THE BEHAVIOR OF ORGANISMS: *PURPOSIVE BEHAVIOR AS A TYPE OF REFLEX*

WILLIAM TIMBERLAKE

INDIANA UNIVERSITY

... *operant behavior is essentially the field of purpose* (Skinner, 1966, p. xi).

Skinner's major goal in writing *The Behavior of Organisms* (1938) was to extend the rigor, simplicity, and lawfulness of the study of spinal reflexes (Crozier & Hoagland, 1934; Magnus, 1924; Sherrington, 1906) to the study of purposive behavior. As Skinner (1938) notes, "the older study of spinal reflexes and the modern study of behavior are logically and to a great extent historically continuous" (p. 378). Skinner's goal contrasts with the common tendency in psychological theory to draw a sharp distinction between reflexive and purposive behavior. Control of reflexive behavior typically is attributed to an external stimulus that need not engage a nervous level higher than the spinal cord. In contrast, purposive behavior has been attributed to mentalistic causes residing in the organism, such as thoughts, goals, purposes, attitudes, intentions, reason, will, desire, information processing, and decision making.

Even among fellow learning psychologists interested in a reflex analysis of purposive behavior (e.g., Guthrie, 1930; Hull, 1937; Pavlov, 1928; Watson, 1916), Skinner's approach was unique. For most learning theorists, the extension of the reflex to purposive behavior was mediated by the mechanism of Pavlovian conditioning. Purposive behavior was explained as the chaining of covert conditioned reflexes. For Skinner, the common framework underlying reflexes and purposive behavior did not depend on any Pavlovian intermediary.

To achieve his aim of a common framework for the study of purposive and reflexive behavior, Skinner had to surmount two obstacles.

First, he had to free the concepts of stimulus, response, and reflex from their obvious physiological substrates. To fail in this was to disallow the jump from reflex to purpose, because it was clear that purposive behavior depended on more than the restricted set of neurological pathways determining spinal reflexes. Skinner dephysiologized the reflex by (a) effectively arguing the importance of a strictly behavioral analysis and the lack of advantage or outright disadvantage of neurologizing; (b) defining stimuli and responses in external terms such as environmental change and movements rather than in terms of sensory apparatus and neural effectors (p. 9); and (c) defining a reflex as an observable, highly positive correlation between a stimulus and response.

A critical aspect of Skinner's approach was that stimuli and responses were defined conditionally; that is, they existed only as parts of a reflex (see Dewey, 1896; Kantor, 1933). Thus, the definitions of the three basic terms were interrelated. A stimulus was not just any environmental change, it was an environmental change that produced a response; a response was not just any movement, but a movement in response to a stimulus; and a reflex was the resultant stimulus-response unit.

The second obstacle to treating reflexes and purposive behavior in the same framework was the importance of consequent as opposed to antecedent stimuli in determining purposive behavior. Skinner separated himself from biologists such as Loeb (1918), who advanced a stimulus-driven account of all behavior, by defining purposive behavior as emitted rather than elicited. In other words, by definition no external stimulus initially "goaded" the animal into purposive behavior. Even when purposive behavior came under stimulus control through discrimination training, Skinner took pains to emphasize that this discriminative

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stimulus was not a goad but an "occasion setter." For example, responses following such a stimulus showed longer response latencies and a lack of relation between the intensity of the stimulus and the strength of the response.

Skinner resolved the differences between simple reflexive and purposive behavior by using a similar conceptual and procedural strategy. He structured purposive behavior in terms of three near-data-level concepts (the three-term contingency): the antecedent stimulus (occasion setter), the response, and the consequent stimulus (reinforcer). Following his strategy for the reflex, he defined these concepts simultaneously and conditional on the occurrence of a reinforcement effect. Thus, a reinforcer was a stimulus that changed the rate of a response upon which it was contingent, a response was that movement (environmental effect) changed in rate by the contingent presentation of a reinforcer, and a stimulus was that aspect of the environment that set the occasion for the change in rate of the response.

It is tempting to speculate that Skinner's use of mutual conditional definition was facilitated by his exposure to the procedures typically used in studying reflexive behavior (Skinner, 1979). Experimenters dealing with a reflex preparation, such as the scratch reflex in frogs, focus on a target movement and then "tune" the preparation by altering the intensity, location, and rate of repetition of the stimulus and adjusting the response measure until the reflex occurs strongly and reliably (Skinner, 1938, pp. 34-35).

Skinner appears to have applied a similar tuning technique in working with operant "reflexes." In the process of modifying the environment, procedures, and response measures to produce a reliable "orderly" change in responding, he altered, refined, and often abandoned particular response classes (Skinner, 1979). For example, among the changes he explicitly documented in developing the lever press were automating and finally eliminating alley running that led to the food area (Skinner, 1956). He then installed screening to prevent lever pressing when the rat was exploring above the lever and increased the diameter of the level to prevent gnawing (Skinner, 1938, pp. 49-50). After such careful tuning, it is hard to view the lever press as an arbitrary response.

