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IN THE BEGINNING ...

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I stumbled across systematic psychology almost by accident. I had entered the graduate program at Columbia, intending to specialize in social and clinical psychology. But to make ends meet (and master some of the fundamentals more securely while doing so), I had accepted a stipend of $37.50 per month to serve as the "reader" in the introductory course for male undergraduates (Columbia College). The first two semesters were fairly uneventful intellectually, but in the spring of 1945 Fred S. Keller returned from his Morse Code research with the U.S. Army Signal Corps and resumed his teaching duties, which included that course. When he handed out a mimeographed outline that proposed a "consideration [of] principles derived from the analysis of human and animal behavior by the experimental method," I realized that something was up. Soon he was into such esoteric matters as the static laws of the reflex, Type S and Type R conditioning, extinction, generalization, stimulus discrimination, response differentiation, chaining, and their application to verbal behavior and thinking.

By then I was beginning my fourth semester (plus two summers) of graduate study. That was just enough training for me to have made two disturbing observations: First, in R. S. Woodworth's laboratory course I had discovered that even in "experimental psychology" many of the best known theories yielded no predictions sufficiently specific to be subjected to empirical test. If they made no predictions, they could not be very useful. Second, the different areas within psychology seemed to be completely compartmentalized, with no broader principles crossing their boundaries to link them together. By contrast, the processes Keller was talking about were objective descriptions of what happened in the laboratory and could readily be replicated. When he further suggested that "the modern psychologist ... seeks to achieve a systematic interconnection of the basic facts of human and animal behavior," I was intrigued. When he proceeded to do so, I was hooked. "Response" as an abstract category seemed to me to be a promising stand-in for whatever specific activity might be of interest in a given situation and laws of "response strength" a promising system for psychology as a whole.

As most readers will recognize, Keller's section on verbal behavior came from a manuscript version of the book that did not appear in print until 12 years later (Skinner, 1957). But almost without exception, the fundamental concepts throughout the rest of the outline could be found in *The Behavior of Organisms: An Experimental Analysis* (1938). Aside from a series of articles that were rarely consulted because the important material was duplicated in the book, that was all there was. In its black cover, the "B of O," as everyone called it, served in the early days as the bible of our movement.

In the years that followed, a large number of writers have commented on Skinner's work, and they have credited him with many and varied contributions—methodological, empirical, and philosophical. The purpose of the present review, however, is to single out the one or two contributions that in my opinion have been most critical to his impact, not only on my own thinking but on the field of psychology as a whole.

A RESEARCH TECHNOLOGY

One factor that could readily be overlooked nowadays is Skinner's contribution to experimental technology. When an innovation

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is widely adopted, it becomes commonplace, and no one any longer gives it much thought. This is what has happened to the arrangement that serves as the discriminative stimulus for that ubiquitous tact, "Skinner box"—the provision of a switch that springs back into place after it is operated by the subject. As control of the experimental routine by an electronic system requires some sort of switching device as an input, it seems likely that when computers finally entered the behavioral laboratory psychologists would inevitably have discovered lever pressing, disk pecking, or some similar device. But Skinner adopted the lever back in 1931, and it was the switch that led to the complex networks of electromechanical programming modules—crude equivalents to today's computers—rather than the other way around. In the decades immediately following publication of the book, there were many investigators who carefully dissociated themselves from the rest of Skinner's thinking but who made use of his lever or his key in their experimental work. Today, a very large part of the research on learning and cognition conducted with nonhuman subjects depends on these devices, including, for example, the use of conditioned suppression and autoshaping to study Pavlovian processes and the use of the matching-to-sample technique to study memorial processes and attention. Research technology with human subjects has also caught up, but in most cases this owes more to the advent of the computer than to the historical influence of Skinner.

