

WHAT IS THE EXPERIMENTAL ANALYSIS OF BEHAVIOR?¹

B. F. SKINNER

HARVARD UNIVERSITY

A new division of the American Psychological Association calls for some effort to define the enterprise that has come to be known as the Experimental Analysis of Behavior. It is sometimes easier to place a piece of research correctly within that field or to exclude what seems to be similar work than to give one's reasons for doing so. Although sharp boundaries can seldom be drawn in defining any scientific area, certain distinguishing features are worth pointing out.

The Dependent Variable

A natural datum in a science of behavior is the probability that a given bit of behavior will occur at a given time. An experimental analysis deals with that probability in terms of frequency or rate of responding. Like probability, rate of responding would be a meaningless concept if it were not possible to specify topography of response in such a way that separate instances of an operant can be counted. The specification is usually made with the help of a part of the apparatus—the “operandum”—which senses occurrences of a response. In practice, responses so defined show a considerable uniformity as the organism moves about in a framework defined by its own anatomy and the immediate environment. Changes in rate are usually recorded and inspected in the ubiquitous cumulative record, although distributions of interresponse times and on-line computer analyses of rates and changes in rate are increasingly used. An

emphasis on rate of occurrence of repeated instances of an operant distinguishes the experimental analysis of behavior from kinds of psychology which proceed in one or more of the following ways.

(1) Behavior is taken merely as the sign or symptom of inner activities, mental or physiological, which are regarded as the principal subject matter. Rate of responding is significant only because it permits us to follow a process (such as learning or maturation) or to determine a state or condition (such as an excitatory tendency or alertness or wakefulness) or to detect available psychic energy or the strength of a drive or emotion, and so on. The observed behavior is not expected to be very orderly because it is only a rather noisy “performance”, from which presumably more stable states and processes are to be inferred with the help of statistical procedures. These practices have discouraged a careful specification of behavior, and the data obtained with them are seldom helpful in evaluating probability of response as such.

(2) Behavior is held to be significant only in meeting certain standards or criteria. An organism is described as “adjusting to a situation”, “solving a problem”, “adapting to the environment”, and so on. With respect to normative criteria its behavior may improve or deteriorate, with respect to developmental criteria it may be arrested or accelerated, and so on. In reporting these aspects of behavior the experimenter may not specify what the organism is actually doing, and a rate of responding cannot be satisfactorily inferred.

(3) Changes in probability of response are treated as if they were responses or acts. The organism is said to “discriminate”, to “form concepts”, to “remember”, to “learn what to do” and, as a result, “know what to do”, and so on. These are not, however, modes of response. To discriminate is not to respond but

¹Introductory remarks at a Symposium on the Application of Operant Conditioning, Annual Meeting of the American Psychological Association, Los Angeles, September 6, 1964. Preparation of this manuscript has been supported by Grant K6-MH-21,775 of the National Institute of Mental Health of the U. S. Public Health Service, and by the Human Ecology Fund. Reprints may be obtained from the author, Dept. of Psychology, Harvard University, 33 Kirkland St., William James Hall, Cambridge, Mass. 02138.

to respond differently to two or more stimuli. To say that an organism has learned to discriminate between two stimuli is to report a possibly useful fact, but it is not to say what the organism is actually doing.

(4) The dimensions studied, though quantifiable, are not related in any simple way to probability of response. The force with which a response is executed and the time which elapses between stimulus and response—called, often inaccurately, latency or reaction time—are popular measures. When they change under differential reinforcement, they are relevant to an experimental analysis, but they may not throw much light on probability. Other common measures, such as the time required to complete a task—to get through a maze, to solve a problem, or to cross out all letters of a given kind on a page—or the number of errors made or the number of trials taken in meeting a criterion, are still less useful. “Amount remembered,” an aspect of behavior first emphasized by Ebbinghaus, has recently enjoyed a renewed popularity. The experimenter may want to know, for example, how a set of responses comes under the control of a corresponding set of stimuli, but instead of following the change in probability he measures the number of responses “correctly emitted in recall” at a later time.

An experiment is often designed so that the important result is a ratio between two such measures, when the arbitrariness or irrelevance of the aspects measured seems to cancel out. A ratio is still of little help in an experimental analysis. Such measures are chosen primarily because they are quantifiable—force of response can be accurately recorded, number of trials exactly counted, and elapsed time measured on the most accurate of clocks—but quantifiability is not enough. Rate of responding is a basic dimension, not simply because responses can be accurately counted, but because rate is relevant to the central concern of a science of behavior.

