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An Underdiscussed Aspect of Chomsky (1959)

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Chomsky's (1959) review of Skinner's (1957) *Verbal Behavior* has been influential and attributed with a role in the cognitive revolution. However, while counter reviews from within behavior analysis have noted that Chomsky misunderstood the subject matter, certain aspects of his scholarship have been underdiscussed. This includes several instances where Chomsky misquotes Skinner or takes his quotes out of context. Similar to the findings of Sokal (1996a, 1996b), it is speculated that the problems with Chomsky were overlooked by cognitive psychologists because his general outlook was accepted. Implications for the editorial review process are discussed.

> Transgressing disciplinary boundaries . . . [is] a subversive undertaking since it is likely to violate the sanctuaries of accepted ways of perceiving. Among the most fortified boundaries have been those between the natural sciences and the humanities.
>
> —Greenberg (1990, p. 1), as quoted in Sokal (1996a)

> Every man has a right to his opinion, but no man has a right to be wrong in his facts.
>
> —attributed to Bernhard Mannes Baruch (Montapert, 1964, p. 145)

Areas of study are sometimes profoundly affected by people outside them. For example, the linguist Noam Chomsky is familiar to many psychologists for his influence in the cognitive revolution, the major shift in the orientation of American psychology from behavioral to cognitive in the 1960s and 1970s (e.g., Auyang, 2000, p. 105; Erneling, 1997; Hunt, 1982, p. 217; Kosslyn & Rosenberg, 2001, p. 238). Some have even suggested that Chomsky's (1959) review of B. F. Skinner's (1957) *Verbal Behavior,* the latter's theoretical account of how language can be explained in behavioral terms, was *responsible* for initiating the cognitive revolution (e.g., Bialystock, 1997; Harnish, 2002, p. 44).

When Chomsky set out to write his review, he faced a daunting task. While apparently well read in certain areas such as political theory, there is little to indicate he had any previous exposure to psychology except for psychoanalysis (Barsky, 1997) and through linguists who have drawn upon psychology, such as Bloomfield (1933). Psychology in general in the early and mid-20th century was broad and diverse (e.g., Heidbreder, 1933; Hilgard & Bower, 1966), and so too was behavioral psychology. At that time "behaviorism" was used to refer a variety of behavior-oriented work, such as the strict methodological behaviorism of John B. Watson (e.g., Watson, 1913, 1924), the mathematical modeling with intervening variables of Clark L. Hull's school (e.g., Hull, 1943, 1951, 1952), and the eclectic, molar, purposive behaviorism of Edward C. Tolman (e.g., Tolman, 1922, 1932, 1948), each with its own assumptions and terminologies. Skinner's approach, radical behaviorism, differed from all of them in that he recognized the fundamental respondent-operant distinction (Skinner, 1935, 1937, 1938, p. 61ff., 1953, pp. 45–90), accepted the reality of private events, and rejected purely hypothetical mental constructs (Skinner, 1945). *Verbal Behavior* was very specifically based on Skinner's approach, and any reviewer would have to understand this to do the job properly.

Behavior analysts' responses to Chomsky's (1959) review of *Verbal Behavior* have not been positive (e.g., MacCorquodale, 1970; McLeish & Martin, 1975; Richelle, 1976); their counter reviews generally have noted that Chomsky fundamentally misunderstood the

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subject matter and emphasized major points where he differed from Skinner. The existing counter reviews, however, do not exhaust the problems with Chomsky. An especially important yet underdiscussed problem of Chomsky is the poor quality of his scholarship, which, to this author's best knowledge, has not been previously addressed. The purpose of this paper is to address this issue and discuss its implications for future literature reviews.

Chomsky's Review of Skinner

As noted above, Chomsky's apparent understanding of radical behaviorism was poor, and at numerous points he makes claims not supported by the existing literature. Some parts of his review, such as citing a supporting claim that stimuli cannot be manipulated (Chomsky, 1959, p. 31, footnote 5) demonstrate severe problems with his understanding of the subject matter, and his handling of the most basic terms was so poor that Richelle (1976) said his "misunderstanding . . . would prompt most examination graders to read no further."