Recording the opening or closing of a switch as the subject's response has a number of advantages. Prosaic but important is the fact that it permits automation of the experimental procedure, with its attendant promises of greater precision, more complex contingencies, and much more data for the same investment in human time. And because manipulation of a switch does not ordinarily depend on locomotion, this technology permits greater freedom in programming relationships among events. In the runway or the maze, where the recorded behavior is a change in the subject's location, the ratio of individual steps to reinforcer deliveries, time to reinforcer, effort expended, and change in external stimulation all are likely to be confounded. They are difficult or impossible to disentangle as parameters of the subject's behavior. Furthermore, changing one of these parameters may require time out for the use of a hammer and saw, disrupting the continuity of experimental events. When antecedent stimuli, responses, and consequences are linked by electric circuits, all relationships known to nature or that can be concocted by human imagination should be reproducible and their effects capable of being examined within the laboratory setting. Furthermore, in a manner reminiscent of Watson's remarks about the conditioned response as the unit of "habit" (1924, pp. 207–208), larger patterns of behavior, such as ratios, chains, simulations of foraging and observing, and those produced by a variety of complex schedules, can be constructed by linking individual stimuli and responses together in complex contingency networks. A great variety of such patterns have been arranged in which single presses or single pecks are still recorded as elementary constituents. The abstract category "response" serves an integrative function at the theoretical level, and in a somewhat different fashion the concrete instance of a switch closure serves to integrate the data at an empirical level.

EXTENDING THE SYSTEM

But if the closure of a switch provided a lingua franca at the level of instrumentation and the number of closures per unit of time (rate of responding) provided a lingua franca at the level of measurement, these matters were of concern primarily to experimenters using rats, pigeons, and monkeys to study behavioral processes at the most abstract level. As Skinner noted in the second volume of his autobiography (Skinner, 1979, p. 318), until Fred Keller and Nat Schoenfeld began teaching laboratory courses based on *The Behavior of Organisms,* only 550 copies had been sold.

The technology did make it relatively easy, once the necessary equipment was obtained, to conduct research on basic behavioral processes, and during the period when grant money was in reasonable supply the equipment could indeed be obtained. Soon there developed a community of "operant conditioners" who knew each other both professionally and personally, who spoke a common language, and who felt common loyalties. This was the original foundation of the movement

that eventually established two of psychology's most frequently cited journals, Division 25 of the American Psychological Association, and later, an independent organization known as the Association for Behavior Analysis (see Laties, 1987; Peterson, 1978). Many of the pioneers in behavior modification were trained initially in the conditioning laboratory.

A technology for studying the behavioral processes of rats or pigeons, however, could never by itself have generated a system that would extend beyond the laboratory to the world of practical affairs. Although the language of *The Behavior of Organisms* may sound archaic to contemporary ears, the seeds were there. To put it in a way that will be accessible to a broad spectrum of readers, Skinner had begun the task of constructing a set of concepts capable of dealing in a scientific fashion with the heartland of psychology. His vocabulary was quite forbidding to those encountering it for the first time. It required an effort to master. But it was on the one hand more tightly tethered to concrete observations than the language of any other systematic treatment and on the other was capable of transcending the boundaries between the conventional physical categories used in everyday discourse (for a panorama of application, see Lundin, 1974; Martin & Pear, 1983; Miller, 1975).

CONSTRUCTING A THEORY

Because the presidential address he delivered to the Midwestern Psychological Association in May of 1949 was entitled "Are theories of learning necessary?" (published in 1950), Skinner has almost invariably been described in subsequent commentaries as "atheoretical," or even "antitheoretical." But I believe this to be a profound misconception. Finding appropriate terms for the description of behavior is in itself a theoretical enterprise. A revealing glimpse into Skinner's views on theories and how they should be constructed is provided by the remarks he had delivered just 2 years earlier at a symposium conducted on March 4th and 5th, 1947, at the University of Pittsburgh (Skinner, 1947). There he declared quite explicitly and unabashedly that "A theory is essential to the scientific understanding of behavior as a subject matter" (1947, p. 29). In the preceding paragraph, he had specified what he considered a theory to be: "Theories ... are statements about organizations of facts.... But they have a generality which transcends particular facts and gives them a wider usefulness" (1947, p. 28; see also Skinner, 1961, p. 230).

After presenting his usual objections to such explanatory fictions as a controlling mind or a hypothetical neural structure, he went on to suggest that "The first step in building a theory is to identify the basic data" (1947, p. 34; 1961, p. 233). To illustrate this stage of theory construction, he cited the work of Galileo, who chose "to deal with the positions of bodies at given times, rather than with their hardness or size. This decision ... was not so easy as it seems to us today" (1947, p. 38; 1961, p. 235). As the psychological counterpart, he pointed to the problem of delineating "the parts of behavior and environment between which orderly relations may be demonstrated" (1947, p. 34; 1961, p. 233). In its second stage, he suggested, theory should "express relations among data" (1947, p. 37; 1961, p. 235), preferably in terms as general as possible. Then, in the third stage, new concepts might emerge, as the concept of acceleration had emerged from the second-stage relation between position and time.

