4 The experimental analysis of behavior

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 set by its own anatomy and the immediate environment.

An emphasis on rate of occurrence of repeated instances of an operant distinguishes the experimental analysis of behavior from kinds of psychology which observe one or more of the following practices.

(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), 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. 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," or "adapting to the environment." With respect to normative criteria its behavior may improve or deteriorate; with respect to developmental criteria it may be arrested or accelerated.

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 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 come 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 fact that the "tests" from which the data are obtained are very loosely controlled 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. The 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 (see page 116).

The independent variables

One 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, as we have seen, the character of a goad, something which forced an organism to respond. That 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 a level of maturation, as a variable. He sometimes uses a collateral dependent variable—but not as a measure. He may use weight, for example, in lieu of a history of deprivation, but it is simply another effect of deprivation, not a measure of hunger as a state.

(3) Contingencies of reinforcement are an important feature of the independent variables studied in an experimental analysis, 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 (see Chapter 6).

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 evalua-

tion 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. A study of schedules of reinforcement (46) can proceed in a rather Baconian fashion, as a table of the possibilities generated by combinations of clocks, counters, and speedometers, fixed and variable sequences, and so on is completed. Most of the contingencies examined in theories of probability, decision-making, and games can be 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? 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.

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 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. The conditions which prevail at the precise moment of reinforcement are important, 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" (49). 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 oversimplified, that it ignores important facts, that a few obvious exceptions demonstrate that its formulations cannot possibly be adequate, and so on. 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 a 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. Bixenstine (16) has attributed an unwarranted optimism in all behavioral science to the methodological position taken by experimental analysts. This is perhaps to overestimate their influence, but 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.

Note 4.1 Independent variables

The stimulus. To the psychophysicist psychology is "the analysis of the stimulus." Students of perception, particularly under the influence of Gestalt psychology, emphasize the ways in which stimuli force us to respond to them. Students of feelings and emotion search for the things felt: hunger is stimulation arising from stomach contractions and thirst from a dry throat. Obese people eat more than normal because they are affected differently by "cues," and people are neurotic and psychotic because they see the world in a different way.

This predilection for stimuli owes much to the secure dimensions of physical things. Stimuli have duration and extent; they occupy an unquestioned position in time and space; they exist before anyone does anything about them and they survive afterwards. In contrast, behavior is evanescent. What men do and say are things of the moment. There is nothing left when a response has been completed except the responding organism. The behavior itself has gone off into history.

In spite of the fact that stimuli are thus reassuringly substantial, the psychologist is nevertheless seldom willing to deal with them as a physicist does. He shines a light into the eye of his subject as an engineer might shine a light into a photocell, but he wants to talk about what his cell—the organism—sees. Or he may inject a reference to the organism's history—for example, by calling a stimulus "novel." ("Familiar" more clearly refers to past history and there have been those, among them Gestalt psychologists, who have argued that familiarity is "in the stimulus." Some of the kinds of organization which are said to make stimuli particularly effective, by forcing a corresponding organization in the behavior of perceiving them, are also not physical properties. Past, present, or future responses may be used to impute "meaning" to a stimulus. (And nonmeaning as well; the nonsensical character of a list of syllables is not a physical property.) Psycholinguists are particularly likely to specify stimuli in terms of earlier contingencies in which they have appeared. "Sequential probabilities," "ambiguities," and "redundancies" are not "in the stimulus." A more obvious appeal to behavior is made in describing stimuli as anxiety provoking, frustrating, confusing, and so on.

On the other hand, physical properties of stimuli are sometimes invoked for the sake of objectivity or quantification when they are irrelevant. We accept the fact that not all properties of the environment are worth specifying. Visual stimuli are not important when our subject is blind, nor is electromagnetic radiation outside the visible range when our subject has normal vision. But other dimensions cannot be dismissed for such obvious reasons. Suppose we are interested in how accurately a person can estimate the number of spots on a page. The number ranges, say, from one to one hundred. This is an objective fact, but the numbers 1 to 100 do not therefore compose a single dimension of the stimulus to which speed or accuracy of estimation can be related. (For one thing the behavior of looking at a small number of spots differs from the behavior of looking at a large number.) The pattern of a maze and its length, like the pattern and length of a list of nonsense syllables, is a physical fact, but not therefore necessarily a useful property of a "stimulus." (Overemphasis on quantifiability causes trouble with other kinds of independent variables. A "twenty-four-hour hunger" describes an objective condition, which can be reproduced by other experimenters, but twenty-four does not describe a quantity of hunger. "Number of reinforced trials" is an objective but possibly useless measure of a history of reinforcement.)

