4

A Review of B. F. Skinner's *Verbal Behavior*

Noam Chomsky

1

A great many linguists and philosophers concerned with language have expressed the hope that their studies might ultimately be embedded in a framework provided by behaviorist psychology, and that refractory areas of investigation, particularly those in which meaning is involved, will in this way be opened up to fruitful exploration. Since this volume [*Verbal Behavior* (New York: Appleton-Century-Crofts, 1957)—*Ed.*] is the first large-scale attempt to incorporate the major aspects of linguistic behavior within a behaviorist framework, it merits and will undoubtedly receive careful attention. Skinner is noted for his contributions to the study of animal behavior. The book under review is the product of study of linguistic behavior extending over more than twenty years. Earlier versions of it have been fairly widely circulated, and there are quite a few references in the psychological literature to its major ideas.

The problem to which this book is addressed is that of giving a "functional analysis" of verbal behavior. By functional analysis, Skinner means identification of the variables that control this behavior and specification of how they interact to determine a particular verbal response. Furthermore, the controlling variables are to be described completely in terms of such notions as *stimulus*, *reinforcement*, *deprivation*, which have been given a reasonably clear meaning in animal experimentation. In other words, the goal of the book is to provide a way to predict and control verbal behavior by observing and manipulating the physical environment of the speaker.

Skinner feels that recent advances in the laboratory study of animal behavior permit us to approach this problem with a certain optimism, since "the basic processes and relations which give verbal behavior its special characteristics are now fairly well understood . . . the results [of this experimental work] have been surprisingly free of species restrictions. Recent work has shown that the methods can be extended to human behavior without serious modification (3).[1]

It is important to see clearly just what it is in Skinner's program and claims that makes them appear so bold and remark-

From *Language* 35, no. 1 (1959): 26-58. Reprinted by permission of the Linguistic Society of America and the author. Sections 5-10 have been omitted (the notes are therefore not numbered consecutively).

able. It is not primarily the fact that he has set functional analysis as his problem, or that he limits himself to study of *observables*, i.e., input-output relations. What is so surprising is the particular limitations he has imposed on the way in which the observables of behavior are to be studied, and, above all, the particularly simple nature of the *function* which, he claims, describes the causation of behavior. One would naturally expect that prediction of the behavior of a complex organism (or machine) would require, in addition to information about external stimulation, knowledge of the internal structure of the organism, the ways in which it processes input information and organizes its own behavior. These characteristics of the organism are in general a complicated product of inborn structure, the genetically determined course of maturation, and past experience. Insofar as independent neurophysiological evidence is not available, it is obvious that inferences concerning the structure of the organism are based on observation of behavior and outside events. Nevertheless, one's estimate of the relative importance of external factors and internal structure in the determination of behavior will have an important effect on the duration of research on linguistic (or any other) behavior, and on the kinds of analogies from animal behavior studies that will be considered relevant or suggestive.

Putting it differently, anyone who sets himself the problem of analyzing the causation of behavior will (in the absence of independent neurophysiological evidence) concern himself with the only data available, namely the record of inputs to the organism and the organism's present response, and will try to describe the function specifying the response in terms of the history of inputs. This is nothing more than the definition of his problem. There are no possible grounds for argument here, if one accepts the problem as legitimate, though Skinner has often advanced and defended this definition of a problem as if it were a thesis which other investigators reject. The differences that arise between those who affirm and those who deny the importance of the specific "contribution of the organism" to learning and performance concern the particular character and complexity of this function, and the kinds of observations and research necessary for arriving at a precise specification of it. If the contribution of the organism is complex, the only hope of predicting behavior even in a gross way will be through a very indirect program of research that begins by studying the detailed character of the behavior itself and the particular capacities of the organism involved.

Skinner's thesis is that external factors consisting of present stimulation and the history of reinforcement (in particular, the frequency, arrangement, and withholding of reinforcing stimuli) are of overwhelming importance, and that the general principles revealed in laboratory studies of these phenomena provide the basis for understanding the complexities of verbal behavior. He confidently and repeatedly voices his claim to have demonstrated that the contribution of the speaker is quite trivial and elementary, and that precise prediction of verbal behavior involves only specification of the few external factors that he has isolated experimentally with lower organisms.

Careful study of this book (and of the research on which it draws) reveals, however, that these astonishing claims are far from justified. It indicates, furthermore, that the insights that have been achieved in the laboratories of the reinforcement theorist, though quite genuine, can be applied to complex human behavior only in the most gross and superficial way, and that speculative attempts to discuss linguistic behavior in these terms alone omit from consideration factors of fundamental importance that are, no doubt, amenable to scientific study, although their specific

character cannot at present be precisely formulated. Since Skinner's work is the most extensive attempt to accommodate human behavior involving higher mental faculties within a strict behaviorist schema of the type that has attracted many linguists and philosophers, as well as psychologists, a detailed documentation is of independent interest. The magnitude of the failure of this attempt to account for verbal behavior serves as a kind of measure of the importance of the factors omitted from consideration, and an indication of how little is really known about this remarkably complex phenomenon.

The force of Skinner's argument lies in the enormous wealth and range of examples for which he proposes a functional analysis. The only way to evaluate the success of his program and the correctness of his basic assumptions about verbal behavior is to review these examples in detail and to determine the precise character of the concepts in terms of which the functional analysis is presented. Section 2 of this review describes the experimental context with respect to which these concepts are originally defined. Sections 3 and 4 deal with the basic concepts—*stimulus*, *response*, and *reinforcement*—Sections 6 to 10 with the new descriptive machinery developed specifically for the description of verbal behavior. In Section 5 we consider the status of the fundamental claim, drawn from the laboratory, which serves as the basis for the analogic guesses about human behavior that have been proposed by many psychologists. The final section (Section 11) will consider some ways in which further linguistic work may play a part in clarifying some of these problems.

