

Chomsky's Nativism Reconsidered

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The preceding article was written in the spring of 1981 as a term paper in a graduate linguistics course at the University of Massachusetts and was later included by invitation in a collection of essays on verbal behavior (Chase & Parrott, 1986). I had long considered Chomsky's criticism of Skinner to be just a polemical exercise with few valid challenges to a behavioral interpretation of language, but I felt that one should not merely dismiss his criticisms without considering his alternative proposals, and I embarked on an earnest program to learn them. I admit that I hoped to find fault, and as the paper reveals, this ambition was fully gratified. But although I was not disinterested, I realized that, to make a useful contribution, criticism must be sound, and I aspired to a balanced appraisal. In this regard, I believe that Chomsky's (1959) review of Skinner's (1957) *Verbal Behavior* will eventually prove to have been harmful to linguistics, however inspirational it appears to have been until now. By misrepresenting the power of behavioral interpretations, Chomsky persuaded a generation of linguists to dismiss the law of effect as an important variable in the interpretation of grammar. It is still commonplace among linguists to remark that, however formidable the difficulties facing their position, it is the only coherent proposal on offer (e.g., Cook & Newson, 1996, p. 103). My several linguistics professors always treated my objections with humorous indulgence, as one might gently tolerate a fanatic who believes that the

world is flat, but otherwise they paid no attention. It is as if "only one path emerged in a yellow wood"; perforce they have taken it and have lost much time.

I have had nearly two decades to reconsider my arguments, and as Schoneberger (2000) reveals, Chomsky's position has evolved considerably during that interval. He has abandoned transformational rules in favor of "principles and parameters," and they in turn are yielding to his "minimalist position." A reappraisal is due.

First a correction: I argued in my paper that a weakness in Chomsky's position is that it depends upon the grammatical intuitions of native speakers, and that this is a shaky foundation for a formal system. But as Schoneberger points out, Chomsky regards grammatical intuitions as only one line of evidence, and by no means a final arbiter of grammaticality. In his view, the linguist is a kind of detective, weaving together wisps of evidence from every possible source into a theory of grammar. Grammatical intuitions are a kind of behavior and are therefore seen as subject to all of the irrelevant disturbances inherent in any linguistic performance. Although this point blunts one of my criticisms of Chomsky, it does so only by removing his system further from the kind of data behaviorists regard as fundamental. Moreover, the force of most of Chomsky's arguments depends upon consensus that certain expressions are, or are not, well formed; the more suspect this consensus, the more tentative the argument. So far as I can see, the foundations of Chomskyan linguistics remain shaky.

However, because we are all seeking a natural-science account of language, we might ask whether we might find a middle ground with Chomsky. One

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might argue that we are merely interested in different aspects of language: Chomsky in structure, we behaviorists in function. True, Chomsky remains dismissive of behavior analysis and indeed of any general learning account of language, any account that does not appeal to an innate language-specific faculty. But even this latter claim need not be incompatible with behaviorism. Many species have "faculties" peculiar to their way of making a living. Rats have been trained to sniff out land mines, and no one would dream of using pigeons for that purpose. There is nothing implausible about the proposal that the "language faculty" is one of the adaptations peculiar to our species, and there is a sense in which this is obviously true. But so conceived, this faculty may be a heterogeneous assortment of characteristics such as a nimble tongue, control of rapid integrated sequences of motor responses, sensitivity to reinforcement by a wide range of arbitrary stimuli, the ability to adapt a well-practiced motor sequence to the demands of an arbitrary task (as when a man juggling oranges is passed a baton and doesn't miss a beat; the "frame" of juggling is filled in with precise molecular responses appropriate to the object being juggled). These characteristics are presumably only quantitatively different from those of other animals; in any case, they are not specific to rules governing reflexive pronouns, phrase structure, question formation, and so on. Philosophical differences between Skinner and Chomsky are profound and fundamental (see Palmer & Donahoe, 1992, for a discussion of Chomsky's essentialism and Skinner's selectionism), but the dispute about verbal behavior is narrower. The controversy is not whether verbal behavior has both ontogenetic and phylogenetic antecedents but whether there must be innate mechanisms that constrain the ordering of elementary verbal responses.

