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Author(s): D. Terence Langendoen

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examine it. One could bicker over omissions of some information and possible overemphases of other, but the extensive and authoritative coverage of the senses is impressive. It takes more than just a "sensory generalist" (Geldard's term) to write such a book in 1972.

Billy R. Wooten, *Brandeis University*
Donald C. Hood, *Columbia University*

The American Heritage Word Frequency Book

By J. B. Carroll, P. Davies, and B. Richman. New York: Houghton Mifflin, 1971. Pp. liv, 856.

The American Heritage Word Frequency Book is constructed to provide an index of the frequency with which words appear in the printed texts encountered by school children. Like the Kučera and Francis (1967) count for adult reading material, this count was prepared using computer tallying of selected samples of running text from a large and comprehensive corpus. Alphabetical and rank-access lists are provided. For the alphabetical list, a breakdown is given for each word, showing its relative frequency in each of the grade levels three through nine, and for each of the categories of material included. Separate rank lists by grade level are not given, making the selection of equal-frequency words at a given level difficult.

Although it delineates the scope of the vocabulary children encounter, the book is less likely to be useful to psychologists interested in language development than to teachers and lexicographers. Unlike counts using adult material, one cannot assume that the written material in the corpus reflects accurately the relative frequency of words in children's vocabulary, since this material is written by adults for consumption by children. This factor particularly affects the reliability of the grade-by-grade frequency breakdowns provided.

R. E. Warren, *Columbia University*

Phrase and Paraphrase: Some Innovative Uses of Language

By Lila R. Gleitman and Henry Gleitman. New York: W. W. Norton, 1970.

This is a beautiful book in several respects. I am particularly impressed by the authors' wit, sometimes gentle and sometimes outrageous; I am also impressed by their grasp of the march of modern psycholinguistics. A not inconsiderable number of persons have written synopses of recent developments in this field; the Gleitmans' second chapter is one of the best treatments currently available.

They also write about the Chomskyan revolution in linguistics (chapter 1). There they are much sketchier, as befits their purposes, but even so, their discussion of certain matters, such as the competence/performance distinction, is exemplary. They also raise there an issue returned to throughout the book: Does every mature speaker of a language possess equal linguistic competence? Chomsky, they point out, was quick to delegate most individual differences among speakers of the same language to performance, while Katz, writing in

1964 ("Semi-sentences," in *The Structure of Language*, pp. 400–416), essentially defined linguistic competence as being held in common by all speakers of a given language. This claim, say the Gleitmans (p. 12), "lacks the stamp of intuitive authority that marks the primary conceptualizations of generative grammar" and "requires from the believer [in it] a certain act of faith." (More recently, Chomsky has also made equicompetence a matter of definition: "By definition, a person knows his language [or several dialects and languages] perfectly"; *Problems of Knowledge and Freedom*, 1971, p. 21.)

Now, the chief behavioral clue to a person's linguistic competence is his ability to render grammaticality judgments about potential sentences of his language. Designing protocols that would enable an investigator to elicit such judgments freely and naturally is obviously extremely difficult; for this reason, most grammarians have resorted to the expedient of asking for the judgments of only an elite group, namely, linguistically trained informants. If one could, however, find a task that both taps competence directly and is commonly used by people in everyday life, it should be a relatively straightforward matter to appropriate that task for experimental purposes. Such a task, say the Gleitmans, is the rendering of paraphrases.

Clearly, paraphrasing one's own or another's remarks is a commonplace activity. It also appears to involve manipulating linguistic structures in much the way that linguists do when they study a problem of grammar. In fact, the ability of people to paraphrase is so taken for granted that there has been very little direct empirical study of this ability. In their survey of the recent literature, the Gleitmans turned up exactly one such study, by W. H. Livant ("Productive grammatical operations: I. The noun-compounding of five-year-olds," *Language Learning*, 1961, 12:15–26), in which the twelve compound noun phrases that can be built up out of the three words *bird*, *black*, and *house* were submitted to three different subjects, who were asked to interpret—to provide a paraphrase of—each. Livant's subjects were reported to have performed flawlessly. The Gleitmans could not replicate his results; their subjects had varying degrees of difficulty with the compound expressions, and none of their twelve subjects performed flawlessly.

