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On Chomsky's Appraisal of Skinner's *Verbal Behavior*: A Half Century of Misunderstanding

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The history of the writing of *Verbal Behavior* (Skinner, 1957), Chomsky's review (1959), and MacCorquodale's rebuttal (1970) are briefly summarized. Chomsky's recent reflections on his review are analyzed: Chomsky's refusal to acknowledge the review's errors or its aggressive tone is consistent with his polemical style but comes at a minor cost in consistency and plausibility. However, his remarks about the place of Skinner's work in science reveal misunderstandings so great that they undercut the credibility of the review substantially. The gradual growth in the influence of Skinner's book suggests that its legacy will endure.

Key words: Chomsky, MacCorquodale, Skinner, verbal behavior

Science is like a river, flowing inexorably downstream, freshened and swollen by rivulets of data. Our attempts to dam or divert it are too puny to prevail for long against the gathering weight; sooner or later all obstacles are swept away, and the river resumes its natural course. We can force Galileo to recant, but we cannot force the earth to stand still; eventually the astronomer is vindicated.

From the perspective of most behavior analysts, Chomsky's review of Skinner's *Verbal Behavior* was a kind of ill-conceived dam in the progress of science, a rhetorically effective but conceptually flawed document that would eventually be overborne. But from the perspective of most cognitive scientists, it served just the opposite purpose: The review was the dynamite that destroyed the obstructions that behaviorists had placed in the way of free-flowing scientific inquiry. The flood of work in cognitive science that followed the review seemed to support that view, for some remarkably parched intellectual provinces began to blossom. Who was right? Even after half a century it is too early to say, but ultimately debate is irrelevant: The river will eventually find its own way regardless of what we believe.

Nevertheless, I think the present interview with Chomsky will help the prognosticator, for it reveals something about the context in which the review was written and therefore helps make sense of it. (The meaning of behavior is to be found not in its structure but in the independent variables of which it is a function.) For behaviorists, it helps to dispel puzzlement and annoyance—two reactions commonly occasioned by the review. Among other things, the interview underscores how alien to Chomsky was Skinner's enterprise and how stereotyped was his view of its conceptual and empirical foundations.

BACKGROUND

In 1934, over aperitifs, the distinguished and kindly British philosopher Alfred North Whitehead remarked to the brash and newly fledged Skinner that behaviorism, however sound it might be as a strategy for understanding nonverbal behavior, could not explain language. He challenged Skinner to account for utterances that allude to stimuli conspicuously absent from the environment of the speaker and that therefore appear to require conceptual tools unavailable to the behaviorist, utterances such as, "No black

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scorpion is falling upon this table." That night, Skinner began outlining a behavioral interpretation of the subject, and he was to continue working on it intermittently for over 20 years. Supported by a Guggenheim Fellowship, he devoted most of 1944 to the project, and in 1947 he summarized his progress in the William James lecture series at Harvard. Mimeographed copies of the lectures were passed from hand to hand, and according to Osgood (1958), they were widely read by students of language, a point echoed by Chomsky in the Virués-Ortega interview (2006). Skinner rounded out the text during a sabbatical in 1955 at Putney, Vermont, and the book appeared in 1957. In its early pages, he explained that the book was not itself an experimental analysis of verbal behavior, but rather an interpretation of everyday facts that invoked only those behavioral concepts and principles that had been established through independent experimental analyses of nonverbal behavior. In part, he attempted to show that the conceptual tools of the behaviorist are adequate to embrace all behavior, verbal as well as nonverbal.

The book pays no tribute to traditional structural formulations; it redefines the domain of interest as behavior and organizes topics in the light of what was known about behavioral processes. For example, it classifies verbal operants according to their controlling variables: Some verbal operants are responses to text; some are verbal chains; some are controlled by characteristic consequences; some are controlled by the stimulus properties of objects or events and are maintained by generalized social reinforcement. Some second-order verbal operants are controlled by dimensions and patterns of first-order verbal operants. Advanced topics emphasize the simultaneous effect of multiple sources of control, audience effects, self-editing, control by covert events, and thinking. Little reference is made to prevailing linguistic theories, and there is no summary of supporting research. Rather, the book rests entirely on the conceptual foundations of *The Behavior of Organisms* (1938), *Science and Human Behavior* (1953), and related work. Of this decision, Skinner later wrote,

> I had collected a lot of experimental data on verbal behavior—on how people learn strings of nonsense syllables, or the nonsense names of nonsense figures, and I had my own results on verbal summation, alliteration, and guessing. They began to clutter up the manuscript without adding much by way of validation. They threw the book as a whole badly out of balance because I could not find experiments for the greater part of the analysis. I was still the empiricist at heart, but I did not think it would betray that position if my book were not a review of established facts. I was *interpreting* a complex field, using principles that had been verified under simpler, controlled conditions. Except for certain aspects of the solar system, most of astronomy is interpretation in this sense, its principles being derived from laboratory experiments. I decided to leave out all laboratory experiments. (1979, p. 282)

