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BOOK REVIEWS

The Psychology of Language. By J. A. Fodor, T. G. Bever, and M. F. Garrett. McGraw-Hill, New York, 1974, xviii + 537 pp., $18.00.

This book is currently the major text in experimental psycholinguistics. Any card-carrying member of the clan must have read it by now; and even if he hasn't, he can probably tell you what he thinks of it. Which is not as outrageous as it sounds. Fodor, Bever, and Garrett have provided a detailed account of what surely has been the dominant view of our field over the past 10 years or so. To review the book is to review the field. So, the issues are obvious: Are we doing psycholinguistics in a coherent way? Are our results sound and interesting? Are our methods defensible? Where do we go from here?

First, a word or two about plot. This is essentially a book about the relationship between linguistic knowledge and linguistic behavior. As the authors (hereafter FBG) put it, "we shall be trying to work out part of a theory of how certain facets of verbal behavior are controlled by the speaker's knowledge of the grammatical structure of his language" (pp. 6-7). According to FBG, this problem has two parts: it requires, first, a specification of what the speaker knows about his language and, second, a theory of the psychological mechanisms which apply this knowledge in the course of language use. FBG set out to solve these problems via the standard division of labor according to which linguistics supplies a hypothesis about linguistic knowledge and psycholinguistics attempts to determine how such knowledge is employed.

Within this framework, FBG begin by reviewing the "Hullian" psycholinguistics of the 1950s. They find this earlier tradition wanting because, although it offered an account of language learning and use based on established psychological principles, it assumed an inadequate characterization of linguistic knowledge (roughly, phrase structure grammar plus structuralist phonology). After reviewing the standard claims about the clumsiness of phrase structure, FBG nominate transformational grammar (1965 version) as a more nearly adequate description of linguistic knowledge and then turn to the major questions of the book: Does transformational grammar provide a psychologically accurate description of linguistic knowledge? If so, how is it employed?

The answers are familiar (*cf.* Fodor and Garrett, 1966). They add together to form what might be called the *weak theory of the psychological reality of transformational grammar.* In effect, the weak theory says that the structural descriptions provided by the grammar are psychologically real, but the rules used to generate those structures are not. In support, FBG marshal the results of a small army of experiments which appear to show that numerous kinds of experimentally obtained linguistic behaviors correlate with properties of both the deep and surface phrase markers but that comparable behavioral measures fail to yield correlations between linguistic behavior and properties of transformational derivations. Accordingly, FBG conclude that the structural descriptions supplied by the grammar provide a psychologically accurate account of what the language user knows about the structure of the sentences in his language. However, they also conclude that the rules used to generate the structural descriptions are not directly involved in the application of this knowledge. According to the terms of this hypothesis, structural descriptions are directly realized during language use: the comprehension of a sentence involves the internal recovery of its structural description; the production of a sentence involves the internal assembly of a surface form with a structural description appropriate to the speaker's intended message. Thus both comprehension and production are viewed as psychological processes which relate sentences and structural descriptions. Since the grammar provides an abstract description of the relation between sentences and structural descriptions, it places a strong constraint upon psychological models of comprehension and production by specifying their input and output. But the grammar does not further constrain the internal operations of these models. In this respect, FBG's position can be distinguished from an earlier and stronger theory of the psychological reality of transformational grammar according to which models of comprehension and production incorporate the grammar as a subcomponent and make direct use of the transformational rules in getting from a sentence to its structural description and back (Miller and Chomsky, 1963).

No one who knows the literature would doubt that FBG have given the stronger theory a proper burial. But what about the weak theory? Having lived with it awhile, I begin to wonder whether we should continue to believe it. I also have my doubts about its effects on the field. And I question whether it provides an adequate theoretical basis for psycholinguistics. Let me explain.

For openers: Is the weak theory true? To illustrate the issues involved, we can focus on one of the weak theory's major subclaims: that in the course of comprehending a sentence the listener determines its deep structure phrase marker, as specified by the grammar. This is a particularly critical claim because it is deep structure which is peculiar to the structural descriptions provided by transformational grammar and hence distinguishes these structural descriptions from those provided by other grammatical theories. So it is not surprising that a number of us have done experiments designed to discover whether deep structure is determined during comprehension.

