JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 1970, 13, 83-99 NUMBER 1 (JANUARY)

ON CHOMSKY'S REVIEW OF SKINNER'S VERBAL BEHAVIOR

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Skinner's book, *Verbal Behavior*, was published in 1957. Chomsky's review of it appeared in 1959. By the criterion of seminal influence in generating controversy and stimulating publication, both must be counted major successes, although the reputation and influence of the review are more widely acknowledged. It has been reprinted at least three times (The Bobbs-Merrill Reprint Series in the Social Sciences, No. A-34; Fodor and Katz, 1964; Jakobovits and Miron, 1967), and Chomsky has recently written (in Jakobovits and Miron, 1967, p. 142) that he would take back little of it if he were rewriting it now.

Skinner's *Verbal Behavior* is an analysis of speech in terms of its "controlling relations" which include the speaker's current motivational state, his current stimulus circumstances, his past reinforcements, and his genetic constitution. Skinner has accepted the constraints of natural science in his basic analytical apparatus in that all of its terms are empirically definable. He intends to account only for the objective dimensions of verbal behavior and to invoke only objective, nonmentalistic and nonhypothetical entities to account for it. The notion of control, anathema to the politically oversensitive, means only "causation" in its purely functional sense, and need not alarm. It is not arguable nor criticizable that behavior is an orderly, controlled datum, sensitive to the circumstances of the behaver; this is simply a fact which has been amply confirmed.

Chomsky's review was, to put it mildly, displeased. It was also a virtuoso performance whose echoes are still reverberating in psychology and whose dust has still not settled after 10 years. It has two parts. The first is an extended criticism of the basic analytical apparatus which Skinner brought to verbal behavior. So much occupies over one-half of the lengthy paper; the second part is a brief, actually rather casual, criticism of the application itself, as if the demolition of the basic explanatory apparatus had made serious discussion of its relevance to verbal behavior superfluous.

The fact that the review has never been systematically replied to (although partial replies have appeared in Wiest, 1967 and Katahn and Koplin, 1968) has become the basis for an apparently wide-spread conclusion that it is in fact unanswerable, and that its criticisms are therefore essentially valid, a belief which Chomsky shares (Jakobovits and Miron, 1967, p. 142). There are, in truth, several sufficient reasons for the lack of rejoinder and none of them have anything to do with the merits of either Chomsky's or Skinner's case. First, because not all S-R psychologists are sympathetic to Skinner's version many of them felt themselves out of Chomsky's range and were not moved to defend themselves *or* Skinner. This is somewhat ingenuous of them, however, since Chomsky's actual target is only about one-half Skinner, with the rest a mixture of odds and ends of other behaviorisms and some other fancies of vague origin. No behaviorist escaped untouched. On the other hand, most Skinnerians correctly concluded that *their* behaviorism was not particularly the focus of the review, much of which they frankly did not understand. For example, the review devotes six utterly bewildering pages (Chomsky, 1959, pp. 39-44) to yet another refutation (they must

[1]I am greatly indebted to Professor Stephen Winokur who read an earlier version of this paper and made many valuable suggestions.

[2]Preparation of this paper was supported in part by grants to the University of Minnesota Center for Research in Human Learning from the National Science Foundation (GS-1761) and the National Institute of Child Health and Human Development (HD-01136) and the Graduate School of the University of Minnesota. Reprints may be obtained from the author, Department of Psychology, University of Minnesota, Elliott Hall, Minneapolis, Minnesota 55455.

number now in the hundreds) of the drive-reduction theory of reinforcement, which has long since disappeared from everyone's behaviorism, I believe, and which *never* characterized Skinner's (Wiest, 1967, makes the same observation). Finally, and it must be said, probably the strongest reason why no one has replied to the review is its tone. It is ungenerous to a fault; condescending, unforgiving, obtuse, and ill-humored. For example, the perfectly well-defined word "response" is consistently called a "notion" which creates, in time, an overwhelming atmosphere of dubiety with respect to the word. The review's one kind word is in a footnote (Chomsky, 1959, p. 32). It is almost impossible to reply to whatever substantive points the review might have made without at the same time sounding either defensive and apologetic, or as truculent as the reviewer. I have hesitated until now because I am an editor for the Series in which *Verbal Behavior* was published. *Caveat lector.* I believe that the review is, in fact, quite answerable. In spite of its length it is highly redundant; nearly all of Chomsky's seeming cornucopia of criticisms of Skinner's basic behaviorism reduce in fact to only three, which can be addressed in finite, if necessarily somewhat extended, space and time, and one can avoid the provocation to an *ad hominem* reply. This discussion will be organized about these three points, followed by a very brief comment concerning Chomsky's criticisms of the application to verbal behavior *per se.*

The reader should realize in advance that there were and are no directly relevant facts to be brought to bear in this discussion. Although his thesis is empirical, Skinner's book has no experimental data involving the laboratory manipulation of verbal responses which definitively demonstrate that the processes he invokes to explain verbal behavior are in fact involved in its production, although reinforcement has been shown to be effective in controlling verbal responses (Baer and Sherman, 1964; Brigham and Sherman, 1968; Holz and Azrin, 1966; Krasner, 1958; Lovaas, Berberich, Perloff, and Schaeffer, 1966; Salzinger, 1959; Salzinger, Feldman, Cowan, and Salzinger, 1965). Chomsky had no data to disprove the thesis of *Verbal Behavior,* nor does he yet. This can be said in the face of rather frequent statements subsequent to the review which assert, for example, that "Chomsky's paper shows that verbal behavior cannot be accounted for by Skinner's form of functional analysis (Fodor and Katz, 1964, p. 546)." Chomsky *showed* no such thing; he merely asserted it. Chomsky's criticisms of Skinner are, then, necessarily methodological. The disagreement is fundamentally an epistemological one, a "paradigm clash" as Katahn and Koplin have put it (Katahn and Koplin, 1968). It is therefore most peculiar that Chomsky nowhere refers to Skinner's earlier book, *Science and Human Behavior* (Skinner, 1953), the source to which Skinner specifically sends the reader of *Verbal Behavior* for elaboration of general methodological matters (Skinner, 1957, pp. 11, 23, 130, 145, *et seq.*). It may be seen there, and in *Cumulative Record* (Skinner, 1959, 1961), that Skinner has never been reticent about his methodological convictions nor vague as to his reasons for maintaining them. By omitting all reference to these arguments Chomsky creates the highly erroneous impression that Skinner has innocently and impulsively blundered along unmindful of the difficulties inherent in what he was doing. This simply is not so. His application of the basic operant model to verbal behavior has been evolving since 1934 (Skinner, 1957, vii). It has survived explication, and criticism by informed but not universally convinced students, in the classroom intermittently since then, and in the William James Lectures at Harvard in 1947. The 1957 book is, then, hardly the result of a momentary enthusiasm. It deserves a more thoughtful hearing.

In what follows I shall consider Chomsky's three basic methodological criticisms in turn and compare each with what Skinner in fact said. The reader should understand that the italicized statements of *Criticism* are nowhere explicit in Chomsky's review, which merely adumbrates them.