The distinction between an experimental analysis and an interpretation, the relation between the two, and the centrality of the latter in science were lost on Chomsky and on many others. As I have argued elsewhere (e.g., Donahoe & Palmer, 1989, 1994; Palmer, 1991, 2003), interpretation is not the stray scraps from the table of science; it is the main course. We run experiments to discover principles that can be extrapolated to events outside the laboratory. Only a relative handful of natural phenomena have been, or ever will be, subjected to controlled study. However, the plausibility of our interpretations of the world rests entirely on the validity of the concepts adduced; thus, tightly controlled laboratory studies underlie the interpretive enterprise. It is this very relation with experimental analysis that sets Skinner's interpretations

apart from most speculation in psychology and linguistics.

Some early reviews of the book were positive, others mixed, but all were respectful (e.g., Broadbent, 1959; Dulaney, 1959; Gray, 1958; Mahl, 1958; C. Morris, 1958; Osgood, 1958; see Knapp, 1992, for a comprehensive summary of the reviews). Chomsky's paper appeared in the journal *Language* in 1959. Whereas the typical review was brief and dispassionate, Chomsky's was 33 pages long and was written in an aggressive debating style, common then and now among linguists and philosophers. Most of the review was devoted to disputing the relevance of concepts derived from the animal laboratory to an interpretation of language. The central point, repeated in the Virués-Ortega (2006) interview, was that Skinner's analysis obviously could not be taken literally; however, when taken metaphorically, it was merely common sense dressed up in jargon:

> [Skinner] utilizes the experimental results [from the animal laboratory] 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 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. … [I will 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. (pp. 30–31)

Chomsky illustrates this point at great length, arguing that laboratory concepts, such as *stimulus, stimulus control, response, probability*, and *response strength*, are inadequate when applied to human behavior. For example,

> 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 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" (p. 425), and more generally, its strength (pp. 425–429). 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. (p. 34)

As this passage indicates, Chomsky was being intentionally obtuse for the purpose of scoring a debating point. In his interpretive work, Skinner often left the task of supplying obvious qualifiers and conditions as exercises for the reader, and part of the force of Chomsky's review rests on his refusal to participate in such exercises. If Skinner's prose permitted an absurd interpretation, then Chomsky embraced it. He appears to have relied on the plausible assumption that the reader would not bother turning to the book itself to check citations. In the case under discussion, for example, one would find Skinner's position (1957, pp. 418–431) to be fully immune to Chomsky's ridicule. According to Skinner, the degree to which an event "confirms" a statement depends entirely on one's history with the additional controlling variables that have been brought to bear, their nature, the reliability of relevant speakers, the kinds of verbal operant offered, and so on. A tact is more influential than an echoic, for example. Moreover, the reader would find the passage embedded in an original discussion about the pragmatic na-

ture of scientific truth that is by no means a mere paraphrase of conventional wisdom.

In spite of such polemical devices, Chomsky's review raised a valid point: The extension of technical vocabulary from the animal laboratory to the domain of verbal behavior might not be justified. The analysis of human behavior might require additional or perhaps even entirely separate principles. In effect, Chomsky was betting that human verbal behavior is qualitatively different from the behavior of nonverbal organisms; Skinner was betting that it isn't. Notwithstanding the tacit verdict of cognitive science, this is not a matter to be decided by debate. Chomsky's review raised the flag of a competing paradigm, but he did not, in my opinion, succeed in capturing Skinner's.

Chomsky sent a draft of his review to Skinner, who was annoyed by its argumentative tone and set it aside, unfinished (Skinner, 1972, pp. 345–346). But the article was read with relish by partisans of the rising field of cognitive psychology, who embraced it as a kind of Emancipation Proclamation, a justification for rejecting the methodological constraints of behaviorism. Other critiques of behaviorism soon appeared (e.g., Breger & McGaugh, 1965; Koch, 1964; Miller, Galanter, & Pribram, 1960). Chomsky's review became just the most prominent symbol of the emergence of a new paradigm in psychology.