Uncontrollable independent variables. The ethologists study behavior as a function of *species status.* A graylag goose behaves in a given way because it is a graylag goose. To change the behavior we should have to change the species. No matter how important genetic variables may be, we do not manipulate them as such in predicting and controlling the behavior of a given organism.

Age is not unrelated to genetic variables since most of the behavior attributed to species status is not present at birth but must mature, possibly during critical periods of development. Age is

taken as the principal independent variable in studying the development of various sensory and motor skills and so-called traits, concepts, and mental processes. The development of speech, for example, is sometimes followed simply as an increase with age in the number of words or grammatical forms a child uses. Delinquent behavior in a given culture is said to show "a peak in theft at fourteen and in rowdyism at seventeen."

Cycles are another kind of temporal patterning. A squirrel runs and rests in its squirrel cage, the stock market rises and falls, a nation swings from a warlike to a peaceful mood and back, and romantic periods of history alternate with classical. A progressive change establishes a *trend.* Autocorrelational techniques can be used to clarify cycles and trends, but unless we know that a cycle will maintain its period or a trend continue, we cannot use the results for purposes of prediction. Nor, of course, can time be manipulated as an independent variable.

Controllable variables are also lacking when behavior is predicted from other behavior. The tests used in mental measurement evoke samples of behavior from which characteristics of similar behavior, usually on a larger scale, can be predicted—but only because the sample and the predicted behavior are functions of common variables, usually not identified. The traits or factors extracted from test scores seem to have the status of independent variables, but they cannot be manipulated as such.

Note 4.2 The dependent variable

Topography of behavior can be recorded in many ways. The graphic arts first made it possible to represent an organism in action, and films and videoscopes are modern equivalents. The alphabet was invented as a means of recording verbal behavior, and the tape recorder now permits greater accuracy. A mere record of topography, however, will not suffice for a functional analysis. We cannot break behavior into parts of convenient size on the basis of topography alone. Thus, we cannot simply describe a bit of behavior and call it an operant, even if everyone agrees to abide by our specifications. Even in reflexes which have been

surgically isolated, the response must be defined in terms of a correlated stimulus (126). An operant must behave like one; it must undergo orderly changes in probability when independent variables are manipulated. The effect on the environment is such a variable, and we can construct an operant by making reinforcement contingent upon a given topography. But false starts are common; what is taken as an operant may not behave like one and something else may. In an apparatus containing two operanda, for example, it may be necessary to consider not only the response to each, but the behavior of changing from one to the other (134).

An explicit description of topography is sometimes avoided by characterizing the dependent variable in more general terms. For example, behavior is classified as procreative, maternal, or combative. Classifications of this sort always involve independent variables. The topography of fighting, copulating, and caring for young is usually related to phylogenic and ontogenic variables which define useful classes, but this is not always true, and even so the consequences of behavior—together with their phylogenic or ontogenic significance—are involved.

Emphasis naturally falls on topography when behavior is studied as a function of the inaccessible or uncontrollable variables already mentioned. There are established fields in which the description of behavior is mainly narration. Ethology tells us how a bird of a given species builds its nest, courts its mate, and defends its territory. Developmental psychology tells us how a baby of a given age raises its head, turns over, and grasps objects. If the important independent variables are indeed only to be found in the phylogeny of the species or in age, this is perhaps all that can be done. But it is a mistake to confine an analysis to the structure of behavior when other variables are available.

An emphasis on topography of behavior at the expense of controlling relations is an example of the Formalistic Fallacy. It is common in linguistics and psycholinguistics. By rearranging fragments of recorded verbal behavior (e.g. "words") new records (e.g. "sentences") are generated, which are then treated as though they were verbal responses. By adding *not* to *It is raining,* for example, we generate *It is not raining,* and we may proceed to test its truth or falsity. But no one has yet said *It is not raining,*

except in reading the words thus arranged, and a textual response is not true or false but merely accurate or inaccurate. The generated "sentence" looks like a record of verbal behavior but the behavior it appears to record has never been emitted under the control of characteristic variables. There is a great difference between the response *It is raining* written in the presence of appropriate stimuli and the same pattern produced by rearranging words on slips of paper.