Although he never made it to the third stage, I believe this account of the first two stages to be a reasonably accurate description of what Skinner considered his own activities to have been when he was writing *The Behavior of Organisms* some 10 years earlier. After choosing "stimulus" and "response" to represent relevant facets of the environment and the behavior, respectively, he began to examine some of the kinds of relations that were possible between these categories of event.

To those who have not read Skinner, the terms stimulus and response have an unfortunate connotation. They were borrowed, via Pavlov, from the science of physiology, and to the casual observer, the physiology of the reflex appears extremely atomistic. Although "stimulus" and "response" may confer an aura of material respectability upon a seemingly more ephemeral subject matter, they also conjure up visions of much too simply determined relations between physical energies impinging on the sensory receptors, on the one hand, and muscular contractions and

glandular secretions on the other (e.g., Breger & McGaugh, 1965). This stereotyped picture of Skinner's views may be responsible for the demand that he and other behaviorists take up the burden of accounting for complex patterns of activity in physiological terms—a task that is actually quite distinct from that of the experimental analysis of behavior—and for some of the charges frequently leveled by critics who do not seem to be familiar with what has actually been said. If the terms stimulus and response were not available, however, others would have to be invented in their stead. As Skinner has pointed out (e.g., 1938, p. 9), there is no way to study changes in the totality of behavior as a function of changes in the totality of the environment. In order to measure, or even to record, it is necessary to select some part of that behavior. And when it is shown that a given part of the subject's behavior can be changed by changing some part of the environment, that part is known as a "stimulus."

There are other concepts of systematic importance to be found in *The Behavior of Organisms*. Somewhat to my surprise, when I returned to the book after a number of years, I found that these concepts were not represented by formal definitions. Most of the content is devoted to empirical determinations of the behavioral processes governing the rate of lever pressing by the albino rat. This is the second stage of theory construction, as outlined by Skinner. But each of the "dynamic laws of the reflex" has embedded within it a classificatory scheme. One may look in vain, for example, for a direct and explicit definition of what is meant by a "discriminative stimulus." But what might be termed a low-profile and incomplete definition can be found on p. 178: "Only in the presence of S^D is R^0 followed by S^1"; and a more complete definition is implied in "THE LAW OF THE DISCRIMINATION OF THE STIMULUS IN TYPE R. The strength acquired by an operant through reinforcement is not independent of the stimuli affecting the organism at the moment, and two operants having the same form of response may be given widely different strengths through differential reinforcement with respect to such stimuli" (1938, p. 228). From *The Behavior of Organisms* it is possible to extract such broad concepts as positive reinforcement, negative reinforcement (in those days meaning punishment), extinction, stimulus generalization (induction) and discrimination, response induction and differentiation, successive approximation (shaping), chaining, and conditioned reinforcement. In later years, as the system has expanded, other concepts have been added.

CONCEPTS AND DEFINITIONS

The full meaning of the word "concept" is difficult to grasp unless one is familiar with at least one illustrative study of the concrete behavioral relationships to which the term is applied. In laboratory studies, the experimental subject is often human, and some new and undefined word has typically been used as the response (e.g., Hanfmann & Kasanin, 1937; Hull, 1920; Kuo, 1923); but the subject may also be nonhuman, in which case the closure of a certain switch will do just as well (e.g., Kelleher, 1958). What is recorded in either case is the subject's response to each member of a population of stimuli that differ in a number of respects. In essence, to have a concept is to react in the same way to a certain group of stimuli that presumably have something in common but not to react in that way to other stimuli that do not share the common characteristic. A variety of different groupings may be possible but only one is currently acceptable to the experimenter.

Outside the laboratory, too, nature may be sliced in a variety of ways. To take a literal example, picture two butchers slicing up a leg of beef, one in Bloomington, Indiana, and the other in Laconia, New Hampshire. They will cut the beef in different places and divide it into different parts. If you ask for "round steak" in Bloomington, you will get a certain cut with certain properties, to be prepared in certain ways, but if you ask for the same cut in Laconia you will get a puzzled look. And it is not merely a matter of the name by which they identify it: The butcher in Laconia will have nothing entirely equivalent, with quite the same properties. The concept does not exist.