2

Although this book makes no direct reference to experimental work, it can be understood only in terms of the general framework that Skinner has developed for the description of behavior. Skinner divides the responses of the animal into two main categories. *Respondents* are purely reflex responses elicited by particular stimuli. *Operants* are emitted responses, for which no obvious stimulus can be discovered. Skinner has been concerned primarily with operant behavior. The experimental arrangement that he introduced consists basically of a box with a bar attached to one wall in such a way that when the bar is pressed, a food pellet is dropped into a tray (and the bar press is recorded). A rat placed in the box will soon press the bar, releasing a pellet into the tray. This state of affairs, resulting from the bar press, increases the *strength* of the bar-pressing operant. The food pellet is called a *reinforcer*; the event, *a reinforcing event*. The strength of an operant is defined by Skinner in terms of the rate of response during extinction (i.e., after the last reinforcement and before return to the pre-conditioning rate).

Suppose that release of the pellet is conditional on the flashing of a light. Then the rat will come to press the bar only when the light flashes. This is called *stimulus discrimination*. The response is called a *discriminated operant* and the light is called the *occasion* for its emission: this is to be distinguished from elicitation of a response by a stimulus in the case of the respondent.[2] Suppose that the apparatus is so arranged that bar-pressing of only a certain character (e.g., duration) will release the pellet. The rat will then come to press the bar in the required way. This process is called *response differentiation*. By successive slight changes in the conditions under which the response will be reinforced, it is possible to shape the response of a rat or a pigeon in very surprising ways in a very short time, so that rather complex behavior can be produced by a process of successive approximation.

A stimulus can become reinforcing by repeated association with an already reinforcing stimulus. Such a stimulus is called a *secondary reinforcer*. Like many

contemporary behaviorists, Skinner considers money, approval, and the like to be secondary reinforcers which have become reinforcing because of their association with food, etc.[3] Secondary reinforcers can be *generalized* by associating them with a variety of different primary reinforcers.

Another variable that can affect the rate of the bar-pressing operant is drive, which Skinner defines operationally in terms of hours of deprivation. His major scientific book, *Behavior of Organisms*, is a study of the effects of food-deprivation and conditioning on the strength of the bar-pressing response of healthy mature rats. Probably Skinner's most original contribution to animal behavior studies has been his investigation of the effects of intermittent reinforcement, arranged in various different ways, presented in *Behavior of Organisms* and extended (with pecking of pigeons as the operant under investigation) in the recent *Schedules of Reinforcement* by Ferster and Skinner (1957). It is apparently these studies that Skinner has in mind when he refers to the recent advances in the study of animal behavior.[4]

The notions *stimulus, response, reinforcement* are relatively well defined with respect to the bar-pressing experiments and others similarly restricted. Before we can extend them to real-life behavior, however, certain difficulties must be faced. We must decide, first of all, whether any physical event to which the organism is capable of reacting is to be called a stimulus on a given occasion, or only one to which the organism in fact reacts; and correspondingly, we must decide whether any part of behavior is to be called a response, or only one connected with stimuli in lawful ways. Questions of this sort pose something of a dilemma for the experimental psychologist. If he accepts the broad definitions, characterizing any physical event impinging on the organism as a stimulus and any part of the organism's behavior as a response, he must conclude that behavior has not been demonstrated to be lawful. In the present state of our knowledge, we must attribute an overwhelming influence on actual behavior to ill-defined factors of attention, set, volition, and caprice. If we accept the narrower definitions, then behavior is lawful by definition (if it consists of responses); but this fact is of limited significance, since most of what the animal does will simply not be considered behavior. Hence, the psychologist either must admit that behavior is not lawful (or that he cannot at present show that it is—not at all a damaging admission for a developing science), or must restrict his attention to those highly limited areas in which it is lawful (e.g., with adequate controls, bar-pressing in rats; lawfulness of the observed behavior provides, for Skinner, an implicit definition of a good experiment).

Skinner does not consistently adopt either course. He utilizes the experimental results as evidence for the scientific character of his system of behavior, and analogic guesses (formulated in terms of a metaphoric extension of the technical vocabulary of the laboratory) as evidence for its scope. This creates the illusion of a rigorous scientific theory with a very broad scope, although in fact the terms used in the description of real-life and of laboratory behavior may be mere homonyms, with at most a vague similarity of meaning. To substantiate this evaluation, a critical account of his book must show that with a literal reading (where the terms of the descriptive system have something like the technical meanings given in Skinner's definitions) the book covers almost no aspect of linguistic behavior, and that with a metaphoric reading, it is no more scientific than the traditional approaches to this subject matter, and rarely as clear and careful.[5]

3

Consider first Skinner's use of the notions *stimulus* and *response*. In *Behavior*

of Organisms (9) he commits himself to the narrow definitions for these terms. A part of the environment and a part of behavior are called *stimulus* (eliciting, discriminated, or reinforcing) and *response*, respectively, only if they are lawfully related; that is, if the *dynamic laws* relating them show smooth and reproducible curves. Evidently, stimuli and responses, so defined, have not been shown to figure very widely in ordinary human behavior.[6] We can, in the face of presently available evidence, continue to maintain the lawfulness of the relation between stimulus and response only by depriving them of their objective character. A typical example of *stimulus control* for Skinner would be the response to a piece of music with the utterance *Mozart* or to a painting with the response *Dutch*. These responses are asserted to be "under the control of extremely subtle properties" of the physical object or event (108). Suppose instead of saying *Dutch* we had said *Clashes with the wallpaper, I thought you liked abstract work, Never saw it before, Tilted, Hanging too low, Beautiful, Hideous, Remember our camping trip last summer?*, or whatever else might come into our minds when looking at a picture (in Skinnerian translation, whatever other responses exist in sufficient strength). Skinner could only say that each of these responses is under the control of some other stimulus property of the physical object. If we look at a red chair and say *red*, the response is under the control of the stimulus *redness*; if we say *chair*, it is under the control of the collection of properties (for Skinner, the object) *chairness* (110), and similarly for any other response. This device is as simple as it is empty. Since properties are free for the asking (we have as many of them as we have nonsynonymous descriptive expressions in our language, whatever this means exactly), we can account for a wide class of responses in terms of Skinnerian functional analysis by identifying the *controlling stimuli*. But the word *stimulus* has lost all objectivity in this usage. Stimuli are no longer part of the outside physical world; they are driven back into the organism. We identify the stimulus when we hear the response. It is clear from such examples, which abound, that the talk of *stimulus control* simply disguises a complete retreat to mentalistic psychology. We cannot predict verbal behavior in terms of the stimuli in the speaker's environment, since we do not know what the current stimuli are until he responds. Furthermore, since we cannot control the property of a physical object to which an individual will respond, except in highly artificial cases, Skinner's claim that his system, as opposed to the traditional one, permits the practical control of verbal behavior[7] is quite false.