Wiest (1967) was the first to publish a systematic response to these critiques. He argued that Chomsky and other critics failed to appreciate the interpretive nature of Skinner's account, that in an interpretation, concepts such as *stimulus, response*, and *reinforcer* need only be plausible; they will not have the objective status of laboratory observations. Supposed shortcomings in Skinner's account arose from a confusion of critics' theoretical constructs with the data from which such constructs were inferred. As a behaviorist, Skinner was obliged to explain behavioral data; he could not be faulted for failing to address the constructs of a competing theory:

> While the speaker does show behavioral regularities in the understanding and production of speech that the linguist may wish to call grammar, let us remain clear on the distinction between the observed behavioral regularity and the *inference* of an internalized set of grammatical structures. A theory which refers to internalizing complex sets of rules or "plans for speech" (Miller et al., 1960) may be a possible way to conceptualize the acquisition of verbal behavior. The important point, however, is that such a theory is *not required* by the regularity of verbal behavior. (p. 220)

Wiest's (1967) paper was not entirely ignored. In response, Katahn and Koplin (1968) invoked Kuhn (1962) to suggest that Wiest was wasting his breath: The conflict between behaviorism and its critics was paradigmatic and could not be resolved by dispute. Only time would tell, they predicted, which paradigm would prevail. Whatever the merits of this dispassionate assessment, it implicitly supported Skinner's policy of ignoring his critics, but the authors made no mention of his restraint.

Wiest's (1967) paper was broad in scope and touched only briefly on Chomsky. It was left to MacCorquodale to write a comprehensive rebuttal of Chomsky's review. He submitted his manuscript to *Language*, where the review itself had been published. For reasons that I have been unable to discover, it was rejected by that journal's editors. Considering the polemical nature of Chomsky's paper, its evident influence, and the detailed nature of MacCorquodale's response, that editorial decision is baffling.

MacCorquodale then submitted his paper to the *Journal of the Experimental Analysis of Behavior*, where it was published in 1970. He distilled Chomsky's arguments down to three, paraphrased as follows:

1. Skinner's book is no more than an untested hypothesis. MacCorquodale (1970) agreed, but he noted that this in no way justifies Chomsky's conclusion that because Skinner's interpretation had not been proven it was therefore wrong:

Chomsky's only real argument for his conclusion that the terms of the theory do *not* in fact apply to verbal behavior … depends upon the amazing possibility that "real-life" and laboratory *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. (p. 86)

In other words, Chomsky argued that the burden of proof was on Skinner to show that laboratory concepts could be extrapolated to verbal behavior; MacCorquodale argued that, conversely, the generality of principles should be assumed until they are found to be inadequate (cf. Palmer, 2003, p. 169).

2. Skinner's technical vocabulary is a mere paraphrase of traditional terms. MacCorquodale (1970) vigorously disputed this point, case by case, arguing that Skinner's terms are more objective than the diffuse concepts of the vernacular.

3. Speech requires a complex, mediational, neurological-genetic theory. To this, MacCorquodale (1970) responded with the standard argument that, however welcome knowledge of underlying mechanisms might be, a science of behavior is possible without it:

"One would naturally expect that the prediction of the behavior of a complex organism (or machine) would require, in addition to knowledge of 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 note 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. (p. 91)

Finally, MacCorquodale (1970) observed that Chomsky either misrepresented or misunderstood the complexity of Skinner's analysis. Chomsky appeared to think that when Skinner identified a putative controlling variable, he was asserting that it was the only relevant variable and always a sufficient one, as though speech were a collection of reflexes.

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. … Multiple causality is never mentioned in the review; it is mentioned throughout *Verbal Behavior*. (p. 98)

The reader is urged to read all three relevant documents: Chomsky's review, MacCorquodale's reply, and of course, *Verbal Behavior* itself. As a partisan, I am no doubt unable to discuss them objectively. On my reading, Chomsky's review is unsound, MacCorquodale's reply devastating, and Skinner's book a masterpiece. However, not all behavior analysts agree with this one-sided assessment. For example, Hayes, Barnes-Holmes, and Roche (2001), Place (1981), Stemmer (2004), and Tonneau (2001) have identified a range of problems with Skinner's analysis from the trivial to the

fundamental. However, in each case, their criticisms were accompanied by a proposed behavior-analytic improvement. It is unlikely that their proposals would satisfy Chomsky.

It is unfortunate that MacCorquodale's (1970) response was published in a behavioral journal, for it is almost never cited outside the field of behavior analysis. Chomsky himself chose to respond only obliquely, in a footnote, a fact he recalled in the Virués-Ortega (2006) interview. Because the typical behavior analyst is likely to read MacCorquodale's paper as a successful and thorough rebuttal of Chomsky's review, it is only fair to present his response in full:

> Interesting reading, in this connection, is MacCorquodale [1970]. I cannot take the space here to correct the many errors (e.g., his misunderstanding of the notion of "function," which leads to much confusion). The major confusion of the article is this. MacCorquodale assumes that I was attempting to disprove Skinner's theses, and he points out that I present no data to disprove them. But my point, rather, was to demonstrate that when Skinner's assertions are taken literally, they are false on the face of it (MacCorquodale discusses none of these cases accurately) or else quite vacuous (e.g., when we say that the response "Mozart" is under the control of a subtle stimulus), and that many of his false statements can be converted into uninteresting truths by employing such terms as "reinforce" with the full imprecision of "like," "want," "enjoy," etc. (with a loss of accuracy in transition, of course, since a rich and detailed terminology is replaced by a few terms that are divorced entirely from the setting in which they have some precision). Failing to understand this, MacCorquodale "defends" Skinner by showing that quite often it is possible to give a vacuous interpretation to his pronouncements, exactly my point. The article is useful, once errors are eliminated, in revealing the bankruptcy of the operant conditioning approach to the study of verbal behavior. (Chomsky, 1973, p. 24)

This was to be virtually the last word on the subject in the entire field of cognitive science for 30 years. I leave it to the reader to decide if this footnote adequately answers MacCorquodale's critique.

In this quotation and in the Virués-Ortega (2006) interview, Chomsky confirms that his central criticism of Skinner's analysis is that because it is obviously false when taken literally, it must be intended metaphorically, in which case it is merely a poor paraphrase of conventional wisdom. Before going further, it would be well to respond to this argument. One could make an equal case that, outside the laboratory, Newton's laws of motion, if taken literally, are obviously false, but if taken metaphorically are merely paraphrases of the rules of thumb of craftsmen. Skinner did *not* intend that his analysis be taken metaphorically. He was making the strong claim that the principles of behavior derived from the laboratory study of behavior are applicable, in their technical sense, to the interpretation of verbal behavior. That was the point of his book. Skinner himself said as much in a note he wrote after reading passages from Chomsky's (1971) review of *Beyond Freedom and Dignity*:

> "When Skinner tells us that a fascinating hobby is 'reinforcing' [writes Chomsky], he is surely not claiming that the behavior that leads to indulging in this hobby will be increased in probability." That is precisely what I did claim. "Rather he means that we enjoy the hobby." That is precisely what I did not mean. (Skinner, 1983, p. 320)

It is clear that Chomsky thinks that Skinner's position can easily be refuted by offering examples in which, for example, a putative reinforcer clearly does not lead to an increase in response frequency. But human behavior is notoriously complex, and prevailing conditions are constantly in flux, partly because of verbal behavior itself; predicting the frequency of a response requires a consideration of all relevant variables, not just the most salient one. When we tell a joke, for example, we convert our audience from one that has not heard it before to one that has. However hearty the laughter,

a second telling is likely to be punished, not reinforced, and it is a rare person who has not learned this fact of social life. The reinforcing effect of the laughter is observed by an increased tendency to tell the joke to a naive audience under similar conditions.

It is true that our interpretations of complex cases are speculative, but that arises from the difficulty of controlling all of the relevant variables and from the complexity of our subject matter: Relevant behavior may be covert (i.e., beneath the threshold of observability given current technology); ethical considerations prohibit exerting tight experimental control over humans; and environment–behavior relations are highly sensitive to history. (Newton complained that the problem of determining the gravitational interactions of just three bodies—the earth, moon, and sun—made his head ache. Imagine the intensity of his headache if the interactions of those bodies varied with their experiences!) The tentative nature of our interpretations means that the field is open to competing accounts, but all scientists are limited by the same constraints. The problems of interpreting human behavior do not go away by fleeing to a more permissive paradigm. The virtue of Skinner's account is that it invokes only variables that have been established by an independent laboratory science.

THE INFLUENCE OF CHOMSKY'S REVIEW

By the time MacCorquodale's (1970) paper was published, the expanding fields of cognitive psychology and structural linguistics were so active that they could no longer be seen as merely a reaction to behaviorism, and any merits or defects in Chomsky's paper became irrelevant. Nevertheless, the review became an intellectual landmark, and it remains one today. It is commonly cited as one of the precipitating events of the "cognitive revolution"; rarely does a textbook in cognitive psychology fail to mention it, often in reverential tones, and the validity of its arguments is unquestioned. Consider the following examples:[1] "Chomsky's review has come to be regarded as one of the foundational documents of the discipline of cognitive psychology, and even after the passage of twenty-five years it is considered the most important refutation of behaviorism" (Newmeyer, 1986, p. 73). "In 1959 Noam Chomsky wrote a scathingly negative review of B. F. Skinner's attempt to account for language in behaviorist terms, and he was successful in convincing the scientific community that adult language use cannot be adequately described in terms of sequences of behaviors or responses" (Hoff, 2005, p. 231). Chomsky's "arguments against behaviorism … were considered absolutely devastating. Like most of Chomsky's finest arguments, his case against Skinner is as effective emotionally as it is intellectually" (Harris, 1993, p. 55). Chomsky's review was said to be "electric: Noam at his best, mercilessly out for the kill, daring, brilliant, on the side of the angels … in the same category as St. George slaying the dragon" (Bruner, 1983, pp. 159–160). Such statements are commonplace in the cognitive literature, but never are they accompanied by evidence that the author has read either Skinner's book or MacCorquodale's paper.

