—circumstances that favor internal or dispositional (organism-based) interpretations. Most readers of Skinner's type of interpretation have not been exposed to the types of circumstances that occasioned his interpretations. Furthermore, a psychologist thoroughly immersed in a strongly differing tradition of research and interpretation may even have difficulty in understanding Skinner's type of experiment.³ Thus, the directionality of Skinner's interpretations is unconventional when offered separate from the extended context of his experiments, in just the way that his experiments were unconventional, but the former lack supporting observations.

A closely related point: Attribution theory is presented as an attempt to characterize the intuitive psychologizing of "the person on the street" (or the equally naive college sophomore), rather than that of the psychologist trained in scientific method. The present thesis is that attribution theory applies to formal psychological theorizing as well, but it must be recognized that there are major differences between the specific occasions of the professional and of the "intuitive" psychologist's interpretive activity. In the interpretations that the theory typically addresses, experimental subjects are asked to account for acts in particular, isolated sets of circumstances. A coherent, general interpretation, independent of particular situations, is neither solicited nor examined. In contrast, Skinner's consistent program of experimental manipulations led to a systematic set of interpretive principles, which were then applied to other subjects' actions in other situations. Of course this is the standard way for a scholarly psychologist to proceed. But in Skinner's case, this meant that his type of interpretation, although culturally normal in the context of the scientific experiments that occasioned it, was offered on occasions in which that type of interpretation was not culturally normal.

Thus, perhaps the interpretations of individual experiments were not what was con-

³ Brady (1987) recounts a clear instance of this, describing the perplexity of a distinguished researcher in animal behavior (and President of the American Psychological Association) when visiting a laboratory that used Skinner's research strategies and techniques.

troversial; instead, the generalizing of those interpretive principles beyond the experiments was the main provocation. Why this should be can be understood in terms of some other characteristics of ordinary interpretive language identified by attribution theory.

A second major factor identified by attribution theorists describes behavior-analytic interpretation as more distinctly countercultural. In a widely cited essay, Jones and Nisbett (1971) assert that attribution is strongly affected by the relation of the interpreter to the action in question. That is, if one is interpreting one's own action, one tends to speak or write in environment-based locutions. On the other hand, if one is observing someone else's action, one tends to interpret it in terms of characteristics of the actor. Even though the tendency of an observer of an action to give disproportionate credence to dispositional factors has been characterized as "the fundamental attribution error" (Ross, 1977), attribution theorists themselves conform to this cultural convention. For example, Kelley's (1967) analysis-of-variance model is construed not as identifying the variables that control directionality of interpretive talk but rather as characterizing the interpreter's intuitive process of inferring causation. Ironically, and fundamental error or not, the cultural convention that Jones and Nisbett expose in their article is illustrated by their own exposition; they describe that convention in terms of *biases* on the part of the actor and on the part of the observer.

To be sure, any serious theorist states the interpretations of action from the viewpoint of observer: Objective psychology is the "science of the other one." As attempts at such, most psychologies conform to the cultural norm that Jones and Nisbett (1971) identify, as observers giving organism-based interpretation. Skinner's interpretive stance also conforms to the standard "other-one" convention of serious psychological theory. However, in so doing, Skinner offers, from the viewpoint of observer, an account whose directionality would be culturally conventional only if it were offered from the viewpoint of the actor. That may explain why behavior analysis has always been outside the mainstream of psychology. (For evidence of the nonmainstream status of behavior analysis even during its presumed heyday, see rem-

iniscences of the founding of a behavioral journal, Himeline & Laties, 1987.)

Finally, the factor of "consistency," identified in Kelley's (1967) covariance model, seems to have a pervasive but mostly unnoticed role in controversies surrounding Skinnerian interpretations. Its subtle but determining role in directionality is well illustrated by the following two statements:

I have carried out an extensive set of observations, with myself as subject, and have discovered that wine is stronger than beer. Based on a comparable set of observations, I have discovered that I am more allergic to tulips than to roses.