(5) The inner entities of which behavior is said to be a sign or symptom include the traits, abilities, attitudes, faculties, and so on, for which various techniques of psychological measurement have been designed. But even the most impeccable statistical techniques and the most cautious operational definitions will not alter the facts that the “tests” from which the data are obtained are very loosely con-

trolled experimental spaces and that the “scores” taken as measures have some of the arbitrary features just mentioned. The important issues to which these techniques have been directed—for example, the covariation in probability of groups of responses—must be studied in other ways before the results will be useful in an experimental analysis.

(6) Instead of observing behavior, the experimenter records and studies a subject’s statement of what he would do under a given set of circumstances, or his estimate of his chances of success, or his impression of a prevailing set of contingencies of reinforcement, or his evaluation of the magnitude of current variables. Observation of behavior cannot be circumvented in this way, because a subject cannot correctly describe either the probability that he will respond or the variables affecting such a probability. If he could, he could draw a cumulative record appropriate to a given set of circumstances, but this appears to be out of the question.

The Independent Variables

The task of an experimental analysis is to discover all the variables of which probability of response is a function. It is not an easy assignment, but it is at least an explicit one. It distinguishes an experimental analysis of behavior from other approaches at many points.

(1) The stimulus is, of course, an important independent variable. An early association with the concept of the reflex gave it the character of a goad, something which forced an organism to respond. This was perhaps as wrong as the traditional view that the organism forced the environment to stimulate—to become visible, audible, and so on. The position of an experimental analysis differs from that of traditional stimulus-response psychologies or conditioned reflex formulations in which the stimulus retains the character of an inexorable force. It does not follow, however, that the organism acts upon the environment in the manner suggested by terms like *detect*, *identify*, *perceive*, *experience*, *classify*, and *judge*, or by terms which appear to describe later responses to stimuli, such as *recall how something looked* or *remember what happened*. Such terms, like expressions borrowed from computer technology which describe the organism as processing information, do not specify what the organism is actually doing.

The concept of the discriminative stimulus (the well known "S^D") and the related notion of stimulus control assign to stimuli a more reasonable role as independent variables.

An experimental analysis describes stimuli in the language of physics. The experimenter does not ask whether a stimulus looks the same to the organism as it does to him. In studying a generalization gradient with respect to wave length of light, for example, lights are sometimes matched for brightness, so that the gradient will represent a reaction to color only; but this is an unwarranted intrusion into the data. To guess what an organism sees when a stimulus is presented and to suppose that what is guessed is what is being presented would be to abandon all that physics has to offer by way of specifying environmental events. The importance of certain classical problems is not thereby denied. Stimuli are often difficult to specify in physical terms. Different stimuli may appear to have the same effect and the same stimulus different effects under different conditions. But it is no solution to fall back upon the response of an experimenter to achieve some sort of invariance. Similarly, any reference to "parameters relating to the complexity of a task" or to "frustrating" or "anxiety-generating" properties of a situation is also objectionable, whether the subject or the experimenter serves as indicator of the complexity or the emotion.

(2) Other independent variables are found in the classical fields of motivation and emotion. The experimental analyst does not manipulate inner states as such. He manipulates, not hunger, but the intake of food; not fear as an acquired drive, but aversive stimuli; not anxiety, but preaversive stimuli. He administers a drug, not the physiological effects of a drug. He takes the age of an organism, not some level of maturation, as a variable. He sometimes uses a collateral dependent variable—but not as a measure. He may use body-weight, for example, in lieu of a history of deprivation, but it is simply another effect of deprivation, not a measure of hunger.

(3) The so-called "contingencies of reinforcement" are an important feature of the independent variables studied in an experimental analysis. A few contingencies, such as conditioning, extinction, and delay of reinforcement are familiar. Somewhat more complex contingencies, such as those responsible

for stimulus discrimination and response differentiation, are also fairly well known. But many psychologists are unaware of the complexity of the contingencies now commonly studied. In addition to many standard schedules of reinforcement, reinforcement may be contingent on rate of responding, rate of change in rate, or specific patterns of rate changes detected by on-line computer analyses. Contingencies may involve several stimuli and responses interrelated in various ways. Considerable skill may be needed to design programs of instructional contingencies which will bring behavior under the control of complex terminal contingencies of this sort. The importance of programming is, indeed, often completely overlooked. For example, the statement that a given type of organism or an organism of a given age "cannot solve a given kind of problem" is meaningless until the speaker has specified the programs which have been tried and considered the possibility that better ones may be designed.

Describing a set of contingencies in instructions to the subject is no substitute for exposing the subject to the contingencies, particularly when they need to be programmed. Instructions have effects, of course, depending in part on the verbal history of the subject, but the behavior of a subject to whom an experimenter has explained how a piece of apparatus works will not necessarily resemble one who has come under the control of the terminal contingencies established by that apparatus.