Similarly, any population of instances can be divided in a number of ways—for example, according to number, according to shape, according to size, according to color, or according to much subtler properties (e.g., Gardner & Gardner, 1984; Herrnstein & Loveland,

1964; Smoke, 1932). Some of these distinctions may be useful for a given purpose, others not. Technically speaking, what we are dealing with here is the set of discriminative stimuli controlling a given response. In verbal behavior, this relation between stimuli and a response is what Skinner (1957) called a tact. Usually, selective reinforcement determines the boundaries. When the rules as to what is included and what is excluded can themselves be verbalized—which is not always the case—we have a definition. Definitions are rules governing the application of the words they define.

A science is no better than its definitions. Rules for the application of words to events are critical steps both in testing a theory and in applying it. In some sense—this is not the place to spell out the details—all scientific theory must ultimately be reducible to a set of propositions of the form that circumstances A (for antecedent) lead to outcome C (for consequent). These are descriptive statements that at their most basic level merely summarize sequences of events. When testing or applying such statements, we make the added assumption that the A–C relationships observed in the past will continue to hold for the future.

The statements relating verbal antecedent (VA) and verbal consequent (VC) are isomorphic with the relations between observed antecedent (OA) and observed consequent (OC), and the role of definition is to translate words into observations and observations into words, moving from one level to the other and back again, just as we might translate back and forth between two languages. When testing a theory, we begin with the verbal specification of antecedent circumstances (VA), use a definition to select an experimental procedure (OA), carry out that procedure, observe the outcome (OC), and translate the data by way of definition back into the terms of the theory (VC). If the outcome of the experiment is consistent with the outcome specified by the theory (OC = VC), we keep that theory; if not, we modify what we have said.

In a very broad and abstract sense, the uses we make of theories come under the headings of prediction and control. To make a prediction, we observe the current circumstances (OA), find a theory that applies to those circumstances—has a corresponding antecedent (VA)—take the outcome specified by that theory (VC), and translate that back into an observable outcome (OC), which constitutes the prediction. When we use a theory to control events, we begin with the desired result (OC), find a theory with a corresponding outcome (VC), backtrack to the conditions stated by the theory for such an outcome (VA), and translate those conditions back into the necessary practical operations (OA).

Although definitions were not at all conspicuous in *The Behavior of Organisms,* Skinner's behavior at the first Conference on the Experimental Analysis of Behavior less than a decade later confirmed my budding suspicion that they are extremely important. Quite a bit of his time was allocated to tentative specifications for a variety of basic terms to be used in the science of behavior (see also Figure 2 in Dinsmoor, 1987). In the early days of *JEAB,* a number of readers complained about technical vocabulary that made material in the new journal difficult to follow. But without that vocabulary, our authors could not make sensible statements about their procedures, let alone their conclusions. I have discovered that undergraduate students and cognitive psychologists also experience difficulty with behavioral language and often feel that it is merely a pedantic version of ordinary English, capable or almost capable of a literal, one-to-one translation into their mother tongue.

INADEQUACY OF POPULAR VOCABULARY

This is not the case. The linguistic categories that develop and are passed along from generation to generation within a given culture are presumably of some effectiveness in dealing with the exigencies of daily life, but they are not the same as those needed for valid and general scientific statements. Consequently, the words that are reinforced and the circumstances under which they are reinforced in everyday conversation are not the same as those in various scientific communities. To put it succinctly, different concepts are needed.

That Skinner recognized the inadequacies of the boundaries established by popular usage is evident from the following passage:

> [The] science of behavior ... must not take over without careful consideration the schemes which underlie popular speech. The vernacular is clumsy and obese; its terms overlap each other, draw unnecessary or unreal distinctions, and are far from being the most convenient in dealing with the data. They have the disadvantage of being historical products, introduced because of everyday convenience rather than that special kind of convenience characteristic of a simple scientific system. It would be a miracle if such a set of terms were available for a science of behavior, and no miracle has in this case taken place. (1938, p. 7)

It is difficult to conceive of a way in which any of the physical or biological sciences could have achieved even a semblance of their current effectiveness without the construction of technical vocabularies. The observations must be classified in appropriate ways to make possible the generation of valid descriptive statements of any substantial degree of generality. If the whale were to be classified as a fish, for example, or the tomato as a vegetable, otherwise correct statements about fish or about vegetables would for these cases be rendered incorrect. However, when whales are classified as mammals and tomatoes as fruit, all is well. Consistency is preserved.