Many serious problems in this area have already been discussed by the above-mentioned counter reviewers; these include instances where Chomsky misattributed views to Skinner that clearly belong to others. For example, Chomsky (1959, p. 28) in describing response strength claimed: "The strength of an operant is defined by Skinner in terms of the rate of response during extinction. . . ." Skinner never claimed this, but rather defined it as the probability a response will occur under certain circumstances (Skinner, 1957, p. 22). MacCorquodale (1970) noted that rate of response during extinction was actually Hull's definition of probability (Hull, 1943, pp. 260–262), and claiming that Skinner accepted this definition given his criticism of Hull's work (Skinner, 1944) would be difficult to justify. Several other of Chomsky's errors may have resulted from his failing to distinguish Skinner's views from those of others. For example, Chomsky claimed that Skinner said "that children can learn language only through 'meticulous care' on the part of adults who shape their verbal repertoire through careful differential reinforcement" (Chomsky, p. 42) and that it is part of "the doctrine of Skinner and others that slow and careful shaping of verbal behavior through differential reinforcement is an absolute necessity" (op. cit., p. 42). Aside from the fact that other people's doctrines are irrelevant here, nowhere does Skinner make this claim. Chomsky may have actually been reacting to Miller and Dollard (1941, p. 82), who in a discussion on language acquisition wrote: "The child is given meticulous training in connecting words to objects and connecting acts to words." Skinner (1957), however, says almost nothing about language acquisition and certainly did not describe it as arising from a laborious and intentional shaping procedure. Chomsky (pp. 35, 52), perhaps supposing all responses would act like respondents, also wrongly indicated that a "strong" (operant) verbal response should, according to Skinner, necessarily be a high-pitched scream. However, as Skinner (1950, p. 196) noted,

> In the sort of behavior adapted to the Pavlovian experiment (respondent behavior) there may be a progressive increase in the magnitude of response during learning. But we do not shout our responses louder and louder as we learn verbal material, nor does a rat press a lever harder and harder as conditioning proceeds. In operant behavior the energy or magnitude of response changes significantly only when some arbitrary value is differentially reinforced—when such a change is what is learned.

Not all of Chomsky's errors involved confounding views or pure misunderstandings. There are several instances in Chomsky (1959) where Skinner is quoted out of context or the quote is incorrect such that Skinner's views are not accurately portrayed. A few examples follow.

1. The above-mentioned false definition of response strength was again made when Chomsky (1959, p. 34) claimed that "Skinner has argued that this [rate of emission during extinction] is 'the only datum that varies significantly and in the expected direction under conditions which are relevant to the "learning process." The actual quote from Skinner (1950, p. 198) began with "Rate of responding appears to be the only datum which [*sic*] varies significantly. . . ." Extinction was not mentioned at all. Furthermore, this occurred in a discussion of learning theories, far removed from anything Chomsky discussed.

2. Chomsky (1959, p. 34) claimed that Skinner said that "the frequency of the response may be 'primarily attributable to the frequency

of occurrence of controlling variables' . . . ." The actual sentence in Skinner (1957, p. 27) was "Nor can we be sure that frequency is not primarily attributable to the frequency of occurrence of controlling variables." This occurred in a discussion of factors affecting frequency, during which Skinner emphasized that, contrary to the practice of "word counts" used in formal analyses of language, "it is also important to know the prevailing conditions," that is, the circumstances under which they occur. Skinner was expressing a possibility or an uncertainty, which Chomsky failed to acknowledge.

3. Chomsky (1959, p. 34, footnote 9) quoted from Skinner (1950, p. 199) regarding allegedly using "the notion of probability in analyzing or formulating instances of even types of behavior which are not susceptible to this analysis." Chomsky used this quote in a discussion in which he attacked Skinner's usage of probability (and failed; see MacCorquodale, 1970). However, the passage from Skinner occurred at the end of a paragraph discussing complex activities that, as a whole, may never be repeated by the individual. In the same paragraph, Skinner (p. 199) said,

> They are not simple unitary events lending themselves to prediction as such. If we are to predict marriage, success, accident, and so on, in anything more than statistical terms, we must deal with the smaller units of behavior which lead to and compose these unitary episodes. If the units appear in repeatable form, the present analysis may be applied.

