

## A Reply to "On Structure-Dependent Grammars: A Reply to Mabry" by Nathan Stemmer

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I'm flattered and honored to have received the reply by Dr. Stemmer. I have to say that he is quite correct that my paper (Mabry, 1993) will have little impact on cognitivists given how successfully they have ignored our literature during the past thirty years. My major concerns were, hopefully, to diminish the hold our traditional grammatical training has had in responding *experimentally* to the questions likely to be raised by grammarians and the like and to state more concisely what Skinner's position on grammar seemed to be.

Stemmer claims that I ignored structural dependence. Most frequently the term is used to refer to phrase structured grammars, like those made popular by Chomsky (Chomsky, 1957) where a sentence is segmented into noun phrases, verb phrases, and verb phrases are further said to hold a verb plus a noun phrase, and finally the immediate constituents which are hung below the rest in a sort of mobile type of arrangement. Specifically, Chomsky had stated that the phrase preserves its integrity when statements are said to be inverted to form questions and active statements are said similarly to be inverted into passive. Stemmer's footnote 2 gives the specifics of Chomsky's charge for the inversion of *The man who is tall is in the room*. Never, according to the footnote and Chomsky, would a child make the mistake of simply moving the first *is* to the position in front of the sentence to form a question as: *Is the man*

*who tall is in the room?* I'm not sure how many children Chomsky has observed, but I suspect that this is another of his intuitive judgments.

For the active-to-passive inversions, which interest Stemmer, he states "There is abundant behavioral evidence showing that speakers often emit verbal responses consisting of 'new' passive statements." Stemmer however does not cite any of these abundant studies. On the other of his concerns, statement-to-question inversions, there is Crain and Nakayama (1987) and mentioned prominently by Pinker (1994). In their case quite young children were reasonably good at asking the question "Is the boy who is sleeping on the blue bench?" given a sample statement of the complexity of "The boy who is sleeping is on the blue bench." It should be noted that the younger group of children tested (mean age 4-3) had nearly half the percentage of grammatical sentences (38%) than the group averaging a year older (80%). The mistakes that they did make were interesting however. The ungrammatical responses recorded in Experiment 1, for example, were by adding an extra auxiliary verb *is* or by making a separate final clause using a pronoun: "is he unhappy" and similar. They did not, however, make a question by moving the first auxiliary verb (*is*) to the front of the sentence. It is possible that their structuring of the task for the children could have influenced the result; the modeling of the correct phrase was always consistent. Nor do I think that the preservation of phrases or other functional units had caused Skinner any problems,

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and as I pointed out (Mabry, 1993, p. 80). I did say he was very wary of the traditional and largely arbitrary segmentation of speech into words, for example, and preferred to talk about the functional unity of larger or smaller units.

Although Pinker (1994) cites several examples of active-to-passive inversions all seem to be anecdotal or theoretical; he clearly would cite positive demonstrations if he could. It should be noted that I have not done an exhaustive search. There are several reasons why there may be few empirical studies which can demonstrate the uniqueness of a given response which is labeled an inversion. Analyzing the methodology used by many psycholinguists, including Crain and Nakayama, it would be difficult to show that instances are ever entirely new. In areas where there has been an attempt to control for uniqueness with parent diaries or pre-tests we are still left some serious concerns. Many studies do not submit intermediate forms to our judgment. In the case of Crain and Nakayama's studies, percentages of ungrammatical responses by age group and by type that they present, may be an indication of the type of intermediate responses to expect, but are not identified with individual children, even as to the individual ages. Clearly many phenomena similar to this are age-dependent with success shown to increase with increased age, and this too is apparent in the Crain and Nakayama's groupings. Unfortunately, most studies originate from a tradition that often discounts the vitality of environmental variables, and entering repertoires, and can then only point to age or the accompanying physical maturity as independent variables. I see no true way around the problem unless it is through a different type of experimental analysis.

Dr. Stemmer has put forth an argument, both in his reply to me and in a 1990 (Stemmer, 1990) paper that words like "hold" act as a relational term, and if a crude analogy is permitted, a fulcrum around which the inversion is made. Stemmer has detailed his arguments in the various papers which he cites. When I

commented on his assertions I said that they seemed at least plausible. Charles Hockett (Hockett, 1968, p. 89), a noted pre-Chomskyan linguist, in attempting to explain why a child would respond to a hot room as "It's three hot in here" as an extension of "It's too hot in here" submitted the following:

I'd like two pieces	:	I'd like three pieces:
We waited two minutes	:	We waited three minutes:
Give me two hot cakes:	:	Give me three hot cakes:
...	:	...
It's too hot in here	:	It's three hot in here.

(The three dot clusters in the above indicate other instances of the same exposures.)