Logically, the choice of an environment-based description in the first case and of an organism-based description in the second is arbitrary: The potency of a beverage is one's susceptibility to it, and one's allergy to a flower is its potency to induce sneezing and related behavior. Furthermore, although each potency or susceptibility has a recognized underlying physiological mechanism (one involves neurotransmitters and the other involves the immune system), it is unlikely that these are the bases of the differing directionality, for the verbal patterns surely antedated the understanding of those mechanisms. Even today most people could not identify the difference of mechanism, but virtually all would engage in the same attributional patterns.

The basis for the differing attributions, and for the interpretive directionality that flows with them, is more evident if we convert each statement to culturally atypical form:

I have carried out an extensive set of observations, with myself as subject, and have discovered that I am more easily intoxicated by wine than by beer. Based on a similar set of observations, I have discovered that tulips are more allergenic than roses.

Both the intuitive and the professional psychologist are likely to find something odd here, reacting to the first by remarking: "This is news?" and to the second: "Speak for yourself!" (Note that the evident sarcasm of these reactions might constitute mild punishment of the culturally atypical attributions if the reactions were overt.) Kelley's (1967) "intuitive analysis of variance" version of attribution theory identifies the key dimension here. That

is, a half-liter of wine is more potent than the same quantity of beer for virtually everyone. In contrast, tulips produce sneezing in some people but not in others. Within the culture, then, occasions in which everyone is affected similarly are occasions for one directionality of causal description: from environment to behavior. In contrast, occasions in which individuals are affected differently are occasions for the other directionality: from disposition to behavior. Applied to Skinnerian theory, this identifies an irony: Even though Skinnerian experimentation focuses upon the behavior of individuals rather than upon groups of individuals; even though Skinnerian prescriptions for educational or therapeutic practice are necessarily tailored to the particular needs of the individual; even though Skinnerian interpretation explicitly declines to blame the individual when addressing antisocial behavior or failures to learn—these are all stated with a directionality that, in the verbal practices of the culture, is occasioned by situations in which everyone is alike. This, then, may be a key factor in reactions to Skinner's theory as "dehumanizing" in its more popularized versions, and as non-mainstream in the domain of learning theory.

Both the social psychologist and the nonpsychologist, then, are likely to react to behavior-analytic interpretation as insensitive to a very important dimension in the texture of social discourse. In its own technical terms, behavior-analytic interpretation risks being reacted to as a failure of stimulus control. Just as a person may react with defensive apprehension to the "word salad" of schizophrenic speech while unable to specify what is strange or threatening about that speech, that person may also act as if threatened by Skinnerian interpretation.

Conventional and Unconventional Attributions in The Behavior of Organisms

To summarize this behavior-analytic rendition of attribution theory's account of behavior analysis: Both organism-based and environment-based interpretations of behavior are standard vernacular repertoires, but they normally occur in different types of situations. Skinner's thoroughgoing environment-based interpretations of his early experiments were culturally conventional in the context of the type of experiments that he did. Also, it was

scientifically conventional to develop individual experiments into an integrated body of work, with the resulting interrelated concepts then extrapolated to account for behavior outside of as well as within experimental settings. Yet many of Skinner's extrapolations, and those of behavior-analytic theory more generally, offer environment-based interpretations of behavior for situations in which organism-based interpretations are the firmly entrenched cultural pattern. Arguments supporting those interpretations have focused on claims of "objectivity," or on the pragmatic superiority of environmental variables over hypothetical constructs. But the cultural resistance to Skinnerian explanations (and in turn, to behavior-analytic approaches to problems in the world at large) may arise from this other dimension that is not addressed by those arguments. That is, the source of resistance to a behavior-analytic approach may be the largely unnoticed but unconventional directionality of attributions that are characteristic of that approach.