The Formalistic Fallacy is most damaging when verbal behavior is analyzed as if it were generated through the application of rules. This is most likely to happen when verbal behavior is studied as a function of uncontrollable variables, since contingencies of reinforcement are not then available as an alternative to the generation of behavior from rules (see Chapter 6). In a study of "the child's acquisition of syntax" Bellugi and Brown (14) recorded the appearance of new words and new grammatical structures in the speech of two children over a period of time. As an example of a "generative" rule, they give the following: "In order to form a noun phrase select first one word from the small class of modifiers and select, second, one word from the large class of nouns." Thus, to say *My hand* the child first selects *my* from a list of modifiers and then *hand* from a list of nouns. No reference is made to the relation of the "generated" phrase to the circumstances under which it is acquired or emitted. How often has the child echoed the verbal stimulus *my hand*? How often has he heard stories in which characters referred to their hands? How often has he heard *hand* when his own hand has been important as a stimulus—when, for example, it has been hurt, touched, washed, or shaken? What verbal history has sharpened the distinction between *my* and *your*? How many other responses containing *hand* and *my* has the child already learned? It seems safe to overlook all this material if the child selects words and puts them together to compose phrases or sentences by applying rules with the help of a mental mechanism. But selection and composition in that sense are rare forms of verbal behavior, characteristic mainly of logicians, linguists, and psycholinguists. Only the Formalistic Fallacy suggests that the products of selection and composition are equivalent to the behavior acquired under the contingencies arranged by a verbal community.

Probability of response. A further qualification of the dependent variable in a science of behavior is needed. We are not so much concerned with the topography of a response as with the probability that it will be emitted. Probability is a difficult concept. For many purposes we may be content with rate of responding, but this is awkward when a single instance of behavior is attributed to more than one variable. Similar problems arise, together with many others, when probability is inferred from the occurrence or nonoccurrence of a response in a given "trial." Behavior at a choice point does not provide independent measures of the probabilities associated with the choices. A rat may turn right rather than left in a T-maze, but we can infer only that a right turn was more probable than a left. The percentage of right or left turns in a series of trials will not complete the account because the organism presumably changes from trial to trial, and averages for groups of rats exposed to the same contingencies are still less useful.

A common practice is to evaluate probability of response in terms of the magnitude of an *independent* variable. A response evoked by a brief stimulus, for example, is felt to be stronger than one which requires a longer exposure. The probability seems to lie on a continuum between the time which guarantees a response and the time at which no appropriate response is ever made. Similar continua seem to be established by making stimuli incomplete—as by omitting letters in a text, filtering out some frequencies in recorded speech, or putting visual stimuli out of focus. The probability is inferred from the point at which the response fails to occur as the duration, clarity, or completeness of the stimulus is reduced. In psychoanalytic theory a response is inferred to have unusual strength if it occurs when it is not particularly appropriate to the occasion. Rorschach patterns and the vague auditory stimuli of the Verbal Summator (127) are presumed to evoke responses having special strength.

Probability of response is also sometimes inferred from how quickly the response is acquired or brought under stimulus control. If a response of complex topography is acquired only slowly, it is assumed that it began in very low strength. When an organism has been conditioned to respond to a given pattern, the probability that it will respond to a different pattern is sometimes argued

from the speed with which it forms a discrimination. If it learns to distinguish patterns quickly, it is assumed that learning to respond to one pattern does not make a response to the other highly probable. Speed of learning is also sometimes used to measure probability attributed to deprivation or aversive stimulation.

Speed of forgetting is also, as we have noted, used to infer probability; a response which can be recalled a long time after acquisition is presumed to have been stronger when acquired. The principle is also basic to psychoanalysis; the responses we now recall were the strong responses of long ago. Further information can be extracted by varying the conditions under which recall occurs. A recollection which has little relevance to a current situation suggests unusual strength. A response recalled in the presence of distractions or conflicting variables is also held to be strong. (The number of psychological experiments which use "amount remembered" as a dependent variable is not to be taken as showing an extraordinary interest in the process of forgetting, for many of them are concerned with processes which could be more directly investigated with measures of probability.)