A SCIENCE OF BEHAVIOR

Because they do not use a mentalistic vocabulary, Skinner and other behaviorists frequently have been accused by cognitive psychologists of ignoring or denying all that is most interesting and most central to psychology. But what Skinner was avoiding was not the processes themselves, as should be obvious from his later writings, but only words that were inadequately related to their controlling stimuli:

> The traditional description and organization of behavior represented by the concepts of "will," "cognition," "intellect," and so on, cannot be accepted so long as it pretends to be dealing with a mental world, but the behavior to which these terms apply is naturally part of the subject matter of a science of behavior. What is wanted in such a science is an alternative set of terms derived from an analysis of behavior and capable of doing the same work. . . . . Traditional concepts are based upon data at another level of analysis and cannot be expected to prove useful. They have no place in a system derived step by step from the behavior itself. (1938, p. 441)

In common with those used by other learning theorists, Skinner's concepts occupy a position that is central to psychology as a whole. It was no accident that the first attempts to communicate his views to a broader audience (Keller & Schoenfeld, 1950; Skinner, 1953) took the form of textbooks to be used for introductory courses rather than for some specialty area within the discipline. Almost all behavior of interest to psychologists in such areas as personality and social, clinical, industrial, educational, and developmental psychology is behavior that is greatly influenced if not wholly shaped by a process of learning; although some interventions by applied psychologists take the form of changes in the situation, more often they take the form of changing the individual's reactions to that situation, a process that in most cases can be classified as learning.

Although they differ greatly in the data bases they employ and in their scientific rigor, there is a curious similarity between behavior analysis and psychoanalysis. Perhaps it is more than a coincidence that they both apply the word "analysis" quite broadly, one to behavior and the other to the psyche. Both have rejected the conscious mind of common-sense psychology as the arbiter of behavior and have presented themselves as alternative systems capable of integrating a large part of their subject matter. The frequent use of the word "dynamic" in *The Behavior of Organisms* is reminiscent of the use of the word "psychodynamic" in clinical writings. Perhaps some reference was intended to the underlying emotions or motivations—whatever these words may mean—that affect the behavior in a way that is orthogonal to its immediate context (see Skinner, 1938, pp. 21–26). But a more rigorous and more inclusive characterization is that both systems attempt to describe the laws of motion governing the characteristic reactions of the individual to certain situations that recur from time to time. That is, there is reference to the prior history of the organism. Writers like Dollard and Miller (1950) and Mowrer (1950), closely associated with Hull, devoted considerable attention to the behavioral interpretation of

psychoanalytic mechanisms, and so, in later writings, did Skinner (e.g., 1953, 1961).

INTERVENING VARIABLES

Some of the terminology used in *The Behavior of Organisms* has virtually disappeared from current usage (e.g., Type S and Type R conditioning, now called by other names, or the reflex reserve, a construct that has since been discarded); but the fruitfulness of other concepts is indicated not only by their continued use in research on conditioning but frequently by their extension to applied settings (e.g., see Bijou & Baer, 1961, 1965; Lundin, 1974; Martin & Pear, 1983; Miller, 1975). The same cannot be said for most of the terms suggested by rival theorists from the same period (e.g., Guthrie, 1935; Hull, 1943; Tolman, 1932), although they, too, were attempting to deal with the central problems of psychology at the most general level.

One of the reasons for the popularity of Skinner's concepts among those interested in the application of psychology may lie in his sparing use of mediating constructs. True, his attitude toward such constructs has never been quite as proscriptive as that ascribed to him by many current writers, both critical and friendly. So far as I can determine, he has never denied their scientific legitimacy; he has only questioned their strategic value for the conduct of research in the science of behavior. In fact, in *The Behavior of Organisms,* he himself made use of such "hypothetical middle term[s]" as reflex strength, the reflex reserve, and particularly the "intermediate state[s]" (p. 24) of emotion and drive—concepts that he explicitly identified as corresponding in their status within the system to the mediating constructs that had recently been proposed by Tolman: "What has here been treated as a 'state,' as distinguished from the operation responsible for the state, is called by Tolman an 'intervening variable'" (1938, p. 437; note also on p. 25 his recognition of the distinction between learning and performance). What is more, in a footnote he established his precedence by citing an earlier publication (Skinner, 1931).