Criticism 1: Verbal Behavior *Is an Untested Hypothesis Which Has, Therefore, No Claim upon Our Credibility*

Neither Skinner nor Chomsky uses the word "hypothesis" to characterize *Verbal Behavior,* but it is one, in fact. Skinner avoids the word but is perfectly clear about what he is up to: "The emphasis [in *Verbal Behavior*] is upon an orderly arrangement of well-known facts, in accordance with a formulation of behavior derived from an experimental analysis of a more rigorous sort. The present extension to verbal

behavior is thus an exercise in interpretation rather than a quantitative extrapolation of rigorous experimental results (Skinner, 1957, p. 11)." And that, of course, is a hypothesis. The data to be accounted for are readily available. As Skinner says: "The basic facts to be analyzed [verbal behavior] are well known to every educated person and do not need to be substantiated statistically or experimentally at the level of rigor here attempted (Skinner, 1957, p. 11)." The explanatory apparatus he invokes does indeed differ from that in most psychological hypotheses since it does not contain any fictional or hypothetical events or mechanisms, being composed instead of well-verified laws of behavior based upon observation of non-verbal organisms emitting non-verbal responses. The hypothesis of *Verbal Behavior* is simply that the facts of verbal behavior are in the domain of the facts from which the system has been constructed. Skinner's stratagem is to find plausible referents in the speech episode for the laws and terms in his explanatory system: stimulus, response, reinforcement, and motivation. The relevance of these laws and their component variables for the verbal events is hypothesized only; it is not dogmatically claimed (Chomsky, 1959, p. 43). The hypothesis may prove to be wrong, but our antecedent confidence in its correctness is at least enhanced by the fact that the basic laws which it invokes have become very sophisticated and impressively well-researched (see Honig, 1966). They have also been shown to be "surprisingly free of species restrictions. Recent work has shown that the methods can be extended to human behavior without serious modification (Skinner, 1957, p. 3)." Skinner also makes the cogent point elsewhere that "It would be rash to assert at this point that there is no essential difference between human behavior and the behavior of lower species; but until an attempt has been made to deal with both in the same terms, it would be equally rash to assert that there is (Skinner, 1953, p. 38)." *Verbal Behavior* is such an attempt for the case of speech.

Skinner's reasons for avoiding the word "hypothesis" in this connection can only be guessed. Psychologists readily confuse "hypothesis" with "hypothetical" in the sense of "fictional", and it is a strong point in Skinner's hypothesis that it contains no reference to fictional causal entities. All of the events, processes, and mechanisms invoked are themselves empirical, and therefore the hypothesis containing them is in principle fully testable and possibly disconfirmable. A more potent reason for his avoiding the word, however, is probably that "hypothesis" has somewhat curiously come to imply the possibility of *experimental* test, which Skinner has not performed and which he does not seem to consider feasible, although *Verbal Behavior* is rich in observational evidence. According to his hypothesis speech is the product of the convergence of many concurrent and interacting variables in the natural environment, which does not sustain the experimental separation and detection of the relevant component variables. Yet anything less than concurrence and interaction of many variables would not, according to the hypothesis, generate speech. Skinner's situation resembles that of the astronomer "explaining" tides as the resultants of many interacting attractions. No one has ever experimentally tested that hypothesis directly either, yet it is highly plausible and supported by much observational evidence which is probably the strongest conclusion we shall ever be able to make for it.

Chomsky avoids the word "hypothesis" in favor of more picturesque terms: "[Skinner] utilizes the experimental results [of laboratory studies of infra-human, non-verbal behavior] 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 . . . 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 . . . with a metaphoric reading, it is no more scientific than the traditional approaches to this subject matter . . . (Chomsky, 1959, pp. 30-31. Italics added)." Which is really only to say that the technical language of Skinner's system is used in a hypothesis about verbal behavior; *all* scientific terms in untested hypotheses are necessarily "metaphoric extensions" and "analogic guesses".

What is puzzling, therefore, is the pejorative aspect which "metaphor" and "analogic" assume in the passage quoted.

Even more puzzling is the giddy speed with which the argument moves from its insight that the terms in the hypothesis are for now metaphoric and analogic, proceeds to the possibility that this *may* prove to be all they are, and concludes flatly with the verdict that the technical terms used *do not* describe verbal behavior. This goes too fast! *That* remains to be seen. Until the hypothesis is tested the literal (non-metaphoric, non-analogic) applicability of its explanatory terms remains in doubt, at worst. Chomsky's only real argument for his conclusion that the terms of the theory do *not* in fact apply to verbal behavior is given in the quotation above. It depends upon the amazing possibility that "real-life" and laboratory behavior *may* be different, as if somehow nature maintains two sets of natural laws, one for laboratories and the other for the rest of the world so that any law observed in the laboratory is *prima facie* suspect when applied to events outside. Entrancing though this idea is, it seems unparsimonious to suppose it. That really does not sound like nature.

The fact is simply that we do not yet know if verbal behavior is within the domain of Skinner's system and whether the technical terms stimulus, response, reinforcement are literally applicable to verbal behavior and correctly parse it into its functional parts of speech.

Chomsky raises special considerations for doubting that each particular term of the basic theory applies to the verbal case. These will be briefly noted.

The stimulus: Chomsky holds Skinner severely accountable for hypothesizing certain stimulus-response relations in *Verbal Behavior,* such as "a piece of music" as a stimulus for the response "Mozart", or a certain painting for "Dutch", and a red chair for "red" or "chair". "Since properties are free for the asking, 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." He then goes on to say: "Stimuli are no longer part of the outside physical world; they are driven back into the organism (Chomsky, 1959, p. 32)." This is a *non sequitur.* Stimuli are "free for the asking" only in hypotheses. Their *quid pro quo* is payable in empirical demonstrations of the evoking power of the putative stimuli. None of the purported stimuli listed above seems outrageously improbable for those responses, and not until such an empirical test of their evocative control has failed is anyone entitled to conclude that these are *not* stimuli for those responses. Chomsky's conclusion that a putative stimulus has lost its objectivity because it occurs in a hypothesis is merely muddled. Skinner did not hypothesize a (hypothetical) stimulus. The stimulus is as real as ever. He hypothesized that there is a controlling relation between the real stimulus and the real response. As for his conclusion that the stimulus in a hypothesized stimulus-response relation has somehow been "driven back into the organism", the rationale is harder to reconstruct. Reading Chomsky on the subject of the stimulus here and elsewhere in his review arouses a growing suspicion that he imagines that by naming one stimulus for a verbal response we name its only stimulus, and that one stimulus somehow preempts a response. He criticizes Skinner's characterization of the responses "Eisenhower" and "Moscow" as proper names, controlled by the man or the city, because one frequently says "Eisenhower" and "Moscow" when the man and the city are not present (Chomsky, 1959, p. 32). Indeed one does, but this only shows, as *Verbal Behavior* repeatedly and clearly insists, that a verbal response may be controlled by different stimuli on different occasions. Verbal behavior does not obey any "one response-one stimulus" rule and it makes no sense to speak of *the* stimulus for anything. "Eisenhower" and "Moscow" are said for many reasons, among which are the presence of the man and the city. Perhaps Chomsky's conclusion that Skinner's stimuli for verbal responses have receded into the mind of the speaker is based upon this point: if I say "Eisenhower" when there is no Eisenhower then he must be in my mind. Is that the difficulty? Only if one is misguidedly determined to preserve Eisenhower as the only stimulus for *Eisenhower.* It is really impossible to be sure. However clear it is in its conclusions, the review is not much help on matters of rationale.

