PREFACE TO THE BEHAVIOR OF ORGANISMS¹

B. F. SKINNER

HARVARD UNIVERSITY

Two other theoretico-experimental analyses of behavior compose the historical setting in which this book should be evaluated. Tolman's *Purposive Behavior in Animals and Men* preceded *The Behavior of Organisms* by six years; Hull's *Principles of Behavior* followed it by five. The three books differed in many ways: they undertook to solve different problems and they sought solutions in different places.

My debt to Sherrington, Magnus, and Pavlov is obvious in my continuing use of the word "reflex." I held to the term even after I had begun to distinguish between elicited and emitted behavior—between "respondents" and "operants"—for I wanted to preserve the notion of reflex strength. In my thesis I had argued that "'conditioning,' 'emotion,' and 'drive,' so far as they concern behavior, are essentially to be regarded as changes in reflex strength." But in my first experiment I followed changes in the rate of responding rather than in latency, magnitude of response, or after-discharge, as in traditional practice. Probability of response thus emerged as my basic dependent variable. Its importance was clarified by the use of a cumulative record, in which changes in rate over relatively long periods of time can be seen at a glance. A simple mechanical device which generates a continuous plot of this sort while the organism is responding "makes behavioral processes visible."

With the exception of a running wheel my only apparatus was the now familiar "box" containing a food- or water-dispenser operated by a lever which could be depressed by a rat. A few relays and timers turned buzzers and lights on and off and connected or disconnected the lever and the dispenser. In the chronological order in which they were studied experimentally, my independent variables

consisted of deprivation and satiation, reinforcement and nonreinforcement, schedules of reinforcement, differential reinforcement with respect to properties of stimuli and responses, aversive consequences, and a few behavioral drugs.

As I pointed out in *Operant Behavior: Areas of Research and Application* (edited by Werner Honig), operant behavior is essentially the field of purpose. Tolman made a more explicit acknowledgment of the traditional significance of his subject. His experiments were designed to make purpose visible in spatial terms, in the movement of an organism toward or away from a goal object. Almost all the figures in his book which describe apparatus are maps. (It is not surprising that his experiments led to the concept of cognitive mapping and emphasized distinctions between place- and response-learning.) Tolman's docile behavior is no doubt close to operant behavior, but docility, like purpose, was a property or characteristic of behavior rather than a relation to an independent variable.

Tolman classified his independent variables as "stimuli, initiating physiological states, and the general heredity and past training of the organism." But this phrase appears near the end of his book in a chapter entitled "Summary and Conclusions for Psychologists and Philosophers" and is less a summary than a foreshadowing of a different formulation which he was to publish three years later and which contains the expression:

$$B = f(\text{SPHTA}).$$

This is close to the

$$R = f(S, A)$$

of my 1931 paper. Discussions which I had with Tolman during the summer of 1931, when he taught summer school at Harvard University, are, I believe, relevant. I had finished my experiments on "drive and reflex

¹ This preface is slightly revised by the author from one that appeared in the seventh printing (1966) of *The Behavior of Organisms*.

strength," and I was arguing that rate of eating, or of pressing a lever when pressing was reinforced with food, was to be described as a function of a "third variable"—that is, a variable in addition to stimulus and response, in this case a history of deprivation and satiation. Although I continued to use the concept of drive for many years (J. R. Kantor eventually convinced me of its dangers), I regarded it simply as a convenient way of referring to *environmental* variables. Tolman, however, made it an *intervening* variable, apparently in his avowed concern with replacing or reinterpreting mental processes. The "B" in his equation represents behavior, but it was to be quantified, if at all, as a "behavior ratio." The maze had been simplified to the form of a "T," but no further, and a remnant of spatial purpose thus survived. Behavior at a choice point was as close as Tolman came to the concept of probability of response.

Hull's dependent variable was Habit Strength. "Habit" came from 19th-century studies of animal behavior, but "strength" was in the reflex tradition, and Hull accordingly used such measures as latency and magnitude of response. When he and his students turned to the use of a modified "Skinner Box" (Hull seems to have been responsible for the expression, which I have never used), the notion of probability of response began to emerge, but it appears relatively late in his book and only in passing, and has not been emphasized by subsequent workers in the Hullian tradition. Hull's independent variables are exemplified by stimuli, number of reinforcements, and deprivation.

Although all three books are ostensibly concerned with explaining observed behavior in terms of observable conditions and events, both Tolman and Hull quickly became preoccupied with internal states and processes. Some such move is inevitable so long as an effort is made to characterize the interchange between organism and environment as input and output. Output can seldom if ever be related to input in any simple way, and internal activities are therefore invented to make adjustments. It is not surprising that modern cognitive psychologists should have been strongly influenced by information theory, where a system is said to convert input into

output by acquiring, processing, storing, and retrieving information. Activities of this sort are modern versions of Tolman's substitutes for mental processes. Although Tolman insisted that his behavior-determinants were "to be discovered, in the last analysis, by . . . experiments" and that "they have to be inferred 'back' from behavior," he nevertheless made them his primary object of inquiry, and this has set the pattern for cognitive psychology.