Other examples of *stimulus control* merely add to the general mystification. Thus, a proper noun is held to be a response "under the control of a specific person or thing" (as controlling stimulus, 113). I have often used the words *Eisenhower* and *Moscow*, which I presume are proper nouns if anything is, but have never been *stimulated* by the corresponding objects. How can this fact be made compatible with this definition? Suppose that I use the name of a friend who is not present. Is this an instance of a proper noun under the control of the friend as stimulus? Elsewhere it is asserted that a stimulus controls a response in the sense that presence of the stimulus increases the probability of the response. But it is obviously untrue that the probability that a speaker will produce a full name is increased when its bearer faces the speaker. Furthermore, how can one's own name be a proper noun in this sense? A multitude of similar questions arise immediately. It appears that the word *control* here is merely a misleading paraphrase for the traditional *denote* or *refer*. The assertion (115) that so far as the speaker is concerned, the relation of reference is "simply the probability that the speaker will emit

a response of a given form in the presence of a stimulus having specified properties" is surely incorrect if we take the words *presence, stimulus,* and *probability* in their literal sense. That they are not intended to be taken literally is indicated by many examples, as when a response is said to be "controlled" by a situation or state of affairs as "stimulus." Thus, the expression *a needle in a haystack* "may be controlled as a unit by a particular type of situation" (116); the words in a single part of speech, e.g., all adjectives, are under the control of a single set of subtle properties of stimuli (121); "the sentence *The boy runs a store* is under the control of an extremely complex stimulus situation" (335); "*He is not at all well* may function as a standard response under the control of a state of affairs which might also control *He is ailing*" (325); when an envoy observes events in a foreign country and reports upon his return, his report is under "remote stimulus control" (416); the statement *This is war* may be a response to a "confusing international situation" (441); the suffix *-ed* is controlled by that "subtle property of stimuli which we speak of as action-in-the-past" (121) just as the *-s* in *The boy runs* is under the control of such specific features of the situation as its "currency" (332). No characterization of the notion *stimulus control* that is remotely related to the bar-pressing experiment (or that preserves the faintest objectivity) can be made to cover a set of examples like these, in which, for example, the *controlling stimulus* need not even impinge on the responding organism.

Consider now Skinner's use of the notion *response.* The problem of identifying units in verbal behavior has of course been a primary concern of linguists, and it seems very likely that experimental psychologists should be able to provide much-needed assistance in clearing up the many remaining difficulties in systematic identification. Skinner recognizes (20) the fundamental character of the problem of identification of a unit of verbal behavior, but is satisfied with an answer so vague and subjective that it does not really contribute to its solution. The unit of verbal behavior—the verbal operant—is defined as a class of responses of identifiable form functionally related to one or more controlling variables. No method is suggested for determining in a particular instance what are the controlling variables, how many such units have occurred, or where their boundaries are in the total response. Nor is any attempt made to specify how much or what kind of similarity in form or *control* is required for two physical events to be considered instances of the same operant. In short, no answers are suggested for the most elementary questions that must be asked of anyone proposing a method for description of behavior. Skinner is content with what he calls an *extrapolation* of the concept of operant developed in the laboratory to the verbal field. In the typical Skinnerian experiment, the problem of identifying the unit of behavior is not too crucial. It is defined, by fiat, as a recorded peck or bar-press, and systematic variations in the rate of this operant and its resistance to extinction are studied as a function of deprivation and scheduling of reinforcement (pellets). The operant is thus defined with respect to a particular experimental procedure. This is perfectly reasonable and has led to many interesting results. It is, however, completely meaningless to speak of extrapolating this concept of operant to ordinary verbal behavior. Such "extrapolation" leaves us with no way of justifying one or another decision about the units in the "verbal repertoire."

Skinner specifies "response strength" as the basic datum, the basic dependent variable in his functional analysis. In the bar-pressing experiment, response strength is defined in terms of rate of emission during extinction. Skinner has argued[8] that this is "the only datum that varies significantly and in the expected

direction under conditions which are relevant to the 'learning process.'" In the book under review, response strength is defined as "probability of emission" (22). This definition provides a comforting impression of objectivity, which, however, is quickly dispelled when we look into the matter more closely. The term *probability* has some rather obscure meaning for Skinner in this book.[9] We are told, on the one hand, that "our evidence for the contribution of each variable [to response strength] is based on observation of frequencies alone" (28). At the same time, it appears that frequency is a very misleading measure of strength, since, for example, the frequency of a response may be "primarily attributable to the frequency of occurrence of controlling variables" (27). It is not clear how the frequency of a response can be attributable to anything BUT the frequency of occurrence of its controlling variables if we accept Skinner's view that the behavior occurring in a given situation is "fully determined" by the relevant controlling variables (175, 228). Furthermore, although the evidence for the contribution of each variable to response strength is based on observation of frequencies alone, it turns out that "we base the notion of strength upon several kinds of evidence" (22), in particular (22-28): emission of the response (particularly in unusual circumstances), energy level (stress), pitch level, speed and delay of emission, size of letters etc. in writing, immediate repetition, and—a final factor, relevant but misleading—over-all frequency.