Reinforcement. Inevitably Chomsky finds Skinner's functional definition of a reinforcer unsatisfactory (that it increases the strength of any operant which precedes it), saying that it

is "perfectly useless . . . in the discussion of real-life [*sic*] behavior, unless we can somehow characterize the stimuli which are reinforcing . . . (Chomsky, 1959, p. 36)." He is complaining because reinforcers can only be postdicted from the fact of reinforcement, since they cannot be "characterized" in terms of any universal, independently knowable correlated property such as drive-reducing power. Many psychologists share this dissatisfaction. But the fault, if any, is in nature, not in our theories. Reinforcers seem in fact to have only one universal property: they reinforce, and no amount of dissatisfaction will either add a correlated property nor disprove the fact that they do reinforce.

To be quite correct, whether a specific stimulus will be reinforcing for the behavior of any specific organism *can* be predicted without actually trying it. That is, reinforcers can be predicted, since all reinforcers are either species-characteristic (the unconditioned reinforcers) or they have, in the history of the behaver, been paired with an unconditioned reinforcer (the conditioned reinforcers). Both of these classes are knowable before any behavioral test of their effect upon behavior is made (although it is technically infeasible to enumerate the members of the second class in the human case.) Furthermore, as Premack's data have shown, all reinforcing stimuli are at least partially transituational; they will reinforce any operant whose initial probability is less than the consummatory or preconsummatory behavior which the reinforcing stimulus itself occasions. Therefore, a prediction of future reinforcing effect must be made given a fact of past reinforcing effect for any stimulus as well as information concerning the momentary probabilities of the operant to be reinforced and the behavior occasioned by the reinforcer. These considerations, in addition to providing bases for prediction as to which stimuli will reinforce which responses, also act as constraints upon the illicit invocation of *ad hoc* reinforcers. Together they remove the concept of reinforcement from "perfect uselessness". Reinforcement is a real and powerful behavioral influence. Its inclusion in a theory of verbal behavior is decided on the basis of its own claim; it becomes a necessity whether it is "useful" in analyzing an instance of casual conversation or not.

Chomsky seems convinced that Skinner *claims* that "slow and careful" reinforcement applied with "meticulous care" is *necessary* for the acquisition and maintenance of verbal behavior (Chomsky, 1959, pp. 39, 42 [twice], 43). Chomsky does not cite *Verbal Behavior* in this context, and the fact is that Skinner does not say or imply that the reinforcement for verbal behavior must be carefully arranged or that differential reinforcement must be "careful", applied with "meticulous care", and "slow and careful" (Chomsky, 1959, p. 42). The idea is preposterous and the implication that Skinner said it is both careless and false.

Skinner does not, in fact, explicitly claim that any reinforcement is necessary for verbal behavior, although Chomsky supposes he does (Chomsky, 1959, pp. 36, 37, 38). His references are to statements in *Verbal Behavior* which say no such thing, and to Miller and Dollard (1941), who may. Skinner *does* claim that reinforcement is a potent influence upon verbal behavior, and, in fairness, he specifies no other strengthening operation for it. Nothing whatever is at stake in excluding from the hypothesis such alternative response-strengthening mechanisms as learning by imitation or by latent (non-reinforced) learning, *if* these should become demonstrable. The system would not then be destroyed or disproved; it would simply be supplemented by laws which specify the conditions under which these processes occur. Chomsky suggests that it is well-known that much language learning in children proceeds by imitation (Chomsky, 1959, p. 43). So, in fact, does Skinner (1957, pp. 55-65) but he further specifies that the imitative repertoire (which he calls echoic in the verbal case) is itself a product of reinforcement. The evidence for an innate imitative tendency is very weak, so that the problem as Skinner saw it was to explain echoism when it does occur, and to account for the facts that the imitative tendency gradually restricts itself to the small segment of the vocal spectrum which the parent language uses, that its flexibility disappears with age, and that the echoic repertoire contains quite different dimensions in different speech communities (such as pitch in some, and not in others). These are all consistent with a reinforcement interpretation of the echoic's origins.

As for latent (unreinforced) learning, it is certainly incorrect to conclude that "Few investigators still doubt the existence of the phe-

nomenon (Chomsky, 1959, p. 39)." The many studies which Chomsky cites in support of the existence of latent learning revealed mostly that the methodological problems involved in a crucial experiment on that question are overwhelming. The matter was not resolved. It was dropped.

Probability. Chomsky criticizes Skinner's " 'extrapolation' of the notion [*sic*] of probability" as being, "in effect, nothing more than a decision to use the word 'probability' (Chomsky, 1959, p. 35)." This is the same objection that has been made to "stimulus" and "reinforcer", *i.e.*, the word occurs in a hypothesis, and therefore we need not reconstruct the argument on either side. Chomsky says, in addition, that "The term 'probability' has some rather obscure meaning for Skinner in this book (Chomsky, 1959, p. 34)." Small wonder, since he cites (Chomsky, 1959, pp. 29, 34) *Hull's* definition of probability (resistance to extinction) as Skinner's basic indicator of probability or "strength" rather than Skinner's, which is simply the likelihood of occurrence of a response, measured as a rate where possible, but as a relative frequency in any case. Skinner thus defines probability quite as any other natural scientist does. Much more ominously for Skinner's purposes, Chomsky seems not to grasp the difference between the overall probability of occurrence of an item in a speaker's verbal repertoire, which is the frequency with which it occurs in his speech over time without regard to his momentary circumstances, and the momentary probability of a given response in some specified set of circumstances. (See, for example, Chomsky, 1959, p. 34.) The two probabilities are very different. The overall probability that any speaker will say, for example, "mulct", is very low; it occurs rarely in comparison with such responses as "the" or "of". The probability that he will say "mulct" may become *momentarily* extremely high, as when he sees the printed word. Of the two, overall probability is a typically linguistic concern, while momentary probability shifts are, in a sense, the very heart of the psychologists' problem, since they reflect the relation between speech and its controlling variables. Under what conditions does an organism speak an item from his repertoire? Simply knowing the repertoire tells us precisely nothing about that. If Chomsky really did not, in fact, see this difference it is impossible to imagine what the rest of *Verbal Behavior* could have meant to him, and no wonder that he regarded it with such astonishment and dismay.

Verbal behavior's momentary probabilities are difficult to assess in practice because the most sensitive experimental indicator in nonverbal research, rate, is not useful: strong verbal responses are not normally repeated several times. Skinner mentions some production effects which on occasion may reflect the strength of a non-repeated, single utterance, such as loudness, speed of production, or repetition if it does occur. Skinner says quickly and explicitly of these, however, that they are untrustworthy: "It is easy to overestimate the significance of these indicators (Skinner, 1959, p. 25; additional warnings are given on pp. 27 and 141)." It is somewhat shocking, therefore, that in spite of Skinner's disclaimers Chomsky imputes to his hypothesis an entailment that a strong response must be "shrieked (Chomsky, 1959, p. 35)" or shouted "frequently and in a high-pitched voice (Chomsky, 1959, p. 52)."

So much for the emigration of the system out of the laboratory. Chomsky faults the argument because it did so.

Criticism 2: Skinner's Technical Terms Are Mere Paraphrases for More Traditional Treatments of Verbal Behavior

This point is at very high strength in Chomsky's review. It is made in turn for Skinner's terms "stimulus" (Chomsky, 1959, pp. 32, 33, 48, 50), for "deprivation" (Chomsky, 1959, pp. 46, 47), "reinforcement" (Chomsky, 1959, p. 38) and for "probability" (Chomsky, 1959, p. 35).