Most of *The Behavior of Organisms* presents empirical support for his approach by relating

"operant reflexes" to the laws of spinal and Pavlovian reflexes, to independent variables such as response timing and effort, and to concepts such as emotion, drive, and the reflex reserve. These relations were explored in an effort to induce general functional laws. One can sense in Skinner's final critical summary of the field his satisfaction that he had provided a unique and highly useful combination of rationale, technique, and apparatus for the study of purposive behavior. By freeing his approach from dependence on physiology and internal concepts, the study of reflexes and purposive behavior could be dealt with on the same plane.

SECRETS OF SUCCESS

Rather than an overall evaluation of *The Behavior of Organisms* (e.g., Verplanck, 1954), I am concerned here with two simple issues. Relative to his goal of providing a common framework for the analysis of reflexive and purposive behavior, what did Skinner accomplish, and what did he fail to accomplish? In this section and the next I offer an evaluation of his successes and shortcomings. I then suggest how his approach might be modified to better achieve his goal.

Skinner wanted to reframe the study of purposive behavior, and in many respects he was successful. His approach has been applied fruitfully in a large number of areas ranging from basic learning research to mental illness and planned communities, from animal training to aggression and drug effects. Skinner's experimental box took over Hull's laboratory within a few years and the rest of whole-animal psychology within the next 20 years. Even after the recent "cognitive revolution," the study of operants in both animals and humans remains a dominant approach to the analysis and control of purposive behavior.

Who would have expected this iconoclastic, thoroughly self-referenced, and theoretically underdeveloped system to outlive its apparently better constructed and more influential competitors? Certainly not the initial reviewers who, while applauding Skinner's experimental facility, tut-tutted his insular analysis and especially his failure to link his work to that of other researchers (Finan, 1940; Hilgard, 1939; Krechevsky, 1939; Wolf, 1939). Although Skinner's influence on the study of

animal learning was immediate (see Hilgard & Marquis, 1940), the global influence his system later achieved remained in doubt.

By 1946 Skinner's book had sold only 550 copies of an original printing of 800 (Skinner, 1979) and the plates had gone into scrap because of the shortage of lead during the war. But Columbia's Keller and Schoenfeld adopted *The Behavior of Organisms* in their new course based on reinforcement principles and a second printing of the book was ordered. Skinner's seedling had taken definite root. By now, the 50th anniversary of *The Behavior of Organisms*, both Skinner and his system have outdistanced their contemporaries in longevity and influence.

What are the sources of this obvious success? In rather speculative and un-Skinnerian fashion, let me suggest the following determinants. First and foremost, Skinner provided a conceptually and empirically robust framework for the study of behavior. He used highly abstract concepts (i.e., stimulus, response, and reinforcer) that were not well developed or related at a theoretical level. Instead they were linked by definition, example, procedure, and the practice of "tuning" the circumstances to produce orderly behavior. Skinner's typical strategy was to focus on an environmentally defined but reasonably coherent class of movement, make guesses about effective manipulations of stimuli and reinforcers, and then look for regular relations between the manipulations and changes in the rate of the movement class. The result was an approach to behavior that focused on control and finding examples rather than on refining and relating concepts.

Second, though Skinner's adamant avoidance of complex theoretical concepts and physiological mechanisms annoyed many of his contemporaries, it was a stand that in retrospect had much to recommend it. In the 50 years since *The Behavior of Organisms*, Skinner's system suffered little obvious damage from his refusal to ground his work in physiology. His approach did not rule out the use of physiological variables and measures, but it avoided the considerable (and continuing) vacillations in the presumed physiology of learning and reinforcement. Further, Skinner's (1950) avowed lack of concern with the conceptual nature of the learning process avoided many unresolvable disputes grounded in differences in language and interpretation.

A third contribution to the success of Skinner's system was his experimental chamber, the well-known box, and his device for simultaneously recording and displaying response rate in real time, the cumulative recorder. In essence the chamber was a flexible puzzle box that allowed no escape and provided repeated exposures to the puzzle. As a device capable of simulating purposive behavior, the box offered distinct advantages over apparatuses such as the maze, the escape-puzzle box, the straight alley, and the jumping stand. These devices simulated behavior in terms of choice because choice was the classical unit for assessment of intelligent behavior. Isolated choices, however, are not a particularly good guide to the dynamic "texture" of real world behavior, a potential strength of the box when coupled with the cumulative recorder.

A fourth reason for success was the theoretical and empirical openness of the system. To conduct an original and important maze study in the late 1930s required thorough acquaintance with a large literature of empirical results and complex theorizing and a good deal of time for the many single trials necessary to show acquisition. In contrast Skinner's box invited putting an animal in and "playing around." There were many variables to manipulate and an hour or so was often enough to establish a reliable change in responding.

The coming together of the apparatus, procedures, and data language of Skinner's approach resembled in several respects the recent emergence of the personal computer. In the language of computing, Skinner supplied a group of avid "hackers," previously disenfranchised or forced to work with expensive, slow mainframes and very complex languages, with their own personal machines and a very simple but powerful language. An explosion of applications resulted. Anyone could "play" without being an expert on hypothetico-deductive techniques and historical precedent. Admittedly, scientists continuing to use the "mainframes" of learning research often looked down on Skinnerian "hackers," but there is little argument about the interest and energy that they generated. The first 5 years of the *Journal of the Experimental Analysis of Behavior* were what initially attracted me to operant psychology. There were interesting applications and a sort of try-anything attitude that was exciting and fun.

