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The Case Against B. F. Skinner 45 Years Later: An Encounter with N. Chomsky

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Chomsky's (1959) review of *Verbal Behavior* (Skinner, 1957) has been hailed as the most influential document in the history of psychology. Although many rejoinders to Chomsky's review have been published, their impact has been minimal. Moreover, Chomsky has not answered them in detail. To invite Chomsky to revisit a number of matters concerning the review, he was interviewed. The principal topics addressed by Chomsky were (a) historical factors associated with behaviorism after World War II; (b) circumstances surrounding the preparation of the review; (c) likely compatibility between formal and functional analyses of language; and (d) Chomsky's current point of view on aspects of the content of his review and on the evolution of behavior analysis.

Key words: behaviorism, cognitivism, Chomsky, Skinner, *Verbal Behavior*

The review of Skinner's *Verbal Behavior* (1957) by the linguist Noam Chomsky (1959) has seemingly had a deep impact on research trends in psychology and attitudes toward behaviorism among several generations of psychologists. Leahey (1987) stated that "Chomsky's review is perhaps the single most influential paper published since Watson's Behaviorist manifesto" (p. 347). According to Arthur W. Staats, psychologists who study language from different behavioral standpoints were affected: "There was a group called Group for the Study of Verbal Behavior. O. H. Mowrer, C. E. Osgood, J. Deese, L. Postman, and myself, among others, all people studying verbal learning were there. They were nominally behavioral, but they weren't radical behaviorists. There were none at that time. They were behavioral but without a specific behavior-analytic background. Chomsky's article really affected them. It had a big impact" (A.W. Staats, personal communication, January 18, 2004). Knapp (1992) reported that from 1972 to 1990 Chomsky's review "was cited once for each two citations of *Verbal Behavior* itself … perhaps a unique relationship [between a book and its review] in the history of social sciences" (p. 87). According to Marc Richelle, a European commentator of Chomsky's review, this "might just reflect the fact that many scientists satisfy themselves with second-hand sources" (M. Richelle, personal communication, March 2, 2004).

Outside the field of behaviorism, Chomsky's paper is considered to be a classic, and is cited as final evidence of the inadequacy of behaviorism as a general framework for animal behavior and human affairs. "Chomsky's paper *demonstrates* [italics added] that verbal behavior cannot be explained by Skinner's functional analysis" (Fodor & Katz, 1964, p. 546). Smith (1999) stated that, "[Chomsky's] review of Skinner's major book … [is] perhaps the most devastating review ever written. … [It] sounded the death-knell for behaviorism" (p. 97). Probably thousands of students in cognitive psychology classes throughout the world have been confronted with Chomsky's re-

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view as conclusive evidence of the case against behaviorism. This last example may not be naive, inasmuch as a number of authors have considered the review to be not only a criticism of Skinner's book but also a foundational text of cognitive psychology. Smith claimed that the review "laid the foundation for current mentalist linguistics and cognitive science more generally" (1999, p. 97). Mehler concluded that "the decline of behaviorism appears to be linked to the birth of modern psycholinguistics" (Mehler, 1969, as cited by Richelle, 1973/1976, p. 209). The fact that the opening issue of *Cognition* began with a long article by Chomsky, restating his review of *Beyond Freedom and Dignity* (Skinner, 1971) published in *The New York Review of Books* (Chomsky, 1971), may suggest that both facts are connected. It should be noted too that the limited influence of *Verbal Behavior* on research may be attributable to other reasons (e.g., Hayes, Blackledge, & Barnes-Holmes, 2001; McPherson, Bonem, Green, & Osborne, 1984).

Initially, behaviorists showed little interest in Chomsky (1959). Skinner himself found the review difficult to answer. He considered Chomsky's tone to be emotionally charged, and the content to be lacking basic knowledge of behavioral analysis: "Chomsky simply does not understand what I am talking about and I see no reason to listen to him" (as cited by Andresen, 1991, p. 57). Julie S. Vargas has stated that "Skinner felt that by answering critics (a) you showed that their criticism affected you; and (b) you gave them attention, thus raising their reputation. So he left replies to others" (J. S. Vargas, personal communication, July 7, 2003). Nevertheless, rejoinders took no less than 8 years in coming (Andresen, 1991; MacCorquodale, 1970; Richelle, 1973/1976; Wiest, 1967). Skinner, in "A Lecture on 'Having' a Poem" (1972), at last mentioned the review, albeit briefly. None of the rejoinders were systematically revisited by Chomsky, who has mentioned the subject a number of times with no virtual modification of his earlier positions (e.g., Chomsky, Place, & Schoneberger, 2000; Rondal, 1994).