Behavior as a dependent variable is often neglected when the investigator turns his attention to internal processes, real or fancied. The study of verbal learning, for example, is more likely to be concerned with proactive and retroactive inhibition, reminiscence, or obliviscence than with the actual behavior of the subject, which is often not carefully analyzed. Behavior studied as a function of time—as growth, development, trends, or cycles—also often takes second place to supposed underlying processes. And no matter how elegant the mathematical procedures used in quantifying traits and abilities, they are almost always applied to relatively crude measures (for example, responses to a questionnaire) evoked under relatively uncontrolled conditions (the questionnaire). The emphasis is not upon the behavior but upon what seems to lie behind it.

Note 4.3 Significance

The psychological literature contains a prodigious number of charts, graphs, tables, and equations reporting quantitative relations among unimportant or useless variables. Much of this may be attributed to professional contingencies of reinforcement, under which what a psychologist says must be above all irrefutable. He can satisfy the contingencies by selecting a measurable aspect of behavior and a measurable condition and examining the relation between them. If he uses the right instruments and treats his data in the right ways, his result will be statistically "significant" even when no relation is found. The significance can be increased by devising a hypothesis which the result confirms or disproves or a general principle which it illustrates. The main thing is to avoid being wrong.

There are no contingencies in which positive results figure in a comparable way. Scientific progress is usually slow, and an important step is not necessarily recognized as such as soon as it is taken. Only a few discoveries are sudden enough to be contingent upon the scientist's investigatory behavior in such a way as to shape and maintain it. The dedication of the scientist is usually the product of a favorable program of weak reinforcements. Additional sources of reinforcement are therefore important. The experimental analysis of behavior has no doubt profited from the fact that its results have led rather quickly to a behavioral technology, but the laboratory scientist profits from any result which clarifies his central conception. It is reinforcing to find variables which change in an orderly fashion and which permit one to formulate behavior as a scientific system, in the sense in which that term was used, for example, by Willard Gibbs.

A concern for basic dimensions helps the young psychologist in another way. When Freud first turned from biology to psychoanalysis, he wrote to a friend (49): "What horrifies me more than anything else is all the psychology I shall have to read in the next few years." The literature faced by the young psychologist today is several thousand times as extensive. It cannot be read as a whole. A field of specialization helps, but most fields are still large. Some principle of selection is needed, and a useful guide is the significance of the variables studied. A glimpse of the coordinates of the graphs in an article will usually suffice. A good rule of thumb is as follows: do not spend much time on articles in which changes in behavior are followed from trial to trial or in which graphs show changes in the time or number of errors

required to reach a criterion, or in amount remembered, or in percent of correct choices made, or which report scores, raw or standard. Sometimes a look at the apparatus will help. Dimensions are probably suspect if the work was done with mazes, T-mazes, jumping stands, or memory drums. The young psychologist will miss something in following these rules (he will find something of value almost everywhere), but he must run some risk. It is a matter of personal strategy, and an emphasis on basic dimensions makes it possible to plan a promising campaign.

Note 4.4 Progress

This is not the place for a survey of data, but some indication of the technical progress which has been made in the experimental analysis of behavior may be useful. Current practices contrast sharply with those reported thirty years ago in *The Behavior of Organisms* (129).

(1) The experimental space is more carefully controlled. Many versions have been standardized.

(2) Experiments last, not for an hour, but for many hours, days, weeks, or even months.

(3) The past history of the organism is more carefully controlled, possibly from birth.

(4) Many more species have been studied, including man (retardates, psychotics, normal children, and normal adults).

(5) Stimuli are more precisely controlled.

(6) Topography of response, including intensive and temporal properties, is more accurately reported and measured.

(7) An operant as a class or response is better defined and cumulative records are therefore smoother.

(8) Many more reinforcers have been studied—including, in addition to food and water, sexual stimulation, the opportunity to behave aggressively, and the production of novel stimuli.

(9) Rate of responding continues to be represented in a cumulative record, but details are clarified in analyses of interresponse times and with on-line computer processing—the latter, in particular, when contingencies are based on characteristics of rate or changes in rate.