The fifth and most important reason for success may well be Skinner himself (more properly his behavior, or the nexus of causal forces we call Skinner). One of his most salient characteristics, at least as modern learning theorists go, is his *extreme* willingness to have a go at applying his approach anywhere. He has dealt with language, self and consciousness, private events, programmed teaching, evolution and development, culture, his age and work habits, Shakespeare, gambling, and so on. To be sure, few of these forays have pleased all the residents of a given field, but each sally has displayed some flair or insight worth noting. Skinner also can be quite willing to go over the same material repeatedly with new audiences, as shown by his extensive replies to critics of selected examples of his work in the special 1984 issue of *Behavioral and Brain Sciences*.

Another important quality of Skinner's behavior was an incredible appreciation of the organization and control of behavior and how to modify and use it. He could with apparent ease get animals to do remarkable things. Pigeons played ping pong and guided bombs; rats completed complex chains of responses beyond the talents of many undergraduates without written instructions. Skinner (1956) has attributed his laboratory successes to serendipity, but this seemed to play an adjunct role. More important was his concern with how the subject interacted with the environment, reflected in his care in designing and redesigning apparatus (Skinner, 1938, pp. 48-51).

Furthermore, Skinner has been a master of the clever story and the telling illustration. His ingenuity along these lines makes it difficult to dismiss his analyses out of hand. However, there is a negative side to this talent. I think his verbal facility has interfered with the development and refinement of his concepts and with investigation of the assumptions underlying his approach. As a result, although Skinnerian language and procedures can be applied to a variety of topics ranging from evolution to physiology, there has been less integration and modification than might have been expected. In this sense the preservation of Skinner's formal system reasonably intact over 50 years may speak to failure as well as success.

ASPECTS OF FAILURE

In evaluating the extent to which Skinner failed to develop a satisfactory common frame-

work for the analysis of spinal reflexes and purposive behavior, I will consider first the formal aspects of Skinner's approach (see also Schick, 1971; Zuriiff, 1985) and then the pragmatics of its use.

The Basic System: Formalities

Skinner defined his basic concepts interdependently and conditionally, in other words as a predominantly self-referenced definitional system. A definitional system can be contrasted with a theoretical system in that elements of the former are linked by definitions, whereas elements of the latter are linked by hypotheses or laws. Though no conceptual system tied to the real world is exclusively one or the other, Skinner is unique among learning psychologists in his reliance on definition.

Because the system is largely self-referenced, the complete definition of any concept reproduces the entire system. Thus, an operant reflex consists of a stimulus (an aspect of the environment) that controls the rate of a response (movement criterion) that is followed closely in time by a reinforcer (presentation or removal of a stimulus) and accompanied by reinforcement. Reinforcement is an orderly increase or decrease in the rate of a response that is followed by a reinforcer and controlled by a stimulus. (A decrease in response rate subsequently was referred to as punishment, but this does not change the nature of the present arguments.)

I will consider two issues, whether Skinner has a well constructed, adequate definitional system, and whether reliance on a definitional system is an appropriate approach to the study of behavior. Examination of this system reveals two components, a set of major concepts (i.e., stimuli, responses, reinforcers, reinforcement), and a set of relations (e.g., "orderly increase or decrease in rate"). A notable characteristic of the system is that the local definition of each concept is incomplete; it specifies only broad sets of possible concept "candidates." For example, a stimulus is locally an "aspect of the environment," but a particular aspect of the environment is not a stimulus unless, in combination with other concept candidates present, it satisfies all elements and relations of the system.

The configuration of the concepts in Skinner's system and their tie to the real world are specified by several relations: (a) "an orderly

increase or decrease in rate," (b) "controls the rate of," and (c) "followed closely in time by." The critical relation tying Skinner's system to data is an "orderly increase or decrease in rate." Surprisingly, this relation is defined by Skinner only by pointing at positive examples of orderly changes in cumulative records of responding. This is not an adequate operational definition. What constitutes a change? How rapid and consistent a change is necessary for it to be orderly?

For example, suppose there was an increase and then a decrease in rate of responding, or suppose the increase was delayed, or amounted to one response per hour. Defining a concept only by pointing at positive examples may serve as a beginning point, but as a long-term solution it has elements of the lack of analysis Skinner appropriately rejected in dealing with typical uses of ordinary language. At a pragmatic level this objection can be met partly by tuning the circumstances to reproduce the qualities of the example, but at a formal level this is not sufficient. At the least a set of positive and negative examples is necessary to facilitate discrimination.

Specifying the relation of discriminative control between a stimulus and rate of responding simply adds more complexity to the problem of defining an orderly change in response rate. How much of a rate differential is required? How reliable must a rate change be and how long must it persist? What are acceptable patterns of stimulus presentation? Finally, the relation "followed closely in time" is not tied specifically to proximity or reliability. Any example of this relation only designates a candidate that must be confirmed by the proper outcome.