What follows is an interview with Noam Chomsky. He was invited to revisit a number of aspects surrounding the publication of the review and, in addition, to address some of its likely shortcomings. The interview took place at the Massachusetts Institute of Technology (Cambridge, Massachusetts) on March 23, 2004, in his office at the Ray and Maria Stata Center. The final part of the interview was conducted by exchange of e-mail correspondence from June 2004 through January 2005. Chomsky edited the interview manuscript before its submission for publication. Where necessary, the author has added related references in square brackets and footnotes for a more balanced understanding of the statements in the text.

This topic is still of great interest to behavioral and nonbehavioral readers. This interest can be inferred from the number of references and articles devoted to this subject. A search in *PsycInfo*, with the terms "Chomsky" and "Skinner," identified 340 results for the years 1996 to 2005 and 72 for the years 1966 to 1995. These findings suggest the unresolved nature of this polemic and the merit of further elaboration on this topic. This interview clearly portrays Chomsky's current views on behaviorism when confronted with recent developments in behavior analysis. It highlights and provides significant detail of Chomsky's account of the circumstances surrounding the publication of his revision. This interview is the result of 2 years of contact with Chomsky, and it is the first in its subject area conducted by a behavioral interviewer.

What intellectual or political events favored behaviorism in the early 1950s?

Chomsky: It has a tradition in the United States that goes back to before the 1920s; and elsewhere as well. In the 1950s, after World War II, there was a rather unusual period in the United States. The U.S. was the richest country in the world before the war and it had been for a long time, but intellectually and culturally it was kind of backwards. If you wanted to study physics you went to Germany, if you wanted to study philosophy you went to England, if you wanted to be a writer or an artist, France. Being in the United States was like being in central Idaho today. It was not the intellectual and cultural center by any means. It was also not the major political force; it was in its own region but not a global player, like Britain, let's say. But that all changed with World War II; it ended with every other industrial society seriously harmed or destroyed. The United States emerged far richer than it had been. It ended up having literally half of the wealth of the world and an enormous dominance in other respects. And there was a period of triumphalism about what those bad old Europeans weren't doing properly and we had to tell them: now *we* are going to run the world intellectually as well, culturally as well. That is part of the reason why we are so unaware about earlier history, which is just dismissed as irrelevant, and try to do everything from the start.

And there was a lot of prestige surrounding science and technology, in part, because we had worked on technological developments. It immediately came to biologists like Watson and Crick and their predecessors how biology could be related to general biochemistry. Shortly before the war, chemistry and physics had not been united, so for the first time it looked like a unified science involving core physics, chemistry, central parts of biology, and so on. And the next question was okay, let's bring in the mind and behavior, the next frontier of a unified science. We'll do it the American way, not that old European way. In this context, radical behaviorism just fit easily. In fact, the study of human affairs was called behavioral science. It was a very strange notion. 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. But the study of behavior is like calling physics "meter-readings science" because meter readings are the data. But in a serious field, you wouldn't identify the subject with the study of the data. Behavioral science was so superficial that history, sociology, psychology were all called the sciences of behavior, which means the study of data. Of course this was never going to get anywhere. But it did have the reputation, the feel that American hard parts of science would not mix with that old European nonsense. And it just swept the intellectual domain in the 1950s (i.e., psychology of course, philosophy, history and other fields). And it was in this general context that radical behaviorism quickly came to enjoy enormous prestige. Right here in Cambridge was the center of all this. When I got here, by the year 1950, this was the core of that.

What were the reasons that made you write the review of Skinner's Verbal Behavior?

Chomsky: There were a few people, not many, a small group of graduate students—I could actually name them—who just didn't believe the orthodoxy. And Skinner's work was like the core text that was being read all over. It was studied in psychology, in philosophy, and in other fields. That basically solved the problem: There were no more deep problems, it was just a matter of adding more details about reinforcement, stimulus–response and so on. Personally it just looked crazy [to me] … and so did it to a few other people.

His book was circulating around 1950. Before that, it had been his William James lectures, and everybody read them before the book appeared. So in the early 1950s this is what the graduate students at Harvard had in philosophy as orthodoxy. I believe it was extremely damaging to the field; it was undermining the possibilities of a scientific work in any of these areas. So I actually wrote the review before the book was published.

