Behaviorism*

B. F. Skinner

The terms "cause" and "effect" are no longer widely used in science. They have been associated with so many theories of the structure and operation of the universe that they mean more than scientists want to say. The terms which replace them, however, refer to the same factual core. A "cause" becomes a "change in an independent variable" and an "effect" a "change in a dependent variable." The old "cause-and-effect connection" becomes a "functional relation." The new terms do not suggest *how* a cause causes its effect: they merely assert that different events tend to occur together in a certain order. This is important, but it is not crucial. There is no particular danger in using "cause" and "effect" in an informal discussion if we are always ready to substitute their more exact counterparts.

We are concerned, then, with the causes of human behavior. We want to know why men behave as they do. Any condition or event which can be shown to have an effect upon behavior must be taken into account. By discovering and analyzing these causes we can predict behavior; to the extent that we can manipulate them, we can control behavior.

There is a curious inconsistency in the zeal with which the doctrine of personal freedom has been defended, because men have always been fascinated by the search for causes. The spontaneity of human behavior is apparently no more challenging than its "why and wherefore." So strong is the urge to explain behavior that men have been led to anticipate legitimate scientific inquiry and to construct highly implausible theories of causation. This practice is not unusual in the history of science. The study of any subject begins in the realm of superstition. The fanciful explanation precedes the valid. Astronomy began as astrology; chemistry as alchemy. The field of behavior has had, and still has, its astrologers and alchemists. A long history of prescientific explanation furnishes us with a fantastic array of causes which have no function other than to supply spurious answers to questions which must otherwise go unanswered in the early stages of a science. ....

Inner "Causes"

Every science has at some time or other looked for causes of action inside the things it has studied. Sometimes the practice has proved useful, sometimes it has not. There is nothing wrong with an inner explanation as such, but events which are located inside a system are likely to be difficult to observe. For this reason we are encouraged to assign properties to them without justification. Worse still, we can invent causes of this sort without fear of contradiction. The motion of a rolling stone was once attributed to its *vis viva*. The chemical properties of

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bodies were thought to be derived from the *principles* or *essences* of which they were composed. Combustion was explained by the *phlogiston* inside the combustible object. Wounds healed and bodies grew well because of a *vis medicatrix.* It has been especially tempting to attribute the behavior of a living organism to the behavior of an inner agent, as the following examples may suggest.

Neural Causes. The layman uses the nervous system as a ready explanation of behavior. The English language contains hundreds of expressions which imply such a causal relationship. At the end of a long trial we read that the jury shows signs of *brain fag,* that the *nerves* of the accused are *on edge,* that the wife of the accused is on the verge of a *nervous breakdown,* and that his lawyer is generally thought to have lacked the *brains* needed to stand up to the prosecution. Obviously, no direct observations have been made of the nervous systems of any of these people. Their "brains" and "nerves" have been invented on the spur of the moment to lend substance to what might otherwise seem a superficial account of their behavior. ....

Eventually a science of the nervous system based upon direct observation rather than inference will describe the neural states and events which immediately precede instances of behavior. We shall know the precise neurological conditions which immediately precede, say, the response, "No, thank you. These events in turn will be found to be preceded by other neurological events, and these in turn by others. This series will lead us back to events outside the nervous system and, eventually, outside the organism. In the chapters which follow we shall consider external events of this sort in some detail. We shall then be better able to evaluate the place of neurological explanations of behavior. However, we may note here that we do not have and may never have this sort of neurological information at the moment it is needed in order to predict a specific instance of behavior. It is even more unlikely that we shall be able to alter the nervous system directly in order to set up the antecedent conditions of a particular instance. The causes to be sought in the nervous system are, therefore, of limited usefulness in the prediction and control of specific behavior.

Psychic inner causes. An even more common practice is to explain behavior in terms of an inner agent which lacks physical dimensions and is called "mental" or "psychic." The purest form of the psychic explanation is seen in the animism of primitive peoples. From the immobility of the body after death it is inferred that a spirit responsible for movement has departed. The *enthusiastic* person is, as the etymology of the word implies, energized by a "god within." It is only a modest refinement to attribute every feature of the behavior of the physical organism to a corresponding feature of the "mind" or of some inner "personality." The inner man is regarded as driving the body very much as the man at the steering wheel drives a car. The inner man wills an action, the outer executes it. The inner loses his appetite, the outer stops eating. The inner man wants and the outer gets. The inner has the impulse which the outer obeys.