(10) Many more schedules of intermittent reinforcement have been studied.

(11) Concurrent and sequential arrangements of contingencies permit the study of aspects of behavior which were once attributed to higher mental processes, among them many which bear upon decision-making.

(12) The experimental space often contains two or more organisms with interlocking contingencies which generate "synthetic social relations."

Note 4.5 A technology of behavior

Science and technology have always been closely interwoven. Practical problems are often solved first, and the solutions are then taken over by basic science; the craftsman's rules of thumb are the beginnings of scientific laws, as Ernst Mach pointed out long ago. On the other hand, as basic research flourishes, its methods and results come to be applied to practical affairs. Much of the technology which emerges may have no earlier rule-of-thumb counterpart. Psychology offers many examples. Techniques of mental measurement were invented to solve practical problems in education and only later came to be used in basic analyses of traits and abilities. Introspective psychology, on the other hand, emerged from philosophical inquiries into the nature of man's knowledge of the world around him, but it gave rise to instruments and methods which were later used to solve practical problems in adjusting to that world. Studies in learning (and in teaching and training) have almost always been a mixture of basic and applied research.

The technological successes of psychology have not, however, been remarkable. The psychologist often finds himself in a subordinate position; he supplies information but plays little or no part in its use. He determines the facts upon which decisions are made but takes no part in making them. Clinical psychologists often find themselves in this position with respect to psychiatrists. School psychologists report to the teacher or administrator who takes action. It is the statesman or politician who uses the results of opinion polls, and boards of directors who plan production in

the light of market analyses. When a psychologist occasionally moves into a decision-making spot, he is usually no longer regarded as a psychologist. Possibly this only shows good judgment: the psychologist knows what he knows and is unwilling to take responsibility for acting upon it. Another explanation is to be found in the history of psychology. No other science has ever had to move against such a mass of folklore, superstition, and error, and it is not surprising that psychologists have put a high price on the factual and objective. They have struggled assiduously to escape from the limitations of personal experience. Measurement and quantification—in a word, objectivity—have been at a premium. If you want to know what a man actually hears or sees, control the stimulating environment. If you want to know what he actually does or says, record his behavior as precisely as possible. If you want to know what he is inclined to do or say, sample his opinions and beliefs. If you want to know what he is really like, quantify his behavior with inventories, questionnaires, and tests. Guarantee the significance of your answers by examining many cases, and draw your conclusions only with the help of logical and statistical methods.

The social sciences have also advanced beyond earlier treatments of their subject matters mainly by emphasizing objectivity. The social scientist has been called the man with a notebook—observing, sampling, recording what he sees, rather than trusting to casual impression and memory. Even historians have entered upon a phase of this kind, searching for materials which can be treated statistically in lieu of the personal reminiscences of eyewitnesses. The result tends to be a form of structuralism (see page 12) or behavioralism (see page 13) where the emphasis falls on topography to the virtual exclusion of independent variables. It is not surprising that the use of the results should remain in other hands.

There is another result. Psychology as a basic science has failed to supply a conception which recommends itself to specialists in other fields of human behavior. Sociology, anthropology, law and jurisprudence, economics, education, political science, religion, linguistics, literary criticism, philosophy, history—each has its own theory, model, or conception of man, drawn in part from common sense and in part from outmoded philosophical systems,

with local improvisations as needed. A formula evolved in one field proves awkward in another. The student whose behavior is the concern of the educational specialist bears little resemblance to Economic Man. Man the Political Animal is not a promising patient in psychotherapy. Yet it is the same man who is being studied in all these fields, and it ought to be possible to talk about him in the same way. Psychoanalysis has come closest to supplying a common formulation, but it arose as a form of therapy and some touch of psychopathology survives when it is applied to everyday life. In spite of many claims to the contrary, it has not contributed a workable theory which is generally useful.

The experimental analysis of behavior may be on the point of doing so. The scientific method which has made it successful in the laboratory makes it almost immediately available for practical purposes. It is not concerned with testing theories but with directly modifying behavior. Its procedures are therefore relevant whenever a change in behavior is a consideration. It is less interested in the topography or structure of behavior than in the variables of which it is a function. It usually confines itself to the more convenient variables, but the interaction between organism and environment represented by the concept of contingencies of reinforcement has great generality. A particular field no doubt calls for special knowledge and will bring new discoveries, but a basic conception common to all fields is nevertheless a possibility.