Although many reviews were published on Skinner's book (e.g., Broadbent, 1959; Jenkins, 1959; Osgood, 1958) yours had a greater impact, in fact, an impact without precedent in the history of psychology. What do you consider to be the reasons that made the paper so influential?

Chomsky: I suspect the reason was mostly the timing. I mean, by the time the review appeared in 1959, the foundations of behavioral psychology were already beginning to be shaken, and it was still considered orthodoxy. If you read Quine, probably the most influential Anglo-American philosopher in the late 20th century, his book *Word and Object* [1960], is basically some Skinnerian orthodoxy[1], virtually in the 1960s. It was still dominant but not unchallenged. Within a few years, all sorts of evidence was coming in, showing that this couldn't be right. In the review, I discussed some of the work just beginning to come in from European comparative psychology and ethology. Timbergen and others [Hinde & Timbergen, 1958] showed that this picture of animals couldn't be correct. Work on linguistics was beginning, and it showed that language couldn't possibly work like this. Cognitive psychology was just beginning; it was like an interactive amalgam involving very few people. It was beginning to get results. By the early 1960s, a couple of years after the review appeared, there was internal criticism which shattered what was left of the foundations of the subject. Two of Skinner's major students, Keller and Marian Breland, went off into animal training. They were the main animal trainers, they wanted to train all the things, circus animals and so on. What they discovered was that this was just not working [Breland & Breland, 1961].[2] I mean, the trainers, the psychologists, they were actually using the instinctive behavior of the animal and slightly modifying them by a training routine. But then, the animals were just drifting back to their normal instincts, to their behavior, refuting all the theory. It's what is called instinctual drift. By the early 1960s, I remember giving talks in behaviorist psychology departments. I remember one case where someone, a young, well-known, respected and very good behavioral psychologist said, "We are very convinced that these things don't work for humans but you seem to be taking for granted that they work for animals, why do you assume that?" It was a provocative question. I supposed it worked for pigeons. He later began to study it, as did others, and it turned out that it didn't work for pigeons. Within a few years there was no way to uphold anything. Cognitive

[1] Skinner and Quine did agree on essential aspects of language. These aspects include the influence of context on the notion of meaning and the importance of language on self-awareness and private experience (see Malone, 2001).

[2] It should be noted that the Brelands did not intend to disparage behaviorism. They suggested the revision of three tacit assumptions, namely, "that the animal comes to the laboratory as a virtual tabula rasa, that species differences are insignificant, and that all responses are about equally conditionable to all stimuli" (Breland & Breland, 1961, p. 684). Referring to this work, K. Breland wrote, "Perhaps we did not state strongly enough our feeling as to the efficacy of operant conditioning in the control of organisms. This convictions is so 'old hat' with us that I am afraid that we sometimes forget that it is not shared by all American psychologists" (personal communication to B. F. Skinner, November 25, 1961; document made available by Robert Bailey).

science had essentially taken over the field and had moved toward new directions.[3]

A number of authors have suggested that behaviorism's decline and the beginning of modern psycholinguistics are not independent facts. What effect have your reviews of Skinner's works (Chomsky, 1959, 1971, 1972) had in spreading the cognitive psychology model?

That is really for others to answer. By 1971, radical behaviorism, or any other variant, had seriously declined in influence (perhaps outside philosophy, Quine's influence, particularly). There were many factors, work on language being one of them. But even from within behaviorism circles, the basic principles were being challenged. For instance, by the Brelands' work on instinctual drift, by Dulany and others on conditioning and awareness [Dulany, 1961, 1968],[4] and much else.

Skinner stated that "[Chomsky] missed the point" (1972, p. 345). A few authors have pointed out that some of the concepts attributed to Skinner in the review were not Skinner's (e.g., Hineline & Wanchisen, 1989; Luzoro, 1992; MacCorquodale, 1970; Wiest, 1967). For instance, (a) reinforcement by drive reduction (Chomsky, 1959, pp. 39–44), (b) the extinction criterion for response strength (Chomsky, 1959, p. 29), and (c) the neglect of grammar in Skinner's account[5] (Chomsky, 1959, pp. 56–58, see also MacCorquodale, 1970). How do you consider these claims?

Chomsky: I already responded 30 years ago, in a footnote to a review of Skinner's *Beyond Freedom and Dignity* [Chomsky, 1971]. Most is just inaccurate. Of course I discussed drive reduction but did not attribute it to Skinner. The review went far beyond Skinner. As for the rest, the authors missed the point of the review: There is an interpretation of Skinner in which he is taken literally, and it is false; there is an interpretation in which he is taken metaphorically, and it is a bad translation of ordinary mentalistic terminology into terminology borrowed from the lab and deprived of meaning. Their critiques are limited to pointing out that the latter interpretation is possible.