Chomsky, however, attempted to apply this analysis to language behaviors, which typically involve a large number of units that may occur very frequently; thus the limitations that may apply to predicting marriage, success, accident, and so on as wholes are irrelevant to Skinner's program. Chomsky acknowledged none of this.

4. Chomsky (1959, p. 35, footnote 10), in discussing pitch, quoted Skinner as saying "Fortunately, 'In English this presents no great difficulty' since, for example, 'relative pitch levels . . . are not . . . important,'" the "this" apparently referring to that pitch, among things, has linguistic functions (pp. 34–35). He further indicated (op. cit.): "No reference is made to the numerous studies of the function of relative pitch levels and other intonational features in English." The implication is that Skinner ignored the importance of pitch and intonation in actual speech. But what Skinner (1957, p. 25) actually wrote was:

> In English, this [energy level, speed of response, and even repetition entering into the construction of different forms of response] presents no great difficulty. Absolute levels of pitch and intensity are not "distinctive," nor are relative pitch levels important. Changes in pitch, however, distinguish different types of utterance.

Skinner then discussed the functions of pitch in English. While Skinner and Chomsky may have disagreed about their exact functions, it is clear that Skinner did not contend that intonation or pitch in English were completely "unimportant" as Chomsky implied.

5. Chomsky (1959, p. 52, footnote 42) quipped, "Similarly, 'the universality of a literary work refers to the number of potential readers inclined to say the same thing' (. . . i.e., the most 'universal' work is a dictionary of clichés and greetings)." However, the paragraph in Skinner (1957, pp. 274–275) this quote was taken from begins:

> A reader seeks out other works of a given writer or other literature of a given type because of the reinforcement he has received. The reinforcement depends upon his own verbal behavior. A thematic correspondence between a reader and a literary work is likely to involve a matching of variables in the fields of motivation and emotion. The universality of a literary work refers to the number of potential readers inclined to say the same thing, at least in some measure. The success of a book is some indication of the number of people who possess a given kind of verbal behavior in strength.

In other words, a work has "universality" when readers would be likely to say something similar in content to the work, not specific utterances.

6. Chomsky (1959, p. 53) claimed that "Skinner considers a sentence to be a set of key responses (nouns, verbs, adjectives) on a skeletal frame" and then proceeded to criticize Skinner as if this was his position. However, the actual quote was: "Others [other sentences] are nearly complete skeletal 'frames' upon which an exceptional response or two may be hung" (Skinner, 1957, p. 346), and this was not presented as the sole possibility. Rather, it was part of a much longer chapter (pp. 344–

367) in which Skinner discussed how verbal responses are combined. Much of this involves a class of responses called autoclitics (p. 311ff.), which are verbal operants whose occurrence depends in part on the occurrence of other verbal operants; in Skinner's hypothesis, grammar results from an interaction of autoclitics with other verbal operants. For example, when one is confronted by the appropriate stimulating conditions (seeing a hungry man) one would not only emit such responses as *hungry* and *man,* but also autoclitics related to the specificity of the situation (*the*) and occurrence (*is*) to produce a longer, structured utterance (*The man is hungry*; Skinner, p. 345). Not all sentences are necessarily created this way, and in the quote Chomsky cites, Skinner does indicate some may be partially or completely preformed; however, he certainly did not claim *all* were.