Before considering those experiments, however, we need to take a minute to recall how the transformational linguist decides what a deep structure for a given sentence looks like. Simplifying a bit, there appear to be three main considerations: First, the deep structure should display the basic grammatical relations in a uniform way (e.g., the logical subject will always appear as the NP immediately dominated by S). Second, the deep structure should provide the syntactic information necessary for lexical insertion. Third, and most important for this discussion, the deep structure should be configured in such a way as to simplify the operation of the transformational rules which map deep structures onto surface forms. So, for example, Rosenbaum (1967) argues that the deep structure sentoid underlying the parenthesized complement clause in sentence (1) below must be dominated by an NP node in deep structure so that the passive transformation (among others) can apply to the sentoid as if it were a noun phrase in order to produce the passive sentence (2) in the ordinary way:

1. *Everyone knew (that Bill was an idiot).*
2. *That Bill was an idiot was known by everyone.*

To take another example, Chomsky (1957) generates the tense and number marker to the *left* of all other verbal elements in deep structure, not to the *right* of the first verbal element as it appears in surface structure. Again the motivation for this placement is transformation: it permits a single transformation (the famous rule of *affix hopping*) to apply generally to all verbal affixes in the derivation of the surface structure of the English verb.

Why bring all this up? Put somewhat simply, the point is this: why should we believe that deep structure phrase markers are determined during comprehension, when the characteristics of deep structure are partially selected just in order to simplify the operation of the transformational rules, which themselves lack psychological reality? So far as I can see, there is no compelling reason to adhere to such a belief. One can search in vain through the deep structure experiments which FBG review without finding any which test the psychological reality of a transformationally motivated aspect of deep structure. But these are the crucial tests. Experiments which assess the psychological reality of externally motivated aspects of deep structures are inconclusive. For example, suppose an experiment designed to assess the psychological reality of the basic grammatical relations yields positive results. This may indicate that the listener determines the basic grammatical relations during sentence comprehension, but it does not require the conclusion that he has done so by means of recovering deep structure. Deep structure is only one of many ways of displaying the basic grammatical relations. To demonstrate the psychological reality of deep structure, we need to assess its unique features. And, in general, the unique features of deep structure are just those which are motivated transformationally.[1] To date, such features of deep structure have neither been tested directly nor been supported indirectly by positive results on tests of transformational reality. Therefore, although it may be true that some of the information displayed in deep structure is determined during comprehension, it has not yet been shown that the complete deep structure is determined. Moreover, the failure to find experimental evidence for the psychological reality of the transformational rules makes it quite unlikely that such evidence will be forthcoming.

Unless, of course, transformational rules have claim to a psychological reality of some other kind. FBG imagine just such a possibility. The argument goes this way: In the course of writing transformational grammars, linguists have discovered certain constraints on the way in which transformations can be formulated and applied to phrase markers (for examples, see Ross, 1967). Violation of these constraints produces grammatical monstrosities of the most bizarre sort. Yet the constraints are difficult to state within the transformational rules, and they also appear to apply in a general way across rules. As a

[1]It may often be difficult to decide exactly which features of deep structure are unique. However, transformationally motivated features seem the best candidates for uniqueness since these features are introduced just in order to simplify the transformational rules, and transformational rules are unique to transformational grammar. The uniqueness of other features is usually arguable. For example, in 1968, I obtained a result which seemed to show that the effectiveness of a prompt word as an aid to sentence recall is an increasing function of the number of times that word appears in the deep tree of the memorized sentence (see Wanner, 1974). I argued that this result might demonstrate the behavioral effects of a unique feature of deep structure (number of appearances), and FBG echo this claim in discussing the result. However, at the time, I also noted that the effect might be due to correlated properties of semantic structure, and there is now some reason to believe that this is a better account of the effect (Anderson and Bower, 1973).