More disturbingly, the claim that behavioral interpretations of complexity are inadequate has become a kind of axiom in cognitive science, and the review is widely regarded as sufficient justification for assuming it. For example, despite the obvious relevance of the processes of generalization and discrimination to the topic of concepts, a recent compen-

[1] I thank Ted Schoneberger for unearthing these examples.

dium of papers on the subject relegated behavioral contributions to a footnote: "[An alternative view is] that concepts are behavioral or psychological abilities. We take it that behavioral abilities are ruled out for the same reasons that argue against behaviorism in general (see, e.g., Chomsky, 1959)" (Laurence & Margolis, 1999, p. 6).

Such examples suggest that, instead of building principles of behavior into its foundation, cognitive science has cut itself loose from them. Cognitive psychology textbooks neither exploit nor review reinforcement, discrimination, generalization, blocking, or other behavioral phenomena. By implication, general learning principles are peripheral to an understanding of cognitive phenomena. Even those researchers who have rediscovered the power of reinforcement and stimulus control hasten to distance themselves from Skinner and the behaviorists. For example, the authors of a book that helped to pioneer the era of research on neural networks were embarrassed by the compatibility of their models with behavioral interpretations: "A claim that some people have made is that our models appear to share much in common with behaviorist accounts of behavior … [but they] must be seen as completely antithetical to the radical behaviorist program and strongly committed to the study of representations and process" (Rummelhart & McClelland, 1986, p. 121).

With regard to those modern linguists who share Skinner's interest in functional questions, Richelle (1993) remarked, "Few specialists were ready to take the risk among their peers of alluding to Skinner, and even less to suggest that he had foreseen some of the current developments in psycholinguistics" (p. 134). If Chomsky's review is indeed partly responsible for this neglect of behavioral principles in cognitive and linguistic theorizing, its effect on those fields will eventually prove to have been destructive, however inspirational it may have been for a generation of scientists. The law of effect is not a behavioristic fantasy. The review, then, has been extraordinarily influential, whatever its intellectual merits, but the nature of that influence is uncertain.

COMMENTS ON THE VIRUÉS-ORTEGA INTERVIEW

Although Chomsky is commonly viewed as an ideological adversary of Skinner, he has never avoided civil dialogue with behavior analysts. In 1993, the behavior-analytic philosopher Ullin Place opened a correspondence with him about verbal behavior, and Chomsky responded at great length in four successive letters. Moreover, he consented to publish an edited version of this correspondence in *The Analysis of Verbal Behavior* (Chomsky, Place, & Schoneberger, 2000).[2] The Virués-Ortega (2006) interview attests to his continued willingness to talk openly about his position with behaviorists. Undoubtedly Chomsky felt that he was speaking from a position of strength: He qualified nothing, and his tone was confident and uncompromising. Nevertheless, I found the interview somewhat surprising. Chomsky's response to criticism and his characterization of the context in which his paper was written do not strengthen, but rather weaken, the review's authority. I was struck by the following points:

1. Chomsky dismissed one of the foundational assumptions of behav-

[2] That this correspondence was published at all is mainly due to the perseverance of Hank Schlinger, the journal editor, and Ted Schoneberger, who edited the correspondence as a set of dialogues on five controversial topics. Chomsky had objected to a simple reprinting of the letters, because they were occasionally repetitive, careless, and personal, but he found the edited format agreeable.

ior analysis, namely, that behavior is an orderly datum that is an appropriate subject matter of science in its own right:

> Behavior is evidence. It's not what you are studying; what you are studying is competence, capacity. If you study man's insight you want to know what is going on in his brain; behavior gives the evidence for that. … In a serious field, you wouldn't identify the subject with the study of the data. (Virués-Ortega, 2006, p. 245)

By "you" Chomsky apparently means "everyone relevant," but in any discussion of Skinner's system, this peculiar assertion is not merely wrong; it implies a refusal to listen. Skinner's career was devoted to empirical demonstrations that a science of behavior is possible and to conceptual arguments that it is worth doing. Moreover, his discussion of the relation between behavior and the nervous system was extensive and sophisticated (e.g., Skinner, 1938, pp. 418–444; see also E. K. Morris, Lazo, & Smith, 2004), but there is no evidence, here or elsewhere, that Chomsky is familiar with it. His claim can be defended as a statement of personal preference, but it is one that appears to be uninformed by contrary points of view.