As Chomsky's criticisms somewhat tend to, this one has several quite independent facets. First is a sort of premise that the technical Skinnerian vocabulary simply renames an old notion in a new but more prestigious way. I believe this is obviously quite false. Second is a conclusion that, being a paraphrase, the technical term is *therefore* no more objective than its traditional counterpart, which I believe is neither a consequence of the first premise nor correct. Both notions thread through the following sentences from the review, though they may also be found in other examples: "His analysis is fundamentally the same as the traditional one, though much less carefully phrased. In particular, it differs only by indiscriminate paraphrase of such notions as deno-

tation (reference) and connotation (meaning), which have been kept clearly apart in traditional formulations, in terms of the vague concept 'stimulus control' (Chomsky, 1959, p. 48)." Let us see what can be done.

Although Skinner did not do so, it probably would be a service if a scientific and technical paraphrase were given for such traditional mentalisms as "refer", "denote", "meaning", "wanting", "liking", and so forth, each of which Chomsky says Skinner has in fact packed into one or another of his technical terms. To do so, one would start with a traditional term, "refer", for example, and give a functional account of the conditions which control its occurrences (roughly, its "use"). If one were to do so, however, he would quickly discover that not all instances of what we indiscriminately call "referring" involve anything like the same functional controlling relations, and one therefore can find no consistent paraphrase among the terms in a functional account for the reference notion. As we have seen, one may "refer" to Eisenhower *wherever* he is in relation to the speaker, but while the reference relation between the response and the man remains thus constant, the control of the response may vary among such stimuli as the physical presence of the man himself, or his picture, or his printed name, or his name spoken by another, or some other verbal stimulus such as "Ike" or "Mamie's husband". In only one instance is the controlling stimulus for the response also the person referred to. The remaining stimuli control the response but the response does not refer to them.

Reference and stimulation also differ diametrically in their direction of influence: a stimulus acts from the environment upon the speaker to control his verbal behavior, while in reference the speaker's response acts upon the environment to single out its stimulus components. An analogy is with the ancient theory of vision which supposed that vapors emanate from the eye to contact the environment as in the dynamics of reference, as opposed to the modern view that objects are seen when light from them controls the eye, as in the stimulus role. To complete the full catalog of nonequivalence, we must only note that many verbal responses which are controlled by stimuli have no referents whatsoever (try "Oh damn!") and also that the concept of "stimulus control" involves causality, which is not involved in "reference". Reference is simply a relation between the world and an item in the language (as opposed to an item in a speaker's actual behavior, the distinction which persists in eluding Chomsky).

In brief, no one technical term in Skinner's causal verbal analysis covers all instances of reference (nor was any intended to), and the one term *stimulus control* covers much that is not reference in the traditional sense. The same argument can be made for the nonequivalence of the other terms, deprivation, reinforcement, and probability, to other more traditional terms; if these are simply paraphrases, they do not map unequivocally and isomorphically, term for term, into each other. Curiously, Chomsky seems to sense this too, and so criticizes the behavioristic paraphrase for blurring the traditional concepts! Given all this, it seems quite obvious that the term "paraphrase" is simply inappropriate in this context. Skinner's analysis is no more a paraphrase of linguistic-philosophical mentalisms than modern physics is a paraphrase of pantheism. They merely converge, but from quite different directions and with quite different credentials, upon some aspects of the same domains.

Whether it is a "mere" paraphrase of the traditional account or not, Skinner's analysis is far more objective and less vague than the traditional one and therefore scientifically preferable. Every term in Skinner's account names some real thing which must be physically involved and locatable in any verbal event for which it is invoked. *That* is objectivity. If in his hypothesis Skinner invokes a particular stimulus to account for the occurrence of a response, he is saying that at least *some* of the occurrences of that response are due to the physical presence of that particular stimulus. The discovery that the response also occurs at other times does not disprove the facts of stimulus control; it simply means that other controlling variables (usually themselves other stimuli) must be discovered for these occurrences. The notion of control as a relation is itself perfectly objective. To make a similar claim for the objectivity of such terms as "reference" (and "wishing", "wanting", "liking", and so forth) would presuppose first defining them in terms of some physical dimensions. But that would at once be another "mere paraphrase" of these terms in which, if we follow Chomsky, instead of the mentalism's gaining

in objectivity, the defining physical dimensions are doomed, by some logical alchemy, to lose their objectivity. This is an odd thesis.

This particular criticism in Chomsky's review occupies a great deal of its total space and accounts for much of its apparent thrust and its most vivid writing. It is not often that a reviewer becomes so overwrought as to permit himself to characterize his author as entertaining "a serious delusion (Chomsky, 1959, p. 38)." But there it is.

Criticism 3: Speech Is Complex Behavior Whose Understanding and Explanation Require a Complex, Mediational, Neurological-Genetic Theory

Chomsky expresses his surprise at "the particular limitations [Skinner] 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' [*sic*] which, he claims, describes the causation of behavior (Chomsky, 1959, p. 27)." Skinner's basic explanatory system is indeed simple in comparison to the complexity of the domain it is intended to cover. It is not so simple as to reduce to a single function however; it has many variables and at least as many functions. It is customary in scientific analysis to reduce complex phenomena to their component processes, each of which appears simple when defined in isolation by means of the control techniques of the laboratory. In the natural environment (curiously called "real" life by Chomsky) the components recombine and interact to generate properties that none alone fully accounts for. According to the hypothesis in *Verbal Behavior,* one such quasi-emergent property is grammar, of which more later. Suffice to say that a theory of verbal behavior that does not have special grammar-generating laws in it may still be capable of generating outcomes which have grammatical properties. The general relationship of the domain of verbal behavior to general behavior laws is reductionistic; the complex is explained in terms of the simple. A system of simple laws which can generate complex outcomes is said to have scientific elegance. As we move from nonverbal to verbal behavior it is more parsimonious to suppose that nature has not given us a whole new set of behavior laws for just this one aspect of behavior; not even genetic mutations account for that much invention. This supposition that the laws of behavior are thus general enough to account for the verbal case is not a claim that they are sufficient; it is a working assumption that they will prove to be.

It is a curious omission from Chomsky's review, considering that he wonders explicitly at the simplicity of Skinner's account, that he nowhere mentions the possibility that the simple laws which the account contains may act concurrently, and so interact as to modify each other's effects, converging upon a single item of verbal behavior, to make it something which is controlled or contributed by no one of them alone. The omission is all the more peculiar when one discovers that an entire section of *Verbal Behavior* is devoted to elaborating this possibility (Skinner, 1959, Part III: pp. 227-309). References to the possibilities for special effects due to multiple causation begin appearing in *Verbal Behavior* as early as page 42, and follow frequently throughout the rest of the book. A careful reading of the whole book shows that when the whole system is given its due, it is not at all limited to accounting for only simple behavior.

In no area of psychology is the contrast between "simplemindedness" and "muddleheadedness" more poignant and clear than in the case of verbal behavior. The S-R psychologist is indeed at the simpleminded end of things, supposing as he does that verbal behavior can be reduced to its component processes, that these will be simpler functions than speech is, and that they will be familiar. This is the psychology of the nothing-but. In fact, if his analysis does not reveal simple and univocal relationships in a new domain, the S-R psychologist tends to suspect that he has specified the wrong variables on the input side or the wrong dimensions on the output side, and he will try again elsewhere. The alternative to simplemindedness is muddleheadedness, which finds it inconceivable that complexity may be composed of simplicities, and writes off the possibility of simple explanations as "trivial", "very unilluminating" or "not interesting", wanting a theory composed of something-more, and certain that it must be needed. The history of science is probably on the side of simplemindedness. In the case of verbal behavior at present it is the disposition of ignorance which is at issue, as it may always be in a sim-

pleminded-muddleheaded confrontation. Skinner hypothesizes that speech will prove to be like other operant behavior when we understand it, and can be decomposed into component processes. Chomsky finds in its unanalyzed mysteries a justification for presuming causal innovation and complexity. He says: "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 (Chomsky, 1959, p. 30; italics added)." This is a very remarkable statement. The reader is encouraged to contemplate it as a rationale for theory construction; here it is a substitute for knowledge.