It is not the layman alone who resorts to these practices, for many reputable psychologists use a similar dualistic system of explanation. The inner man is sometimes personified clearly, as when delinquent behavior is attributed to a "disordered personality," or he may be dealt with in fragments, as when behavior is attributed to mental processes, faculties, and traits. Since the inner man does not occupy space, he may be multiplied at will. It has been argued that a single physical organism is controlled by several psychic agents and that its behavior is the resultant of their several wills. The Freudian concepts of the ego, superego, and id are often used in this way. They are frequently regarded as nonsubstantial creatures, often in violent conflict, whose defeats or victories lead to the adjusted or maladjusted behavior of the physical organism in which they reside.

Direct observation of the mind comparable with the observation of the nervous system has not proved feasible. It is true that many people believe that they observe their "mental states" just as the physiologist observes neural events, but another interpretation of what they observe is possible.... Introspective psychology no longer pretends to supply direct information about events which are the causal antecedents, rather than the mere accompaniments, of behavior. It defines its "subjective" events in ways which strip them of any usefulness in a causal analysis. The events appealed to in early mentalistic explanations of behavior have remained beyond the reach of observation. Freud insisted upon this by emphasizing the role of the unconscious—a frank recognition that important mental processes are not directly observable. The Freudian literature supplies many examples of behavior from which unconscious wishes, impulses, instincts, and emotions are inferred. Unconscious thought-processes have also been used to explain intellectual achievements. Though the mathematician may feel that he knows "how he thinks," he is often unable to give a coherent account of the mental processes leading to the solution of a specific problem. But any mental event which is unconscious is necessarily inferential, and the explanation is therefore not based upon independent observations of a valid cause.

The fictional nature of this form of inner cause is shown by the ease with which the mental process is discovered to have just the properties needed to account for the behavior. When a professor turns up in the wrong classroom or gives the wrong lecture, it is because his *mind* is, at least for the moment, *absent.* If he forgets to give a reading assignment, it is because it has slipped his *mind* (a hint from the class may re*mind* him of it). He begins to tell an old joke but pauses for a moment, and it is evident to everyone that he is trying to make up his *mind* whether or not he has already used the joke that term. His lectures grow more tedious with the years, and questions from the class confuse him more and more, because his *mind* is failing. What he says is often disorganized because his *ideas* are confused. He is occasionally unnecessarily emphatic because of the force of his *ideas.* When he repeats himself, it is because he has an *idée fixe*; and when he repeats what others have said, it is because he borrows his *ideas.* Upon occasion there is nothing in what he says because he lacks *ideas.* In all this it is obvious that the mind and the ideas, together with their special characteristics, are being invented on the spot to provide spurious explanations. A science of behavior can hope to gain very little from so cavalier a practice. Since mental or psychic events are asserted to lack the dimensions of physical science, we have an additional reason for rejecting them.

Conceptual inner causes. The commonest inner causes have no specific dimensions at all, either neurological or psychic. When we say that a man eats *because* he is hungry, smokes a great deal *because* he has the tobacco habit, fights *because* of the instinct of pugnacity, behaves brilliantly *because* of his intelligence, or plays the piano well *because* of his musical ability, we seem to be referring to causes. But on analysis these phrases prove to be merely redundant descriptions. A single set of facts is described by the two statements: “He eats” and “He is hungry.” A single set of facts is described by the two statements: “He smokes a great deal” and “He has the smoking habit.” A single set of facts is described by the two statements: “He plays well” and “He has musical ability.” The practice of explaining one statement in terms of the other is dangerous because it suggests that we have found the cause and therefore need search no further. Moreover, such terms as “hunger,” “habit,” and “intelligence” convert what are essentially the properties of a process or relation into what appear to be things. Thus we are unprepared for the properties eventually to be discovered in the behavior itself and continue to look for something which may not exist.

The Variables of which Behavior is a Function

The practice of looking inside the organism for an explanation of behavior has tended to obscure the variables which are immediately available for a scientific analysis. These variables lie outside the organism, in its immediate environment and in its environmental history. They have a physical status to which the usual techniques of science are adapted, and they make it possible to explain behavior as other subjects are explained in science. These independent variables are of many sorts and their relations to behavior are often subtle and complex, but we cannot hope to give an adequate account of behavior without analyzing them.

Consider the act of drinking a glass of water. This is not likely to be an important bit of behavior in anyone’s life, but it supplies a convenient example. We may describe the topography of the behavior in such a way that a given instance may be identified quite accurately by any qualified observer. Suppose now we bring someone into a room and place a glass of water before him. Will he drink? There appear to be only two possibilities: either he will or he will not. But we speak of the *chances* that he will drink, and this notion may be refined for scientific use. What we want to evaluate is the *probability* that he will drink. This may range from virtual certainty that drinking will occur to virtual certainty that it will not. The very considerable problem of how to measure such a probability will be discussed later. For the moment, we are interested in how the probability may be increased or decreased.