Sometimes this unconventionality may be reacted to simply as explanation insensitive to its occasioning context, as in the observer's directionality of interpretation that, according to Jones and Nisbett (1971), is culturally appropriate only from the stance of actor. In other cases it may be reacted to as implicitly asserting that all people are alike, when vernacular explanations appeal to distinct inferred characteristics of each person. Thus, from the viewpoint of attribution theory it is remarkable that the resulting approach could ascend to prominence and endure within a culture whose interpretative patterns are so thoroughly at odds with it. Given the massive inertia of cultural conventions, the manner in which behavior analysis was introduced may have been crucial to its gaining a foothold; thus, its early introduction as well as the circumstances of its development bear examining in terms of the relationships that have been identified here.

With the publication of *B of O*, the distinctive characteristics of Skinner's empirical approach were substantially developed and in place. Close examination of that book reveals, however, that Skinner's initial theorizing was not entirely pure in its environment-based stance. In addition, a joint focusing of attribution theory and some principles of

behavior analysis, both addressed to the expository sequencing in *B of O*, provides side-lights that may illuminate how that environment-based theory occurred to its progenitor as well as how it was introduced to others.

The book opens with several prefatory paragraphs, the first stating Skinner's criticism of the common practice of granting mentalistic concepts preferential explanatory status presumably because they are internal to the behaving organism and thus inaccessible to disproof. The foregoing discussion suggested that the differences between organism-based and environment-based interpretation can be framed more generally. The second paragraph, however, fits hand in glove with the present account in addressing the status of behavior with respect to interpretation: Organism-based theories treat behavior as mere index of process, rather than as psychological phenomenon in and of itself. Next, Skinner explicitly rejects vernacular terms "that imply conceptual schemes," although he otherwise favors ordinary rather than technical language. One can infer from his examples that such schemes appeal to an organism's agency, or to intentionality, as initiating or guiding its actions. For Skinner, the basic role of acceptable language is characterized as narration ("a running account of a sample of behavior as it unfolds itself in some frame of reference . . ." p. 8). His is to be a "descriptive theory," beginning with the identification of analytical units rather than theoretical constructs. The reflex is offered as an example, and he is on his way.

Conceptually, the unconditioned reflex is an obvious starting point as a likely simplest unit of behavior. Furthermore, Skinner's seminal papers during the early 1930s can be seen as attempts to extend a basic reflexology that already had ample precedent in the recognized physiology of behavior and in Watson's behaviorism (Coleman, 1984). We shall see that the flavor of classic reflexology lingers in many pages of the 1938 book. However, even in a treatise whose main thrust is to depart from pure reflexology, the reflex also may be an effective starting point for a purely expository reason: By definition, unconditioned reflexes are features that all members of a species have in common, and so it is culturally conventional to give an environment-based account of them. A puff of air

or a bit of dust in the eye is said to produce an eye blink; one is not tempted to appeal to the organism for initiation of such behavior. Thus Skinner's pattern of interpretation is culturally conventional as he enumerates the "static laws of the reflex"—threshold, latency, magnitude, after-discharge, and temporal summation. These are described as general principles rather than as specific empirical results; it is uncontroversial that they give salience to environmental determinants, for it is well established that they are essentially uniform across individuals. Next come the dynamic laws, which include two—facilitation and inhibition (of the kind that Pavlovians would call *external inhibition*)—that give as much salience to stimuli as to responses. Other dynamic laws—refractory phase and reflex fatigue—give greater emphasis to responses than to their eliciting stimuli and might induce one to appeal to fluctuations within the organism. But these are still characteristic of all individuals, so again most any reader will find environment-based interpretation appropriate. Those initial pages of exposition build upon the fact that all readers have repertoires of environment-based interpretation by describing situations that are normal occasions for those repertoires. With the environment-based pattern of interpretation established through varied but conventional repetitions, the reader is prepared for the subsequent process of "fading" by successive steps—maintaining environment-based locutions while gradually introducing features that normally would occasion organism-based ones.