The second issue concerning Skinner's system is whether his definitional approach is appropriate to the study of behavior. The approach is obviously not appropriate if it makes indefinite contact with the real world. But presuming the possibility of improved specification of relations, such as "orderly increase in rate," Skinner's system has several advantages. All candidates must pass the ultimate test of combining with others to produce an appropriate effect before they can be taken as adequate examples of the concepts. Because of this final test the integrity of the system as a whole cannot be breached. When combined with the practice of tuning procedures and circumstances to produce recognizable output, the re-

sult is a system that can be applied widely without modification of its concepts.

However, these advantages have accompanying drawbacks. Because the system is self-contained and impervious, there is no encouragement to develop and refine the definitions of individual concepts and relations. For example, more can be said about particular responses than that they are movements that meet a criterion, but this knowledge is not included in the formal system. It is included in laboratory lore or buried in specific experimental reports. Further, in the long run, simultaneous definition of concepts puts the entire system at the mercy of the weakest concept and the poorest tie to data.

Perhaps most important for a field of scientific endeavor, such a definitional system has difficulty generating falsifiable predictions. The fundamental problem is that the antecedent conditions are not stated independently of the outcome. In the case of Skinner's system this problem has been pointed out as the issue of circularity in the definition of a reinforcer, but the problem is more general than this. All of Skinner's concepts are defined conditionally, and thus are circular in this sense. Formally, one cannot identify a stimulus, a response, a reinforcer, or a reinforcement effect unless all candidate criteria and relations are satisfied simultaneously. Perhaps the only potentially falsifiable prediction Skinner can make is that his candidate criteria and relations form a null set.

A familiar technique for generating predictions in a definitional system is to incorporate a specific example of a presumed consequence as an antecedent condition, and open the system. For example, by replacing the requirement of an *accompanying* reinforcement effect with the requirement of a *previous* reinforcement effect under the same candidate circumstances, one can predict that an accompanying reinforcement effect should occur again. The problem with this solution is that, though it allows us to test an hypothesis, the hypothesis is only that elements in the system are reliably grouped. Confirmation of this hypothesis is important, for without repeatability there is little possibility of fruitful scientific investigation, but confirmation says very little about causality and the accuracy of the concepts.

What repeatability establishes is that somewhere in a set of candidates are circumstances sufficient to produce an effect, but we do not

know exactly what these circumstances are. This question can only be answered through systematic research in which the results feed back on the concepts and relations of the system. Without such hypothesis-driven refinement of concepts and relations we cannot reasonably open the system to generalize about reinforcement even so far as was done by Meehl (1950). He assumed a critical causal role for the reinforcer and predicted that reinforcers identified in one situation would act as reinforcers in another situation. It should not be surprising, given the lack of conceptual development, that Meehl's generalization made obviously incorrect predictions. These failures can be viewed as sharply questioning the reinforcer concept (Premack, 1965; Timberlake & Allison, 1974), but Skinner's formal system severely restricts development of an alternative.

In short, the "knowability" of Skinner's formal system is limited because of its relatively closed, definitional nature. All Skinner's concepts are indefinite because they are tied to the real world through the inadequately defined notion of an orderly change in response rate. Even if "orderly change" were well defined, little development and refinement of the system is possible. If more concepts were defined independently, as in most scientific systems, the hypotheses and assumptions required to relate them could be tested. Defining one's concepts interdependently requires less explanatory overhead, but the system cannot be mapped adequately to the real world. Even at a pragmatic level, the deficit in independent ties makes it more difficult to decide what constitutes an example of a concept.

The Basic System: Pragmatics and Implicit Theories

Despite the problems with Skinner's formal system, he and others appear able to use his approach. One is reminded of theorists arguing in principle that people cannot fly, or go to the moon, and yet someone takes on the task and accomplishes it. According to a reasonable analysis of Skinner's system, one cannot predict the occurrence of reinforcement and yet reinforcement effects are readily produced. In our best estimate, Skinner's system is inaccessible, yet researchers readily generate functional relations between the experimenter's manipulations and outcomes. How can this happen?

I think three interdependent conclusions can be made at this pragmatic level. First, the empirical "knowability" apparent in reliable combinations of procedures and results indicates that there are phenomena present on which Skinner's system and practices impinge. Put bluntly, Skinner has a hold of something. His apparatus and procedures frequently affect some process or set of processes that generate reliable outcomes. We have little precise evidence that Skinner's concepts map the controlling variables well (and some evidence that they do not; e.g., Timberlake, 1980, 1984). However, that the experimental circumstances can be tuned to produce robust functional relations between variables argues strongly for the system's potential for reasonably reliable contact with the real world.

Secondly, Skinner's criterion of orderliness, though not well defined, can nonetheless be used in the mapping of relations between variables. If one establishes a particular change in a cumulative record as an example of reinforcement, then other changes can be referenced to it. Thus, given the steepness of a particular cumulative record, the steepness of other cumulative records can be compared to it, forming a scale that can be related to values of independent variables. For example, up to a point lever-press rate increases with the value of a fixed-ratio schedule or the concentration of amphetamine injected.