Although a technology of behavior is thus in the making, we are not on the verge of solving all our problems. Human behavior is extraordinarily complex (it is no doubt the most complex subject matter ever submitted to scientific analysis), and a great deal remains to be learned. Technical knowledge is needed. We cannot deal effectively with human behavior by applying a few general principles (say, of reward and punishment) any more than we can build a bridge simply by applying the principles of stress and strain. The two fields in which an experimental analysis of behavior has already yielded the most extensive technology (education and psychotherapy) are those closest to psychology itself and hence those to which specialists in behavior are most likely to turn. Even there, however, a strong tradition favoring pure research keeps many of those who would be most successful away from technical applications. Elsewhere much of what is known

has not yet been put to use because those who are in a position to use it either do not know that it is available or are put off by misunderstandings of its nature or its implications. A new kind of professional training, preferably with laboratory experience, is needed. In the long run, the effective management of human affairs will probably require a change in the way in which everyone thinks about himself and those with whom he comes into contact.

The need for an effective technology of behavior is obvious enough. Every generation seems to believe that the world is going to the dogs, but (to be ethological for a moment) we must also not forget the boy who cried Wolf! It is quite possible that we are in serious trouble. Man may be foolish enough to set off a nuclear holocaust—not by design but by one of those accidents which are so much admired by those who oppose design. We have not yet brought the powerful methodology of science to bear on many of our problems. Prescientific formulations of human behavior are still widely used, and supported by prescientific philosophies. A sweeping change is needed, and a successful science of behavior is perhaps the necessary first step.

Note 4.6 The critics

The experimental analysis of behavior is misunderstood in many ways and for many reasons, particularly in its implications for human affairs. With respect to its use in education, Paul Goodman writes (56): "To be candid, I think operant-conditioning is vastly overrated. It teaches us the not newsy proposition that if an animal is deprived of its natural environment and society, sensorily deprived, made mildly anxious, and restricted to the narrowest possible spontaneous motion, it will emotionally identify with its oppressor and respond—with low-grade grace, energy, and intelligence—in the only way allowed to it. The poor beast must do something, just to live on a little." Jules Henry, an anthropologist, has commented on "the uncritical extrapolation of experimental results from animals to man" in the following way: "Learning theory has two simple points to make and does so with talmudic ingenuity, variability, intricacy, and insistence. They are

reinforcement and extinction. What has to be left out, because the subjects are mostly animals, is thought" (63). It would be interesting to try to apply these analyses to an issue of the *Journal of the Experimental Analysis of Behavior.*

Classroom demonstrations are often cited as if they epitomized the analysis. Pigeons have been taught to play a kind of ping-pong (146) and simple tunes on a toy piano, and these trivial achievements are offered as representing the nature and scope of operant conditioning. The analysis is often dismissed as "all a matter of conditioned reflexes" or of "habit formation in mazes." Reinforcement is sometimes said to be synonymous with reward or bribery or necessarily a matter of drive reduction. The range of the analysis is not recognized. Krutch (87) has argued that conditioned reflexes "shortcircuit" important processes in human behavior, which are presumably out of reach of a behavioral analysis. Ashby has written (6):

> Children do behave like pigeons. And this is why the technique is so dangerous. Pigeons can be taught to play the piano but they cannot be taught to understand music; and except for very limited purposes (such as the memorizing of telephone numbers) rote learning without understanding is useless. Now the chief weakness of programmed instruction is that it rewards rote learning, and worse than that—it rewards only those responses which are in agreement with the programme. The doubter, the dissenter, the questioner—in short, anyone with an original mind—can get no stimulus or satisfaction out of the programme. Furthermore, it is the declared aim of those who compose programmes to make the steps so simple that the learner does not make mistakes, and so gets his reinforcement at every step. But making mistakes is an essential experience in learning.

But the behavior involved in understanding music can be analyzed experimentally, operant conditioning is not rote learning, programs can promote original behavior, and what is learned from making mistakes can be taught in other ways (155). Problem solving, creative thinking, intellectual and ethical self-management, and behavior governed by rules are also often said to be out of reach. Some of these will be discussed in Chapters 5 and 6. A very common complaint, to which we shall return in Chapter 8, is that consciousness is "ignored."