Continuing with dynamic laws of the reflex, Skinner briefly describes Pavlovian conditioning (denoted as Type S), which introduces hints of differences between individuals. But even here, in the conditioned reflex, the form of response does not differ between individuals; only the particular eliciting stimuli that produce it are individualized. Furthermore, conditioned as well as unconditioned reflexes inherently involve immediately attendant stimuli that qualify as "entities" in an attributional account, and these support the continuing environment-based interpretation. Then, operant conditioning and extinction are added to the set of dynamic laws, but these phenomena are introduced in a way that minimizes the fact that they might induce

different individuals to act differently. Skinner construes the defining feature of operant behavior as the absence of eliciting stimuli, so it is important that the continuing environment-based interpretation is supported by a language of "everyone is similar," even though the similarities are now based on *absence* of environmental events proximal to the behavior at issue.

With that much in place, and more than 20 pages into his account, Skinner interrupts the gradual succession, venturing to deal with relationships for which organism-based theorizing would be far more conventional:

The operations characterizing drive and emotion differ from the others listed in that they effect concurrent changes in *groups* of reflexes. The operation of feeding, for example, brings about changes in all the operants that have been reinforced with food and in all the conditioned and unconditioned respondents concerned with ingestion. Moreover, a single operation is not unique in its effect. There is more than one way of changing the strength of the group of reflexes varying with ingestion or with an emotional stimulus. In addition to the formulation of the effect upon a single reflex, we must deal also with *the* drive or *the* emotion as the "state" of a group of reflexes. This is done by introducing a hypothetical middle term between the operation and the resulting observed change. "Hunger," "fear" and so on, are terms of this sort. (*B of O*, p. 24)

In defending his inclusion of these terms, which seem inconsistent with the main thrust of his approach, Skinner acknowledges the need for meticulous parsimony in their use:

In the present system hypothetical middle terms ("states") will be used in the cases of drive and emotion, but no other properties will be assigned to them. A dynamic law always refers to the change in strength of a single reflex as a function of a single operation, and the intermediate term is actually unnecessary in its expression. (*B of O*, p. 24)

He is arguing, then, for confining the status of such intervening terms to that of intervening variables—summary terms for environmental contributors—rather than of hypothetical constructs, which impute mediating events within the organism. This distinction was clarified a few years later by MacCorquodale and Meehl (1948).

However, the next term that is introduced in *B of O*—the reflex reserve—has the distinct flavor of hypothetical construct. The term is defined initially as a property of the behavior itself, or of the environmental operations that affect behavior:

... we may speak of a certain amount of *available activity*, which is exhausted during the process of repeated elicitation and of which the strength of the reflex is at any moment a function. . . . In one sense the reserve is a hypothetical entity. It is a convenient way of representing the particular relation that obtains between the activity of a reflex and its subsequent strength. . . . The notion applies to all operations that involve the elicitation of the reflex and to both operant and respondent behavior, whether conditioned or unconditioned. (*B of O*, p. 26)

But there immediately follows the suggestion of an underlying process distinct from the behavior to be accounted for:

One distinction between an unconditioned and a conditioned reflex is that the reserve of the former is constantly being restored spontaneously, when it is not already at a maximum. In the particular case of reflex fatigue, a spontaneous flow into the reserve is evident in the complete recovery from fatigue that takes place during rest . . . In conditioned reflexes the reserve is built up by the act of reinforcement, and extinction is essentially a process of exhaustion comparable with fatigue. The conception applies to both types of conditioning and leads to a much more comprehensive formulation of the process than is available in terms of mere change in strength. (*B of O*, p. 27)

This is more than a momentary lapse, for in the continuing discussion of reflex reserve, we find that behavior has been displaced from primary focus. It is now a barely mentioned index of underlying process:

In a phasic respondent the refractory phase suggests a smaller subsidiary reserve which is either completely or nearly completely exhausted with each elicitation. This subsidiary reserve is restored from the whole reserve, but the rate of restoration depends upon the size of the latter. Thus, during the fatigue of such a respondent the refractory phase is progressively prolonged. (*B of O*, p. 28)

Thus we see a dramatic departure from purely environment-based theory and an illustration

of changes that attend such a shift: Even in Skinner's own prose, inclusion of intervening "states" makes behavior mainly an index of underlying process.