Third, and most important, Skinner's empirical success in applying his system strongly argues that he has unstated criteria (implicit theories) that allow him to select good candidates for concepts and procedures. I believe that the use of these implicit theories is responsible for an important part of the practical success of his system. If acknowledged and developed, these assumptions could play an important role in advancing Skinner's aim of a common framework for reflexive and purposive behavior.

Because Skinner touts induction, argues incisively against unnecessary and misleading theorizing, and ties his concepts very closely to data, it is easy to overlook his own implicit theories. However, I see little evidence that any scientist is a pure inductionist. Certainly Skinner did not simply manipulate a chance assortment of variables in a random fashion with no preconceptions of what constitutes an effective variable, how and in what order they should be manipulated, and what measures

should be taken. Instead, Skinner made highly appropriate choices arguably based on implicit theories about behavior.

Skinner's implicit theories are clear at two levels. First, at the level of naming concepts, basic terms such as reinforcer, inhibition, elicitation, and drive all have relations to previous theories about the functioning of the organism. No matter that Skinner carefully defines these terms and claims that they have no excess meaning, it is nearly impossible to avoid theoretical baggage in their use. Second, Skinner's criteria for choosing candidates for his concepts and relations were influenced by theoretical assumptions in at least three important areas: (a) the procedures of reinforcer presentation and the interpretations of outcomes, (b) the choice and interpretation of the results of motivating procedures, and (c) the choice of concept candidates, apparatus, and procedures appropriate to individuals and species.

Reinforcement procedures and outcomes. A key theoretical assumption influencing Skinner's choice of candidates for reinforcers and the procedures of their presentation appears to be the notion that learning consists of the strengthening of responding. Skinner's use of the term "strengthening," although explicitly excluded from referring to an internal connection between stimulus and response, is obviously indebted to connectionist ideas in classical learning theory and serves much the same function.

The strengthening model appears to have influenced the concept of reflex reserve, Skinner's theoretical answer to the problem of putting together learning and performance. Reinforcers strengthen responses and produce cumulations in the reserve; when reinforcement is omitted the store is paid out as responses. Unfortunately, despite its prominence in *The Behavior of Organisms*, the reflex reserve did no better than its competitors in dealing with phenomena such as performance on intermittent schedules, even given that the rate of exchange between behavior and the reserve was allowed to vary at both the input and output (see Killeen, 1988). Perhaps as a result Skinner dropped it, leaving him without a comprehensive way to predict many effects in maintenance and extinction. Idiosyncratic accounts of phenomena then emerged, as when Ferster and Skinner (1957) described strain on fixed-ratio schedules as abulia, literally the loss of willpower.

Despite the disappearance of the reflex reserve concept, the strengthening metaphor lived on in the focus on response-reward proximity and frequency, and in the limitations researchers placed on reward density to ensure monotonic functions with performance (Timberlake & Peden, 1987). The influence of the strengthening assumption is nowhere clearer than in Skinner's (1948) analysis of superstitious behavior in the pigeon. In this procedure pigeons were presented food periodically in a response-independent fashion (a fixed-time schedule), and some movement typically increased in rate. Skinner attributed this result to reinforcement based on the assumption of differential temporal contiguity between the food and the movement. In this use, reinforcement was not related to any production operations, but was inferred solely from an outcome and attributed to an unsubstantiated proximity relation between food and an unspecified response candidate. This use of reinforcement seems uncomfortably similar to the invoking of mentalistic causes that Skinner himself argued against so well.

Motivation. The theoretical assumption behind Skinner's criteria for selecting motivational procedures and interpreting resultant changes in behavior appears to be that animals engage in time-bound compensation for forced deficits. The rate of feeding within a session was attributed to compensation for deprivation of food and/or water outside the chamber. Local increases in response rate following short interruptions were explained as immediate compensation.

On the whole, Skinner's treatment of motivation is more interesting than most. He carefully showed that rates of feeding were unchanged by requiring a small number of lever presses for each pellet (Skinner, 1932). He described the compensatory reaction to food deprivation in mathematical terms. At one point he even argued the ethological view that responding may fall into classes related to drives. However, little additional work was done in any of these areas. For example, although he called attention to the trade-off between compensation and cost of operant responding, he did not pursue this issue or try to establish the timing characteristics for local compensatory reactions (see also Killeen, 1988).

Zuriff (1985) indicated that Skinner later dropped the concept of drive because it could be translated directly into operations, and, thus,

had no surplus meaning. But this focus on operations misses the point that the operations were embedded in a theory of compensation. To claim to depend solely on operations after using a compensatory model to develop and apply the operations seems more like a ploy for argumentation than an accurate analysis of behavior. Many researchers in the operant tradition developed a rule of thumb that assumed a specific form of compensatory theory without testing it: If responding does not decline across a session, then there is no change in the tendency to compensate. Timberlake and Peden (1987) recently argued that this assumption is questionable.