This led the Gleitmans to undertake a much more systematic investigation of the ability of English speakers to paraphrase three-word compound noun phrases, and also to look more closely at the grammatical processes themselves that generate nominal compounds. Chapter 3 is a report of their linguistic investigations; chapter 4 gives the details of their experimental investigation into the ability to paraphrase; and chapter 5 concludes with some extremely interesting speculations on how people interpret semantically and syntactically deviant constructions (or try to).

The Gleitmans' challenge to the doctrine of equal linguistic competence has potentially explosive political implications. It is therefore of more than ordinary importance to assess the arguments they advance in support of their challenge. The most important piece of evidence that they present is that there is an overwhelmingly significant population difference in the ability to render correct paraphrases of three-word nominal compounds in English.

In their study, they divided their subjects, all women, into three groups. Group A consisted of seven graduate students and holders of the Ph.D. Group B consisted of seven undergraduates and college graduates who had no intentions of pursuing a graduate degree. Group C consisted of seven secre-

taries with high-school diplomas and no college experience. Correctness of the paraphrases was determined by two scorers, a linguist and a graduate student, on the basis of the Gleitmans' own grammatical description and "on the basis of [the scorers'] intuition about the correctness of the paraphrases" (p. 110). There was very high reliability between the two scorers, and all inconsistencies were resolved. In all, 144 separate compounds had to be paraphrased by each subject.

The Gleitmans subjected their data to a number of different statistical analyses in order to pinpoint more specifically the nature of the population difference in question. They distinguished among four different error types; they distinguished among the parts of speech of the words that they varied in the compounds (the two nouns *bird* and *house* were used in all the compounds); they distinguished between semantically plausible and semantically implausible compounds; and they distinguished between clearly syntactically well-formed and questionably formed compounds. Finally, the Gleitmans performed a retest, using the same 144 compounds as stimuli but requiring the subject to choose between a correct and incorrect paraphrase, which were presented to her on cards. What they found was that groups B and C made not only more but different kinds of errors; that certain constructions were more difficult than others, uniformly for all groups; and that the population differences persisted in the retest without significant change.

First, let us consider difficulty. Compare the expressions *black house-bird* and *house-bird black*. The former may be correctly interpreted as 'a house-bird that's black'; the latter either 'as black as a house-bird' (analogous to *coal-black*) or even less plausibly, 'substance for making house-birds black' or 'person who makes house-birds black' (analogous to *car clean* [?] or *chimney sweep*). On the basis of both intuition and grammatical analysis, the phrase *black house-bird* and its paraphrase are clearly intelligible and grammatical; the phrase *house-bird black* is both semantically implausible and grammatically queer, and its paraphrases are all implausible. The Gleitmans, accordingly, divided their stimuli into two groups, core and penumbral, core expressions being those that are totally unproblematic on both semantic and syntactic grounds; penumbral ones being all the rest. The results: group A was wrong for 7% of the core expressions and 17% of the penumbral; group C was wrong for 41% of the core and 77% of the penumbral (p. 155).

When group A went wrong, it was mainly a result of interpreting the compound with a different order of words (e.g., interpreting *house-bird black* as if it were *black house-bird*). Groups B and C, in addition, made large numbers of errors of grouping (e.g., paraphrasing *bright-bird house* as if it were *bright bird-house*) and errors that represent combinations of order and grouping (the limiting case; one could err no further).

Clearly, then, there is a large systematic difference in the behavior of these population groups on the Gleitmans' tasks. How do we interpret it? The Gleitmans argue that the difference cannot be accounted for by any known performance factor or combination of factors and that the difference must therefore be one of underlying competence. The conclusion does not follow from the premises, but suppose we grant the conclusion for the moment. The question still remains whether we have to do here with linguistic competence in the sense defined by Chomsky and accepted by the Gleitmans.