An experimental analysis of behavior is necessarily a science in progress. The assertion that it cannot explain some aspect of behavior must be qualified with the phrase "as of this date." The analysis has grown steadily more rigorous and powerful, and it is constantly reaching into new areas, but it no doubt has a long way to go. We do not dismiss the early stages of other sciences because they were not complete. Boyle's Law, as originally stated, was quite inadequate and had to be changed as other variables were considered and as more exact measures were taken. It was not discarded, however; it was simply qualified and extended.

The use of concepts and laws derived from an experimental analysis in the interpretation of daily life is also a source of misunderstanding. An analogy from another science may be helpful. Geophysics interprets the present condition of the accessible parts of the earth in terms of presumed conditions in the mantle and core. It appeals quite freely to physical laws derived from laboratory analyses of matter under various pressures and temperatures, even though it is merely an assumption that comparable states actually prevail in the interior of the earth. In the same way familiar facts about verbal behavior are interpreted with principles derived from the laboratory study of contingencies of reinforcement (141), even though the contingencies maintained by the verbal environment cannot be precisely ascertained. In both these examples, principles derived from research conducted under the favorable conditions of the laboratory are used to give a plausible account of facts which are not at the moment under experimental control. Neither account can at the present time be proved, but both are to be preferred to treatments which lack the same kind of experimental support.

Another common misunderstanding concerns extrapolation from animal to human behavior. Those who study living organisms—say, in genetics, embryology, or medicine—usually start below the human level, and students of behavior have quite naturally followed the same practice. The experimenter needs an organism which is readily available and cheaply maintained. He must submit it to daily regimens, often for long periods of time, confine it in easily controlled environments, and expose it to complex contingencies of reinforcement. Such organisms are almost necessarily

simpler than men. Nevertheless, with very few exceptions, those who study them are primarily concerned with human behavior. Very few people are interested in the rat or pigeon for their own sakes.

The relevance of research on lower organisms to human behavior is sometimes flatly denied. Jules Henry, for example, has written, "When I extrapolate the laws of rat or pigeon learning to man, I break the law of homologous extrapolation because rats and pigeons are not homologous with man" (63). It turns out, however, that two species are homologous only if laws can be extrapolated from one to the other. Another writer has argued that although "theories . . . based on experimentation with pigeons [have] had considerable influence for good in education and clinical psychology . . . yet it seems likely that . . . pigeon results will be too simplistic for extensive use with humans" (6). This is almost certainly correct, since differences must always be taken into account, but useful similarities have been demonstrated over a fairly wide range of species. The fact is that methods first developed for the study of lower organisms, as well as the concepts and principles arising from that study, have been successfully applied to human behavior, both in a basic analysis and in many technological applications.

Although it is sometimes said that research on lower animals makes it impossible to discover what is distinctly human, it is only by studying the behavior of lower animals that we can tell what *is* distinctly human. The range of what has seemed to be human has been progressively reduced as lower organisms have come to be better understood. What survives is, of course, of the greatest importance. It must be investigated with human subjects. There is no evidence that research on lower organisms contaminates research on men or that those who study animals can have nothing important to say about men.

It is frequently implied that human dignity is threatened when principles derived from the study of lower animals are applied to man; but if we really believe that the proper study of mankind is man, we must not reject any relevant information. The use of animal vaccines in the treatment and prevention of human illness was once attacked on grounds of dignity, but medical science

without the help of animal research is unthinkable. We not only study the endocrine systems of animals and apply the results to man, we use animal hormones.

A similar concern for human worth or dignity underlies a common misunderstanding of the practices of a scientific analysis. As Bannister has put it (10):

> In order to behave like scientists we must construct situations in which our subjects are totally controlled, manipulated and measured. We must cut our subjects down to size. We construct situations in which they can behave as little like human beings as possible and we do this in order to allow ourselves to make statements about the nature of their humanity. I can think of no simple formula which can allow us to escape from this paradox but I think we might have the decency to acknowledge its presence. We ought not to use the curious notions of reductionism in order to try to convince ourselves that our chaining of our subjects is the ideal way to go about things. It may be that an imprisoned, miniscule man is all we are capable of studying but let us acknowledge that we do miserable experiments because we lack the imagination to do better ones, not claim that these are scientifically ideal because they are simple minded.