2. Chomsky refused to admit any errors, even reasonable ones, but at a cost in coherence. MacCorquodale had remarked that Chomsky's review devoted six pages to criticizing views on drive reduction that Skinner never held and that all other behaviorists had abandoned (MacCorquodale, 1970, pp. 83–84). In addition, Virués-Ortega cited two other examples of positions the review attributed to Skinner that he did not hold (2006, p. 247). One would expect Chomsky to have been mildly chagrined by these errors, and to have pointed out that it was not unreasonable for him to make a mistake or two when critiquing a vast field outside his own domain of expertise. But he waved them away with the remark, "Of course I discussed drive reduction but did not attribute it to Skinner" (p. 247). Why "of course"? Whose theory of drive reduction was he was discussing? What possible relevance could it have to a review of Skinner's *Verbal Behavior*?

The answer, apparently, is that Chomsky intended the review to go "far beyond Skinner," to embrace the entire family of behavioral approaches. One cannot help suspecting that this is a post hoc reinterpretation of his goals, a bolt-hole to avoid the charge of sloppy scholarship. It is true that Chomsky acknowledged in his review that Skinner rejected the drive reduction theory of reinforcement. Moreover, he has claimed for many years that the target of his review was a kind of generalized behaviorism: "I had intended this review not specifically as a criticism of Skinner's speculations regarding language, but rather as a more general critique of behaviorist (I would now prefer to say 'empiricist') speculation as to the nature of higher mental processes" (Chomsky, 1967, p. 142). But if that were indeed his intention at the time, it is quite odd that he did not say so in the review. Moreover, this maneuver fails to answer the criticism on two grounds. First, as MacCorquodale (1970) indicated, behaviorists in general had abandoned the drive reduction theory; Chomsky was hurling spears at a shadow. Second, and more important, by shifting the supposed target of the review to a generic behaviorism, Chomsky vitiated his own arguments. Skinner's position was not a subset of this generic behaviorism, as Chomsky admitted explicitly in the review and implicitly in the interview. Some of the review is aimed at one target, some at another, but the reader is not brought in on the secret. If the review is without factual errors, then it is incoherent.

3. Chomsky declined to acknowledge that the review was written in a strident tone. Tone is relative, of

course, and one must take into consideration the standards of discourse to which one is accustomed. Relative to the tone adopted among linguists when arguing among themselves, as documented in Harris' *The Linguistics Wars* (1993), Chomsky's tone in his review of *Verbal Behavior* is polite and restrained. Because the shrillness of controversies in his own field may have dulled Chomsky's ear to the nuances of professional discourse, I suggest he read the reactions of a neutral observer:

> His conclusions about Skinner's project are unconditional … Skinner's claim is not simply "false," it is "quite false" (p. 32) … Skinner is seldom simply in error, but "grossly in error" (p. 46) … The term "reinforcement" is not just "useless," it is "perfectly useless" (p. 38); things are not just wrong, but "obviously wrong" (p. 47) … "Skinner's account [differs] mainly in the use of *pseudoscientific* terms" (p. 53) and is "a kind of *play-acting* at science" (p. 39) … "*A moment's thought* is sufficient to demonstrate the impossibility of [classifying responses according to the behavior of the listener]" (p. 47) … Apparently Skinner did not take that moment of thought. (Czubaroff, 1988, p. 324)

Such loaded terms are commonly read, I believe, as condescending and antagonistic. When Chomsky suggests that Skinner is suffering "a serious delusion" (p. 38), he strikes a partisan and adversarial pose that suggests that dialogue is not likely to be fruitful.

However, the charge that Chomsky's tone was "angry" was made by Skinner after Chomsky's (1971, 1973) reviews of *Beyond Freedom and Dignity* (Skinner, 1971) and is irrelevant here. The tone of the review of *Verbal Behavior* is aggressive, not angry. But Chomsky's remarks about *Beyond Freedom and Dignity* are relentlessly shrill from beginning to end ("It would be hard to conceive of a more striking failure to comprehend even the rudiments of scientific thinking," Chomsky, 1973, p. 46). Readers are invited to read the review for themselves to see if Skinner's characterization is fair and if Chomsky's umbrage in the closing paragraph of the interview is justified.

4. Chomsky's summary of developments in behavior analysis in the years following the review was so inaccurate that it retrospectively undercut his authority in the review and elsewhere. To paraphrase: Skinnerian orthodoxy developed cracks because of work in comparative psychology, ethology, and linguistics, and it was finally brought to ruin by internal criticism, mainly by the Brelands, who found that their animal training consisted of no more than slight modifications of instinctive behavior, which would soon reassert itself, "refuting all the theory." The collapse was complete: The procedures didn't even work for pigeons (Virués-Ortega, 2006, p. 246). Skinner's legacy is a few experimental techniques and some special therapeutic procedures. "There are few if any theses of more than the most limited significance" for any organism (p. 249).