Of course, Skinner recognizes that these measures do not co-vary, because (among other reasons) pitch, stress, quantity, and reduplication may have internal linguistic functions.[10] However, he does not hold these conflicts to be very important, since the proposed factors indicative of strength are "fully understood by everyone" in the culture (27). For example, "if we are shown a prized work of art and exclaim *Beautiful!*, the speed and energy of the response will not be lost on the owner." It does not appear totally obvious that in this case the way to impress the owner is to shriek *Beautiful* in a loud, high-pitched voice, repeatedly, and with no delay (high response strength). It may be equally effective to look at the picture silently (long delay) and then to murmur *Beautiful* in a soft, low-pitched voice (by definition, very low response strength).

It is not unfair, I believe, to conclude from Skinner's discussion of response strength, the *basic datum* in functional analysis, that his *extrapolation* of the notion of probability can best be interpreted as, in effect, nothing more than a decision to use the word *probability*, with its favorable connotations of objectivity, as a cover term to paraphrase such low-status words as *interest, intention, belief*, and the like. This interpretation is fully justified by the way in which Skinner uses the terms *probability* and *strength*. To cite just one example, Skinner defines the process of confirming an assertion in science as one of "generating additional variables to increase its probability" (425), and more generally, its strength (425-29). If we take this suggestion quite literally, the degree of confirmation of a scientific assertion can be measured as a simple function of the loudness, pitch, and frequency with which it is proclaimed, and a general procedure for increasing its degree of confirmation would be, for instance, to train machine guns on large crowds of people who have been instructed to shout it. A better indication of what Skinner probably has in mind here is given by his description of how the theory of evolution, as an example, is confirmed. This "single set of verbal responses . . . is made more plausible—is strengthened—by several types of construction based upon verbal responses in geology, paleontology, genetics, and so on" (427). We are no doubt to interpret the terms *strength* and *probability* in this context as paraphrases of

more familiar locutions such as "justified belief" or "warranted assertability," or something of the sort. Similar latitude of interpretation is presumably expected when we read that "frequency of effective action accounts in turn for what we may call the listener's 'belief' " (88) or that "our belief in what someone tells us is similarly a function of, or identical with, our tendency to act upon the verbal stimuli which he provides" (160).[11]

I think it is evident, then, that Skinner's use of the terms *stimulus, control, response,* and *strength* justify the general conclusion stated in the last paragraph of Section 2. The way in which these terms are brought to bear on the actual data indicates that we must interpret them as mere paraphrases for the popular vocabulary commonly used to describe behavior and as having no particular connection with the homonymous expressions used in the description of laboratory experiments. Naturally, this terminological revision adds no objectivity to the familiar *mentalistic* mode of description.

4

The other fundamental notion borrowed from the description of bar-pressing experiments is *reinforcement.* It raises problems which are similar, and even more serious. In *Behavior of Organisms,* "the operation of reinforcement is defined as the presentation of a certain kind of stimulus in a temporal relation with either a stimulus or response. A reinforcing stimulus is defined as such by its power to produce the resulting change [in strength]. There is no circularity about this: some stimuli are found to produce the change, others not, and they are classified as reinforcing and nonreinforcing accordingly" (62). This is a perfectly appropriate definition[12] for the study of schedules of reinforcement. It is perfectly useless, however, in the discussion of real-life behavior, unless we can somehow characterize the stimuli which are reinforcing (and the situations and conditions under which they are reinforcing). Consider first of all the status of the basic principle that Skinner calls the "law of conditioning" (law of effect). It reads: "if the occurrence of an operant is followed by presence of a reinforcing stimulus, the strength is increased" (*Behavior of Organisms,* 21). As *reinforcement* was defined, this law becomes a tautology.[13] For Skinner, learning is just change in response strength.[14] Although the statement that presence of reinforcement is a sufficient condition for learning and maintenance of behavior is vacuous, the claim that it is a necessary condition may have some content, depending on how the class of reinforcers (and appropriate situations) is characterized. Skinner does make it very clear that in his view reinforcement is a necessary condition for language learning and for the continued availability of linguistic responses in the adult.[15] However, the looseness of the term *reinforcement* as Skinner uses it in the book under review makes it entirely pointless to inquire into the truth or falsity of this claim. Examining the instances of what Skinner calls *reinforcement,* we find that not even the requirement that a reinforcer be an identifiable stimulus is taken seriously. In fact, the term is used in such a way that the assertion that reinforcement is necessary for learning and continued availability of behavior is likewise empty.

To show this, we consider some examples of *reinforcement.* First of all, we find a heavy appeal to automatic self-reinforcement. Thus, "a man talks to himself . . . because of the reinforcement he receives" (163); "the child is reinforced automatically when he duplicates the sounds of airplanes, streetcars . . ." (164); "the young child alone in the nursery may automatically reinforce his own exploratory verbal behavior when he produces sounds which he has heard in the speech of others" (58); "the speaker who is also an accomplished listener 'knows when he

has correctly echoed a response' and is reinforced thereby" (68); thinking is "behaving which automatically affects the behaver and is reinforcing because it does so" (438; cutting one's finger should thus be reinforcing, and an example of thinking); "the verbal fantasy, whether overt or covert, is automatically reinforcing to the speaker as listener. Just as the musician plays or composes what he is reinforced by hearing, or as the artist paints what reinforces him visually, so the speaker engaged in verbal fantasy says what he is reinforced by hearing or writes what he is reinforced by reading" (439); similarly, care in problem solving, and rationalization, are automatically self-reinforcing (442-43). We can also reinforce someone by emitting verbal behavior as such (since this rules out a class of aversive stimulations, 167), by not emitting verbal behavior (keeping silent and paying attention, 199), or by acting appropriately on some future occasion (152: "the strength of [the speaker's] behavior is determined mainly by the behavior which the listener will exhibit with respect to a given state of affairs"; this Skinner considers the general case of "communication" or "letting the listener know"). In most such cases, of course, the speaker is not present at the time when the reinforcement takes place, as when "the artist . . . is reinforced by the effects his works have upon . . . others" (224), or when the writer is reinforced by the fact that his "verbal behavior may reach over centuries or to thousands of listeners or readers at the same time. The writer may not be reinforced often or immediately, but his net reinforcement may be great" (206; this accounts for the great "strength" of his behavior). An individual may also find it reinforcing to injure someone by criticism or by bringing bad news, or to publish an experimental result which upsets the theory of a rival (154), to describe circumstances which would be reinforcing if they were to occur (165), to avoid repetition (222), to "hear" his own name though in fact it was not mentioned or to hear nonexistent words in his child's babbling (259), to clarify or otherwise intensify the effect of a stimulus which serves an important discriminative function (416), and so on.