Everyday experience suggests several possibilities, and laboratory and clinical observations have added others. It is decidedly not true that a horse may be led to water but cannot be made to drink. By arranging a history of severe deprivation we could be “absolutely sure” that drinking would occur. In the same way we may be sure that the glass of water in our experiment will be drunk. Although we are not likely to arrange them experimentally, deprivations of the necessary magnitude sometimes occur outside the laboratory. We may obtain an effect similar to that of deprivation by speeding up the excretion of water. For example, we may induce sweating by raising the temperature of the room or by forcing heavy exercise, or we may increase the excretion of urine by mixing salt or urea in food taken prior to the experiment. It is also well known that loss of blood, as on a battlefield, sharply increases the probability of drinking. On the other hand, we may set the probability at virtually zero by inducing or forcing our subject to drink a large quantity of water before the experiment.

If we are to predict whether or not our subject will drink, we must know as much as possible about these variables. If we are to induce him to drink, we must be able to manipulate them. In both cases, moreover, either for accurate prediction or control, we must investigate the effect of each variable quantitatively with the methods and techniques of a laboratory science.

Other variables may, of course, affect the result. Our subject may be "afraid" that something has been added to the water as a practical joke or for experimental purposes. He may even "suspect" that the water has been poisoned. He may have grown up in a culture in which water is drunk only when no one is watching. He may refuse to drink simply to prove that we cannot predict or control his behavior. These possibilities do not disprove the relations between drinking and the variables listed in the preceding paragraphs; they simply remind us that other variables may have to be taken into account. We must know the history of our subject with respect to the behavior of drinking water, and if we cannot eliminate social factors from the situation, then we must know the history of his personal relations to people resembling the experimenter. Adequate prediction in any science requires information about all relevant variables, and the control of a subject matter for practical purposes makes the same demands.

Other types of "explanation" do not permit us to dispense with these requirements or to fulfill them in any easier way. It is of no help to be told that our subject will drink provided he was born under a particular sign of the zodiac which shows a preoccupation with water or provided he is the lean and thirsty type or was, in short, "born thirsty." Explanations in terms of inner states or agents, however, may require some further comment. To what extent is it helpful to be told, "He drinks because he is thirsty"? If to be thirsty means nothing more than to have a tendency to drink, this is mere redundancy. If it means that he drinks because of a state of thirst, an inner causal event is invoked. If this state is purely inferential—if no dimensions are assigned to it which would make direct observation possible—it cannot serve as an explanation. But if it has physiological or psychic properties, what role can it play in a science of behavior?

The physiologist may point out that several ways of raising the probability of drinking have a common effect: they increase the concentration of solutions in the body. Through some mechanism not yet well understood, this may bring about a corresponding change in the nervous system which in turn makes drinking more probable. In the same way, it may be argued that all these operations make the organism "feel thirsty" or "want a drink" and that such a psychic state also acts upon the nervous system in some unexplained way to induce drinking. In each case we have a causal chain consisting of three links: (1) an operation performed upon the organism from without—for example, water deprivation; (2) an inner condition—for example, physiological or psychic thirst; and (3) a kind of behavior—for example, drinking. Independent information about the second link would obviously permit us to predict the third without recourse to the first. It would be a preferred type of variable because it would be non-historic; the first link may lie in the past history of the organism, but the second is a current condition. Direct information about the second link is, however, seldom, if ever, available. Sometimes we infer the second link from the third: an animal is judged to be thirsty if it drinks. In that case, the explanation is spurious. Sometimes we infer the second link from the first: an animal is said to be thirsty if it has not drunk for a long time. Iii that case, we obviously cannot dispense with the prior history.

The second link is useless in the *control* of behavior unless we can manipulate it. At the moment, we have no way of directly altering neural processes at appropriate moments in the life of a behaving organism, nor has any way been discovered to alter a psychic process. We usually set up the second link through the first: we make an animal thirsty, in either the physiological or the psychic sense, by depriving it of water, feeding it salt, and so on. In that case, the second link obviously does not permit us to dispense with the first. Even if some new technical discovery were to enable us to set up or change the second link directly, we should still have to deal with those enormous areas in which human behavior is controlled through manipulation of the first link. A technique of operating upon the second link would increase our control of behavior, but the techniques which have already been developed would still remain to be analyzed.

The most objectionable practice is to follow the causal sequence back only as far as a hypothetical second link. This is a serious handicap both in a theoretical science and in the practical control of behavior. It is no help to be told that to get an organism to drink we are simply to "make it thirsty" unless we are also told how this is to be done. When we have obtained the necessary prescription for thirst, the whole proposal is more complex than it need be. Similarly, when an example of maladjusted behavior is explained by saying that the individual is "suffering from anxiety," we have still to be told the cause of the anxiety. But the external conditions which are then invoked could have been directly related to the maladjusted behavior. Again, when we are told that a man stole a loaf of bread because "he was hungry," we have still to learn of the external conditions responsible for the "hunger." These conditions would have sufficed to explain the theft.

