Selections from *Science and Human Behavior*

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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 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.

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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.

The sciences of neurology and physiology have not divested themselves entirely of a similar practice. Since techniques for observing the electrical and chemical processes in nervous tissue had not yet been developed, early information about the nervous system was limited to its gross anatomy. Neural processes could only be inferred from the behavior which was said to result from them. Such inferences were legitimate enough as scientific theories, but they could not justifiably be used to explain the very behavior upon which they were based. The hypotheses of the early physiologist may have been sounder than those of the layman, but until independent evidence could be obtained, they were no more satisfactory as explanations of behavior. Direct information about many of the chemical and electrical processes in the nervous system is now available. Statements about the nervous system are no longer necessarily inferential or fictional. But there is still a measure of circularity in much physiological explanation, even in the writings of specialists. In World War I a familiar disorder was called "shell shock." Disturbances in behavior were explained by arguing that violent explosions had damaged the structure of the nervous system, though no direct evidence of such damage was available. In World War II the same disorder was classified as "neuropsychiatric." The prefix seems to show a continuing unwillingness to abandon explanations in terms of hypothetical neural damage.

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, as we shall see in Chapter XVII. 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 nonhistoric; 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. In 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.

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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, for 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 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.

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Goals, Purposes, and Other Final Causes

It is not correct to say that operant reinforcement "strengthens the response which precedes it." The response has already occurred and cannot be changed. What is changed is the future probability of responses in the same *class*. It is the operant as a class of behavior, rather than the response as a particular instance, which is conditioned. There is, therefore, no violation of the fundamental principle of science which rules out "final causes." But this principle is violated when it is asserted that behavior is under the control of an "incentive" or "goal" which the organism has not yet achieved or a "purpose" which it has not yet fulfilled. Statements which use such words as "incentive" or "purpose" are usually reducible to statements about operant conditioning, and only a slight change is required to bring them within the framework of a natural science. Instead of saying that a man behaves because of the consequences which *are* to follow his behavior, we simply say that he behaves because of the consequences which *have* followed similar behavior in the past. This is, of course, the Law of Effect or operant conditioning.

It is sometimes argued that a response is not fully described until its purpose is referred to as a current property. But what is meant by "describe"? If we observe someone walking down the street, we may report this event in the language of physical science. If we then add that "his purpose is to mail a letter," have we said anything which was not included in our first report? Evidently so, since a man may walk down the street "for many purposes" and in the same physical way in each case. But the distinction which needs to be made is not between instances of behavior; it is between the variables of which behavior is a function. Purpose is not a property of the behavior itself; it is a way of referring to controlling variables. If we make our report after we have seen our subject mail his letter and turn back, we attribute "purpose" to him from the event which brought the behavior of walking down the street to an end. This event "gives meaning" to his performance, not by amplifying a description of the behavior as such, but by indicating an independent variable of which it may have been a function. We cannot see his "purpose" before seeing that he mails a letter, unless we have observed similar behavior and similar consequences before. Where we have done this, we use the term simply to predict that he will mail a letter upon this occasion.

Nor can our subject see his own purpose without reference to similar events. If we ask him why he is going down the street or what his purpose is and he says, "I am going to mail a letter," we have not learned anything new about his behavior but only about some of its possible causes. The subject himself, of course, may be in an advantageous position in describing these variables because he has had an extended contact with his own behavior for many years. But his statement is not therefore in a different class from similar statements made by others who have observed his behavior upon fewer occasions. As we shall see in Chapter XVII, he is simply making a plausible prediction in terms of his experiences with himself. Moreover, he may be wrong. He may report that he is "going to mail a letter," and he may indeed carry an unmailed letter in his hand and may mail it at the end of the street, but we may still be able to show that his behavior is primarily determined by the fact that upon past occasions he has encountered someone who is important to him upon just such a walk. He may not be "aware of this purpose" in the sense of being able to say that his behavior is strong for this reason.

The fact that operant behavior seems to be "directed toward the future" is misleading. Consider, for example, the case of "looking for something." In what sense is the "something" which has not yet been found relevant to the behavior? Suppose we condition a pigeon to peck a spot on the wall of a box and then, when the operant is well established, remove the spot. The bird now goes to the usual place along the wall. It raises its head, cocks its eye in the usual direction, and may even emit a weak peck in the usual place. Before extinction is very far advanced, it returns to the same place again and again in similar behavior. Must we say that the pigeon is "looking for the spot"? Must we take the "looked for" spot into account in explaining the behavior?

It is not difficult to interpret this example in terms of operant reinforcement. Since visual stimulation from the spot has usually preceded the receipt of food, the spot has become a conditioned reinforcer. It strengthens the behavior of looking in given directions from different positions. Although we have undertaken to condition only the pecking response, we have in fact strengthened many different kinds of precurrent behavior which bring the bird into positions from which it sees the spot and pecks it. These responses continue to appear, even though we have removed the spot, until extinction occurs. The spot which is "being looked for" is the spot which has occurred in the past as the immediate reinforcement of the behavior of looking. In general, looking for something consists of emitting responses which in the past have produced "something" as a consequence.

The same interpretation applies to human behavior. When we see a man moving about a room opening drawers, looking under magazines, and so on, we may describe his behavior in fully objective terms: "Now he is in a certain part of the room; he has grasped a book between the thumb and forefinger of his right hand; he is lifting the book and bending his head so that any object under the book can be seen." We may also "interpret" his behavior or "read a meaning into it" by saying that "he is looking for something" or, more specifically, that "he is looking for his glasses." What we have added is not a further description of his behavior but an inference about some of the variables responsible for it. There is no *current* goal, incentive, purpose, or meaning to be taken into account. This is so even if we ask him what he is doing and he says, "I am looking for my glasses." This is not a further description of his behavior but of the variables of which his behavior is a function; it is equivalent to "I have lost my glasses," "I shall stop what I am doing when I find my glasses," or "When I have done this in the past, I have found my glasses." These translations may seem unnecessarily roundabout, but only because expressions involving goals and purposes are abbreviations.

Very often we attribute purpose to behavior as another way of describing its biological adaptability. This issue has already been discussed, but one point may be added. In both operant conditioning and the evolutionary selection of behavioral characteristics, consequences alter future probability. Reflexes and other innate patterns of behavior evolve because they increase the chances of survival of the *species*. Operants grow strong because they are followed by important consequences in the life of the *individual*. Both processes raise the question of purpose for the same reason, and in both the appeal to a final cause may be rejected in the same way. A spider does not possess the elaborate behavioral repertoire with which it constructs a web because that web will enable it to capture the food it needs to survive. It possesses this behavior because similar behavior on the part of spiders in the past has enabled *them* to capture the food *they* needed to survive. A series of events have been relevant to the behavior of web-making in its earlier evolutionary history. We are wrong in saying that we observe the "purpose" of the web when we observe similar events in the life of the individual.