From this sample, it can be seen that the notion of reinforcement has totally lost whatever objective meaning it may ever have had. Running through these examples, we see that a person can be reinforced though he emits no response at all, and that the reinforcing *stimulus* need not impinge on the *reinforced person* or need not even exist (it is sufficient that it be imagined or hoped for). When we read that a person plays what music he likes (165), says what he likes (165), thinks what he likes (438-39), reads what books he likes (163), etc., BECAUSE he finds it reinforcing to do so, or that we write books or inform others of facts BECAUSE we are reinforced by what we hope will be the ultimate behavior of reader or listener, we can only conclude that the term *reinforcement* has a purely ritual function. The phrase "X is reinforced by Y (stimulus, state of affairs, event, etc.)" is being used as a cover term for "X wants Y," "X likes Y," "X wishes that Y were the case," etc. Invoking the term *reinforcement* has no explanatory force, and any idea that this paraphrase introduces any new clarity or objectivity into the description of wishing, liking, etc., is a serious delusion. The only effect is to obscure the important differences among the notions being paraphrased. Once we recognize the latitude with which the term *reinforcement* is being used, many rather startling comments lose their initial effect—for instance, that the behavior of the creative artist is "controlled entirely by the contingencies of reinforcement" (150). What has been hoped for from the psychologist is some indication how the casual and informal description of everyday behavior in the popular vocabulary can be explained or clarified in terms of the notions developed in care-

ful experiment and observation, or perhaps replaced in terms of a better scheme. A mere terminological revision, in which a term borrowed from the laboratory is used with the full vagueness of the ordinary vocabulary, is of no conceivable interest.

It seems that Skinner's claim that all verbal behavior is acquired and maintained in "strength" through reinforcement is quite empty, because his notion of reinforcement has no clear content, functioning only as a cover term for any factor, detectable or not, related to acquisition or maintenance of verbal behavior.[16] Skinner's use of the term *conditioning* suffers from a similar difficulty. Pavlovian and operant conditioning are processes about which psychologists have developed real understanding. Instruction of human beings is not. The claim that instruction and imparting of information are simply matters of conditioning (357-66) is pointless. The claim is true, if we extend the term *conditioning* to cover these processes, but we know no more about them after having revised this term in such a way as to deprive it of its relatively clear and objective character. It is, as far as we know, quite false, if we use *conditioning* in its literal sense. Similarly, when we say that "it is the function of predication to facilitate the transfer of response from one term to another or from one object to another" (361), we have said nothing of any significance. In what sense is this true of the predication *Whales are mammals?* Or, to take Skinner's example, what point is there in saying that the effect of *The telephone is out of order* on the listener is to bring behavior formerly controlled by the stimulus *out of order* under control of the stimulus *telephone* (or the telephone itself) by a process of simple conditioning (362)? What laws of conditioning hold in this case? Furthermore, what behavior is *controlled* by the stimulus *out of order*, in the abstract? Depending on the object of which this is predicated, the present state of motivation of the listener, etc., the behavior may vary from rage to pleasure, from fixing the object to throwing it out, from simply not using it to trying to use it in the normal way (e.g., to see if it is really out of order), and so on. To speak of "conditioning" or "bringing previously available behavior under control of a new stimulus" in such a case is just a kind of play-acting at science (cf. also 43*n*).

* * *

11

The preceding discussion covers all the major notions that Skinner introduces in his descriptive system. My purpose in discussing the concepts one by one was to show that in each case, if we take his terms in their literal meaning, the description covers almost no aspect of verbal behavior, and if we take them metaphorically, the description offers no improvement over various traditional formulations. The terms borrowed from experimental psychology simply lose their objective meaning with this extension, and take over the full vagueness of ordinary language. Since Skinner limits himself to such a small set of terms for paraphrase, many important distinctions are obscured. I think that this analysis supports the view expressed in Section 1, that elimination of the independent contribution of the speaker and learner (a result which Skinner considers of great importance, cf. 311-12) can be achieved only at the cost of eliminating all significance from the descriptive system, which then operates at a level so gross and crude that no answers are suggested to the most elementary questions.[46] The questions to which Skinner has addressed his speculations are hopelessly premature. It is futile to inquire into the causation of verbal behavior until much more is known about the specific character of this behavior; and there is little point in speculating about the process of acquisition without

much better understanding of what is acquired.

Anyone who seriously approaches the study of linguistic behavior, whether linguist, psychologist, or philosopher, must quickly become aware of the enormous difficulty of stating a problem which will define the area of his investigations, and which will not be either completely trivial or hopelessly beyond the range of present-day understanding and technique. In selecting functional analysis as his problem, Skinner has set himself a task of the latter type. In an extremely interesting and insightful paper,[47] K. S. Lashley has implicitly delimited a class of problems which can be approached in a fruitful way by the linguist and psychologist, and which are clearly preliminary to those with which Skinner is concerned. Lashley recognizes, as anyone must who seriously considers the data, that the composition and production of an utterance is not simply a matter of stringing together a sequence of responses under the control of outside stimulation and intraverbal association, and that the syntactic organization of an utterance is not something directly represented in any simple way in the physical structure of the utterance itself. A variety of observations lead him to conclude that syntactic structure is "a generalized pattern imposed on the specific acts as they occur" (512), and that "a consideration of the structure of the sentence and other motor sequences will show . . . that there are, behind the overtly expressed sequences, a multiplicity of integrative processes which can only be inferred from the final results of their activity" (509). He also comments on the great difficulty of determining the "selective mechanisms" used in the actual construction of a particular utterance (522).