The answer here, I submit, is negative. I give three arguments. First, there

is some confusion in the Gleitmans' conception of linguistic competence (despite the clarity with which they expound the notion in chapter 1), and that confusion tends to be vitiating. Second, their characterization of paraphrase is inaccurate, and that inaccuracy is definitely vitiating. Finally, the experimental task failed to get at the underlying linguistic processes involved in the derivation of compounds, so that competence at interpreting compounds turns out to be no more reflective of underlying linguistic competence than is competence at interpreting individual monomorphemic words.

Concerning linguistic competence itself, we must distinguish between that notion and the much broader notion of verbal facility, which includes such abilities as writing and speaking well, making puns, and for that matter, doing grammatical analysis. Clearly, one cannot make a case for equal endowment of verbal facility. But it seems that the Gleitmans are too ready to conclude from the unequal endowment of verbal facility that linguistic competence is also unequally endowed. They say in answer to the potential objection that the population differences in their results may 'merely' be differences in ability or inclination to play with linguistic structures: "Still, some of the outcome of the experiments can be understood as a difference in the ability or inclination to manipulate linguistic entities consciously and systematically. But this distinction cannot fruitfully be called 'mere.' (Differences between Nabokov and the rest of us cannot so lightly be shrugged aside.) Whatever it is, it is hardly extralinguistic" (p. 170).

Granted their point about the inappropriateness of "mere," it is nevertheless mistaken to confuse the conscious ability to manipulate linguistic structures with the unconscious ability—true linguistic competence—on which it is founded. But even supposing that the population difference is linguistic, that is, has to do with different internalizations of English, it is mistaken to conclude that two groups, each of which has internalized the same grammar, except that one of them has an additional (in this case, optional) rule that the other completely lacks, have different linguistic competence by virtue of the different grammars that each has internalized. They merely have different grammars. To show a difference in linguistic competence, one would have to show that there was some rule or rules that one group could learn and the other could not. (For further discussion on this criterion, see below.) This the Gleitmans have not shown. At most, they have uncovered a dialect difference that correlates with educational status.

Next, concerning the notion of paraphrase, the following problem emerges. The Gleitmans define 'paraphrase' as "a sentence with the same meaning as some other sentence, a sentence assigned the same deep structure as some other sentence" (p. 3). Apart from the obvious quibble that since the Gleitmans' experiments did not involve sentences, their subjects did not, strictly speaking, render paraphrases, we may note that the ordinary, mundane phenomenon that goes by the name 'paraphrasing' is *not* the expression of exactly the same meaning in different words. The ordinary phenomenon is much less exacting than that; its purpose in everyday life is to clarify (and hence, to change, if necessary) the meaning of something that may have been misunderstood or realized as potentially misleading. Thus there is no reason to assume that people can regularly produce surface alternatives for a single deep structure simply because people can paraphrase in the nontechnical sense just given. Just as naive informants are not good at rendering grammaticality judgments, so, I

imagine, they are not particularly skilled at producing true surface alternatives of a given deep structure. At least, the Gleitmans have given no real evidence to the contrary.

The careful reader will notice, in fact, that I have consistently failed to use the word 'paraphrase' and its close relatives in connection with what their subjects did in the experiments. Rather, I used the more neutral term 'interpret.' This failure is a consequence of a refusal on my part to grant that paraphrasing is what they did, or better, what they were asked to do. My impression is that the subjects responded to the task as if they were being asked to *define* the compounds in question. If this is so, then the Gleitmans' results are hardly surprising, and hardly of any bearing on linguistic theory, because, as we all know even without the benefit of the relevant experiments, people differ systematically in their ability to define words, and these differences correlate with educational attainment.