DISCUSSION

The publication of factually inaccurate papers is far from unknown. Perhaps the most famous case in recent years is that of Sokal's (1996a) publication of an article purportedly about science as a social construct. It was afterwards revealed by the author (Sokal, 1996b) that the paper was a parody in which the major points were supported, if at all, by non-sequitirs and nonsense. Sokal (1996b) suggested that even though the editors who reviewed the first article did not understand it, they accepted it because, in part, of its extensive use of jargon and because they agreed with its conclusions. Although Sokal's (1996a) work was a parody and his target (postmodernist literary critics) was different than Chomsky's (1959) audience (linguists and psychologists), the situations are in some respects similar. Just as most social critics probably are not likely to understand the finer points of physics and mathematics, most linguists are probably not familiar with behavioral psychology. Sokal (1996b) claimed his parody was intended to have been detectable as such by any competent undergraduate majoring in physics or mathematics; the problems with Chomsky were sufficiently extreme that anyone well acquainted with behavior analysis should know that something was awry with it. Sokal (1996b) suggested that the editors who accepted his parody did not consult someone who actually was sufficiently competent in the subject matter; given that there was no accompanying note of dissent from a knowledgeable person, it seems likely this was the case with Chomsky as well.

Chomsky (1959) was not the only review of Skinner (1957), of course. Knapp (1992) reviewed several others that were written by people in a number of disciplines. He noted that, while not uncritically accepting, they were relatively neutral or positive and often included constructive suggestions for expanding Skinner's analysis or integrating it with other views. In contrast, as MacCorquodale (1970) and others have noted, Chomsky was extremely negative and gave Skinner credit for getting very little correct. These differences between Chomsky's review and these others may explain why only Chomsky's has been so influential. Chomsky presumed a mentalistic standpoint wherein the presence of unobserved entities within the person was assumed to be necessary to understand behavior. It is reasonable to suppose that someone with mentalistic views who was not well acquainted with behavioral psychology might miss the psychological errors in Chomsky entirely. For someone looking for a bludgeon to use against behaviorism (and, as Skinner [1957, pp. 274–275] would imply, there probably were many), Chomsky (1959) would be the perfect citation to assert the folly of applying rat- and pigeon-psychology to complex human behaviors.

Nevertheless, not only are there serious problems with Chomsky's understanding of the subject material, but, as demonstrated above, in many instances he did not accurately portray Skinner's words or their contexts. Behavior analysis is a difficult topic for many, and *Verbal Behavior* is a difficult book, so problems in understanding the material would not be surprising; Chomsky's failure to set aside his mentalistic assumptions may have made understanding all the more difficult. However, Knapp (1992) fails to mention any serious problems of misunderstanding the material among the reviews he covers despite the bulk of them coming from outside behavior analysis. Furthermore, such an explanation does not apply to many of the errors in Chomsky (1959) discussed above; many of these are unrelated to the technical aspects of the material, or even when they do contain technical material, the citations are obviously in error. It would be misleading to portray Chomsky as a radical

behaviorist by selectively quoting him saying "Pavlovian and operant conditioning are processes about which psychologists have developed real understanding" (p. 38) and "Reinforcement undoubtedly plays a significant role [in language acquisition]. . ." (p. 43). Nevertheless, Chomsky similarly misrepresented Skinner's work. If Chomsky actually was influential in the cognitive revolution, the quality of his review suggests those he influenced were not familiar with behaviorism (or at least Skinner's variety), rather than making an informed rejection of behaviorism in all forms.

Nevertheless, despite the problems with Chomsky (1959), other academics should not be discouraged from venturing beyond the areas in which they are trained; on the contrary, disciplines have often benefited from relevant borrowings from other fields. For example, behavior analysis was heavily influenced in its origins by importations from physiology such as Pavlov (1927), and behavior analysis has in turn influenced other areas such as education and animal training. But while such explorations are to be encouraged, they must be done cautiously. Misrepresentation of work in psychology has been previously documented, and the misrepresentation of Skinner's work and behavior analysis by those outside the field has been common (e.g., Gaynor, 2004; Todd & Morris, 1992). Academics in any area should be aware of the limits of their knowledge and, when faced with confusing material, may find it beneficial to consult someone who knows more. Similarly, editors reviewing interdisciplinary papers should take appropriate measures to ensure the quality of such papers, such as consultation with relevant experts and basic checking of logic and quotations against the source material. Real-life examples as egregious as Sokal (1996a) are (hopefully) rare, and with the proper care will continue to remain so.

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