result, linguists have tentatively elevated such constraints into the theory of transformational grammar. Once elevated, a constraint places a limitation upon what transformational rules can accomplish, and the limitation takes on the status of a hypothetical universal. That is, if the theory of transformational grammar provides a correct characterization of human language, then it should be possible to write a transformational grammar for any human language. But this will only be true in case there are no languages requiring rules which violate the constraints stated in the theory. In fact, the universality of many of the constraints so far proposed has yet to be very thoroughly tested across languages. And it is a bit of an embarrassment when the constraint proposed on the basis of research in English is violated by a language as closely related as Dutch (Comrie, 1977). Nevertheless, FBG accept the claim that some constraints on transformational rules are (or will prove to be) universal. They then proceed to worry that the existence of such universal constraints may prove embarrassing for the weak theory of the psychological reality of transformational grammar. As they see it,

> there are linguistic universals which serve precisely to constrain the form in which information is represented in grammars (i.e., the form of grammatical rules). The question is: If these universals do not also constrain the form in which linguistic information is represented in a sentence-processing system, how is their existence to be explained? Surely, if universals are true of anything, it must be of some psychologically real representation of a language. But what could such a representation be if it is not a part of the sentence encoding-decoding system? (pp. 369-370)

FBG consider several possible answers, but their favorite is this: Suppose that the child comes equipped with a genetically determined language acquisition system which is biased toward the development of a transformational representation of linguistic structure. On this familiar assumption, the child learns a language by assembling a transformational grammar which is consistent with the growing mass of linguistic data to which he is exposed. However, given the failure of the strong theory of transformational grammar, we can be certain that even if it is true that the child does acquire a transformational grammar, that cannot be all that he acquires. The child must also learn procedures for understanding and producing sentences. The core of FBG's proposal concerns the manner in which such procedures might be learned. Thus they speculate that such procedures might "be constructed by a simple and general algorithm from grammars that satisfy the universals and *only* from such grammars" (p. 371). On this view, "the process of learning a (first) language involves internalizing the grammar and applying this algorithm to construct the corresponding [comprehension or production] procedure" (p. 371).

This speculative account of language acquisition has several obvious advantages for the weak theory of transformational reality. First, it provides a psychological status for the grammar, namely, as that representation of the language which is acquired initially by the child and from which all procedures underlying linguistic use must be derived. Second, it provides a way of explaining the existence of universal constraints on transformational rules by attributing them to innate biases in the language acquisition system. According to this position, a language which violates such constraints should not be learnable by a human child. Finally, this view provides a rationale for the weak theory's claim that the structural descriptions, but not the transformational rules, are realized during language use. For example, it is now possible to imagine why the deep structure of a sentence, including even its transformationally motivated aspects, might be determined during comprehension: If the child constructs a transformational grammar internally, then he will implicitly share the linguist's motivation for arranging deep structures in such a way as to simplify the operation of the transformational rules. Moreover, if the child's comprehension procedures are derived algorithmically from the grammar in such a way as to preserve the mapping between sentences and structural descriptions provided by the grammar, then such a child must also develop into an adult who confirms the weak theory by realizing structural descriptions in a nontransformational manner during language use.

But is there any evidence that children actually acquire language in this way? When, toward the very end of their book, FBG get around to answering this question, they reach a conclusion which, if I understand it correctly, is in direct conflict with their own speculations about language acquisition:

> How plausible is it to view the grammar as psychologically real? . . . In the case of language learning, [this issue] arises in the context of theories which hold that a standard transformational grammar is the output of the child's innate language acquisition mechanisms.
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> We have argued that neither the empirical nor the theoretical basis for this view of language acquisition is secure. There exist no satisfactory models of a language acquisition device capable of inducing a grammar from a corpus, and there is very little unequivocal evidence that such an induction does, in fact, take place. (p. 502)

There is, I think, every reason to accept this conclusion. It is based on an intelligent review of what is known about language acquisition. Among other things, FBG argue persuasively against the view that syntactic development can be construed as a gradual internal accumulation of transformational rules, and I think it is fair to say that this conclusion is now generally accepted (*cf.* de Villiers and de Villiers, 1977; Maratsos, 1976).