The terms of this dispute have not changed since 1981. Chomsky's specific proposals about grammar have

changed, but his nativist claims have remained the same. To recapitulate, his argument proceeds thus: Native speakers know many things about the syntax of their language; they cannot have inferred them from their observations; they have not received relevant instruction from parents and teachers; therefore this knowledge must be innate. In the 1970s this argument was illustrated with rules of transformational grammar; today the same argument is illustrated, unchanged, with principles and parameters. (See Cook & Newson, 1996, for recent examples. These authors correctly point out that analogous arguments are not restricted to syntax but can apply to any domain in which one cannot imagine an explanation; p. 85. They cite cheerfully, and with no note of concern, that before Darwin offered an alternative interpretation, the same form of argument was used to claim that the adaptiveness of organisms implies the existence of a creator.)

There is nothing absurd about the proposal that grammar is innately constrained. Everyone accepts that nest building, courtship rituals, maternal behavior, songs, and many other examples of species-specific behavior in other animals are innately constrained. However, grammar is not behavior but a model of behavior. Nativist claims about grammar must be translated into physical or biological terms before the hypothesis can be evaluated. There is now a relevant literature on the nativist interpretation of grammar (see Rice, 1996, for one collection of papers). However, the contributors to this literature all accept Chomsky's argument as a starting point for discussion rather than as a hypothesis needing support. No one has identified relevant structures or mechanisms; no one has suggested plausible evolutionary origins, nor has anyone explained how an innate grammatical module could respond to raw, unanalyzed stimuli or how it could initiate or influence actual behavior. Some have attempted to justify the evolutionary origins of language (Pinker, 1994; Pinker & Bloom,

1990). Unfortunately, these suggestions merely remind us that language is wonderfully useful; they do not justify Chomsky's model of grammar; they do not explain, for example, why one's genes would suffer a competitive disadvantage if one used a reflexive pronoun to refer to an antecedent in a different clause. Chomsky's nativist claims today, like those of two decades ago, rest not on direct evidence or even plausible interpretation, but on logical arguments. Unfortunately, his arguments have no force.

Let us begin with the claim that "native speakers know various rules of syntax." One might raise petty objections to this claim (hardly anybody knows rules of syntax; the word *know* has no content; etc.), but we can at least accept that people's observed verbal behavior can usually be modeled by the grammatical rules of the linguist. But these rules are formal models derived from observations of behavior. At the level of our observations, there are not two variables, behavior and rules, just one: behavior. That one can model behavior does not imply that the behaving subject "knows the model." A pigeon on concurrent schedules of reinforcement knows nothing of the matching law; in flight it knows nothing of principles of aerodynamics. But according to Chomsky, grammatical rules are not merely models of behavior; they are things that native speakers know. The model has slipped out of the linguist's notebook into the speaker's head. But this scheme is not a fact, something that can lend force to an argument; it is a hypothesis.

The hypothesis is appealing only because it appears to solve a mystery: Why do people speak in the orderly ways they do and not in other ways equally simple, or even simpler? However, in order to offer an explanation of a puzzling phenomenon, an account must interpret it with established terms that are not themselves as mysterious as the phenomenon to be explained. This point distinguishes sharply be-

tween Skinner and Chomsky. However unsatisfactory one might regard Skinner's accounts of grammar, and opinions differ widely on this point, his account offers an actual explanation. That is, he interprets the phenomena of verbal behavior in terms of principles that have an independent status: The principle of reinforcement rests on a wealth of highly replicable experiments; moreover, it is easy to suggest physiological mechanisms that are capable of implementing the principle (e.g., Donahoe & Palmer, 1994); finally, the principle has evident adaptive significance and is easily integrated with evolutionary accounts of complexity in nature. Chomsky's proposals share none of these features. He resorts to nativism by default, because he can imagine no alternative, but the nativist account is just as mysterious as the phenomenon to be explained. As argued earlier, if a feature of syntax is arbitrary and without discriminable effect, differential selection cannot get a purchase on it either during the organism's lifetime or over evolutionary time.