Mediational Terms

Skinner's laws are called functional because they describe direct relations between each of the several controlling variables (evoking stimuli, reinforcement, motivational states) and momentary probabilities of different behaviors in an individual's repertoire. That is to say, he does not invoke other events, processes, or mechanisms which are hypothesized or invented for the purpose of mediating between behavior and its empirical determinants. This omission is sometimes misconstrued as a denial that mediating mechanisms exist; they obviously do, they are obviously neurological and they are also obviously themselves lawful (see Skinner, 1953, p. 28; 1957, p. 435). Because they are themselves lawful, these mediating events, processes, and mechanisms generate and maintain lawful functional covariations between the controlling variables of molar behaviorism and the behavior they control. The argument is simplicity itself, and Skinner has made it abundantly available (see especially Skinner, 1959, 1961, pp. 39-69). He considers such theoretical terms unnecessary; they may generate research whose only usefulness is to disconfirm the mediating entity or redefine it without increasing our knowledge of behavior's controlling variables; they can become the absorbing focus of an inquiry and so deflect attention from behavior itself; and they can become a "refuge from the data", as motivation has tended to be in psychology. It is often simply "what varies so as to account for otherwise unaccounted-for variability in behavior."

Chomsky does not refer to Skinner's discussions of why he omits mediational constructs, but he was apparently little affected by them, finding the violation of his own preconceptions a sufficient reason for ignoring them: "One would naturally expect that the 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 information and organizes its own behavior" (Chomsky, 1959, p. 27). Perhaps one would, but he *need* not. It is perfectly feasible and sufficient to know merely *that* the speaker's "internal structure . . . processes information" so as to generate lawful relations between the speaker's circumstances (past and present) and his speech. Unless one is a neurophysiologist it is not necessary in the least to know *how* the internal structure goes about doing so nor which structures are involved. The psychologist's knowing how it does so would not improve the precision of predicting behavior from knowledge of the speaker's circumstances, nor would this knowledge make existing functional laws of behavior any more true, nor could it show them to be untrue. It is simply false, of course, that one cannot accurately predict behavior, even complex behavior, without knowing and taking into account the behaver's structure and internal processes; we do it all the time. In point of fact, our current knowledge of the functional laws of behavior far precedes and outweighs both our knowledge of, and even our theories about, the mediating mechanisms involved. For example, so far as I can tell, the behavioral facts of reinforcement are by now so well-known and dependable that theories of the details of its mediation are no longer of great interest. Where interest in the mediating structures survives, it is behavioral data which illuminate them, not the other way around.

Neurological-Genetic Mediators

Although Chomsky locates the missing mediator variously, now, in the organism's internal *structure*, now in some prebehavioral processing and organizing *activity*, or, sometimes, in deeper grammatical *processes*, it is clear from his most detailed examples that he intends to locate them precisely in the brain, and moreover that he supposed they got there in large part by genetic predetermination or preprogramming.

So far as I can see, he is almost certainly correct on both counts, neither of which has the slightest relevance to the question of the validity of Skinner's hypothesis, although they apparently have some crucial significance to Chomsky however elusive it proves to be when one tries to characterize it.

With regard to neurological mediators in general he says: "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 [*sic*] specifying the response in terms of the history of inputs (Chomsky, 1959, p. 27)." The psychologist finds himself standing here in a strange light, making-do with "the only data available", behavior, but really only marking time until neurology can catch up and give him all of the real explanations of behavior. Aside from its condescension, the facts and the logic in Chomsky's statement are both wrong. The facts are that we are not merely *trying* to "specify" behavior in terms of its past history and current cirmumstances (the "input" referred to), we are doing so, and with increasing accuracy. The (functional) law of reinforcement is an enormously powerful predictive (specifying?) device. At least it is for nonverbal behavior; and no one can say that it is not powerful for verbal behavior too. The logic, as we saw in the previous section, is that a valid functional law can be completely established on the basis of the only data available, and does not need "independent neurophysiological evidence". The functional law of reinforcement, in addition to being powerful, is an established empirical fact. It is not a theory awaiting neurological validation.

The possibility that certain aspects of verbal behavior may be genetically predetermined seems to be loaded with special significance for Chomsky. He appears to draw at least two conclusions from the possibility; one is that if the brain is in fact genetically preprogrammed for such behavior, it becomes all the more obvious that the structure of the brain must be "considered" in the explanation of that behavior. The second is that the fact of genetic predetermination is incompatible with the facts of reinforcement. Are those valid inferences? Consider the first. Recall that Skinner explained imitative verbal behavior as the product of reinforcement for "echoic" responses. Chomsky says of this ". . . however, it is possible that ability to select out of the complex auditory input those features that are phonologically relevant may develop largely independently of reinforcement, through genetically determined maturation." He then goes on to say: "To the extent that this is true, an account of the development and causation of behavior that fails to consider the structure of the organism will provide no understanding of the real processes involved (Chomsky, 1959, p. 44)." One hardly knows where to begin. First, it is not necessary to "consider the structure of the organism" in psychological laws *no matter how* the brain got programmed. There is nothing unique about the logical status of a genetically programmed mediator. So long as the brain is programmed it will maintain lawful covariations between "phonologically relevant stimuli" and echoic behavior, and a functional law referring only to such stimuli and behavior can be written without reference to the brain and its "program". Second, if genetic preprogramming is a characteristic of the "real processes involved" in echoic responding, that fact will be revealed through "considering" the behavior of the organism, not its structure, and will appear normally as a reinforcement parameter. Genetically determined behavior is what does not have to be learned. Although at one level the brain explains behavior, in the tactics of scientific discovery it is behavior that explains the brain. And, yet once more, from the psychologist's point of view the "real" processes involved in echoics are the presentation of a "phonologically relevant stimulus" and the occurrence of an echoic response.

Chomsky's second neurological example appears to say simply that grammatical behavior may be similarly preprogrammed. Its import is more complex than that, however, since some experience is obviously necessary for grammatical behavior in addition to the genetic head start. The example is doubly important because it seems to have been taken very seriously by many psycholinguists (see especially Lenneberg, 1964, 1967). Chomsky said: "As long as we are speculating, we may consider the possibility that the brain has evolved to the point where, given an input of

observed Chinese sentences, it produces (by an 'induction' of apparently fantastic complexity and suddenness) the 'rules' of Chinese grammar, and given an input of observed English sentences, it produces (by, perhaps, exactly the same process of induction) the rules of English grammar; or that given an observed application of a term to certain instances it automatically predicts the extension to a class of complexly related instances. If clearly recognized as such, this speculation is neither unreasonable nor fantastic . . . (Chomsky, 1969, p. 44)." Nor, alas, it is particularly relevant. As we noted in discussing echoic or imitative verbal behavior, the mere fact that the brain has evolved does not force its introduction as a mediator into a functional law.