But such a conclusion should have a devastating impact upon the weak theory of transformational reality. If there is no compelling evidence that the child internalizes a transformational grammar in the course of acquiring language, then there is no longer *any reason* to believe that either sentence comprehension or production should involve the realization of the complete structural descriptions provided by a transformational grammar. In effect, this conclusion knocks the last leg out from under the weak theory of transformational reality. Recall the state of the evidence. The weak theory escapes the fate of the strong theory by postulating that the structural descriptions, but not the transformational rules, are realized during language use. But, although there is evidence for the psychological reality of some aspects of structural descriptions, there is no evidence for the psychological reality of the unique, transformationally motivated aspects of structural descriptions. Nor can the weak theory provide any explanation of why transformationally motivated aspects of structural descriptions *should* be realized during language use unless the weak theory adopts an account of language acquisition in which the procedures underlying language use are derived from a previously internalized transformational grammar. But by FBG's own account, there is no compelling evidence that such an internalization takes place.

It is puzzling that FBG do not appear to be upset by their own conclusions about language acquisition. Perhaps they take the evidence from child speech lightly. They might, for example, invoke the competence-performance distinction. Thus the child might be internalizing a transformational grammar, but the structure of his utterances might not show it if those utterances were controlled by performance procedures derived from, but only indirectly related to, the internalized grammar. This line of argument is plausible, but perilous, since it comes close to claiming that the internalization of a transformational grammar will proceed without any observable trace. In this case, however, the whole appeal to language acquisition is without empirical content, and the only empirical claim left to the weak theory is that the structural descriptions specified by the grammar are realized during language use. But now we have come full circle; for, as we have seen, the crucial aspects of this last claim have never been tested.

Then again, perhaps there is another reason why FBG are not disturbed by the language acquisition data. Perhaps they feel that the evidence for the existence of universal constraints on transformation is enough to force the conclusion that transformational grammar must be internalized in some way during language acquisition, despite the scarcity of any other confirming evidence.[2] But I do not find this view compelling. Suppose we assume, before all the evidence is in, that there are some constraints on transformations

[2]A careful reading of pp. 510-513 may support the guess that FBG do, in fact, hold this view.

which will prove to be universal. Even given this assumption, we need not conclude for the psychological reality of transformational grammar. For it is hard to imagine a constraint on transformations which cannot be restated as a constraint on surface structures. Once restated, possible explanations can be sought in terms of constraints on nontransformational representations of linguistic knowledge, or in terms of the cognitive limitations of the processing systems involved in determining surface structure. In any event, the fact that a universal property of linguistic structure can be stated as a constraint on transformations does not, so far as I can see, require us to conclude for the universal internalization of transformational grammar.

So what is the upshot? Just this: Despite FBG's substantial and sophisticated arguments for the weak theory of transformational reality, I can find no conclusive evidence for the theory in their book. Certainly some of the properties of structural descriptions have been shown to correlate with some aspects of linguistic behavior. But none of the unique, transformationally motivated aspects of such structures has been either directly or indirectly shown to have psychological reality. Therefore, so far as I can see, there is no conclusive reason to accept FBG's claim that the structural descriptions supplied by transformational grammar provide a constraint on the input or output of psychologically adequate models of comprehension or production.

This conclusion, negative as it may be, is important. The weak theory of transformational reality has had the effect of preserving what might be called derivative psycholinguistics. Basic to this brand of psycholinguistics is the attempt to derive psychological theories of linguistic performance from linguistic theories of language structure.[3] The clearest cases of derivative psycholinguistics are found among the early performance models which literally incorporated the grammar as a proper part. In rejecting such models, and the strong theory of transformational reality on which they were based, FBG have taken a long step away from a strictly derivative psycholinguistics. The question we must now face is whether this step has been long enough.