Although present-day linguistics cannot provide a precise account of these integrative processes, imposed patterns, and selective mechanisms, it can at least set itself the problem of characterizing these completely. It is reasonable to regard the grammar of a language L ideally as a mechanism that provides an enumeration of the sentences of L in something like the way in which a deductive theory gives an enumeration of a set of theorems. (*Grammar*, in this sense of the word, includes phonology.) Furthermore, the theory of language can be regarded as a study of the formal properties of such grammars, and, with a precise enough formulation, this general theory can provide a uniform method for determining, from the process of generation of a given sentence, a structural description which can give a good deal of insight into how this sentence is used and understood. In short, it should be possible to derive from a properly formulated grammar a statement of the integrative processes and generalized patterns imposed on the specific acts that constitute an utterance. The rules of a grammar of the appropriate form can be subdivided into the two types, optional and obligatory; only the latter must be applied in generating an utterance. The optional rules of the grammar can be viewed, then, as the selective mechanisms involved in the production of a particular utterance. The problem of specifying these integrative processes and selective mechanisms is nontrivial and not beyond the range of possible investigation. The results of such a study might, as Lashley suggests, be of independent interest for psychology and neurology (and conversely). Although such a study, even if successful, would by no means answer the major problems involved in the investigation of meaning and the causation of behavior, it surely will not be unrelated to these. It is at least possible, furthermore, that such a notion as *semantic generalization*, to which such heavy appeal is made in all approaches to language in use, conceals complexities and specific structure of inference not far different from those that can be studied and exhibited in the case of syntax, and that consequently the general

character of the results of syntactic investigations may be a corrective to oversimplified approaches to the theory of meaning.

The behavior of the speaker, listener, and learner of language constitutes, of course, the actual data for any study of language. The construction of a grammar which enumerates sentences in such a way that a meaningful structural description can be determined for each sentence does not in itself provide an account of this actual behavior. It merely characterizes abstractly the ability of one who has mastered the language to distinguish sentences from nonsentences, to understand new sentences (in part), to note certain ambiguities, etc. These are very remarkable abilities. We constantly read and hear new sequences of words, recognize them as sentences, and understand them. It is easy to show that the new events that we accept and understand as sentences are not related to those with which we are familiar by any simple notion of formal (or semantic or statistical) similarity or identity of grammatical frame. Talk of generalization in this case is entirely pointless and empty. It appears that we recognize a new item as a sentence not because it matches some familiar item in any simple way, but because it is generated by the grammar that each individual has somehow and in some form internalized. And we understand a new sentence, in part, because we are somehow capable of determining the process by which this sentence is derived in this grammar.

Suppose that we manage to construct grammars having the properties outlined above. We can then attempt to describe and study the achievement of the speaker, listener, and learner. The speaker and the listener, we must assume, have already acquired the capacities characterized abstractly by the grammar. The speaker's task is to select a particular compatible set of optional rules. If we know, from grammatical study, what choices are available to him and what conditions of compatibility the choices must meet, we can proceed meaningfully to investigate the factors that lead him to make one or another choice. The listener (or reader) must determine, from an exhibited utterance, what optional rules were chosen in the construction of the utterance. It must be admitted that the ability of a human being to do this far surpasses our present understanding. The child who learns a language has in some sense constructed the grammar for himself on the basis of his observation of sentences and nonsentences (i.e., corrections by the verbal community). Study of the actual observed ability of a speaker to distinguish sentences from nonsentences, detect ambiguities, etc., apparently forces us to the conclusion that this grammar is of an extremely complex and abstract character, and that the young child has succeeded in carrying out what from the formal point of view, at least, seems to be a remarkable type of theory construction. Furthermore, this task is accomplished in an astonishingly short time, to a large extent independently of intelligence, and in a comparable way by all children. Any theory of learning must cope with these facts.

It is not easy to accept the view that a child is capable of constructing an extremely complex mechanism for generating a set of sentences, some of which he has heard, or that an adult can instantaneously determine whether (and if so, how) a particular item is generated by this mechanism, which has many of the properties of an abstract deductive theory. Yet this appears to be a fair description of the performance of the speaker, listener, and learner. If this is correct, we can predict that a direct attempt to account for the actual behavior of speaker, listener, and learner, not based on a prior understanding of the structure of grammars, will achieve very limited success. The grammar must be regarded as a component in the behavior of the speaker and listener

which can only be inferred, as Lashley has put it, from the resulting physical acts. The fact that all normal children acquire essentially comparable grammars of great complexity with remarkable rapidity suggests that human beings are somehow specially designed to do this, with data-handling or "hypothesis-formulating" ability of unknown character and complexity.[48] The study of linguistic structure may ultimately lead to some significant insights into this matter. At the moment the question cannot be seriously posed, but in principle it may be possible to study the problem of determining what the built-in structure of an information-processing (hypothesis-forming) system must be to enable it to arrive at the grammar of a language from the available data in the available time. At any rate, just as the attempt to eliminate the contribution of the speaker leads to a "mentalistic" descriptive system that succeeds only in blurring important traditional distinctions, a refusal to study the contribution of the child to language learning permits only a superficial account of language acquisition, with a vast and unanalyzed contribution attributed to a step called *generalization* which in fact includes just about everything of interest in this process. If the study of language is limited in these ways, it seems inevitable that major aspects of verbal behavior will remain a mystery.