First, Chomsky evidently knows little about the Brelands' work or its implications. His caricature suggests that their attempts to train animals were futile, that the most they could accomplish was a slight but temporary modification of instinctive behavior. But precise prediction and control are the bread and butter of the commercial animal trainer. Keller Breland, Marian Breland Bailey, and Robert Bailey used operant conditioning procedures as the foundation of a successful commercial enterprise for nearly 50 years, during which time they revolutionized the field of animal training. They showed that the principles do indeed work for pigeons and for many other organisms as well: They trained over 15,000 animals, from more than 140 species, in a wide variety of performances, featuring, among many other things, rabbits playing pianos, chickens dancing and playing tic-tac-toe, and a pig that would turn on a radio, operate a vacuum cleaner, pick up laundry, and select the sponsor's

brand over a competitor's (Bailey & Gillaspy, 2005). This was far more than the slight modification of instinctive behaviors. Far from "refuting all the theory," their work offered dramatic support for the power and generality of operant principles, just the opposite of what Chomsky implied.

Second, Chomsky errs in imagining behavior analysis to be a rigid and unchanging set of propositions, a kind of dogma, rather than an evolving science. Skinner's approach was explicitly inductive and pragmatic; he eschewed model construction, formal theorizing, and theory-driven research; his guiding maxim was simply to seek order in his data (e.g., Skinner, 1938, 1956). In the domain of verbal behavior alone, that tradition has opened up a variety of new avenues of work including joint control (e.g., Lowenkron, 1998), naming (e.g., Horne & Lowe, 1996), relational frame theory (e.g., Hayes et al., 2001), stimulus equivalence (e.g., Sidman, 1994), and automatic shaping (e.g., Donahoe & Palmer, 1994; Sundberg, Michael, Partington, & Sundberg, 1996). This is not the picture of a field goose-stepping to Skinner's drum.

Verbal Behavior was an exercise in the extrapolation of empirical principles as they were known at the time, not the application of dogma. In contrast, Chomsky exalts theory construction, formal models, and theory-driven research, and perhaps for that reason he appears to believe that science cannot survive prediction errors. But science is a self-correcting enterprise. The Brelands' data were undoubtedly surprising at the time—nobody understood the power of shaping better than the Brelands, and they themselves were surprised. They discovered that certain inductive generalizations did not hold under some conditions. However, the data were accommodated by higher order generalizations, and the "Breland effect" was easily integrated into the science of behavior. There was no discontinuity, no crisis, and the field has grown steadily ever since. Just a few years after the Brelands' paper was published, Skinner cited it favorably and discussed at length the phylogenetic and ontogenetic origins of behavior (Skinner, 1966). Regarding the intrusion of species-wide forms of food-getting behavior into ongoing performance of a task, he wrote,

> Since other reinforcers were not used, we cannot be sure that these phylogenic forms of food-getting behavior appeared because the objects were manipulated under food-reinforcement. The conclusion is plausible, however, and not disturbing. … The facts do not show an inherently greater power of phylogenic contingencies in general. Indeed, the intrusion may occur in the other direction. (p. 1210)

Indeed, it may. In the face of the most fundamental of phylogenetic imperatives, anorexic teenagers starve themselves, monks vow celibacy, and an inexhaustible supply of young men in their reproductive prime eagerly volunteer to blow themselves up in Baghdad and New York. The origins of behavior are complex, and a science of behavior must analyze them all.

The genetic endowment plays some role in verbal behavior, as it does in all behavior. Whatever that role is, it will be embraced by behavior analysis, whenever it is understood. If it happens that Chomsky's work indeed illuminates that role, he will be acknowledged, strange to say, alongside Skinner as an important figure in the history of the science of behavior. However, as I have argued elsewhere (Palmer & Donahoe, 1992), Chomsky's conceptual tools are incommensurate with the phenomena he hopes to explain. (As one example among many, the grammatical sentence is an inappropriate unit of analysis in a selectionist science.) I therefore think it unlikely that he will make a permanent contribution to a science of verbal behavior.

5. Chomsky remarked that the William James lectures were orthodox for Harvard philosophy students, that "everybody read them," and that "I actually wrote the review before the book was published" (Virués-Ortega, 2006, p. 246).[3] Moreover, behaviorism "just swept the intellectual domain of the 1950s. … Radical behaviorism quickly came to enjoy enormous prestige," and there were so few dissenters that he could actually list them (p. 245). These comments are valuable for the light they shed on the way Skinner was viewed at Harvard, in that era, by people outside his field. However, with the benefit of hindsight, we can see that Chomsky would have seen things differently if he had lived anywhere else. Skinner may have been a charismatic and influential figure at Harvard in the 1950s, but radical behaviorism was by no means a dominant and monolithic force in psychology. At the time *Verbal Behavior* was published, Skinner had perhaps a few dozen students and colleagues, but they had so much trouble getting their papers published in mainstream psychological journals that they started their own. Nevertheless, that Chomsky saw radical behaviorism as a juggernaut helps to explain the polemical nature of his attacks against it.