Intervening variables (by which I mean to include hypothetical constructs, which are sometimes considered a separate category) play a central and a critical role in such matured sciences as physics, chemistry, and genetics. But examination of the difficulties encountered by Tolman and by Hull with their respective systems suggests that Skinner took the wiser course in staying as close as possible to his data, holding such constructions to a minimum. The problems with Tolman's and Hull's systems were opposite in nature, and the failures at both extremes suggest that such a fate may have been inevitable at any point along the continuum.

In Tolman's case, the proposed variables were borrowed from traditional common-sense psychology and hence were intuitively appealing, but they were largely programmatic. He offered only a tentative list of what the appropriate theoretical terms might be, and he was understandably reluctant to labor over the details of their definition. That could come later. Tolman's strategy allowed considerable flexibility and made it easy for him to "explain" results that posed difficulties for Hull and perhaps even for Skinner. But as a consequence of that strategy, Tolman's system never achieved the specificity of prediction needed to compete as a viable alternative to those offered by Skinner and Hull.

Hull went to the opposite extreme. He constructed a highly formalized network of intervening variables in which most of the relationships, including those with observable events, were specified with a degree of rigor that was, within psychology, quite rare. Looking back, the image that often occurs to me is that of Icarus, who strapped on wings of feathers and wax to escape from an island prison. He flew too high, the sun melted the wax, and he fell into the sea. Hull's ambition was too lofty, and his goals could not be achieved. Perhaps he was naive to think that such an enterprise could succeed, but one can still admire his daring and the height to which he flew.

Attempts were made by others to extend Hull's system to the realm of practical affairs. Dollard and Miller (1950) constructed a greatly simplified version of Hull's system—in itself a significant move—and used it to "explain" a variety of psychodynamic processes noted in the clinic. (Unfortunately these processes were themselves open to question on empirical grounds.) Although in the United States the first behavior modifiers were inspired by Skinner (see Ullmann & Krasner,

1965; Ulrich, Stachnik, & Mabry, 1966), in Britain they usually cited Hull (e.g., Eysenck, 1960). But Skinner's system is still with us, whereas the Hullian contribution seems to have petered out. The essential difficulty, in my view, was that Hull's structure was too complex to be practical in application. It was difficult and time-consuming to learn and cumbersome to use, and, errors aside, the benefits simply were not of sufficient magnitude to compensate for the costs.

By avoiding such lengthy verbal linkages between theory and data, Skinner was able to keep his concepts precise, simple, and relatively few in number. They map fairly directly onto the observations themselves, in a manner that Skinner suggested was "purely descriptive" (1938, p. 426). But Skinner's descriptions were certainly couched in terms different from those used by the layperson, based on different criteria.

FUNCTIONAL DEFINITIONS

As stated earlier, the stimuli impinging upon an organism can be categorized in a variety of ways. The classificatory scheme used in *The Behavior of Organisms* is especially effective for a systematic science of behavior because it cuts across a number of important boundaries. What Skinner's definitions ignore is almost as important as the criteria to which they attend. They ignore, for example, the species of the subject, the distinction between the laboratory and the natural world, different settings within the natural world, and different topographies of behavior in each of those settings. From a conceptual point of view, at least, there are no barriers to extrapolation across the face of psychology.

Compare the concepts in Skinner's system with the categories normally used by the nonpsychologist. When asked to describe a scene, the average person might simply list the objects that are present, giving each the name supplied by the national language and perhaps its spatial relation to other objects: "two chairs and a table, a window through which one can see some trees outside. . .," and so on. (Significantly enough, when asked to describe someone's *actions*, the same person might give a highly inferential account, couched in terms belonging to what might best be described as a contemporary folk psychology. One of the things that may be learned from laboratory work with rats and pigeons is to use physical rather than inferential categories in describing behavior.) For inanimate objects, the conventional categories maintained by our culture draw sharp distinctions in terms of immediate physical characteristics, such as shape, size, and color, because these attributes are quickly and reliably discriminated and the distinctions among different objects are important for everyday uses, such as deciding where to sit, what to eat, when to flee, and so on. But if used for a psychological system, this kind of physical specification would impose restrictions that would prevent us from transferring the concept from one experiment to another, let alone to the settings for such target behaviors as studying, personal grooming, inviting on a date, attaching seat belts, or sequences of verbal behavior.