This exposition bears more than passing resemblance to those given by Stemmer (1990) for the active-passive inversion. Stemmer states in the current reply that it is such "generalizations" from specific instances that produce the final "passive capacity." To paraphrase his argument from the 1990 paper: By learning several instances of "Daddy receives the book" and seeing daddy receiving books in the presence of someone talking about receiving books, etc. the child is able to respond appropriately when other things are received. Separate contingencies for "the book is received by Daddy" would complete the set so that received becomes a "relational term," and through generalization new instances are generated. In the example of "The man who holds the book receives the ball" words such as holds and receives are "relational words" which account for the transformation of the active to the passive voice by the child. Generalization of this "functional property" to new instances is called upon to clarify the problem in behavioral terms (Stemmer, 1990). In the present instance Stemmer adds the assertion that the inversion occurs at the proper structural boundaries, thus what he offers is a explanation of structure dependent grammar. If I'm not mistaken, Crain and Nakayama would

consider Stemmer's semantic features they apparently claimed was disallowed of the above study.

In his example, the emphasis on the traditional properties of generalization and "generalization" question begging that Stemmer's outcome is, by generalization. For assumed that a "functional property" generalization; the specialized pattern separately to generalizes a family. Stemmer says it is aware of Chomsky's generalization. Nevertheless, the detailed nature allows him to identify generalization to the outcome. While although plausible, Stemmer's account. What is finally account is a further analysis of the nature of such relations would be required to identify patterns of generalization as in Hockett's common extensions of the verb tense and phrase boundary (Skinner, 1957). Phrase boundaries, those are forewarned and safely tucked away, must meet the definition of a phrase clause, must be more than the usual noun and verb, a mystery.

There is one argument while a

consider Stemmer's relational terms as semantic features, rather than structural, as they apparently did when evaluating a previous Stemmer assertion which they claimed was disconfirmed in Experiment 3 of the above study.

In his examples, Stemmer puts major emphasis on the "generalization of functional properties." Unfortunately, his use of generalization, especially a "process of generalization" as he now states, is surely question begging. If things happen the way that Stemmer says they do then the outcome is, by common definition, generalization. For centuries it has been assumed that patterns similar to Stemmer's "functional properties" were examples of generalization; the term analogy applied to specialized patterns of sound changes and separately to grammatical inflections represents a familiar example in linguistics. Stemmer says in his reply to me that he is aware of Chomsky's (1959) objection to generalization as essentially "pointless." Nevertheless, Stemmer asserts that the detailed nature of his present account allows him to imply causality to the term generalization that he has used to describe the outcome. What it comes down to is that although plausible both Hockett's and Stemmer's accounts are really incomplete. What is finally lacking in Stemmer's account is a functional (i.e., experimental) analysis of the stimulus control properties of such relational terms. A similar account would be required to effectively characterize patterns of analogous extensions such as in Hockett's account and in similar common extensions or overextensions of regular verb tense endings, as well as Skinner's (Skinner, 1957) use of autoclitic tags. Phrase boundaries themselves, unless those are forever to remain unanalyzed and safely tucked away inside the organism, must meet the same challenge. The definition of a phrase, including Stemmer's clauses, must rest upon some better definition than the usual word class definition, noun and verb, which remain something of a mystery.

There is one final caution. So far I have argued while accepting the basic premise

that a question is an inversion of the corresponding declarative statement and the passive is an inversion of the active sentence. Many cases of supposed occurrences of inversions may not, however, involve simply the inversion of its opposite. Instead they could very well be a result of the induction of passives to passives or answers to answers, and so on, which are then bound with the inverted member of the proposed pair only by common thematic, formal or intraverbal elements e.g., "What did he say?" - "He said..." This is no simpler to explain perhaps, but not often tendered or examined as a possibility in a structural analysis. This non-inversion possibility is also reflected in the types of incorrect answers given by Crain and Nakayama's children in the above citation. For example, when the phrase contained terms such as *should*, which were less familiar to the children, some would substitute a more familiar, but equivalent, phrase than the one coaxed, e.g., "Is that boy *should* have an umbrella?", "Is that boy supposed to have an umbrella?", "Does that boy have to have an umbrella?" (p. 540). Much the same can be said of the mistakes made in the first experiment and alluded to above. These mistakes may have conformed, or not, to the "phrase structure" boundaries, but they clearly were not an inversion. The supposed inversions that did occur could have as easily been handled by a different analysis, e.g., echoing the experimenter's phrase preceded by an "is." Stemmer's acceptance of the problem, as posed by Chomsky's pairing of sentences on the basis of their being logical inversions, is in fact Stemmer's real connection to structural dependence; in this case the acceptance of the formal logical properties of the terms under examination. Otherwise Stemmer's proposed solution is largely a functional one, though incomplete, which Chomsky's anti-empirical, anti-associationist arguments would just as clearly reject. I think that it would be unfortunate if we allow Chomsky and others to both ask the questions and propose the agenda for answering them. I'm not at all opposed to examining such ques-

tions for merit. In fact, I attempted to propose several in my paper that have been raised by psycholinguists once we are able to separate them from the theoretical and methodological bias they represent.

I will repeat that I find Stemmer's suggestion to have real plausibility just as I accord the same to Hockett's proposal. Those, and Skinner's use of tags and autoclitic ordering as functional descriptions of grammatical behavior (Skinner, 1957) still require some extensive investigation. That was what I was hoping to promote. Perhaps this exchange will keep the issue alive.

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