The objection to inner states is not that they do not exist, but that they are not relevant in a functional analysis. We cannot account for the behavior of any system while staying wholly inside it; eventually we must turn to forces operating upon the organism from without. Unless there is a weak spot in our causal chain so that the second link is not lawfully determined by the first, or the third by the second, then the first and third links must be lawfully related. If we must always go back beyond the second link for prediction and control, we may avoid many tiresome and exhausting digressions by examining the third link as a function of the first. Valid information about the second link may throw light upon this relationship but can in no way alter it.

A Functional Analysis

The external variables of which behavior is a function provide for what may be called a causal or functional analysis. We undertake to predict and control the behavior of the individual organism. This is our "dependent variable"—the effect for which we are to find the cause. Our "independent variables"—the causes of behavior—are the external conditions of which behavior is a function. Relations between the two—the "cause-and-effect relationships" in behavior—are the laws of a science. A synthesis of these laws expressed in quantitative terms yields a comprehensive picture of the organism as a behaving system.

This must be done within the bounds of a natural science. We cannot assume that behavior has any peculiar properties which require unique methods or special kinds of knowledge. It is often argued that an act is not so important as the "intent" which lies behind it, or that it can be described only in terms of what it "means" to the behaving individual or to others whom it may affect. If statements of this sort are useful for scientific purposes, they must be based upon observable events, and we may confine ourselves to such events exclusively in a functional analysis. We shall see later that although such terms as "meaning" and "intent" appear to refer to properties of behavior, they usually conceal references to independent variables. This is also true of "aggressive," "friendly," "disorganized," "intelligent," and other terms which appear to describe properties of behavior but in reality refer to its controlling relations.

The independent variables must also be described in physical terms. An effort is often made to avoid the labor of analyzing a physical situation by guessing what it "means" to an organism or by distinguishing between the physical world and a psychological world of "experience." This practice also reflects a confusion between dependent and independent variables. The events affecting an organism must be capable of description in the language of physical science. It is sometimes argued that certain "social forces" or the "influences" of culture or tradition are exceptions. But we cannot appeal to entities of this sort without explaining how they can affect both the scientist and the individual under observation. The physical events which must then be appealed to in such an explanation will supply us with alternative material suitable for a physical analysis. ....

Reflex Action

Descartes had taken an important step in suggesting that some of the spontaneity of living creatures was only apparent and that behavior could sometimes be traced to action from without. The first clear-cut evidence that he had correctly surmised the possibility of external control came two centuries later in the discovery that the tail of a salamander would move when part of it was touched or pierced, even though the tail had been severed from the body. Facts of this sort are now familiar, and we have long since adapted our beliefs to take them into account. At the time the discovery was made, however, it created great excitement. It was felt to be a serious threat to prevailing theories of the inner agents responsible for behavior. If the movement of the amputated tail could be controlled by external forces, was its behavior when attached to the salamander of a different nature? If not, what about the inner causes which had hitherto been used to account for it? It was seriously suggested as an answer that the "will" must be coexistent with the body and that some part of it must invest any amputated part. But the fact remained that an external event had been identified which could be substituted, as in Descartes's daring hypothesis, for the inner explanation.

The external agent came to be called a *stimulus.* The behavior controlled by it came to be called a *response.* Together they comprised what was called a *reflex*—on the theory that the disturbance caused by the stimulus passed to the central nervous system and was "reflected" back to the muscles. It was soon found that similar external causes could be demonstrated in the behavior of larger portions of the organism—for example, in the body of a frog, cat, or dog in which the spinal cord had been severed at the neck. Reflexes including parts of the brain were soon added, and it is now common knowledge that in the intact organism many kinds of stimulation lead to almost inevitable reactions of the same reflex nature. Many characteristics of the relation have been studied quantitatively. The time which elapses between stimulus and response (the "latency") has been measured precisely. The magnitude of the response has been studied as a function of the intensity of the stimulus. Other conditions of the organism have been found to be important in completing the account—for example, a reflex may be "fatigued" by repeated rapid elicitation.

The reflex was at first closely identified with hypothetical neural events in the so-called "reflex arc." A surgical division of the organism was a necessary entering wedge, for it provided a simple and dramatic method of analyzing behavior. But surgical analysis became unnecessary as soon as the principle of the stimulus was understood and as soon as techniques were discovered for handling complex arrangements of variables in other ways. By eliminating some conditions, holding others constant, and varying others in an orderly manner, basic lawful relations could be established without dissection and could be expressed without neurological theories.