Immediately after introducing and briefly discussing the reflex reserve, the introductory chapter of *B of O* returns to environment-based interpretation. Now the progression is to situations of greater complexity, where there might be greater variability over time or between individuals. These are handled by describing principles of interaction between reflexes when more than one is simultaneously operative, either in combination or in competition. No new initiating principles are required for the more complex or variable behavior; purely combinatorial principles appear to suffice, and so the environment-based pattern is maintained in situations that have still greater tendency to induce the organism-based one.

The final section of the first chapter and the first part of the second are more complex expository sequences that might be better characterized in terms of conventional logic rather than in terms of the simple dimension of interpretive directionality. The exposition of basic principles is punctuated by asides that address a variety of tangential but crucial issues, anticipating criticisms of Skinner's position that have persisted over the decades since *B of O* was written. Among these are accounting for novel behavior by showing how it is accommodated in specifying even the most basic behavior-environment relations (for elaboration of this aspect of "The Generic Nature of the Concepts of Stimulus and Response," see Coleman, 1984, and Hineline & Wanchisen, 1989); acknowledging biologically based separations ("natural lines of fracture") between functional units of behavior; acknowledging and justifying the emphasis upon operant rather than respondent behavior; pointing out limitations and distortions that result from adopting vernacular concepts into scientific language; taking a position on the issue of molar versus molecular scales of analysis; explicitly including the interpreter's behavior in his interpretive system; and providing a definition of verbal behavior that anticipates by nearly two decades his book-length work on that topic. Metatheory is addressed explicitly, characterizing the approach as a descriptive rather than an

explanatory system, but asserting the need for structure in interpretive systems of any kind.

The second chapter, "Scope and Method," includes detailed description of Skinner's basic experimental arrangement. This introduction to specific, innovative features of method prepares the way for descriptions of experiments and results that, as noted earlier, provide an entirely different basis for supporting environment-based interpretation: Unlike the between-group studies that are more typical of experimentation in psychology—whose brief observations of each subject and emphasis on differences between groups of individuals plays into the cultural bias toward organism-based interpretation—Skinner's experiments follow each individual's behavior as it tracks the changes of experimental procedure. Because the individual is a constant during these sequential changes of procedure and behavior, it is culturally conventional to appeal to the environmental context of action rather than to characteristics of the actor when accounting for what the individual does.

In this new mode, the expository sequence alternates between respondent and operant behavior, emphasizing similarities more than differences between them while basic conditioning and extinction are described. Skinner's own data are presented with ample attention given to individual subjects' differing behavior patterns; still, these are handled with minimal appeal to dispositional characteristics of the individuals. For example, in characterizing the original conditioning of each of 78 rats' lever pressing, nine individual cumulative records are presented as representing the range of effects. The few atypical cases are discussed in terms of four types of likely interplay between environmental events and behavior; a fifth, one-sentence alternative, might be viewed as suggesting an organism-based difference—emotional reactivity to novel stimulation—but even there a specific environmental stimulus is said to produce the atypical reaction.

With basic demonstrations of conditioning in place, the sequence again builds from simple to complex, but now the development occurs over the course of several chapters. Delayed reinforcement, conditioned reinforcement, intermittent reinforcement, discrimination and generalization, all are characterized mainly

as behavior–environment interaction, which is to say, they are described in the terms of environment-based theory. Occasional discussions of the implications of data for the reflex reserve have the character of asides or of afterthoughts, appended to accounts of data that appeal directly to procedural determinants. When individual differences are salient—as when periodic reconditioning generates highly individualized and complex performances in particular experimental subjects—the observed differences are accounted for in terms of ancillary environment-based processes such as discrimination, generalization, and specific spatial relations in the conditioning situations, rather than by appealing to differences between the subjects' capacities for establishing a reserve or the like.