Contingencies of reinforcement have been analyzed formally in theories of probability, decision-making, and games, but the theorist often has no way of knowing, aside from observation of his own behavior, what effects a given set of contingencies will have or what kind of program may be needed to make it effective. Certain assumptions—for example, that an organism will behave rationally—are sometimes used in lieu of observations to complete a statement of contingencies. Formal statements of contingencies, like instructions, have their effects and if detailed enough may supply rules which function as prior stimuli to control behavior resembling that which would be generated by prolonged exposure to the contingencies themselves. The two cases must, however, be clearly distinguished. When an organism is brought under the control of

complex contingencies, it is not necessarily "applying the rule" which describes them.

The increasing power of an experimental analysis has made it possible to examine the effects of complex contingencies to which an organism has traditionally been assumed to adjust only by exercising certain cognitive processes. It is sometimes obvious that such processes have been invented simply to account for the behavior in the absence of any better information as to how the contingencies could generate it. The experimenter has not been able to relate the behavior to the contingencies, and he is forced to conclude that the organism has somehow done so mentally. Supposed cognitive processes of this sort may be disregarded. Others, however, may be a sort of internalized version of precurrent behavior—behavior maintained by its effects in maximizing the reinforcement of subsequent responses. Precurrent behavior is part of the subject matter of an experimental analysis. It is usually studied in overt form though it may eventually drop to the covert level. In either case it is defined as behavior which affects behavior rather than as mental activity.

Treatment of Relationships among Variables

The behavioral processes studied in an experimental analysis usually consist of changes in probability (or rate of response) as a function of manipulated variables. The changes are followed in real time rather than from "trial to trial"—a practice derived from accidental features of early psychological research. An emphasis on real time is another reason why cumulative records are useful. (A cumulative record is sometimes used to "smooth" other kinds of data—for example, the errors made during repeated trials in learning a maze or in solving a problem—and it is often implied that a cumulative record of responses in time also gains an unwarranted smoothness of the same sort. The important difference is that the slope of a cumulative curve in real time represents a meaningful state of behavior.)

Relations among dependent and independent variables are seldom explored according to a prior "experimental design", as R. A. Fisher used that term. The null hypothesis finds itself in the null class. Research which is not designed to test hypotheses—physiological, mentalistic, or conceptual—may seem puzzling to those who identify statistics with scientific

method, though it appears perfectly reasonable to physicists, chemists, and most biologists. The usual practice is to construct an experimental space in which stimuli, responses, and reinforcements are interrelated in a set of contingencies. The contingencies depend in part on the behavior which the organism brings to the experiment. Provision is usually made for changing the apparatus as the behavior changes, but seldom according to a predetermined plan. The experimental control of variables is emphasized rather than a later evaluation of their presumed importance through statistical analyses. The number of organisms studied is usually much smaller than in statistical designs, but the length of time during which any one organism is observed is usually much greater.

It is often said to be impossible to distinguish between significant and insignificant facts without a hypothesis or theory, but the experimental analysis of behavior does not seem to bear this out. It has progressed by building upon its past. Improved formulations and techniques have led to more precise and reproducible data over a much greater range, but not to the outright rejection of earlier work. (For one thing, few data have become useless because a theory they were designed to test has been discarded.) In retrospect there appears to have been little random or aimless exploration. Such a field as the systematic analysis of contingencies of reinforcement, for example, does not require a theory. In our study of schedules of reinforcement Ferster and I proceeded in a rather Baconian fashion, filling in a table of the possibilities generated by combinations of clocks, counters, and speedometers, fixed and variable sequences, and so on. Most of the contingencies examined in theories of probability, decision-making, and games are generated in a similar way—the "theory", if any, being concerned with what organisms will do under the contingencies analyzed. The experimental analysis of behavior dispenses with theories of that sort by proceeding to find out.

In addition to the systematic manipulation of contingencies, the interpretation of human affairs is a rich source of suggestions for experiments. Do conditions detected in some episode in daily life actually have the effects observed when more carefully controlled? Can a certain history of reinforcement be shown to

be responsible for a current performance? What changes in contingencies will have different and possibly more acceptable results? And so on. The guesses and hunches with which the experimenter proceeds to answer questions of this sort are not the formal hypotheses of scientific method; they are simply tentative statements for which further support is sought. The philosopher of science may still want to reconstruct the behavior so that it fits a hypothetico-deductive model, but efforts in that direction grow less impressive—particularly as an alternative formulation of the behavior of Man Thinking is glimpsed as one of the more distant reaches of an experimental analysis (Skinner, 1957).

Research which enlarges an established corpus of facts or simplifies an effective formulation is usually less dramatic than research which topples hypotheses or confirms broad theories, but it has its compensations. For those so inclined, theoretical activities are by no means ruled out, even though scientific methodologists have usually been hesitant in accepting the position often adopted in an experimental analysis. Quite aside from testing hypotheses, one may look for simplifying uniformities. For example, one may develop a theory as to why schedules of reinforcement have the effects they have, seeking certain simplifying relations among the many performances generated by different schedules. Ferster and I hazarded some informal guesses along this line, arguing for the importance of the conditions which prevail at the precise moment of reinforcement, but a better theory in this sense is no doubt possible and desirable.