This point does not refute Chomsky's proposals; it merely reminds us that they do not yet comprise an alternative to Skinner's. Chomsky and his followers must identify empirical principles and integrate their proposals into the broader field of biology before they can claim to offer an adequate alternative interpretation of verbal behavior.

As I argued previously, a more fundamental objection than that his theory is incomplete is that, in framing his hypotheses, Chomsky explicitly retreats from the physical and biological world to an essentialistic world of "ideal speakers in a homogenous verbal community." In this world the sentence is taken by fiat to be the appropriate unit of analysis. Sentences are not considered to be behavior but strings of symbols, and words are treated as if they were printed on Scrabble® tiles, to be arranged and rearranged on one's tray. Some arrangements are permitted; some are not. This is a sterile world,

untroubled by stimuli and unconcerned with behavior. In this world, according to the principles and parameters theory, a child hearing speech is effectively presented with strings of tiles tagged as "noun," "verb," "subject," and so on, and as a result, switches are set "in the child's brain." If verbs and prepositions appear on the left of their respective phrases, then their language is a "left-headed" language, and the "head parameter" switch is set accordingly. If declarative sentences appear without subjects, then the null-subject or "pro-drop" parameter is set accordingly, and so on. Japanese children will set their switches one way, Italian children another, and English children a third. According to this theory, the setting of these switches, in conjunction with other variables, enables children to speak grammatically within their respective verbal communities.

But children learn language in the real world of stimulus and response classes. Even if we were to grant that learning a language means setting switches in the brain, we should like to know what the actual stimuli are that are necessary and sufficient to set the switches. Because languages vary from one community to another, there can be no invariant property of the physical stimulus that sets the switches. For the system to work, the child must already be able to translate physical stimuli into strings of grammatical symbols. Chomsky does not tell us how the non-verbal infant is able to accomplish this translation, or in fact how anyone other than a trained linguist might do so. To the contrary, he appears to recognize that there are no operational criteria for identifying the abstract concepts of syntax. But any adequate proposal of language acquisition requires analyzing the interactions of a child with actual, not translated, stimuli.

Here, then, must be the middle ground we have been seeking: Both approaches ultimately rest on a fine-grained analysis of a child interacting with a verbal community. But this is no middle ground at all. The analysis

of the behavior of an organism in its environment is squarely within the behaviorist's domain, and no facts or principles can emerge from such an analysis that will be excluded from our science of behavior. Chomsky and his followers cannot postpone indefinitely the task of analyzing contingencies of reinforcement, for that domain lies between them and an adequate nativist hypothesis. But when they finally address themselves to the task they will necessarily find themselves, in effect, studying verbal operants. Once verbal behavior has been analyzed in terms of a child's interactions with a verbal community, it is unclear that there will be any work left for "parameter setting" to do. This criticism is still valid, despite the evolution of Chomsky's theory.

It is not likely, of course, that Chomskyan linguists will soon be analyzing contingencies of reinforcement. They regard the idiosyncrasies of experience as theoretically uninteresting. Chomsky distinguishes between "E-language" (external language, or language as behavior) and "I-language" (internal language, an essential, innate asset), but he has little interest in the former: "I-language is a central notion, but E-language, if it exists at all, is derivative, remote from mechanisms and of no particular significance, perhaps none at all" (Chomsky, 1991, p. 10). Moreover, a strong antipathy to behaviorism still pervades the entire field of linguistics. For example, the author of an article compatible with a behaviorist account explicitly distinguishes her perspective (interactionism) from behaviorism no later than the title of her paper, apparently lest the reader dismiss her work out of hand (Snow, 1996).