The Transition from Classic Reflexology to the Language of Operant Theory

In beginning *B of O* with reflex relations, Skinner established an environment-based interpretive directionality that he then maintained through transitions to domains in which that directionality is less conventional. These latter, of course, are especially the domains of operant behavior, where one encounters separations of time between behavior and its relevant environmental events as well as widely ranging differences between individuals' actions and capabilities. That transition to a focus mainly on operant behavior is accomplished quite early in the book. The book begins, however, not only with descriptions of reflexive behavior but also with descriptions and discussions couched in the language of classic reflexology, whose frequent references to elicitation continually suggest immediate environmental precursors of behavior even when no specific elicitors are identified. The book's expository shift away from this language of reflexology lags behind the topical progression from reflexes to operant behavior; only after many pages describing operant behavior do we find the language of classic reflexology coming to be replaced by the more contemporary language that we now identify with behavior analysis. This more modern language draws a clear distinction between elicited reflexes and emitted behavior, whose rates of occurrence are not so tightly tied to immediate environmental precursors. It also differs from the vernacular and mainstream

psychology in various other ways, to the extent that it might be considered a distinct dialect (Hineline, 1980). Besides its expository function, the shift may reflect the ontogenesis of environment-based interpretive patterns in Skinner's own behavior.

To be specific: The early parts of the book characterize all behavior—even operant behavior—in the language of elicitation. For example, Skinner initially construes the conditioning of a lever-press response as dealing with a “chain of reflexes involved in pressing the lever and obtaining food” (p. 66). Later on, rates of responding are described rather than rates of elicitation, with “elicitation” occurring only once or twice in the course of several pages. Within that general trend, however, some striking transitions between the two modes of description conveniently illustrate both types. For example, consider first a description of operant behavior in the language of classic reflexology:

... When an organism comes accidentally (that is to say, as the result of weak investigatory reflexes) upon a new kind of food, which it seizes and eats, both kinds of conditioning presumably occur. When the visible radiation from the food next stimulates the organism, salivation is evoked according to Type S. This secretion remains useless until the food is actually seized and eaten. But seizing and eating will depend upon the same accidental factors as before unless conditioning of Type R has also occurred—that is, unless the strength of S^P:food. R:seizing has increased. Thus, while a reflex of Type S prepares the organism, a reflex of Type R obtains the food for which the preparation is made. And this is in general a fair characterization of the relative importance of the two types. (*B of O*, p. 111)

A few pages later, we find a mixture of modern behavior-analytic and classic reflexive language:

An operant may be strengthened or weakened through reinforcement or the lack of it, but the phenomena of acquisition and loss of strength are only part of the field defined by reinforcement as an operation. . . .

In general the states of strength of the conditioned reflexes of an organism are submaximal with respect to the operation of reinforcement. . . . Special properties of conditioned reflexes arise under periodic reconditioning which have no counterpart in the

original conditioning and extinction of a reflex. They are properties of the reflex reserve and of the relation of the reserve to the rate of elicitation. We may approach the subject by examining the effect of periodic reconditioning upon the state of our representative operant. (*B of O*, pp. 116-117)

Then, with descriptions of extended response patterns on fixed-interval schedules, individuals' extended histories and the resulting performances are portrayed mainly in the details of cumulative records, although "rate of elicitation" still appears in the accompanying prose. This descriptive phrase gradually becomes less frequent in the course of pages 117 to 139.