The extension of the principle of the reflex to include behavior involving more and more of the organism was made only in the face of vigorous opposition. The reflex nature of the spinal animal was challenged by proponents of a "spinal will." The evidence they offered in support of a residual inner cause consisted of behavior which apparently could not be explained wholly in terms of stimuli. When higher parts of the nervous system were added, and when the principle was eventually extended to the intact organism, the same pattern of resistance was followed. But arguments for spontaneity, and for the explanatory entities which spontaneity seems to demand, are of such form that they must retreat before the accumulating facts. Spontaneity is negative evidence; it points to the weakness of a current scientific explanation, but does not in itself prove an alternative version. By its very nature, spontaneity must yield ground as a scientific analysis is able to advance. As more and more of the behavior of the organism has come to be explained in terms of stimuli, the territory held by inner explanations has been reduced. The “will” has retreated up the spinal cord, through the lower and then the higher parts of the brain, and finally, with the conditioned reflex, has escaped through the front of the head. At each stage, some part of the control of the organism has passed from a hypothetical inner entity to the external environment.

The Range of Reflex Action

A certain part of behavior, then, is elicited by stimuli, and our prediction of that behavior is especially precise. When we flash a light in the eye of a normal subject, the pupil contracts. When he sips lemon juice, saliva is secreted. When we raise the temperature of the room to a certain point, the small blood vessels in his skin enlarge, blood is brought nearer to the skin, and he “turns red.” We use these relations for many practical purposes. When it is necessary to induce vomiting, we employ a suitable stimulus—an irritating fluid or a finger in the throat. The actress who must cry real tears resorts to onion juice on her handkerchief.

As these examples suggest, many reflex responses are executed by the “smooth muscles” (for example, the muscles in the walls of the blood vessels) and the glands. These structures are particularly concerned with the internal economy of the organism. They are most likely to be of interest in a science of behavior in the emotional reflexes.... Other reflexes use the “striped muscles” which move the skeletal frame of the organism. The “knee jerk” and other reflexes which the physician uses for diagnostic purposes are examples. We maintain our posture, either when standing still or moving about, with the aid of a complex network of such reflexes.

In spite of the importance suggested by these examples, it is still true that if we were to assemble all the behavior which falls into the pattern of the simple reflex, we should have only a very small fraction of the total behavior of the organism. This is not what early investigators in the field expected. We now see that the principle of the reflex was overworked. The exhilarating discovery of the stimulus led to exaggerated claims. It is neither plausible nor expedient to conceive of the organism as a complicated jack-in-the-box with a long list of tricks, each of which may be evoked by pressing the proper button. The greater part of the behavior of the intact organism is not under this primitive sort of stimulus control. The environment affects the organism in many ways which are not conveniently classed as “stimuli,” and even in the field of stimulation only a small part of the forces acting upon the organism elicit responses in the invariable manner of reflex action. To ignore the principle of the reflex entirely, however, would be equally unwarranted.

Conditioned Reflexes

.... The difference between an unskilled conjecture and a scientific fact is not simply a difference in evidence. It had long been known that a child might cry before it was hurt or that a fox might salivate upon seeing a bunch of grapes.

What Pavlov added can be understood most clearly by considering his history. Originally he was interested in the process of digestion, and he studied the conditions under which digestive juices were secreted. Various chemical substances in the mouth or in the stomach resulted in the reflex action of the digestive glands. Pavlov's work was sufficiently outstanding to receive the Nobel Prize, but it was by no means complete. He was handicapped by a certain unexplained secretion. Although food in the mouth might elicit a flow of saliva, saliva often flowed abundantly when the mouth was empty. We should not be surprised to learn that this was called "psychic secretion." It was explained in terms which "any child could understand." Perhaps the dog was "thinking about food." Perhaps the sight of the experimenter preparing for the next experiment "reminded" the dog of the food it had received in earlier experiments. But these explanations did nothing to bring the unpredictable salivation within the compass of a rigorous account of digestion.

Pavlov's first step was to control conditions so that "psychic secretion" largely disappeared. He designed a room in which contact between dog and experimenter was reduced to a minimum. The room was made as free as possible from incidental stimuli. The dog could not hear the sound of footsteps in neighboring rooms or smell accidental odors in the ventilating system. Pavlov then built up a "psychic secretion" step by step. In place of the complicated stimulus of an experimenter preparing a syringe or filling a dish with food, he introduced controllable stimuli which could be easily described in physical terms. In place of the accidental occasions upon which stimulation might precede or accompany food, Pavlov arranged precise schedules in which controllable stimuli and food were presented in certain orders. Without influencing the dog in any other way, he could sound a tone and insert food into the dog's mm h. In this way he was able to show that the tone *acquired* its ability to elicit secretion, and he was also able to follow the process through which this came about. Once in possession of these facts, he could then give a satisfactory account of all secretion. He had replaced the "psyche" of psychic secretion with certain objective facts in the recent history of the organism.