Further, Timberlake (1980, 1984) and others following Premack (1965, 1971) have provided considerable evidence that deprivation outside the experimental session is orthogonal to the determination of session responding. Session responding is determined by the degree of schedule constraint on preferred characteristics of responding within the session. Theorizing about regulatory effects under schedules has gone considerably beyond Skinner's conception of motivation and invaded the explanatory domain previously ruled by reinforcement concepts alone (Gawley, Timberlake, & Lucas, 1986, 1987; Nevin, Smith, & Roberts, 1987; Timberlake, 1980, 1984).

Stimuli, responses, and reinforcers. Finally, although obviously quite important, Skinner's criteria for choosing candidates for stimuli, responses, and reinforcers are not very clear. In much of his work Skinner appears to use discrete attention-getting stimuli, manipulative or locomotor responses potentially related to obtaining food, and reinforcers created by reducing access outside the session. But based on his consistent success in shaping behavior Skinner must have other criteria as well. When Skinner explains how he produced a pigeon that played ping pong, the procedure is marvelously understandable, like the punch line that makes sense of a complex story. Yet when other researchers try the punch line it frequently falls flat. Breland and Breland (1961), in training new animal species using operant techniques, found it easy to get irregular changes in responding, making Skinner's accomplishments with rats and pigeons all the more remarkable.

Clearly Skinner knew his organisms much better than most of us and dealt from a more

precise set of private rules for the selection of his candidates. His discussions of shaping reveal some aspects of these criteria, but he leaves most of them unstated, glossing over them by emphasizing convenience, laziness, or serendipity (Skinner, 1956). In an example from the second volume of his autobiography, Skinner (1979) reports that he selected a response on the basis of a presumed natural response sequence. He located a plate to be pecked to avoid shock so that the pigeon "could strike at the sight of the stimulus, as it would 'naturally' have done if it were, say, being bitten by a predator" (p. 321). A close analysis of more examples might prove an important contribution to a science of behavior.

In summary, both formally and pragmatically Skinner's system falls short of providing an integrative framework for the study of reflexes and purposive behavior. In a formal sense the system is unknowable. At a pragmatic level the system is given coherence by the use of theory and experience-based candidate criteria coupled with the careful tuning of apparatus and procedures. The resultant approach can be used to explore functional relations between variables, but its development has been arrested by its closed definitional formulation and the failure to analyze, develop, and refine its implicit theoretical assumptions.

The production of orderly data is not a sufficient test of a system; it must be further analyzed by testing predictions about the circumstances that produce orderly data. I sympathize with Skinner's (1950) argument that faster progress toward understanding learning often can be made by research that is not primarily designed to test theories. However, if a particular result cannot be reconstructed in terms of testable hypotheses and assumptions, an adequate level of analysis is lacking. This lack will inevitably result in a puzzling inability to predict and control behavior when circumstances are changed.

It might be argued that the dearth of formal theory in Skinner's system is no surprise. Skinner aimed to provide a pragmatic linguistic and procedural framework for controlling reflexive and purposive behavior, not a theoretical edifice. The evidence, though, is that even at a pragmatic level his approach is a surprisingly theory-driven and still incomplete prescription for controlling behavior. Further, I think overemphasizing the pragmatics of

Skinner's approach sells his goals short. He intended to replace all previous theoretical approaches to the study of purposive behavior and to account for reflexes as well. This ambition is worth holding him to.

PURPOSIVE BEHAVIOR, REFLEXES, AND BEHAVIOR SYSTEMS

In the final analysis the conceptual framework Skinner extracted from the analysis of reflexes was not sufficient to account for purposive behavior. This failure is regrettable, for I find Skinner's goal of an integrative framework well worth pursuing. I believe that achievement of that goal can be facilitated by two related tasks: (a) the sorting through and development of Skinner's theoretical assumptions about behavior as revealed in his criteria for the selection of concept candidates and (b) the development of a more accurate and comprehensive view of reflexive and operant behavior than is provided by Skinner's concepts. I have already addressed the first task in some part, so what follows relates to a reconceptualization of the units of analysis.

As Skinner argued, an important aspect of science is generalization from examples (induction). A critical contributor to the success of such generalization is that the conception of the initial examples be sufficiently complex to capture the great majority of the important determinants of behavior. I think a fundamental problem is that Skinner's initial conception of the reflex was incomplete, thus helping limit him to a low-level analysis and a primarily example-driven approach (see also Shimp, 1976).

Skinner was not alone in his treatment of the reflex. According to Fearing (1930), all psychology was dominated by a limited and even imaginary view of the reflex. Even in the 1920s, it was clear that there was no such thing as a simple reflex in an intact animal. As Fearing discussed at some length, simple reflexes cannot "be regarded as isolated units of function in the intact nervous system" (p. 277). Instead, reflexes are modified by motivational states and the nature of the preceding and present stimulus situations.

Most important, reflexes typically evolved as part of a set of adaptive responses related to the achievement of some functional endpoint

or goal. Thus, the eye blink to a palpable stimulus is related to avoiding damage to the eye, but so is raising the arm, ducking or turning the head, throwing the body, or withdrawing from or avoiding the location of the stimulus. It is, in fact, necessary to go to considerable lengths in experiments to avoid engaging these other protection-related responses (many of which also show learned aspects) so that the "pure" eye blink reflex and its conditioning can be measured.