Somewhat later, Skinner's interpretive statements begin to catch up with these evolving descriptions of data, as when he defines "discrimination of Type R":

Although the response is free to come out in a very large number of stimulating situations, it will be effective in producing a reinforcement only in a small part of them. The favorable situation is usually marked in some way, and the organism makes a discrimination of a kind now to be taken up. It comes to respond whenever a stimulus is present which has been present upon the occasion of a previous reinforcement and not to respond otherwise. The prior stimulus does not elicit the response; it merely sets the occasion upon which the response will be reinforced. . . . Three terms must therefore be considered: a prior discriminative stimulus (S^D), the response (R^O), and the reinforcing stimulus (S^I). (*B of O*, p. 178)

Then, as Skinner proceeds to describe various experiments on discrimination, the reflex reserve is mentioned only briefly, and the phrase "rate of elicitation" appears only occasionally. Many descriptions are in terms of "rates" with no modifiers, but vestiges of reflexology are still evident in descriptions in terms of the relative strengths of a reflex (lever pressing) in the presence of different stimuli.

Still later, Skinner refers to "the reinforcement of an operant" rather than of a reflex, and shortly thereafter he explicitly distances the operant from the reflex:

The lack of an eliciting stimulus in operant behavior together with the law of the operant reserve throws considerable weight upon the response alone, and this may seem to weaken

any attempt to group operants under the general heading of reflexes. . . . it should be understood that the operant reserve is a reserve of responses, not of stimulus-response units. Whether the same can be said for respondents is not clear. (*B of O*, p. 228)

And on page 241, we find the modern, non-reflexive language of operant behavior:

The discriminative stimulus (defined with respect to operant behavior) has a very different status from that of the eliciting stimulus (defined with respect to respondent behavior). It is less likely to be regarded as a spur or goad and is perhaps best described as "setting the occasion" for a response. Whether or not the response is to occur does not depend upon the discriminative stimulus, once it is present, but upon other factors. . . . Strictly speaking we should refer to a discriminated operant as "occurring in the presence of" rather than "elicited as a response to" S^D .

At long last, the formal conception has caught up with the changes that could be discerned earlier in the descriptions of experiments and data:

The various functions of stimuli may be summarized in this way: a stimulus may:

- (1) elicit a response ("elicitation"),
- (2) set the occasion for a response ("discrimination"),
- (3) modify the reserve ("reinforcement"), or
- (4) modify the proportionality of reserve and strength ("emotion," "facilitation," and "inhibition"). (*B of O*, p. 243)

Late in the book are chapters addressing the concepts of drive, emotion, and reflex reserve, whose initial characterizations constituted lapses into organism-based theory already described. This time, Skinner introduces the concept of drive with an observation that anticipates by several decades the account in terms of attribution theory:

The problem of drive arises because much of the behavior of an organism shows an apparent variability. A rat does not always respond to food placed before it, and a factor called its "hunger" is invoked by way of explanation. . . . It is because eating is not inevitable that we are led to hypothesize an internal state to which we may assign the variability. Where there is no variability, no state is needed. Since the rat usually responds

to a shock to its foot by flexing its leg, no "flexing drive" comparable to hunger is felt to be required. (*B of O*, p. 341)

The ensuing treatment of his research under the heading of "drive" remains, for the most part, consistent with Skinner's initial environment-based characterization, quoted earlier as having occurred on page 24: Drives are identified in terms of *classes* of operants or of reflexes that are affected collectively by particular variables. When we identify drives, we identify classes of operations having similar effects on a given unit of behavior or classes of units of behavior affected by a single operation: "Whether or not a given reflex belongs to a given drive must be answered by considering covariation rather than any essential property of the behavior itself" (*B of O*, p. 371). Then, treating emotion in a manner analogous to the treatment of drive:

In both cases (drive and emotion) we must describe the covariation of the strengths of a number of reflexes as functions of a particular operation. Drive and emotion are separate fields only because the appropriate operations can be separated into different classes. In many cases, this distinction is thin . . . It is not essential to this formulation that drive and emotion constitute two distinct classes. The important thing is the recognition of a change in strength as a primary datum and the determination of the functional relationship between the strength and some operation. (*B of O*, pp. 408-409)