The process of conditioning, as Pavlov reported it in his book *Conditioned Reflexes*, is a process of *stimulus substitution*. A previously neutral stimulus acquires the power to elicit a response which was originally elicited by another stimulus. The change occurs when the neutral stimulus is followed or "reinforced" by the effective stimulus. Pavlov studied the effect of the interval of time elapsing between stimulus and reinforcement. He investigated the extent to which various properties of stimuli could acquire control. He also studied the converse process, in which the conditioned stimulus loses its power to evoke the response when it is no longer reinforced—a process which he called "extinction."

The quantitative properties which he discovered are by no means "known to every child." And they are important. The most efficient use of conditioned reflexes in the practical control of behavior often requires quantitative information. A satisfactory theory makes the same demands. In dispossessing explanatory fictions, for example, we cannot be sure that an event of the sort implied by "psychic secretion" is not occasionally responsible until we can predict the exact amount of secretion at any given time. Only a quantitative description will make sure that there is no additional mental process in which the dog "associates the sound of the tone with the idea of food" or in which it salivates because it "expects" food to appear. Pavlov could dispense with concepts of this sort only when he could give a complete quantitative account of salivation in terms of the stimulus, the response, and the history of conditioning.

Pavlov, as a physiologist, was interested in how the stimulus was converted into neural processes and in how other processes carried the effect through the nervous system to the muscles and glands. The subtitle of his book is *An Investigation of the Physiological Activity of the Cerebral Cortex.* The "physiological activity" was inferential. We may suppose, however, that comparable processes will eventually be described in terms appropriate to neural events. Such a description will fill in the temporal and spatial gaps between an earlier history of conditioning and its current result. The additional account will be important in the integration of scientific knowledge but will not make the relation between stimulus and response any more lawful or any more useful in prediction and control. Pavlov's achievement was the discovery, not of neural processes, but of important quantitative relations which permit us, regardless of neurological hypotheses, to give a direct account of behavior in the field of the conditioned reflex. ....

The Range of Conditioned Reflexes

Although the process of conditioning greatly extends the scope of the eliciting stimulus, it does not bring all the behavior of the organism within such stimulus control. According to the formula of stimulus substitution we must elicit a response before we can condition it. All conditioned reflexes are, therefore, based upon unconditioned reflexes. But we have seen that reflex responses are only a small part of the total behavior of the organism. Conditioning adds new controlling stimuli, but not new responses. In using the principle, therefore, we are not subscribing to a "conditioned-reflex theory" of all behavior. ....

Learning Curves

One of the first serious attempts to study the changes brought about by the consequences of behavior was made by E. L. Thorndike in 1898. His experiments arose from a controversy which was then of considerable interest. Darwin, in insisting upon the continuity of species, had questioned the belief that man was unique among the animals in his ability to think. Anecdotes in which lower animals seemed to show the "power of reasoning" were published in great numbers. But when terms which had formerly been applied only to human behavior were thus extended, certain questions arose concerning their meaning. Did the observed facts point to mental processes, or could these apparent evidences of thinking be explained in other ways? Eventually it became clear that the assumption of inner thought-processes was not required. Many years were to pass before the same question was seriously raised concerning human behavior, but Thorndike's experiments and his alternative explanation of reasoning in animals were important steps in that direction.

If a cat is placed in a box from which it can escape only by unlatching a door, it will exhibit many different kinds of behavior, some of which may be effective in opening the door. Thorndike found that when a cat was put into such a box again and again, the behavior which led to escape tended to occur sooner and sooner until eventually escape was as simple and quick as possible. The cat had solved its problem as well as if it were a "reasoning" human being, though perhaps not so speedily. Yet Thorndike observed no "thought-process" and argued that none was needed by way of explanation. He could describe his results simply by saying that a part of the cat's behavior was "stamped in" because it was followed by the opening of the door.

The fact that behavior is stamped in when followed by certain consequences, Thorndike called "The Law of Effect." What he had observed was that certain behavior occurred more and more readily in comparison with other behavior characteristic of the same situation. By noting the successive delays in getting out of the box and plotting them on a graph, he constructed a "learning curve." This early attempt to show a quantitative process in behavior, similar to the processes of physics and biology, was heralded as an important advance. It revealed a process which took place over a considerable period of time and which was not obvious to casual inspection. Thorndike, in short, had made a discovery. Many similar curves have since been recorded and have become the substance of chapters on learning in psychology texts.