Neither does the fact that the brain has evolved tell us anything useful about how it "produces" the "rules" of grammar. Whatever that may mean, exactly, there can be no doubt that the human brain has evolved to the point where it has the capacity to mediate the acquisition of grammatical behavior. This says nothing in itself as to whether the acquisition process involves the sort of learning by imitation or observation supposed in the example or, instead, learning by reinforcement. The capacity to learn by imitation or observation is certainly not a peculiarly or uniquely diagnostic symptom of evolutionary advance and, as we have seen, the possibility of some acquisition process other than reinforcement is not itself overwhelming to Skinner's system.

There is no lethal incompatibility or even mild inconsistency between the principles of genetic evolution and the principle of reinforcement. Reinforcement has many necessary points of contact with genetics. Reinforceability is itself a genetically determined characteristic; organisms are simply born reinforceable. They have evolved that way. The fact that organisms behave at all is due to genetic determination. Stimulus generalization and response induction are genetically determined characteristics. The only incompatibility between genetic determination and learning by reinforcement is that if some behavior is wholly genetically determined, as unconditioned reflexes are, then no learning is needed to account for its occurrences. Such behaviors hardly "disprove" reinforcement theory, of course.

Whether, and if any, how much, grammatical behavior is in fact due to genetic predetermination of any great specificity is another matter. Obviously we have not inherited a special set of grammatical neurons, so prespliced as to arrange verbal responses in certain standard orders. At most we may be supposed to have inherited a predisposition to learn grammatical behavior, and to do so in a certain way. The fact, if it is a fact, that there are grammatical universals hardly encourages us to adopt an "inherited grammar nerve-net" hypothesis; if language learners everywhere share a common, somewhat simple, dynamic acquisition mechanism such as reinforcement (which they do) we should expect them to acquire complex behavior repertoires, both verbal and nonverbal, having many properties in common (which they do). Any limitation in behavioral variety suggested by behavior universals may simply reflect a limitation imposed by the reinforcement process and possibly some structural characteristics of the brain such that it can only go about learning to order verbal responses in a distinctly limited number of ways, due, indeed, to the simplicity of the reinforcement process and the fixity of the brain.

The fact that some, but by *no* means all, children acquire grammatical behavior at a rather early age and rather suddenly (Chomsky finds its rapidity "fantastic") does not require a previously laid-down inherited grammatical nerve net nor, even, anything much in the way of a strong genetic prepotency for grammar learning. As we have seen, nothing about the reinforcement process *per se* requires it to be slow and painstaking as Chomsky so insistently asserts it does (Chomsky, 1957, pp. 39, 42). Simple responses appear, in fact, to condition in one reinforcement even in lower organisms, and the child is not a lower organism. The dynamics but not the parameter values of the child's reinforcement processes will resemble those of the pigeon. The applicability of the law to the child is not in question merely because the process proceeds at a more rapid rate.

That a child learns certain orders, such as adjective-noun, and actor-action sequences, on the basis of a relatively small sampling from the enormous universe of such instances shows simply that a child is able to make complex abstractions and to generalize from them to diverse new instances. A parameter value may surprise us, but it does not prove that the proc-

esses of stimulus generalization and response induction are not applicable.

In brief, Skinner's hypothesis concerns how whatever grammar acquisition genetic predetermination leaves remaining to be done is in fact done. The two sorts of determiners are complementary, not antagonistic. On the contrary. It is distinctly inconsistent to argue that while we may have inherited a disposition to grammatical behavior, we could not have learned it by reinforcement. Both evolution and reinforcement theory provide that what survives behaviorally is what increases survival chances, or, roughly, what reinforces.

Grammatical Behavior

Chomsky's discussions of the controlling variables for grammatical behavior suggest that he views the necessity for postulating a mediational mechanism for this particular aspect of speech as especially acute and, apparently, obvious. He says, for example, that "The child who learns a language has in some sense constructed the grammar for himself . . . this grammar is of an extremely complex and abstract character, . . . 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 (Chomsky, 1959, p. 57)." This "grammar" is, then, a theory or, sometimes, "rules" and more recently "competence". It is a *thing* which the child, and later the adult, *has* and *uses*. It reveals itself in two ways: as an understanding device when its possessor listens, and as a generating device when he speaks. As a consequence the word grammar is used in any number of ways. It is the name of the competence or rules or theory which the speaker has constructed or inherited or learned; it is the name of a perceptual property of stimulus sentences he hears or reads; and it is a property of his behavior when he actually speaks himself. The first actually underlies the second two and mediates both indifferently.

As it functions during listening, the grammar construction receives input in the form of heard or read stimulus sentences, which it goes to work upon so as to "distinguish sentences from nonsentences, to understand new sentences (in part), to note certain ambiguities, *etc.* (Chomsky, 1959, p. 56)." Presumably it also communicates its verdicts to the rest of the person somehow, although what interest he could have in a steady stream of diagnoses of sentencehood and nonsentencehood is somewhat puzzling. At any rate the activity involved is readily recognizable simply as stimulus discrimination. There is nothing in that to tax an S-R analysis, and nothing to force us to hypothesize an underlying theoretical construction. Nearly any set of diverse stimulus objects, including the sentences one hears, can be assorted into classes or subsets having some property in common and differing from other subsets in some property. In this sense hearers probably do learn to discriminate sentence types, but there is nothing unique to grammatical stimuli in these discriminations; they do not require any special, separate perceptual mechanism or innovative perceptual process on the part of the hearer. Sentence discrimination, becoming highly sophisticated in linguistics, probably exhausts the empirical basis from which inferences about the structure of a speaker's underlying grammar construction can be made. That is, our knowledge of any speaker's "competence" will necessarily be a product of our perception of his actual emitted sentences, plus some empirically surplus inferences. It will be a highly discriminative and abstract account of what he says, but it is speechbound and takes nothing else into account.

But what can speech alone tell us about actual causes of speech, including its grammatical determiners? Nothing unequivocal; the fact that there is a stimulus dimension in an individual's observed verbal behavior identifiable as "grammar" by no means entails that there is any unique causal variable called "grammar" at work in the production of his verbal behavior. A simple causal system having no pattern axioms at all may generate highly patterned outcomes, with the pattern becoming represented as such only in the outcome, although, one hopes, predictable from an understanding of the components and interactions of the nonpattern causal variables.

So at least Skinner conceptualizes the autoclitic processes, defined in *Verbal Behavior* as "verbal behavior which is dependent upon or based upon other verbal behavior (Skinner, 1956, p. 315)." The formulation is abstruse and difficult and it takes some getting used to. It is certainly the most complex part of Skinner's hypothesis, although its complexities inhere in the interaction it supposes exist among

what are rather simple component processes. According to the formulation, speech may begin when the speaker has something to say, a disposition to respond due to his current stimulus and motivational circumstances. This "primary" speech is fragmentary, in that it does not include purely syntactic forms; it is unordered, in that many responses are concurrently available, and it has no grammar. Given something to say, the speaker can then respond autoclitically to aspects of it, specifically to its strength and origins, by ordering and commenting upon it as it appears in his speech. In terms of Skinner's analysis, such behavior is simply a complex kind of tact. The tact itself, is, however, not generically a grammatical process at all, and it includes much that is not grammar. The grammar does not come first, then, the elements of speech do. These instigate speech in which grammar emerges as the way these elements literally arrange themselves.