For S-R psychologists, response stands for output and stimulus for input, and when the one cannot be accounted for in terms of the other, mediating activities are again invented. (How dangerous this can be is seen in the fact that two of Hull's principles, afferent neural interaction and behavioral oscillation, serve no other function than to account for failure to relate the objective terminal events in a meaningful way.) The properties of Hull's mediating system were also to be inferred from environmental measures, but Hull himself began to insert references to the nervous system.

The Behavior of Organisms is often placed, quite erroneously, in the S-R tradition. The book remains committed to the program stated in my 1931 paper in which the stimulus occupied no special place among the independent variables. The simplest contingencies involve at least three terms—stimulus, response, and reinforcer—and at least one other variable (the deprivation associated with the reinforcer) is implied. This is very much more than input and output, and when all relevant variables are thus taken into account, there is no need to appeal to an inner apparatus, whether mental, physiological, or conceptual. The contingencies are quite enough to account for attending, remembering, learning, forgetting, generalizing, abstracting, and many other so-called cognitive processes. In the same way histories of satiation and deprivation take the place of internalized drives, schedules of reinforcement account for sustained probabilities of responding otherwise attributed to dispositions or traits, and so on.

These characteristics of the present formulation become more significant as we move from contemporary setting to subsequent history. It is instructive to examine a recent issue of the *Journal of the Experimental Analysis of Behavior* or the book *Operant Behavior: Areas*

of *Research and Application*. The cumulative records in *The Behavior of Organisms*, purporting to show orderly changes in the behavior of individual organisms, occasioned some surprise and possibly, in some quarters, suspicion. Today, most of them seem quite crude. Improved experimental control has yielded much smoother curves. Cumulative records are also now often supplemented by distributions of inter-response times and on-line computer processing of changes in rate. The "organism" of my title, the laboratory rat, has been joined by scores of other species, including man. The lever has made room for operanda appropriate to many other topographies of behavior. Independent variables are much more carefully manipulated: buzzers and lights are often replaced by stimuli controlled with the precision characteristic of human psychophysics. To food and water have been added other positive and negative reinforcers. The negative reinforcers have opened up new territory in the study of avoidance, escape, and punishment. Caffeine and benzedrine have proved to be only the beginning of a long line of "psychotropic" drugs.

As the power of the analysis has grown, more and more complex behavior has been studied, under contingencies which approach the subtlety and complexity of the contingencies to be found in the environment at large. More of what the organism is doing at any given time is analyzed. Multiple stimuli and multiple responses compose complex systems of concurrent and chained operants. Experiments may last for weeks rather than for the standard hour of *The Behavior of Organisms*. Special environments may be maintained from birth. The apparatus required for all this is necessarily much more elaborate: simple relays, timers, and counters have given way to solid-state circuitry and computers. All these advances were facilitated by a formulation which emphasized behavior rather than supposed precursors of behavior and observable variables rather than inferred causal states or processes.

Much current basic research is essentially a technological application. The study of stimulus control, for example, has yielded a kind of nonverbal psychophysics. An extensive application to physiology seems to confirm the argument of Chapter 12 that the behavior "mediated" by the nervous system needs to

be rigorously described before a neurological account can be seen to be adequate. Operant research plays an important role in psychopharmacology. The techniques are also applied with increasing frequency to what might be called the biology of behavior, sometimes to the surprise of ethologists. Many of these applications of the techniques of an experimental analysis have meant generous return support for basic research.

Other applications are more in the spirit of a behavioral technology. Toward the end of the book I pointed out that extrapolations to human affairs had been avoided ("Let him extrapolate who will"), but I was already at work on an extension to verbal behavior. My book on that subject, published nearly twenty years later, has not always been understood by linguists and psycholinguists, but it seems to me an essential step in extending the analysis to human behavior. One important application—both verbal and non-verbal—has been made in education. If teaching may be defined as the arrangement of contingencies of reinforcement under which students learn, then the various pieces of apparatus used in the experimental analysis of behavior are teaching machines. Many processes analyzed in *Verbal Behavior*, particularly in the chapter on "Supplemental Stimulation," are basic to programmed verbal instruction. An early example of "shaping" behavior by arranging a program of increasingly complex contingencies appears near the end of Chapter 8.

The application to psychotherapy is not so far advanced, though O. R. Lindsley's pioneer work has been followed by many other studies of psychotic and retarded subjects. Lindsley has developed the notion of a "prosthetic environment," an environment so designed that a person may live in it effectively in spite of behavioral deficiencies. But all designed environments are prosthetic, in the sense that they facilitate or otherwise encourage specific kinds of behavior. My interest in the design of a culture—in government in the broadest sense—led to a fictional treatment in *Walden Two*, in which many issues raised by the possibility of a successful science of behavior are discussed.

So far as the facts are concerned, *The Behavior of Organisms* is out of date. It still seems to me a viable book, however, for it presents

a useful formulation of behavior supported by a selection of illustrative experiments. It may also serve as a reminder that a promising conception of human behavior has been derived from an analysis which began with

simple organisms in simple situations and moved on, but only as its growing power permitted, to the complexities of the world at large.