However, language is behavior, whatever one might wish, and linguistics will be driven ever closer to an analysis of behavior. Even Chomsky appears to be drifting, all unawares, in that direction. In his "minimalist program," Chomsky has moved much of the burden of syntax into the "lexi-

con,” a hypothetical library containing words and information about how they are to be used. For example, along with the entry for *persuade*, one might find a note that it is constrained to be followed by a direct object and that this object is to be persuaded *of* something or *to do* something. It is one of Chomsky’s universal principles (the “projection principle”) that these constraints affect the syntactic organization of sentences. (The reader may notice the kinship of this principle with Skinner’s concept of autoclitic frames; see, e.g., Donahoe & Palmer, 1994; Palmer, 1998; Place, 1992; Stemmer, 1996.) Because entries in the lexicon depend upon one’s experience, this shift of syntactic burden is an unwitting step in the direction of a functional account.

Nevertheless, as long as Chomsky and other linguists continue to regard the sentence as the fundamental unit of verbal behavior, it is unlikely that our perspectives will converge substantially. Behavior analysis determines analytical units empirically, following methodological precepts that evolved from the experimental analysis of behavior (see Skinner, 1935, for a discussion); linguistics has no comparable system for determining units of analysis, so such decisions are left to the intuition of the linguist. Occasionally sentences do function as units, but more commonly the functional unit is smaller. For example, grammar can provisionally be understood as autoclitic frames, with insertion of variable elements determined by the context of the utterance. It may be true that most sentences are novel, but autoclitic frames are not, and they are the orderly and replicable units comprising grammatical phenomena. Chomsky dismisses as vacuous the claim that verbal behavior is under stimulus control, but his argument rests on the mistaken use of sentences as units. Understanding the stimulus control of autoclitic frames is a formidable challenge for behavior analysis, but it is by no means a vacuous or pointless enterprise. Unfortunately, we cannot turn for help to

Chomskyan linguists, the very people whose grasp of the subtleties of verbal phenomena is unparalleled, for we do not agree on appropriate units of study.

Although Chomsky surely remains the most influential linguist today, competing paradigms within linguistics, more congenial to a behavioral interpretation, may now be dominant. The editor of *Language*, reviewing a recent book by Steven Pinker, writes, “There is a fundamental ideological dispute between two schools of researchers studying the same data; Pinker’s school is decidedly in the minority” (Aronoff, 1999, p. 26). (In this context, Pinker’s school is allied with Chomsky, though Pinker is more tolerant of claims that learning is an important variable in language.) Curiously, the bitterness apparent in Chomsky’s (1959) review seems to pervade disputes within the field of linguistics as well (cf. Harris, 1993, reviewed by Mabry, 1995). A textbook on Chomskyan linguistics notes, “The opposition between these two approaches in linguistics has been long and acrimonious. . . . The E-linguist despises the I-linguist for not looking at ‘real’ facts; the I-linguist derides the E-linguist for looking at trivia” (Cook & Newson, 1996, p. 22). A prominent psycholinguist notes that “the negative stereotypes are exacerbated by a slightly paranoid sense within each group that its own views are failing to get a fair hearing and are losing ground” (Snow, 1996, p. 386).

It would be a mistake, then, to generalize from Chomskyan linguistics to all of cognitive science. Most behaviorists are offended by the tone of Chomsky’s review of *Verbal Behavior* and are annoyed by how reliably it is cited by cognitive scientists as definitive, but this petulance should not blind us to the extensive research by psycholinguists on language development that, if not explicitly behavioristic, does study functional relationships between verbal behavior and manipulable variables (see Gallaway & Richards, 1994, and Locke, 1993, for represen-

tative reviews). I see no reason to amend my earlier criticisms of Chomsky's nativism, but it would be detrimental to the progress of our own field to ignore the contributions of his entire discipline.

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