Notes

1. Skinner's confidence in recent achievements in the study of animal behavior and their applicability to complex human behavior does not appear to be widely shared. In many recent publications of confirmed behaviorists there is a prevailing note of skepticism with regard to the scope of these achievements. For representative comments, see the contributions to *Modern Learning Theory* (by W. K. Estes *et al.*; New York: Appleton-Century-Crofts, Inc., 1954); B. R. Bugelski, *Psychology of Learning* (New York: Holt, Rinehart & Winston, Inc., 1956); S. Koch, in *Nebraska Symposium on Motivation*, 58 (Lincoln, 1956); W. S. Verplanck, "Learned and Innate Behavior," *Psych. Rev.*, 52 (1955), 139. Perhaps the strongest view is that of H. Harlow, who has asserted ("Mice, Monkeys, Men, and Motives," *Psych. Rev.*, 60 [1953], 23-32) that "a strong case can be made for the proposition that the importance of the psychological problems studied during the last 15 years has decreased as a negatively accelerated function approaching an asymptote of complete indifference." N. Tinbergen, a leading representative of a different approach to animal-behavior studies (comparative ethology), concludes a discussion of *functional analysis* with the comment that "we may now draw the conclusion that the causation of behavior is immensely more complex than was assumed in the generalizations of the past. A number of internal and external factors act upon complex central nervous structures. Second, it will be obvious that the facts at our disposal are very fragmentary indeed"—*The Study of Instinct* (Toronto: Oxford Univ. Press, 1951), p. 74.

2. In *Behavior of Organisms* (New York: Appleton-Century-Crofts, Inc., 1938), Skinner remarks that "although a conditioned operant is the result of the correlation of the response with a particular reinforcement, a relation between it and a discriminative stimulus acting prior to the response is the almost universal rule" (178-79). Even emitted behavior is held to be produced by some sort of "originating force" (51) which, in the case of operant behavior, is not under experimental control. The distinction between eliciting stimuli, discriminated stimuli, and "originating forces" has never been adequately clarified and becomes even more confusing when private internal events are considered to be discriminated stimuli (see below).

3. In a famous experiment, chimpanzees were taught to perform complex tasks to receive tokens which had become secondary reinforcers because of association with food. The idea that money, approval, prestige, etc. actually acquire their motivating effects on human behavior according to this paradigm is unproved, and not particularly plausible. Many psychologists within the behaviorist movement are quite skeptical about this (cf. 23*n*). As in the case of most aspects of human behavior, the evidence about secondary reinforcement is so fragmentary, conflicting, and

complex that almost any view can find some support.

4. Skinner's remark quoted above about the generality of his basic results must be understood in the light of the experimental limitations he has imposed. If it were true in any deep sense that the basic processes in language are well understood and free of species restriction, it would be extremely odd that language is limited to man. With the exception of a few scattered observations (cf. his article, "A Case History in Scientific Method," *The American Psychologist*, 11 [1956], 221-33), Skinner is apparently basing this claim on the fact that qualitatively similar results are obtained with bar pressing of rats and pecking of pigeons under special conditions of deprivation and various schedules of reinforcement. One immediately questions how much can be based on these facts, which are in part at least an artifact traceable to experimental design and the definition of *stimulus* and *response* in terms of *smooth dynamic curves* (see below). The dangers inherent in any attempt to *extrapolate* to complex behavior from the study of such simple responses as bar pressing should be obvious and have often been commented on (cf., e.g., Harlow, *op. cit.*). The generality of even the simplest results is open to serious question. Cf. in this connection M. E. Bitterman, J. Wodinsky, and D. K. Candland, "Some Comparative Psychology," *Am. Jour. of Psych.*, 71 (1958), 94-110, where it is shown that there are important qualitative differences in solution of comparable elementary problems by rats and fish.

5. An analogous argument, in connection with a different aspect of Skinner's thinking, is given by M. Scriven in *"A Study of Radical Behaviorism," Univ. of Minn. Studies in Philosophy of Science*, I. Cf. Verplanck's contribution to *Modern Learning Theory, op. cit.* pp. 283-88, for more general discussion of the difficulties in formulating an adequate definition of *stimulus* and *response*. He concludes, quite correctly, that in Skinner's sense of the word, stimuli are not objectively identifiable independently of the resulting behavior, nor are they manipulable. Verplanck presents a clear discussion of many other aspects of Skinner's system, commenting on the untestability of many of the so-called "laws of behavior" and the limited scope of many of the others, and the arbitrary and obscure character of Skinner's notion of *lawful relation;* and, at the same time, noting the importance of the experimental data that Skinner has accumulated.

6. In *Behavior of Organisms*, Skinner apparently was willing to accept this consequence. He insists (41-42) that the terms of casual description in the popular vocabulary are not validly descriptive until the defining properties of stimulus and response are specified, the correlation is demonstrated experimentally, and the dynamic changes in it are shown to be lawful. Thus, in describing a child as hiding from a dog, "it will not be enough to dignify the popular vocabulary by appealing to essential properties of *dogness* or *hidingness* and to suppose them intuitively known." But this is exactly what Skinner does in the book under review, as we will see directly.

7. 253f. and elsewhere, repeatedly. As an example of how well we can control behavior using the notions developed in this book, Skinner shows here how he would go about evoking the response *pencil*. The most effective way, he suggests, is to say to the subject, "Please say *pencil*" (our chances would, presumably, be even further improved by use of "aversive stimulation," e.g., holding a gun to his head). We can also "make sure that no pencil or writing instrument is available, then hand our subject a pad of paper appropriate to pencil sketching, and offer him a handsome reward for a recognizable picture of a cat." It would also be useful to have voices saying *pencil* or *pen and* . . . in the background; signs reading *pencil* or *pen and* . . . ; or to place a "large and unusual pencil in an unusual place clearly in sight." "Under such circumstances, it is highly probable that our subject will say *pencil.*" "The available techniques are all illustrated in this sample." These contributions of behavior theory to the practical control of human behavior are amply illustrated elsewhere in the book, as when Skinner shows (113-14) how we can evoke the response *red* (the device suggested is to hold a red object before the subject and say, "Tell me what color this is").

In fairness, it must be mentioned that there are certain nontrivial applications of *operant conditioning* to the control of human behavior. A wide variety of experiments have shown that the number of plural nouns (for example) produced by a subject will increase if the experimenter says "right" or "good" when one is produced (similarly, positive attitudes

on a certain issue, stories with particular content, etc.; cf. L. Krasner, "Studies of the Conditioning of Verbal Behavior," *Psych. Bull.*, 55 [1958], for a survey of several dozen experiments of this kind, mostly with positive results). It is of some interest that the subject is usually unaware of the process. Just what insight this gives into normal verbal behavior is not obvious. Nevertheless, it is an example of positive and not totally expected results using the Skinnerian paradigm.