Chomsky's comment upon this hypothesis is modestly placed in a footnote (Chomsky, 1959, ftn. 45, p. 54) which says: "One might just as well argue exactly the opposite is true", and no doubt at this stage of knowledge one might. And so Chomsky does, supposing, after Lashley (1951), that syntactic structure is "a generalized pattern imposed upon the specific acts as they occur (Chomsky, 1959, p. 55)." Thus, grammar is said to preexist outside verbal behavior and exert a causal influence upon it. Lashley's conclusion was based upon, and is relevant only to, an S-R analysis of grammatical ordering which hypothesizes that grammatical behavior is a result of a left-to-right process of intraverbal chaining. But Skinner's autoclitic hypothesis involves no left-to-right intraverbal chaining. It very adroitly (and almost certainly in full knowledge of Lashley's paper) puts the necessary controlling variables in the interrelationships among the fragmentary "primary" verbal responses which are simultaneously, not serially, available to the speaker.

Skinner accounts for the instigation and determination of verbal behavior, grammar and all, in terms of variables external to the speech episode itself, with a secondary, autoclitic step added once instigation is under way. Chomsky is totally silent, on the other hand, about what might be the form of input which would similarly engage the grammar construct when speech is to be produced, and tell it what to be grammatical about, and how to select a possible transformation to say it in, and so forth. So far as one can tell, Chomsky's one controlling variable for speech production—grammar, rules, competence—rests locked away in the brain somewhere, inert and entirely isolated from any input variables which could ever get it to say something. Unless some external input is permitted one must suppose that the grammar construct regulates itself, a repugnant notion. No one speaks pure grammar. All sentences have grammatically irrelevant properties; they are, in addition, about something. Chomsky elsewhere in the review very firmly rules out control by stimulus and motivational variables, as we have seen. One waits with bated breath to see what is left. The behavior of the grammar construct must now be explained. Until it is we are no further along than we were without it. It is simply "that which controls grammatical behavior". But that, of course, is the question, not the answer. The speaker's cognitions will not do, since they too are theoretical constructions and must in turn be explained. Sooner or later something must *enter* the system. Guthrie complained that Tolman had left the rat "lost in thought" because he provided no relation between the expectancy and behavior. Chomsky leaves the speaker lost in thought with nothing whatever to say.

In sum, the verbally competent person can discriminate a syntactic dimension in speech as a stimulus, and he can emit speech which has syntactic properties in the sense that a hearer can discriminate them. This does not prove in any way that some underlying theory governs both behaviors. A child learns both to walk and to discriminate walking. Nothing is gained by saying that therefore he has constructed a theory of walking which he uses in his perceptions and in his activities. So he may be conceived to learn to speak and to perceive speech, directly and without stopping to construct a theory or apply a rule.

The Extension to Verbal Behavior

Chomsky's criticisms focus principally upon Skinner's basic systematic apparatus, rather than its application to verbal behavior. That he feels there is relatively little left to say is revealed in his introduction to his discussion of the application itself: "Since this system is

based on the notions [*sic*] 'stimulus', 'response', and 'reinforcement', we can conclude . . . that it will be vague and arbitrary (Chomsky, 1959, p. 44)." His treatments of the mand, tact, echoic, and so forth are therefore brief and add little new in the way of specific criticism. A few details which have mostly to do with misinterpretation of psychological fact or misreading of Skinner's text should be noted however.

The Mand

In Skinner's definition, a mand is a "verbal operant in which the response is reinforced by a characteristic consequence and is therefore under the functional control of relevant conditions of deprivation or aversive stimulation (Skinner, 1957, pp. 35, 36)." "Characteristic" in the definition means a consequence having a specific form, not a routine or inevitable consequence as Chomsky misread it to. Chomsky criticizes the definition because it is, as he says, "generally impossible" to have information concerning the speaker's motivational circumstances, and so the behavior analyst cannot make a correct diagnosis of whether a response is a mand or not. Similarly, as Chomsky reasons, the hearer, as reinforcement mediator, could not know whether or how to reinforce "relevantly." These are not real problems at all. The verbal behavior analyst must take into account whatever variables control behavior, no matter how infeasible it is to detect them in ordinary conversation. He will not undertake to test his theory in the drawing room, after all, and since the speaker's motivational circumstances are objectively measurable they may in principle be known. The test of a good theory is not how verifiable it is by a casual and non-expert observer. Modern physics would do very poorly by such a criterion.

As for the reinforcement mediator, he need not know anything whatever about the speaker's motivation in order to play his role effectively as a mand conditioner. If a verbal response specifies characteristic consequences, for example, "pass the salt", "milk, please", or "get off my foot", and the hearer complies, then *if* the speaker has a relevant motivational condition, the reinforcement operation completes itself, so to speak, and a mand, composed of that particular motivational condition controlling that particular response, will be strengthened. But if the speaker mands when he is not suitably motivated, the reinforcer, although presented, is automatically ineffective and the response will extinguish for the unmotivated state, thus sharpening motivational control. The reinforcement mediator need not concern himself with the speaker's motivation; psychology will take care of it.

In questioning the possibility of ever discovering the relevant deprivations for such mands as "give me the book", "take me for a ride" or "let me fix it", Chomsky is forgetting that reinforcers are not necessarily drive-reducers. A book may be a conditioned reinforcer whose momentary effectiveness varies with other motivational conditions: "I cannot finish this paper and go to bed until I have the reference in that book", "I need something to prop this door open", "I hid ten dollars in it". The effectiveness of conditioned reinforcers depends upon deprivation for something else. Chomsky is quite correct in concluding that deprivation is "relevant at most to a minute fragment of verbal behavior (Chomsky, 1959, p. 46)," but he is incorrect if he supposes this is a defect in the system. One of the great insights of *Verbal Behavior* is that human reinforcement-mediators can also reinforce non-motivated, non-mand, disinterested verbal behavior. This fact, rather than a genetic mutation enjoyed only by his species, very likely accounts for the fact that only humans have developed verbal behavior.

Some sort of lapse appears to have occurred where Chomsky erroneously detects an absurdity: "a speaker will not respond properly to the mand 'Your money or your life' unless he has a past history of being killed (Chomsky, 1959, p. 46)." The speaker? The speaker *emits* the mand, he does not respond to it. He needs only a history of having needed money. That is rather common. There may be an absurdity here but it is not in *Verbal Behavior.*

The Tact

Skinner defines the tact as "a verbal operant in which a response of given form is evoked (or at least strengthened) by a particular object or event or property of an object or event (Skinner, 1957, p. 81-82)." Chomsky's principal objection to this treatment is its lack of congruence with the notions of reference and meaning, which has already been dis-

cussed, and which, although true, is a virtue, not a defect.

In addition, however, he criticizes Skinner's formulation of why the hearer reinforces tacting: by doing so the hearer's potential contact with the environment is functionally extended. Once told that "dinner is ready" (a tact) the hearer may behave in a way directly reinforcing to himself. Although the dinner can be seen by the tacter only in the dining room, the tact may be heard by the reinforcement mediator around corners, upstairs, outdoors, or across town. Accurate tacters are, simply, very useful to have around. If they were not, many of civilization's most cultivated institutions such as schools, including professional graduate schools, probably would not exist. Most knowledge about the world exists as talk. Chomsky's objection (1959, p. 48) that the parents of first-borns could not know enough to teach them to tact (because they do not yet have the appropriate history of reinforcement for hearing tacts) ignores the fact that parents already have a lifetime history of hearing tacts from *other* speakers, mostly adults. One does not wait until he has borne children to hear his first assertions about the world and be reinforced for listening. Children's tacts are, in fact, useless and boring. One reinforces them all the same because if the children become good at tacting, their tacts may become very useful indeed.