By upholding the weak theory of transformational reality, FBG preserve a partially derivative psycholinguistics, one in which the input and output of performance models are still derived from the grammar. If the weak theory is correct, it would admittedly provide important benefits for psycholinguistics. It would mean that a large part of the work of building

[3]Notice the resemblance between the research program characteristic of derivative psycholinguistics and FBG's speculative version of language acquisition in which the child derives performance procedures from a previously internalized grammar. It is difficult to resist the observation that derivative psycholinguistics may have fashioned a theory of the child in its own image.

performance models has already been accomplished by linguistics. However, if the weak theory is incorrect, then the consequences for any program of research based on it must surely be disastrous for psycholinguistics. For if psycholinguists sign away the deed to the input and output of their performance models, and if the linguistic characterization of input and output proves inaccurate, then psycholinguistic research would be condemned to a fruitless search for a psychologically real performance mechanism which implements an input-output relation which does not exist.

To be fair, it appears that FBG also shrink from this abyss. Alluding to the continuing disputes within linguistics about the degree of "abstractness" appropriate to deep structure, FBG make the following remark on their concluding pages:

> although the hypothesis that levels of grammatical description correspond closely to psychologically real levels of encoding has thus far often been vindicated, it cannot be relied upon indefinitely. Even as a source of information about the entities which populate descriptive levels, the standard techniques of syntactic analysis may finally be running dry. We have seen that the kinds of constraints that have thus far been brought to bear within syntax seem to be compatible with a variety of assumptions about the degree of abstractness of deep structure. This clearly suggests that the constraints need to be tightened. Since the adequacy of a grammar depends on the psychological reality of the structural descriptions it postulates, it may well be that only direct experimentation on psychological reality will ultimately chose between competing syntactic theories. (p. 512)

Here FBG finally reject the derivative psycholinguistic program. It is surely a sensible move, but it may also be the last straw for the weak theory. Ten years of research have failed to produce either a conclusive empirical case or a coherent theoretical formulation. In addition, the linguistic certainties upon which the weak theory is founded appear to have dissolved. So, perhaps it is time to change the way we are doing psycholinguistics. Perhaps we already have.

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Some Aspects of Communicative Competence and Their Implications for Language Acquisition. By Tom Van Der Geest. Van Gorcum, Amsterdam, 1975 (no price indicated).

Interest in questions of communicative competence in the last decade can be traced to the work of Hymes (1972). Many issues have been raised as the result of his pioneering work; Van Der Geest's (VDG) recent book *Some Aspects of Communicative Competence...* (CC) is an attempt to address some of these issues. The book is divided into two parts. The first details exemplary phenomena the author argues should be included in a theory of communicative competence—stress, intonation, topic and comment, ellipsis, and language functions. The second and far shorter part will be of great interest to those working in the field of language acquisition. This section examines implications of the theoretical considerations in Part I for early child language development, from the single-word stage to multiword utterances during the third year.

The author's articulated purpose for writing this book was to raise a set of issues that require consideration of linguistic phenomena that encompass intrasentential relations: "to present a provisional framework for the description of utterances in terms of communicative appropriateness" (p. i). The volume suffers not so much from the heterogeneity the author fears but from overambitiousness. A more limited range of considerations would likely have afforded the author greater opportunity for integration. Nevertheless, CC does offer the reader important insights into aspects of communicative competence. Since there has been abundant interest in this area, particularly with respect to its implications for theories of language development, this review will focus on Part II of CC.

In a volume that seriously proposes to bring to bear a broad range of contextual phenomena in developing a theory of communicative competence, contextual information is frequently absent. While VDG includes intonational and gestural information in single-word and two-word utterances, he fails to provide us with a description of the context in which an utterance and its accompanying gesture and intonation occurred. For example, we do not know with whom the child was interacting, what utterances by the child and others surrounded the speech event, and so on. More contextual information would have contributed to making the data presented more intelligible and would have provided the reader with information sufficient to draw his own conclusions.