Learning curves do not, however, describe the basic process of stamping in. Thorndike's measure—the time taken to escape—involved the elimination of other behavior, and his curve depended upon the number of different things a cat might do in a particular box. It also depended upon the behavior which the experimenter or the apparatus happened to select as "successful" and upon whether this was common or rare in comparison with other behavior evoked in the box. A learning curve obtained in this way might be said to reflect the properties of the latch box rather than of the behavior of the cat. The same is true of many other devices developed for the study of learning. The various mazes through which white rats and other animals learn to run, the "choice boxes" in which animals learn to discriminate between properties or patterns of stimuli, the apparatuses which present sequences of material to be learned in the study of human memory—each of these yields its own type of learning curve.

By averaging many individual cases, we may make these curves as smooth as we like. Moreover, curves obtained under many different circumstances may agree in showing certain general properties. For example, when measured in this way, learning is generally "negatively accelerated"—improvement in performance occurs more and more slowly as the condition is approached in which further improvement is impossible. But it does not follow that negative acceleration is characteristic of the basic process. Suppose, by analogy, we fill a glass jar with gravel which has been so well mixed that pieces of any given size are evenly distributed. We then agitate the jar gently and watch the pieces rearrange themselves. The larger move toward the top, the smaller toward the bottom. This process, too, is negatively accelerated. At first the mixture separates rapidly, but as separation proceeds, the condition in which there will be no further change is approached more and more slowly. Such a curve may be quite smooth and reproducible, but this fact alone is not of any great significance. The curve is the result of certain fundamental processes involving the contact of spheres of different sizes, the resolution of the forces resulting from agitation, and so on, but it is by no means the most direct record of these processes.

Learning curves show how the various kinds of behavior evoked in complex situations are sorted out, emphasized, and reordered. The basic process of the stamping in of a single act brings this change about, but it is not reported directly by the change itself.

Operant Conditioning

To get at the core of Thorndike's Law of Effect, we need to clarify the notion of "probability of response." This is an extremely important concept; unfortunately, it is also a difficult one. In discussing human behavior, we often refer to "tendencies" or "predispositions" to behave in particular ways. Almost every theory of behavior uses some such term as "excitatory potential," "habit strength," or "determining tendency." But how do we observe a tendency? And how can we measure one?

If a given sample of behavior existed in only two states, in one of which it always occurred and in the other never, we should be almost helpless in following a program of functional analysis. An all-or-none subject matter lends itself only to primitive forms of description. It is a great advantage to suppose instead that the *probability* that a response will occur ranges continuously between these all-or-none extremes. We can then deal with variables which, unlike the eliciting stimulus, do not "cause a given bit of behavior to occur" but simply make the occurrence more probable. We may then proceed to deal, for example, with the combined effect of more than one such variable.

The everyday expressions which carry the notion of probability, tendency, or predisposition describe the frequencies with which bits of behavior occur. We never observe a probability as such. We say that someone is "enthusiastic" about bridge when we observe that he plays bridge often and talks about it often. To be "greatly interested" in music is to play, listen to, and talk about music a good deal. The "inveterate" gambler is one who gambles frequently. The camera "fan" is to be found taking pictures, developing them, and looking at pictures made by himself and others. The "highly sexed" person frequently engages in sexual behavior. The "dipsomaniac" drinks frequently.

In characterizing a man's behavior in terms of frequency, we assume certain standard conditions: he must be able to execute and repeat a given act, and other behavior must not interfere appreciably. We cannot be sure of the extent of a man's interest in music, for example, if he is necessarily busy with other things. When we come to refine the notion of probability of response for scientific use, we find that here, too, our data are frequencies and that the conditions under which they are observed must be specified. The main technical problem in designing a controlled experiment is to provide for the observation and interpretation of frequencies. We eliminate, or at least hold constant, any condition which encourages behavior which competes with the behavior we are to study. An organism is placed in a quiet box where its behavior may be observed through a one-way screen or recorded mechanically. This is by no means an environmental vacuum, but the organism will react to the features of the box in many ways; but its behavior will eventually reach a fairly stable level, against which the frequency of a selected response may be investigated.

To study the process which Thorndike called stamping in, we must have a "consequence." Giving food to a hungry organism will do. We can feed our subject conveniently with a small food tray which is operated electrically. When the tray is first opened, the organism will probably react to it in ways which interfere with the process we plan to observe. Eventually, after being fed from the tray repeatedly, it eats readily, and we are then ready to make this consequence contingent upon behavior and to observe the result.