From such an analysis it should be evident that the idea of a "pure" reflex is an invention of the experimenter, not a property of the animal. Most of what we term reflexes are local time-bound reactions to a just presented stimulus of obvious biological significance (the unconditioned stimulus, or US). When a US is predictable, components of these reactions become controlled by predictive stimuli. But these reactions occur in the context of other behaviors, typically at the end of a sequence of complex skeletal behavior preceding the US. For example, in the case of feeding, salivary reflexes come at the end of a sequence of food acquisition that can involve searching, capturing, manipulating, and biting. Similarly, an eye blink typically accompanies or follows a sequence of starting, dashing, and freezing responses in anticipation of an aversive stimulus.

Skinner's focus on the immediate stimulus-response correlation of the spinal reflex, his isolation of the reflex from physiology, and his dependence on tuning of apparatus and procedures all combined to lead away from a conception of learned behavior as based on and appearing within the larger context of a system of behavior. He was compelled by the absence of a more inclusive conception of behavior to divide the world into the extremes of elicited and emitted behavior. Elicited behavior was an invariable reaction to a stimulus presented to a nonfatigued organism. Emitted behavior was the stimulus-independent appearance of a candidate for a response.

Based on what we now know, responses are not divided into such neat and disparate categories (see Skinner, 1977). Reflexes, tropisms, and the like are not automatic results of stimuli. Even the eye blink, at least to a looming stimulus, is not inevitable. More importantly, all the other responses noted above, such as raising the arm, ducking the head, and escaping or avoiding a situation, have both

elicited and emitted qualities. Each of these responses is sensitive to particular kinds of stimuli and their timing, as well as to particular contexts and ongoing processes (e.g., Fentress, 1968). Each response also typically has relatively well-organized aspects of topography, sequence, and orientation prior to effects produced by the immediate situational contingencies.

Nor is the eye blink and its associated behavior unique. All responses are affected by stimulus filtering, response organization, and motivational processes (Timberlake & Lucas, *in press*). In Skinner's terms, there are degrees of elicitation and emission and much to be learned about the relations of stimulus characteristics and response organization. For example, Skinner himself pointed out that the pressure exerted in pressing a lever was correlated with the proximity of reward, and it was difficult to train an animal to press lightly in order to get food. This relation probably represents an evolved dynamic for manipulation responses and food-getting, and this sort of phylogenetically programmed relation is likely to be commonplace.

A major task in increasing the complexity of our conception of the organism is to take behavior out of the literal and conceptual box in which we have placed it and spend time observing its organization in more obviously animal-oriented environments. Skinner himself argued in favor of more description, at one point claiming (1938, p. 33) "We cannot define the concepts of stimulus and response . . . without taking account of the natural lines of fracture along which behavior and environment actually break."

Ethologists have provided a considerable amount of descriptive work, as well as analytic techniques and relatively well-developed low-level theoretical concepts. There are solid data that behavior is structured in functional systems, controlled by particular stimuli, and produced in particular forms. These data in no sense deny the importance of learning, but actually provide a clear place for learning within these systems (e.g., Hailman, 1967; Nottebohm, 1972). Nor do these data deny the importance of experimental manipulation and control. The manipulation of stimuli and motivational states has been an important tool in inferring the nature of the structures and processes underlying behavior (Lorenz, 1981; Tinbergen, 1951).

There appear to be four potential blocks to introducing a more complex, ethological framework into operant psychology. The first is a resistance to any conception that appears to treat the organism as a system processing inputs and outputs, thereby implying that we are looking inside the organism for another organism. However, "looking inside" does not imply letting one's imagination run wild or abandoning the ground of behavior in favor of homuncular uses of anatomy and neurochemistry. The structures and processes of a behavior system can be tied to environmental stimuli, responses, and functions in a straightforward manner. In fact, Skinner's focus on apparatus design and the tuning of procedures and environment make considerable sense if we assume that their efficacy depends on the existence of system structures and processes that, in conjunction with the environment, guide and limit learned responding.

The second obstacle is Skinner's apparent aversion to most theorizing. It is easy to share Skinner's disdain of scientists "... whose curiosity about nature is less than their curiosity about the accuracy of their guesses . . ." (1938, p. 44). Yet the accuracy of guesses is the way we develop, integrate, differentiate, and discard concepts and their proposed relations to each other and the real world. Skinner abandoned the reflex reserve in part because it made inaccurate predictions. However, instead of attempting to analyze the problems, Skinner retreated to a simple, abstract, and closed account of behavior that was difficult both to disprove and improve. In the long run, as Skinner acknowledged (1950), theories have been critical in the development and integration of most areas of science. Despite the potential for overuse of theories, it is certainly better to make explicit and testable one's theoretical assumptions than simply to use them without acknowledgment or development.