There may be a conceptual vacuity entailed in transferring theoretical elements used in the lab to normal human life. In the 1950s, scarce empirical data were available and most of Skinner's ideas about human behavior could have been regarded as lucubration. Fifty years have gone by,

[3] Chomsky's account of the timing of the development and decline of behaviorism and cognitive science should be contrasted with available scientometric studies demonstrating that (a) there are a number of indexes that indicate that behaviorism continued to be a growing field up to the mid-1970s; (b) the growth of cognitive science has been a long-term phenomenon that developed steadily from the late 1960s to the 1990s; rigorous studies show that a quick exchange of paradigms, as advocated by a number of cognitive authors, did not occur; (c) behavior analysis was not strictly predominant when cognitive science started to grow; (d) behavioral psychology has produced a stable number of publications since the mid-1980s and has displayed a slight steady growth since the mid-1990s (Mandler, 2002; O'Donohue, Ferguson, & Naugle, 2003; Pena, 2003; Robins, Gosling & Craik, 1999).

[4] Dulany has said, "In … [Dulany, 1961, 1968] I was challenging behaviorism in what I saw to be the most fundamental ways: specifically a challenge to the central behavior theory principle of the automatic action of reinforcement and more generally its philosophically naive suspicion of and rejection of consciousness. … At that time, I saw no reformulation of behavior theory principles that would be adequate for either verbal or nonverbal voluntary behavior, and still don't" (D. E. Dulany, personal communication, February 10, 2005).

[5] According to MacCorquodale (1970), "a theory that does not have special grammar-generating laws in it must still be capable of generating outcomes which have grammatical properties" (p. 90). In other words, although Skinner's account is reductionistic, it does not exclude causal pathways particular to "verbal behavior" (Skinner, 1957, Part III). Furthermore, autoclitic processes are considered in extensive sections of the book (Skinner, 1957, Part IV). Further comments on the compatibility of a functional and formal analysis of verbal behavior are available in Moerk (1992) and Richelle (1973/1976).

and a few hundred well-designed studies on the subject have been published. Many empirical reports suggest that these concepts and the applied methods derived from them have some usefulness beyond the laboratory (e.g., applied behavior analysis of pervasive developmental disorders, clinical behavior analysis, operant methods for language acquisition). Considering everything that has been published, would you say that, in particular spheres of human language, an operant analysis could have some heuristic value for human affairs according to the empirical data available?[6]

Chomsky: There is some usefulness in behavior modification, therapy, training, under special circumstances, including the cases you mention. There is quite a lot of use of experimental techniques in industry (say, in testing effects of drugs on animals), and in work in serious psychology. But that was never the issue and still isn't. There is precisely zero in the areas that he was making remarkable claims about. If the claims were made now, the verdict would be exactly the same.[7]

Several authors have pointed out that both analyses, Chomskean and Skinnerian, are not necessarily exclusive, and can even enrich each other (Moerk, 1992; Segal, 1977). Both theoretical bodies seem to have their own range of successful predictions and their own evolving research programs. Accordingly, it has been suggested that "the theory-choice [among Chomsky's and Skinner's proposals] is implicated in value-judgments" (Lacey, 1978, p. 131).

Chomsky: I do not know how to make sense of such comments. The reason is that I know of no theoretical body of Skinnerian work, and the few research programs that remain have to do with quite different topics. To my knowledge, what remains of Skinner's work is a collection of very useful experimental techniques. I do not see any theory choice. And if there were, it would not be a matter of value judgment. If two research programs and the theories they yield are compatible, then there is no issue of choice; we accept both of them, and try to unite them. There is no value judgment.

Is a formal and functional analysis of language necessarily exclusive?[8]

Chomsky: Certainly not. They are both pursued all the time, by the same people in fact. By me, to take one example.

"In his speculations on human behavior, which are to be clearly distinguished from his experimental investigations of conditioning behavior, B. F. Skinner offers a particular version of the theory of human malleability" (Chomsky, 1972, p. 12). It can easily be inferred from these words that Skinner's work is valuable within the limits of conditioning and animal behavior. Nevertheless, the criticisms found in Chomsky (1959) are quite deep and relevant to basic concepts

[6] In a previous communication Chomsky has stated, "I'm not sure what you mean by verbal behavior research. There is some work called that, mostly rather narrow behaviorist. If that's what you mean, I don't follow it very closely, and it seems to me extremely uninteresting" (N. Chomsky, personal communication, June 5, 2003).