Chomsky apparently finding the cash-value explanation of reinforcement for tacting too harsh, wonders if it would not be just as scientific to say that a parent has "a desire to see the child develop and extend his capacities (Chomsky, 1959, p. 48)." No, it certainly would *not*, unless by "capacity" we mean a tendency to tact accurately. Only well-discriminated (and therefore potentially useful) tacting gives the pleasure; babbling, jabbering, prattling, and outright lying may all be elaborately developed and extended as capacities but they do not please parents nearly so much.

Chomsky evidently misunderstood Skinner's operant paraphrase of Bertrand Russell's respondent version of a hearer's response to the tact "fox". This paraphrase is most certainly *not* Skinner's "own equally inadequate analysis (Chomsky, 1959, p. 48)." It is merely a paraphrase into the operant vocabulary of how the events in Russell's example, *if they occurred*, would be analyzed. It was not intended to suggest that those events are plausible, and, in fact, they are not.

Finally, Chomsky alludes to Skinner's treatment of how speakers are able to tact private events. Superficially this capacity seems most mysterious from any point of view. How *do* we learn that the English name for this thing is "headache", for that thing "contentment", and for those other things "thoughts"? For public things, like cows, someone who already knew the name saw the cow we were looking at and told us, and could reinforce our own response "cow" if the cow was *there*. The paradigm is impossible for headaches and contentment and thoughts because the reinforcement mediator cannot share the relevant stimuli. Skinner meets this problem head on; and, I think, brilliantly. The interested reader should study the explanation in *Verbal Behavior* (Skinner, 1957, pp. 130 ff), and see also Skinner (1959, pp. 272-286). Skinner's interesting point is, in fact, that only those internal stimuli that have obvious external correlates which are observable by the reinforcement mediator can become discriminated, so that, as he says, it is the community that teaches one to "know himself". Chomsky dismisses the argument briskly as a "heavy appeal to obscure internal stimuli", a grossly inadequate characterization of a very sophisticated analysis.

The Echoic

An echoic is a response which "generates a sound pattern similar to that of the stimulus (Skinner, 1956, p. 55)." Chomsky criticizes the account principally and afresh because it attributes the echoic repertoire to reinforcement rather than to instinctual imitative mechanisms. The significance of the genetic-reinforcement aspect of this objection has been discussed above.

The Textual

A textual, which is a verbal response to a written stimulus, and which makes no demands upon linguistic competence or grammatical behavior, but is surely verbal and usually more grammatical than any other verbal behavior from the same speaker, is not discussed in the review.

The Intraverbal

Intraverbals, which are verbal responses under control of other verbal behavior, are dis-

missed, but hardly discussed, by Chomsky along with the very important role which Skinner argues they play in nearly all extended intervals of verbal behavior. Once verbal behavior begins we are able to continue speaking, almost endlessly, under the stimulus influence of what we have already said. The role of intraverbal stimuli in instruction is to combine with echoics and textuals so as to produce a response which was not previously available. Chomsky wonders (1959, p. 52) in what sense this can be true for someone who is told (an echoic stimulus) that "the railroads face collapse" since the hearer *could* have said this before. But the point is that he could *not* have said it unless he had a momentary reason to do so. He could as readily have said "the railroads do not face collapse" or "there is a four-banded armadillo in the gazebo". Chomsky is once again overlooking the difference between a speaker's vocabulary as a response repertoire (what he is able to say) and speaking as a response (what he is able to say *now*). Psychology is concerned with both, but principally the latter. They are clearly different.

CONCLUSION

I conclude that Chomsky's review did not constitute a critical analysis of Skinner's *Verbal Behavior*. The theory criticized in the review was an amalgam of some rather outdated behavioristic lore including reinforcement by drive reduction, the extinction criterion for response strength, a pseudo-incompatibility of genetic and reinforcement processes, and other notions which have nothing to do with Skinner's account. Chomsky misunderstood the intent of *Verbal Behavior*, evaluating it as an accomplished explanation of verbal behavior rather than a hypothesis about the causes of verbal behavior. His review rejected in principle the products of Skinner's methodology without having come to terms with his rationale, especially as it concerns the necessity for theoretical mediating variables. The review took a view of extrapolation of laboratory findings that would bring any scientist's methodology to a dead stop. It rejected without discussion the logic of reductionism. It criticized *Verbal Behavior* for not having been about something else, that is, a theory of verbal behavior rather than verbal behavior itself. It redefined verbal behavior by equating it exclusively with the items in the behavioral repertoire rather than the speaker's momentary verbal responding, an obvious and crucial distinction for psychology.

The review completely ignored much that is central to an understanding, application and assessment of Skinner's position. Most importantly it failed to reflect Skinner's repeated insistence that the full adequacy of his explanatory apparatus for complex cases, including verbal behavior, cannot be assessed unless the possibilities for interaction among its several controlling variables acting concurrently were realized; this is what is different between the laboratory and the real world. In the laboratory, variables are made to act "one at a time", for all practical purposes. The real world simply puts the environment back together again. Multiple causality is never mentioned in the review; it is mentioned throughout *Verbal Behavior*. The mystery of its omission from the review is compounded by the fact that Chomsky found it mysterious that Skinner thought something so complex as speech could be accounted for "by a simple function"!

But the review, however approximate, *has* had an enormous influence in psychology. Nearly every aspect of currently popular psycholinguistic dogma was adumbrated in it, including its warlike tone; the new look is a frown. It speaks of itself as a "revolution", not as a research area; it produces "confrontations", not inquiries. So far there have been no telling engagements in the revolution. The declaration of war has been unilateral, probably because the behaviorist cannot clearly recognize why he should defend himself. He has not hurt anyone; he has not preempted the verbal territory by applying his methods to verbal behavior; he has not used up all of the verbal behavior nor has he precluded other scientists from investigating it to their heart's content, with any methods and theories which please them; he need not be routed before they do so.

The behaviorist will not be roused to self-defense by having a few new paradigms rattled at him. New paradigms in psychology are, bluntly, less than a dime a dozen. They come and go. The most illuminating example in the present instance is Gestalt psychology, as Neisser has noted (1967). The behaviorist does, on the other hand, understand new data. He will be the first and best judge of whether they are

incompatible with his own paradigms and he can be trusted to take them into account either way. Psycholinguistics will do itself a disservice by spending more of its time trying to destroy behaviorism, but if it is determined to do so, it should learn first exactly what the behaviorists really said, and how behaviorisms differ from one another. The same amount of time spent developing the positive aspects of its point of view will at least test whether it can define itself as something coherent and positive, rather than merely antibehavioristic. The psycholinguists are probably in a uniquely favorable position to make important advances in speech perception and in discovering what the stimulus dimensions of syntactic structures are. This knowledge will be invaluable to anyone interested in producing a causal system or theory of speech production, but I do not believe that he will be a psycholinguist.

Meanwhile, it *has* been 10 years. One can only agree with another observer of this scene who recently said: "in the flush of their initial victories, many linguists have made extravagant claims and drawn sweeping but unsupported conclusions about the inadequacy of stimulus-response theories to handle any central aspects of language behavior . . . The claims and conclusions so boldly enunciated are supported neither by careful mathematical argument . . . nor by systematic presentation of empirical evidence to show that the basic assumptions of these [stimulus-response] theories are empirically false. To cite two recent books of some importance, neither theorems nor data are to be found in Chomsky (1965) or Katz and Postal (1964), but one can find rather many useful examples of linguistic analysis, many interesting and insightful remarks about language behavior, and many badly formulated and incompletely worked out arguments about the theories of language learning." (Suppes, 1968, pp. 1-2.)

Just so.

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