A third obstacle to the implementation of a more complex framework for behavior is Skinner's aversion to botanizing stimuli and responses. In Skinner's view such a classificatory scheme will likely be impossibly time-consuming and prove useless in dealing with the dynamic aspects of behavior. Yet classification need be neither overwhelming nor separated from the dynamic aspects of behavior. From the ethologists we have learned a considerable amount about how classification schemes and dynamics can be integrated in the concepts of

drive and instinct (Lorenz, 1981; Tinbergen, 1951). Skinner himself has aptly shown that a careful analysis of a few specific examples can lead readily to development of a general classification scheme. Classification has been a tool of tremendous importance in evolutionary biology by contributing to the development and clarification of general theories of evolution, function, mechanism, and development. I see no reason it cannot serve similar functions in psychology.

The final obstacle to acceptance of a more complex framework is the belief that in principle all behavior can be accounted for by reinforcement (e.g., Herrnstein, 1970). Skinner actually refutes this view in emphasizing the contribution of phylogenetic behavior and motivation to responding (Skinner, 1977, 1979), but at other times he has sounded more sympathetic. This belief is inappropriate on several counts. At a formal level no behavior can be accounted for by reinforcement in Skinner's system because this system is not formulated to make predictions. At a pragmatic level, reinforcement remains incompletely understood, too much so to serve as an account of all behavior. Finally, although learning controls and modifies many aspects of functional behavior, to presume that it is the fundamental causal agent of behavior seems to counter what we know of phylogeny. Primitive animals began as functioning (regulating) organisms in the absence of learning. Learning appears to have evolved to the extent that it provides a more accurate and rapid tracking of predictable environmental variation (Nottebohm, 1972; Timberlake & Lucas, in press).

In brief, I believe it is important to deal carefully with behavior using a more complex model of the organism that incorporates aspects of stimulus filtering, response organization, and hierarchies of motivational processes. Both reflexes and purposive behavior have a place in such a framework, but the distinction between them is neither sharp nor of such central importance. The focus is on the common system organization that includes the basis for both sorts of responses, and on how both environment and organization contribute to behavior rather than its being constructed by conditioning paradigms alone. As Skinner himself notes (1938, p. 434) the ultimate goal "lies in obtaining a system of behavior which is a structure determined by the nature of the subject matter itself." If by the

nature of the subject matter he means the nature and fit of the animal and the environment, I could not agree more.

CONCLUSIONS

Skinner presented a deceptively simple approach to the study of purposive behavior. He drew his formal framework from work on the reflex, and expanded it to account for all purposive behavior. He avoided or abandoned most high-level theoretical concepts but at the same time discouraged examination of his own implicit theoretical assumptions by focusing on the impregnable formal properties of his definitional system and the attractiveness of his examples. Skinner strongly encouraged the establishment of functional relations between variables, although the results were not included in a formal way in his system once he abandoned the reflex reserve concept.

As an iconoclastic commentator and tactician Skinner gets high marks for his analysis of the pervasive hold of mentalism on psychology and the conceptual ambiguities and excesses of modern learning theories. However, the frequency with which he has railed against hypothetico-deductive theories has made it easy to assume that he eschews theorizing in all forms. This position does not accurately represent his approach and may have put off other researchers interested in similar behavioral goals. The development of adequate theories is of critical long-term importance for the prediction and control of behavior.

As a systematist, Skinner had many of the earmarks of a good theorist. He proposed a highly influential set of procedures and nomenclature. He showed considerable pragmatic knowledge of behavior, original ideas, and a predilection for abstract statements. Yet he did little to develop or refine his formal system, or to investigate the causation of aspects of responses not under schedule control. Instead he emphasized abstract concepts, compelling examples, and interesting applications.

As an intuitive engineer of animal behavior, Skinner was without peer, at least among academicians. He was a brilliant experimentalist with an unerring grasp of the way the organism and environment fit together. He was able to control the behavior of animals with great cleverness, speed, and precision. However, Skinner provided insufficient information about

the stimuli controlling his own behavior to account for his success. The outcome of his talents was a simple conceptual system, clever and useful procedures, and fascinating examples that masked an incomplete linkage to the organism-specific structures and processes that allow the system to work.

It is possible to sum up Skinner's approach by borrowing terms from behavioral ecology used to distinguish species that specialize in colonizing new or changing environments (r-selected species) versus those competing for resources in more stable environments (k-selected species). In terms of the universe of possible systems, Skinner has advanced an r-selected system of behavior, a system suitable for rapid colonizing of theory environments that are below carrying capacity. Skinner's approach is simply conceptualized, easily taught, focuses on control, and is readily portable because of the apparent lack of theoretical baggage. Following *The Behavior of Organisms* Skinner weeded out encumbering aspects of his system (such as the reflex reserve) and emphasized its strengths. The result was a system best suited for entering new areas, and less well designed for careful continued analyses of the phenomena it identified. The efficacy of Skinner's approach has been demonstrated in the variety of circumstances in which it has been successfully applied. In the crowded current landscape, however, Skinner's approach requires more complete development if it is to continue to compete as an account of behavior rather than survive only as a testing environment, system of nomenclature, or philosophy.

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