A central theme in Part II of CC is the author's criticism of Bloom's theses in *One Word at a Time* (1973). With respect to single-word utterances, VDG argues:

> We will object against [*sic*] that children do not know grammar (whether it is grammatically or semantically specified) until their usage of two or multi-word sentences. It will be argued that intonational contours and accompanying bodily behavior give structure to the child's use of single words. Furthermore, it will be argued that the child's earliest productions can be provided with more complex underlying representations and that the mechanism accounting for ellipsis and stress . . . can deal with the telegraphic method of speaking of the child. With respect to the transitional stage in which two single word sentences are used in an anticipated schema to cover one informational unit, it will be argued that this way of building up bigger units is not restricted to this particular stage. What we see here is an apparent mismatch between a greater semantic cognitive knowledge and a less developed set of realization rules from the side of the child. (p. 5)

In Chapters VI-VIII VDG presents four kinds of evidence to argue in favor of the view that children's single-word utterances are sentences. *Sentence* is taken here to mean having semantic value greater than the referential meaning of the lexical item. The arguments are

1. The word order of successive single-word utterances is the same as the word order might be if those words were produced in a syntactically more sophisticated utterance.
2. Successive single-word utterances within an anticipated action schema form one informational unit that is therefore a unit semantically if not syntactically.
3. Since children's early knowledge of the relations between words is primarily semantic rather than syntactic, children do know more about language than their single-word utterances might suggest.

4. Children's early single-word utterances are accompanied by contrastive intonation and gestural patterns. Taken together, these sytematicities indicate that children mean more than they say.

These arguments are examined individually below.

1. VDG presents successive single-word utterances drawn from *One Word at a Time* in an attempt to demonstrate that successive single-word utterances exhibit the same order as fuller, syntactically more complex utterances. Contrary to VDG's claim, Allison Bloom produced successive single-word utterances that did *not* appear to be consistent with adult syntactic order or were at least open to alternative interpretation.

a. (Allison, having eaten peach pieces that her father cut, picks up another piece of peach and holds it out to her father)

peach/Daddy/

b. (Allison, pretending to cook)
Is the baby cooking?

pot/meat/

c. (Doll is on chair. Allison getting on chair)

up/baby/

VDG also argues that since Allison's utterances with [wĩdə] exhibited regularities in word order, knowledge of syntax at some level can be attributed to the child at the single-word stage. This argument is problematic in that it confuses syntax with word order. While the child may have figured out some very general features of the word order of his native language, it is not necessarily the case that he knows about syntax. That is, syntax is more than word order; it is the order of words as determined by the particular types of relationships that obtain between the words as a function of the roles they assume (Bloom, 1973, pp. 37-38). In agreement with Bloom, it is argued here that one cannot attribute knowledge of syntax to the child based on regularities of word order in their successive single-word productions. In fact, there is doubt whether children's two- and three-word utterances reflect knowledge of syntactic relations. Bowerman argues "that the structural phenomena that motivate the description of adult speech in terms of syntactic phenomena are missing in child speech; hence, there is no clear evidence that children in fact have made these abstractions" (1976; see also Bowerman, 1973*a,b*).

2. VDG's second argument is based on the apparent relationship on informational grounds between successive single-word utterances. VDG shifts

from claiming primitive knowledge of syntax to knowledge of semantic relations. "On semantic grounds one might decide that at least successions within an anticipated schema form just one informational unit (which is a speech event basically) that for the moment is not realized as a unit syntactically but semantically" (p. 194). While it is the case that successive single word utterances like *Daddy/peach/cut/* (Bloom, 1973, p. 41) can be associated with the scene of Daddy cutting a peach, it is not necessarily the case that they represent one coherent informational unit. That the child may cognitively represent them as related events or an anticipated action schema is not the same. While Bloom does not provide evidence against these utterances forming a coherent unit, VDG does not provide convincing evidence that they do.

3. VDG claims that children know more than they say in the early stages of language acquisition, as borne out by their use of semantic processing strategies. While the use of semantic strategies in decoding active and passive sentences (Bever, 1970) is not denied, it has become increasingly evident that young children employ nonlinguistic strategies perhaps to an even greater degree. Clark (1971) and Keller-Cohen (1974) found that children use an order of mention strategy to process sentences with temporal connectives. Clark (1973) reports that children's early knowledge of *in, on,* and *under* is closely tied to perceptual features of the objects "and their possible relations in space, e.g., containers versus surfaces" (Clark, 1976, p. 17). These data argue for a more conservative interpretation of what young children know about language than VDG proposes.