CONCLUSION

Although Chomsky's willingness to discuss his review of *Verbal Behavior* with behavior analysts is a sign of openness and confidence, the Virués-Ortega (2006) interview did not flatter him. His refusal to acknowledge errors of fact, or shrillness of tone, was narrow and defensive. Moreover, he confirmed that he does not understand the distinction between experimental analysis and interpretation, that the extrapolation of laboratory principles to domains in which experimental analysis is not yet possible is standard practice and contributes greatly to our understanding of the world. Finally, Chomsky revealed a naive understanding of the rationale for the behavioral approach, its goals, and its relation to empirical work. His imagined opponent was an extreme environmentalist cleaving to stimulus–response dogma, immune to evidence. Painting an absurd caricature of one's opponent is an effective debating move, but the strategy pays a penalty when it is discovered.

Chomsky is still a prominent figure in cognitive science, but his influence has waned. According to one book devoted to Chomsky's work, "The early demolition of Skinner has remained Chomsky's main influence on psychology, rather than his later work; introductions to psychology seldom mention post-1965 writing" (Cook & Newson, 1996, p. 78). For more than two decades, Chomsky and his followers attempted to model syntax with transformational rules, but that effort was abandoned. As the models increased in complexity, they became less plausible psychologically (Schoneberger, 2000). Chomsky's current theory has reallocated the explanatory burden from one essentialistic construct to another (from the syntactic module to the lexicon), with no advance in plausibility. My own exploration and evaluation of Chomsky's theories (Palmer, 1986/2000a, 2000b) led me to predict that his work will ultimately be seen as a kind of scientific flash flood, generating great excitement, wreaking havoc, but leaving behind only an arid gulch.

It is true that cognitive science has flourished in the decades following Chomsky's review, but behavior analysis has flourished as well and has done so cumulatively. Interest in Skinner's interpretation of verbal

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[3] Hence the "half century" of my title. Chomsky may indeed have written his essay in response to the William James lectures, but if so, he revised it upon publication of Skinner's book.

behavior is stronger than ever. By one measure, work inspired by Skinner's book has increased eightfold in the past 30 years (John Eshleman, personal communication, October, 2003; see also, Eshleman, 1991). This is quite a different trajectory from that implied by Chomsky's remarks.

What of practical application? Applied behavior analysis is now widely used in the shaping of verbal behavior in children with autism or with developmental delays. All such work is rooted in behavioral principles, and some of it is even explicitly guided by Skinner's interpretation of verbal behavior. (Whether that guidance confers an advantage relative to other behavioral approaches is uncertain, pending programmatic research; Carr & Firth, 2005.) I am aware of no one who appeals to Chomsky's analysis of grammar in this context. It appears, then, that the utility of a behavioral approach in the domain of verbal behavior is not "precisely zero," as Chomsky asserted (p. 248). Perhaps Chomsky would take refuge in the claim that autistic children are atypical, that normal children do not require the careful arrangement of contingencies of reinforcement to learn, and that this only underscores his argument that language simply "unfolds" in the child, triggered by exposure to critical examples, irrespective of reinforcement contingencies. But reinforcement contingencies are ubiquitous, and all children profit when they are favorably arranged. One behaviorally designed Internet-based reading program can teach most children to read in under 30 hr of instruction (Layng, Twyman, & Strikeleather, 2003) and has been used even in inner city schools with promising results. The potential of well-designed instructional technology to effect positive social change is enormous. The relevance of this example in the present context is that the construction of this program was explicitly guided, in part, by Skinner's analysis of language. One of its designers remarked, "Without *Verbal Behavior*, there would be no program" (T. V. Joe Layng, personal communication, October 3, 2005).

These examples do not bear directly on Chomsky's arguments. My purpose in relating them is that they indicate that Skinner's analysis of verbal behavior is not a museum piece, a moribund historical curiosity; it is the foundation of an active research program, continuing conceptual development, and of practical applications with potentially far-reaching effects. These are the characteristic signs of science in progress. To return to the metaphor with which I began, each behavioral enterprise is like a rivulet, small in itself, but relentlessly bearing downstream, adding its weight cumulatively to the river of science. In 1957, the tributary represented by Skinner's book was a mere trickle and was easily dammed. But water is now spilling over the top.

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