8. "Are Theories of Learning Necessary?", *Psych. Rev.*, 57 (1950), 193-216.

9. And elsewhere. In his paper "Are Theories of Learning Necessary?" Skinner considers the problem how to extend his analysis of behavior to experimental situations in which it is impossible to observe frequencies, rate of response being the only valid datum. His answer is that "the notion of probability is usually extrapolated to cases in which a frequency analysis cannot be carried out. In the field of behavior we arrange a situation in which frequencies are available as data, but we use the notion of probability in analyzing or formulating instances of even types of behavior which are not susceptible to this analysis" (199). There are, of course, conceptions of probability not based directly on frequency, but I do not see how any of these apply to the cases that Skinner has in mind. I see no way of interpreting the quoted passage other than as signifying an intention to use the word *probability* in describing behavior quite independently of whether the notion of probability is at all relevant.

10. Fortunately, "In English this presents no great difficulty" since, for example, "relative pitch levels . . . are not . . . important" (25). No reference is made to the numerous studies of the function of relative pitch levels and other intonational features in English.

11. The vagueness of the word *tendency*, as opposed to *frequency*, saves the latter quotation from the obvious incorrectness of the former. Nevertheless, a good deal of stretching is necessary. If *tendency* has anything like its ordinary meaning, the remark is clearly false. One may believe strongly the assertion that Jupiter has four moons, that many of Sophocles' plays have been irretrievably lost, that the earth will burn to a crisp in ten million years, and so on, without experiencing the slightest tendency to act upon these verbal stimuli. We may, of course, turn Skinner's assertion into a very unilluminating truth by defining "tendency to act" to include tendencies to answer questions in certain ways, under motivation to say what one believes is true.

12. One should add, however, that it is in general not the stimulus as such that is reinforcing, but the stimulus in a particular situational context. Depending on experimental arrangement, a particular physical event or object may be reinforcing, punishing, or unnoticed. Because Skinner limits himself to a particular, very simple experimental arrangement, it is not necessary for him to add this qualification, which would not be at all easy to formulate precisely. But it is of course necessary if he expects to extend his descriptive system to behavior in general.

13. This has been frequently noted.

14. See, for example, "Are Theories of Learning Necessary?", *op. cit.*, p. 199. Elsewhere, he suggests that the term *learning* be restricted to complex situations, but these are not characterized.

15. "A child acquires verbal behavior when relatively unpatterned vocalizations, selectively reinforced, gradually assume forms which produce appropriate consequences in a given verbal community" (31). "Differential reinforcement shapes up all verbal forms, and when a prior stimulus enters into the contingency, reinforcement is responsible for its resulting control. . . . The availability of behavior, its probability or strength, depends on whether reinforcements *continue* in effect and according to what schedules" (203-4); elsewhere, frequently.

16. Talk of schedules of reinforcement here is entirely pointless. How are we to decide, for example, according to what schedules covert reinforcement is *arranged*, as in thinking or verbal fantasy, or what the scheduling is of such factors as silence, speech, and appropriate future reactions to communicated information?

46. E.g., what are in fact the actual units of verbal behavior? Under what conditions will a physical event capture the attention (be a stimulus) or be a reinforcer? How do we decide what stimuli are in "control" in a specific case? When are stimuli "similar"? And so on. (It is not interesting to be told, e.g., that we

say *Stop* to an automobile or billiard ball because they are sufficiently similar to reinforcing people [46].)

The use of unanalyzed notions like *similar* and *generalization* is particularly disturbing, since it indicates an apparent lack of interest in every significant aspect of the learning or the use of language in new situations. No one has ever doubted that in some sense, language is learned by generalization, or that novel utterances and situations are in some way similar to familiar ones. The only matter of serious interest is the specific "similarity." Skinner has, apparently, no interest in this. Keller and Schoenfeld, *op. cit.*, proceed to incorporate these notions (which they identify) into their Skinnerian "modern objective psychology" by defining two stimuli to be similar when "we make the same sort of *response* to them" (124; but when are responses of the "same sort"?). They do not seem to notice that this definition converts their "principle of generalization" (116), under any reasonable interpretation of this, into a tautology. It is obvious that such a definition will not be of much help in the study of language learning or construction of new responses in appropriate situations.

47. "The Problem of Serial Order in Behavior," in L. A. Jeffress, ed., *Hixon Symposium on Cerebral Mechanisms in Behavior* (New York: John Wiley & Sons Inc., 1951). Reprinted in F. A. Beach, D. O. Hebb, C. T. Morgan, H. W. Nissen, eds., *The Neuropsychology of Lashley* (New York: McGraw-Hill Book Company, 1960). Page references are to the latter.

48. There is nothing essentially mysterious about this. Complex innate behavior patterns and innate "tendencies to learn in specific ways" have been carefully studied in lower organisms. Many psychologists have been inclined to believe that such biological structure will not have an important effect on acquisition of complex behavior in higher organisms, but I have not been able to find any serious justification for this attitude. Some recent studies have stressed the necessity for carefully analyzing the strategies available to the organism, regarded as a complex "information-processing system" (cf. J. S. Bruner, J. J. Goodnow, and G. A. Austin, *A Study of Thinking* [New York, 1956]; A. Newell, J. C. Shaw, and H. A. Simon, "Elements of a Theory of Human Problem Solving," *Psych. Rev.*, 65 [1958], 151-66), if anything significant is to be said about the character of human learning. These may be largely innate, or developed by early learning processes about which very little is yet known. (But see Harlow, "The Formation of Learning Sets," *Psych. Rev.*, 56 (1949), 51-65, and many later papers, where striking shifts in the character of learning are shown as a result of early training; also D. O. Hebb, *Organization of Behavior*, 109 ff.). They are undoubtedly quite complex. Cf. Lenneberg, *op. cit.*, and R. B. Lees, review of N. Chomsky's *Syntactic Structures* in *Language*, 33 (1957), 406f, for discussion of the topics mentioned in this section.