We select a relatively simple bit of behavior which may be freely and rapidly repeated, and which is easily observed and recorded. If our experimental subject is a pigeon, for example, the behavior of raising the head above a given height is convenient. This may be observed by sighting across the pigeon's head at a scale pinned on the far wall of the box. We first study the height at which the head is normally held and select some line on the scale which is reached only infrequently. Keeping our eye on the scale we then begin to open the food tray very quickly whenever the head rises above the line. If the experiment is conducted according to specifications, the result is invariable: we observe an immediate change in the frequency with which the head crosses the line. We also observe, and this is of some importance theoretically, that higher lines are now being crossed. We may advance almost immediately to a higher line in determining when food is to be presented. In a minute or two, the bird's posture has changed so that the top of the head seldom falls below the line which we first chose.

When we demonstrate the process of stamping in this relatively simple way, we see that certain common interpretations of Thorndike's experiment are superfluous. The expression "trial-and-error learning," which is frequently associated with the Law of Effect, is clearly out of place here. We are reading something into our observations when we call any upward movement of the head a "trial," and there is no reason to call any movement which does not achieve a specified consequence an "error. Even the term "learning" is misleading. The statement that the bird "learns that it will get food by stretching its neck" is an inaccurate report of what has happened. To say that it has acquired the "habit" of stretching its neck is merely to resort to an explanatory fiction, since our only evidence of the habit is the acquired tendency to perform the act. The barest possible statement of the process is this: we make a given consequence contingent upon certain physical properties of behavior (the upward movement of the head), and the behavior is then observed to increase in frequency.

It is customary to refer to any movement of the organism as a "response." The word is borrowed from the field of reflex action and implies an act which, so to speak, answers a prior event—the stimulus. But we may make an event contingent upon behavior without identifying, or being able to identify, a prior stimulus. We did not alter the environment of the pigeon to *elicit* the upward movement of the head. It is probably impossible to show that any single stimulus invariably precedes this movement. Behavior of this sort may come under the control of stimuli, but the relation is not that of elicitation. The term "response" is therefore not wholly appropriate but is so well established that we shall use it in the following discussion.

A response which has already occurred cannot, of course, be predicted or controlled. We can only predict that *similar* responses will occur in the future. The unit of a predictive science is, therefore, not a response but a class of responses. The word "operant" will be used to describe this class. The term emphasizes the fact that the behavior *operates* upon the environment to generate consequences. The consequences define the properties with respect to which responses are called similar. The term will be used both as an adjective (operant behavior) and as a noun to designate the behavior defined by a given consequence.

A single instance in which a pigeon raises its head is a *response.* It is a bit of history which may be reported in any frame of reference we wish to use, The behavior called "raising the head," regardless of when specific instances occur, is an *operant.* It can be described, not as an accomplished act, but rather as a set of acts defined by the property of the height to which the head is raised. In this sense an operant is defined by an effect which may be specified in physical terms; the "cutoff" at a certain height is a property of behavior.

The term "learning" may profitably be saved in its traditional sense to describe the reassortment of responses in a complex situation. Terms for the process of stamping in may be borrowed from Pavlov's analysis of the conditioned reflex. Pavlov himself called all events which strengthened behavior "reinforcement" and all the resulting changes "conditioning." In the Pavlovian experiment, however, a reinforcer is paired with a *stimulus;* whereas in operant behavior it is contingent upon a *response.* Operant reinforcement is therefore a separate process and requires a separate analysis. In both cases, the strengthening of behavior which results from reinforcement is appropriately called "conditioning." In operant conditioning we "strengthen" an operant in the sense of making a response more probable or, in actual fact, more frequent. In Pavlovian or respondent" conditioning we simply increase the magnitude of the response elicited by the conditioned stimulus and shorten the time which elapses between stimulus and response. (We note, incidentally, that these two cases exhaust the possibilities: an organism is conditioned when a reinforcer [1] accompanies another stimulus or [2] follows upon the organism's own behavior. Any event which does neither has no effect in changing a probability of response.) In the pigeon experiment, then, food is the *reinforcer* and presenting food when a response is emitted is the *reinforcement.* The *operant* is defined by the property upon which reinforcement is contingent—the height to which the head must be raised. The change in frequency with which the head is lifted to this height is the process of *operant conditioning.*

While we are awake, we act upon the environment constantly, and many of the consequences of our actions are reinforcing. Through operant conditioning the environment builds the basic repertoire with which we keep our balance, walk, play games, handle instruments and tools, talk, write, sail a boat, drive a car, or fly a plane. A change in the environment—a new car, a new friend, a new field of interest, a new job, a new location—may find us unprepared, but our behavior usually adjusts quickly as we acquire new responses and discard old. We shall see in the following chapter that operant reinforcement does more than build a behavioral repertoire. It improves the efficiency of behavior and maintains behavior in strength long after acquisition or efficiency has ceased to be of interest.