Both discussions, however, hint at a departure from pure environment-based interpretation: "An emotion is a dynamic process rather than a static relation of stimulus and response" (p. 409). "It is a poor substitute to measure the operation responsible for the state of a drive until the relation between the state and the operation is accurately known" (p. 402). And in the general comments of his concluding chapter, the problematic status of some of these concepts is acknowledged:

The concepts of "drive," "emotion," "conditioning," "reflex strength," "reserve," and so on, have the same status as "will" and "cognition," but they differ in the rigor of the analysis with which they are derived and in the immediacy of their reference to actual observations. (*B of O*, p. 440)

Beyond The Behavior of Organisms

After the publication of *B of O* in 1938, Skinner's theorizing continued to evolve in some respects, while being addressed to an ever-increasing range of topics. In a 1940 convention paper he reported findings that were not readily accommodated by the concept of reflex reserve (Skinner, 1940). Soon after, the reflex reserve faded from Skinner's prose. Similarly, the locutions of reflexology are seldom found in his later works. Skinner's own confidence in the consistency of his position is evident in a recent exchange: In a commentary on Skinner's work, Blanchard, Blanchard, and Flannelly (1984) sketch a distinction between "organismic" and environment-based interpretation that is very similar to the one presented here, and they suggest that organism-based concepts are essential to an adequate science of behavior. Skinner replies as follows:

I would point out that in my thesis, published in 1931 [which was substantially incorporated into *B of O*], I hazarded a guess that there were three kinds of variables of which behavior would prove to be a function. I referred to them with the terms conditioning, drive, and emotion. Blanchard et al. would call the last two organismic. Over the years I have spent a good deal of time on them, but simply as different sets of variables of which the probability of response is a function. (Skinner, 1984, p. 702)

This claim to have maintained a purely environment-based stance (with its parsimonious superiority implied by the term "simply") is a plausible one, and it does describe most of the work of Skinner's extensive career. Whether he has entirely eliminated emotion as an organism-based locution could be assessed through detailed examination of his subsequent writings.

Although the origins and initial introduction of behavior-analytic concepts may be substantially portrayed in *B of O*, that book may not have been the most effective propagator of those concepts. Indeed, Knapp (in press) asserts that *B of O*, in itself, was not particularly successful, and that it was saved from near intellectual oblivion by Keller and Schoenfeld's program at Columbia University. Two key features of that program were their

own textbook (Keller & Schoenfeld, 1950) and the introduction of behavioral principles through a laboratory course in which each student conditioned the behavior of a laboratory rat. Such laboratory procedures provide a shaping/fading arrangement that may well be more effective than any textbook can provide: The major components for interpreting operant behavior are systematically introduced to the student as they are built into the animal's repertoires of consistently expanding complexity. Hence, the infusion of theory from *B of O* into the culture may be better examined via the details of the behavioral tradition as it developed at and radiated out from Columbia. Nevertheless, Skinner's many other publications surely have been a continuing and prominent component of the survival of behavior analysis as a distinct viewpoint.

Finally, it must be acknowledged that interpretive directionality is not the only unconventional feature of Skinner's approach. Its construing of process extended over time, for example, is another, and Skinner's notorious assertions of strong determinism and rejections of mentalistic terms have contributed to the controversies as well. At the time of its genesis, as well as over the ensuing years, the merits of Skinner's approach were, and have continued to be, argued in terms of issues such as determinism versus autonomy of persons on the one hand (e.g., see Coleman, 1984) and in terms of theoretical coherence on the other (e.g., Verplanck, 1954). Both the conventional logic of the latter and the prescientific value commitments of the former have operated in the controversies surrounding this approach. Nevertheless, even within arguments stated in those terms, the simple but subtle feature of interpretive direction may have much to do with the clashes that occur between behavior-analytic interpretations and the interpretive patterns of the surrounding culture.

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