The experimental analysis of behavior is, of course, an *analysis*. The environment in which human behavior is observed is usually simplified so that one aspect (or at most a very few aspects) can be studied at one time. What we observe may not be very much like the behavior we see in the confusion of daily life, but it is still human behavior.

Simplification of the human environment is not an exclusively scientific practice. Artists, composers, writers, and scientists characteristically maximize the quality and quantity of their work by isolating themselves from unrelated features of the world about them. They build physical and social environments appropriate to a small part of their repertoires, and it is one of the objects of doing so that the behavior thus maximized should not closely resemble the behavior we meet with in daily life. We do not say that they have "cut themselves down to size" or "are behaving as little like human beings as possible" or that "they have imprisoned themselves as minuscule men," or that what they do is "all they are capable of doing." It is true that we often particularly admire

those who think best in the heat of battle or who paint or compose or write in the wild abandon of a misspent life, for they must be unusual people to work under such circumstances and their work may be closer to real life; but though their achievements differ from those of the solitary worker, they will be no more human.

As the techniques of an experimental analysis of behavior become more powerful, more and more complex behavior is analyzed under more and more complex circumstances. We ignore some things for the sake of studying others, but we do not ignore them permanently. They will be studied in their turn. Nothing is lost in the process of analysis which can not be reconstituted. Every science has been subjected to similar criticisms at some time in its history; its methods have seemed to destroy the holistic aspects of its subject matter. But more and more of that subject matter is eventually accounted for.

The fact that it is hard to see what is happening in an experimental space (see page 9) should be carefully considered by those who object to the extrapolation of laboratory results to human affairs. Presumably they object because the extrapolations do not jibe with their observations of the world at large, but if we now ask them to look at the world in small, we shall find that their observations do not jibe with what *we know to be the case*. We know it because we have constructed the contingencies and can analyze their effects under especially advantageous conditions. It is quite possible that so many people have said so many different things about the world at large just because none of them has ever been able to confirm what he thinks he has seen. We extrapolate from relatively simple conditions to relatively complex, not to confirm what someone claims to have seen in the complex case, but to begin for the first time to see it in a new light.

Terminology is another common source of misunderstanding. When speaking or writing casually, the student of behavior is perhaps as likely as anyone else to mention sensations, feelings, ideas, thoughts, decisions, and so on. Critics sometimes cite instances of this to prove inconsistency, lack of logic, or bad faith. The astronomer is similarly inconsistent when he says that the sun rises or that the stars come out at night, but he would be a foolish astronomer indeed if he avoided such expressions in casual

discourse. No one should be seriously misled by such expressions as "The idea occurred to me . . . ," "My memory of him is rather vague . . . ," or "I don't feel like going. . . ." When early astronomers were challenged, as they must have been when they continued to speak of the sunrise, their answer was presumably a quick translation into nongeocentric terms. The student of behavior must also be ready to translate if challenged, and in any serious enterprise he should be alert to the danger in unanalyzed, casual terms.

Another criticism of an experimental analysis of behavior is that it "apes" other sciences. This is not true. It adopts the basic scientific assumption of order and lawfulness in its subject matter, and it freely borrows any method which may be relevant to its subject matter, but it does not do this in order to resemble more prestigious sciences. Compared with information-theory or cybernetics, mathematical models, hypothetico-deductive systems, computer simulation, and general systems theory, it is unusually free of scientific role playing. It is in no hurry to be mathematical. Newton's brilliant success in putting order into a chaotic universe led men almost immediately to wonder whether the same thing might not be done for human behavior and society. Within a century Jean-Jacques Rousseau could exclaim, "Calculators, it is now up to you. Count, measure, compare." (It is tempting to suppose that he was clairvoyant, and that "*calculateurs*" meant "computers.") Another century and Gustav Fechner jumped out of bed with the exciting thought that the physical world and the world of the psyche might be *mathematically* related. Another century and mathematical psychology sustains the hope of avoiding the sheer labor of an empirical analysis—an analysis which is needed if we are to identify the entities and the relations among them which are to be treated mathematically.