This point leads naturally to my third reason for discrediting the conclusion that people differ in linguistic competence. The Gleitmans themselves point out that compounds are equivalent to morphologically simple nouns in that they are in a definitional relation with expressions consisting of a generic noun plus relative clause (p. 97); to illustrate, they give the examples *Eskimo Dog* and *Husky*, both of which may be defined 'dog of no particular breed that is used by Eskimos.' Now, it is true that compounds can be related, perhaps transformationally, to more elaborate syntactic structures which contain the elements that go into the compounds, and that people who can deal with compounds at all have internalized these rules (which are, moreover, recursive). However, the meanings of compounds are in general underdetermined by these syntactic processes, just as (in the limiting case) the syntactic properties of monomorphemic nouns underdetermine their meanings. Thus, there is no surprise at all in the finding that some people (the more educated) are better than others (the less educated) in providing definitions for compounds. Most crucially, the Gleitmans have not shown that the less educated groups have not internalized the fully regular compounding transformations. On the contrary, every one of their subjects *has*, simply by virtue of having understood the experiment's instructions.

I have one further objection to the Gleitmans' argument against the doctrine of equal linguistic competence. This has to do with their contention that because they could find no performance variables on which they could hang the observed population differences, the differences must have to do with competence. But there may have been performance factors involved that simply failed to occur to the Gleitmans. It is not unimaginable, for example, to suppose that had their group C subjects been high on marijuana they might have performed like group A subjects. More to the point, the experimental situation may have been sufficiently unfamiliar and threatening to the group C subjects that their performance was adversely affected. If the semblance of the laboratory situation had been removed, it is possible that their performance would have improved. For all their sophistication, the Gleitmans appear to have fallen into the same error that certain educational psychologists have made in their assessment of the intellectual abilities of ghetto children: bad performance that is inexplicable on grounds known to the experimenter implies impaired competence.

I conclude by returning to the criterion (mentioned above) by which it

could be ascertained that two groups differ in their linguistic competence. What must be demonstrated is that there is some aspect of general linguistic structure that is attainable by one group but unattainable by the other. Suppose, for example, a group of persons were found who seemed to speak English but who systematically lacked relative clauses in their speech and who could not understand English sentences containing relative clauses, and suppose further that relative-clause structures could not be acquired by their children even upon exposure to them on conditions favorable to learning. Then it might be correct to conclude that this group had a different endowment of linguistic competence, through to establish this, one would also have to demonstrate that the group did not possess any semantic equivalent to the process of relative-clause formation. On this criterion, clearly, the doctrine of equicompetence is much less vulnerable to refutation than the Gleitmans suppose, and deserves whatever little faith is required for belief in it.

D. Terence Langendoen, *Department of English, Brooklyn College,
and Ph.D. in Linguistics, City University of New York*

Carmichael's Manual of Child Psychology, Vols. 1 and 2

Edited by Paul H. Mussen. 3d ed. New York: Wiley, 1970. Pp. xii, 1519; xii, 872. \$30.00, \$20.00; \$40.00 the set.

Although the present two-volume set represents a third edition of the *Manual of Child Psychology*, it is more than a mere revision of the two earlier editions. As the editor points out, "this is a completely new *Manual* (p. vii)." Like the first and second editions, edited by Leonard Carmichael and published in 1946 and 1954 respectively, the purpose of the present work is to provide a "comprehensive and accurate picture of the current state of knowledge—the major systematic thinking and research—in the most important research areas of the psychology of human development" (p. vii).

It is indeed an ambitious undertaking to attempt to provide an accurate and comprehensive picture of the state of knowledge of the vast field of human development in only two volumes, even if the present revision has over a 1,000 more pages than the one-volume 1954 edition. This feeling is reinforced when it is recognized that 24 of the 29 chapters on the 'most important' research areas in child psychology are handled by single authors. Obviously, the biases of each particular author color not only those aspects of each area chosen for critical and evaluative treatment but also any conclusions drawn.

As the editor himself readily admits, the enormous growth in quantity of relevant literature, the high degree of specialization, and the changing tone of developmental psychology make it exceedingly difficult to produce a full, balanced, accurate, and up-to-date representation of the current state of the discipline. In some instances, then, the result is either an overly meticulous and overspecialized treatment of a narrow area of research, or a more or less general summary or survey of an area.

Child psychology has changed a lot in the last 15 years, and topics have been added that reflect current interest in applied problems and wider societal and educational issues. One chapter in volume 1 is devoted to the