In terms of progress toward a general theory, the situation is not much more encouraging when we turn to the sensory psychologist. There the dimensions used to describe the stimulus are less immediate and therefore less convenient for everyday purposes—they require instrumentation—but serve more accurately to interrelate physiology and function within the sensory field. Although dimensions like wave length, visual angle occluded, luminous flux, angular velocity, auditory frequency, or sound pressure level may be appropriate for the reporting of experiments in vision or audition, in research on conditioning they occupy center stage only in the methods section; in theoretical discussions, they are relegated to the wings. For the most part, we are not much interested in this type of specification, despite its technical sophistication, but in whether stimuli are discriminative, reinforcing, eliciting, inhibitory, aversive, and so on. These classifications require functional definitions.

The type of definition required for a general science of behavior sounds extremely labored to the untrained ear, and the need for this type of definition is by no means apparent to casual observation. Those who teach this form of analysis encounter substantial resistance from many of their students. The failure to appreciate the inadequacy of the traditional vocabulary and the gains to be achieved through a functional classification may constitute the single most important bar-

rier to more widespread acceptance of a truly scientific perspective among professional psychologists, let alone the lay public. Functional definitions are not based on immediate sense impressions but on the location of the stimulus or the response in question within a network of events stretching across some period of time. To put it another way, a history is required. Such definitions are not the most convenient, to be sure, but they are necessary if we are to break away from the popular mythology of our culture and establish a scientific treatment of our subject matter.

In Skinner's system, stimuli are classified in terms of the operations into which they enter and the effects of those operations on the subject's behavior. As originally described, at least, a (positive) discriminative stimulus is present when the specified response is eligible for reinforcement. The response occurs sooner or at a higher rate in the presence of that stimulus than in its absence. In another example, the stimulus may be delivered a number of times following instances of the designated response; if the response occurs at a higher rate under that circumstance, then the stimulus is classified as reinforcing.

In spite of the fact that these functional definitions transcend the limitations of description in terms of *immediately detectable* physical attributes, as indicated above, ultimately they do refer back to concrete physical description, and in a fairly unambiguous manner. Whether a stimulus is present or absent when a reinforcing event arrives is a matter that in most cases can easily, reliably, and objectively be determined by direct observation or by suitable recording apparatus. It can also be verified by an independent observer, and agreement among observers should be close to perfect. Whether the event that takes place in the presence of the stimulus can be classified as reinforcing is determined by measuring some physically specifiable item of behavior (originally, counting the closures of a switch) across different periods of time.

Although these are not the same observations used by the layperson or the sensory psychologist, they are equally physical in their status, and it is the return to concrete physical specification that provides the objectivity so necessary to scientific discourse. Skinner himself has suggested that effectiveness in controlling behavior is a better criterion for the selection of a scientific vocabulary than is agreement among observers. Perhaps the control has to come first, to make the production of a new term reinforcing to its author. But to establish the collective body of knowledge known as a science, we need the ability to communicate with other scientists. Eventually, we must transmit our findings to our students and to the general public, who we hope will make use of our verbal behavior and will provide economic support for our efforts. The transmission of scientific information depends on agreement between the speaker or writer and the listener or reader in the way in which words are used.

In this review, I have argued that even though Skinner largely avoided the use of intervening variables in his work, he nonetheless constructed a theoretical system; and I have argued that the most important source of his influence on the behavior of other psychologists is to be found in the nature of the concepts he proposed for classifying behavioral events. These concepts are the content of the system. Direct testimony concerning the importance of Skinner's definitional structure has been provided by Arnold Buss, who took a course with him in the 1940s. Preparing a reminiscence for the centennial of Indiana University's psychological laboratories, Buss recorded the following observation: "I took a seminar with him, and he asked the class not to necessarily agree with his position but to use his language so that we could understand one another without confusion. By the end of the semester, I discovered that his language equalled his position, and once you became accustomed to talking his way, you were a Skinnerian."

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