In representing the relationships discovered by an experimental analysis of behavior, little use is made of metaphors or analogies drawn from other sciences. Reports seldom contain expressions like *encode, read out from storage, reverberating circuits, overloaded channels, gating, pressure, flow, drainage, networks, centers, or cell assemblies*. Little use is made of maps or schemata, such as Tolman's sow-bug, Lewin's fields and vectors, or block diagrams representing organisms as adaptive machines. The advantage in representing processes without the use of metaphor, map, or hypothetical structure is that one is not misled by a spurious sense of order or rigor. Early in his career Freud wrote to Fliess that he had put psychology on a firm neurological basis. The theory

permitted him "to see the details of neurosis all the way to the very conditioning of consciousness" (quoted in Fine, 1962). His letter emphasized number, structure, and terms borrowed from neurology, biology, and physics. He spoke of "the three systems of neurones, the 'free' and 'bound' states of quantity, the primary and secondary processes, the main trend and the compromise trend of the nervous system, the two biological rules of attention and defense." Terms of this sort encourage euphoria, and Freud was vulnerable; in his first report he was "wildly enthusiastic". Within a month or so he had abandoned the theory. He had the insight to tell Fliess that it seemed to him in retrospect "a kind of aberration".

Attitudes toward Research

The experimental analysis of behavior is also generally characterized by an unhurried attitude toward the as-yet-unanalyzed or the as-yet-unexplained. Criticism often takes the line that the analysis is over-simplified, that it ignores important facts, that a few obvious exceptions demonstrate that its formulations cannot possibly be adequate, and so on (for example, Miller, Galanter, and Pribram, 1960). An understandable reaction might be to stretch the available facts and principles in an effort to cover more ground, but the general plan of the research suggests another strategy. Unlike hypotheses, theories, and models, together with the statistical manipulations of data which support them, a smooth curve showing a change in probability of response as a function of a controlled variable is a fact in the bag, and there is no need to worry about it as one goes in search of others. The shortcomings and exceptions will be accounted for in time. The strategy is supported by the history of early criticisms of the *Behavior of Organisms*. It was said that the book was not about organisms but about the rat, and very small groups of rats at that. How could one be sure that other rats, let alone animals of other species, would behave in the same way? Only food and water were used as reinforcers, social reinforcers being conspicuously lacking. The stimuli—lights and buzzers—were crude and poorly controlled. Two levers should have been used so that the data would throw light on behavior at a choice point. And, after all, could we be sure that the rat was not pressing

the lever simply because it had nothing else to do? These criticisms have all been answered without effort in the course of time simply as part of the normal development of the analysis.

Patience with respect to unexplored parts of a field is particularly important in a science of behavior because, as part of our own subject matter, we may be overwhelmed by the facts which remain to be explained. Subtle illusions, tricks of memory, the flashes which solve problems—these are fascinating phenomena, but it may be that genuine explanations within the framework of a science of behavior, as distinguished from verbal principles or “laws” or neurological hypotheses, are out of reach at the present time. To insist that a science of behavior give a rigorous account of such phenomena in its present state of knowledge is like asking the Gilbert of 1600 to explain a magnetic amplifier or the Faraday of 1840 to explain superconductivity. Early physical scientists enjoyed a natural simplification of their subject matters. Many of the most subtle phenomena were to come into existence only through technical advances in the sciences themselves. Others, though occurring in nature, were not recognized as parts of their fields. The behavioral scientist enjoys no such

natural protection. He is faced with the full range of the phenomena he studies. He must therefore more explicitly resolve to put first things first, moving on to more difficult things only when the power of his analysis permits.

A final distinction. Those who engage in the experimental analysis of behavior are usually conspicuous for their enthusiasm. In a recent article Bixenstine (1964) attributes an unwarranted optimism in all behavioral science to the methodological position taken by experimental analysts. This is perhaps to overestimate their influence. In any case, he points to the wrong cause. He suggests that the optimism springs from release from the anxiety of theory construction. There is a more obvious explanation: the analysis works.

REFERENCES

- Bixenstine, V. Edward. *Science*, 1964, 145, 464.
 Fine, Reuben. *Freud: A Critical Re-evaluation of his Theories*. New York: McKay, 1962.
 Miller, George A., Galanter, Eugene, and Pribram, Karl H. *Plans and the Structure of Behavior*. New York: Henry Holt, 1960.
 Skinner, B. F. *Verbal Behavior*. New York: Appleton-Century-Crofts, 1957.

Received December 30, 1965