[7] MacCorquodale has stated that "until the hypothesis is tested the literal (nonmetaphoric, nonanalogic) 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 … depends upon the … 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" (1970, p. 91). It should be noted that since MacCorquodale's rejoinder, a number of projects in the applied fields of verbal behavior have been successfully completed (e.g., Goldstein, 2002).

[8] Note that *functional analysis*, *rule*, and other terms may have quite different meanings from behavioristic and mentalistic standpoints (e.g., Dulany, 1997; Malott, 1992).

(e.g., reinforcement, stimulus control, discrimination) that are called "empty." Must we consider that the validity of these concepts depend on the class or complexity of the behavior they refer to?

Chomsky: The inference goes much too far beyond what I said. Skinner's "experimental investigations of conditioning behavior" left useful experimental techniques, which are widely used: for example, in the pharmaceutical industry; and sometimes in serious experimental psychology. But they showed very little about how animal behavior develops or is carried out. In fact, even the existence of conditioning as a psychological phenomenon has been seriously questioned by some of the most outstanding cognitive neuroscientists, Randy Gallistel to take the best-known case.[9]

The concepts that remain are experimental techniques. There are few if any theses of more than the most limited significance, whether for pigeons or mice or any organism.

Some authors have claimed that the review, although making valuable points, was written in an "angry" tone. For instance, MacCorquodale states that "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" (p. 84). In Skinner's own words, "I have never been able to understand why Chomsky became … angry when writing about me" (personal communication to S. Murray in 1977 as cited by Andresen, 1991, p. 57). Considering the epigraph about reinforcement concept as an example, one would say that the language used may probably go beyond what a methodological criticism would require (i.e., "bar-pressing experiments," "perfectly useless," "tautology," "vacuous," "looseness of the term," "entirely pointless," "empty," "no explanatory force," "paraphrase," "serious delusion," "full vagueness," "no conceivable interest," "quite empty," "notion," "no clear content," "cover term," "pointless," "quite false," "said nothing of any significance," "play-acting at science" from Chomsky, 1959, pp. 36–39). Is this point of view acceptable?

Chomsky: I checked to find the context. Here it is: "This is a perfectly appropriate definition for the study of schedules of reinforcement. It is perfectly useless, however, in the discussion of real-life behavior, unless we can somehow characterize" [Chomsky, 1959, p. 36], and so on. That is a simple factual statement, politely describing where the notion is "perfectly appropriate," and pointing out, accurately, that it is "perfectly useless" unless the conditions spelled out can be met. The proper answer is not to say "it is angry," so I cannot respond. It is to show how the notion is useful if those conditions are not met. I checked again. Here's what it says, "As reinforcement is defined, this law becomes a tautology" (footnote stating, "This has been frequently noted," Chomsky, 1959, p. 36). Again, there is nothing angry about repeating a frequently noted factual statement. The notion of "tautology" is descriptive. It is not a four-letter

[9] It should be noted that Gallistel has mainly elaborated on classical conditioning (e.g., Gallistel & Gibbon, 2002), although he has recently stated, "I think the perspective I have been arguing for undermines the idea of operant conditioning as a distinct process. I am currently preparing a paper based on new experiments with matching in the mouse that, I believe, further undermine the idea that the animal adjusts its behavior on the basis of the rewards that the behavior has produced, which is, of course, the key idea in operant conditioning. One has to make the subtle distinction between whether what the animal learns about the world through its behavior (e.g., food is found at that location with frequency X) and what the animal learns about the effect of its behavior on the world (going to that location produces food with frequency X). Our results suggest that it is only the former that matters, whereas, at the theoretical level, emphasis was always placed on the latter in understanding operant behavior" (personal communication, August 3, 2005).

word. If the common observation is inaccurate, let's hear why. The proper response is not to pout about how it is "angry." I don't have time to check the contexts below. But I would be glad to do so, if you would like to provide them. However, the first two examples are quite appropriate and straightforward.

It is not relevant here, but the reaction you quote is particularly offensive in context. Recall the character of the book, and the acolytes, with extraordinary claims about their amazing achievements and contemptuous dismissal of very extensive and hard work which they did not even feel any need to learn anything about, in the light of the self-image they were projecting.

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