4. Clearly the most well-developed argument that VDG presents supporting the single-word utterance equals S position draws on intonational and gestural data from the speech of one child, Hester. In contrast to Bloom's report (1973), VDG reports systematic use of intonation at the single- and two-word stages. This finding is consistent with reports that prosodic development begins quite early (Kaplan, 1969, in Dore, 1975).

VDG found that certain prosodic and gestural patterns were associated with two word classes.

Class	Intonation	Gesture (Dx)
nounlike words	falling, rising	pointing (Dx_1)
verblike words	imperative, rising	grasping (Dx_2)

That Dx_1 + Imper + N, Dx_2 + falling intonation, and Dx_1 + V never occurred was significant in establishing the contrastiveness of these classes.

When all these features of a single-word utterance were viewed along with other properties of the context, VDG was able to establish a basic set of communicative intentions:

1. Pointing out existence.
2. Questioning existence.
3. Requesting possession.
4. Requesting permission to act on.
5. Asserting desire to possess.
6. Asserting desire to act on.

The author then translated these intentions + actual utterances produced into sentential representations. That is, through rich interpretation that combined actual lexical items produced in combination with gesture and intonation, VDG was able to interpret single-word utterances as sentential.

The description of single-word utterances ultimately proposed combines some features of both Ingram (1971) and Antinucci and Parisi (1973). The model is similar to that of generative semantics. VDG's justification for his version of this model will not be discussed here. In brief, however, an S (Utterance) can be described as consisting of a Sentence Qualifier (SQL) and a Proposition (P). The SQL contains information about the speaker's intentions (Ques, Imper) and about the speech event (assignment of speaker-hearer roles). P contains the Qualifier (Tns, Neg) + the Nucleus. The Nucleus contains information about the "State of Affairs," the referential meaning of the utterance. Gesture is formally incorporated into the description of the single-word utterance since it is viewed as part of the propositional structure of an utterance. Intonation is incorporated into the SQL since it provides information about the speaker's intentions.

Even if one acknowledges the systematic use of gesture and intonation that VDG observed, there is still sufficient question about attributing underlying semantic knowledge to the child. Such a position has been taken by Dore (1975). He proposes a speech act view of early word use. Words in his view are seen as *rudimentary referring expressions,* i.e., verbal tools for referring to people, objects, and actions. Intonation is used by the child to indicate the *force* of his utterance, i.e., what he wishes to be accomplished by the production of the single word. Dore includes gesture as part of the nonlinguistic behavior that contributes to determining the force of an utterance. Unlike VDG, Dore does not formally include gesture in analyses of single-word utterances: "from a speech act point of view, contextual features [including gestures] and the child's understanding of the world are denied structural status. In this way a clear distinction is drawn between knowledge of language and knowledge of the world, and this helps prevent basing claims about the former on data about the latter" (p. 34).

The issue here then is whether one wants to consider gestural data to be linguistic data in a model of child language development. The whole question is further complicated by VDG's alternation between linguistic competence and communicative competence. If his argument is that a gestural system is part of the child's *communicative competence,* then his data support such a claim since gestures contribute to communicating different speech functions. If on the other hand he is proposing that gestures are part of the child's linguistic system as he seems to in Chapter VI, then the data do not support his claim.

VDG's final criticism of *One Word at a Time* is the distinction Bloom draws between semantics and cognition. He argues for a contrast instead between "cognition and semantics on the one hand and syntax (or realization) on the other" (p. 196). This *cognitive semantic* knowledge is "the knowledge of the speaker inasfar as it is involved in the content of the message" (p. 5). He therefore combines both what the speaker knows about the world and knowledge of meaning relations as expressed by words and their relations. His view is related to that of Parisi (1971): "By semantic structure we mean a cognitive structure which is constructed with the intent to communicate it. Therefore semantic structures are a subclass of cognitive structures" (p. 102).

There are substantial arguments on both sides of the issue regarding the relationship between cognition and semantic representation. It is argued here that a distinction ought to be maintained between the two even if there is overlap at points. One must be able to distinguish between knowledge the child has about people, events, and objects and their relations and the means he has for encoding this information linguistically. The problem with equating semantics with cognition is the failure to recognize that there exist in every culture cognitive discriminations that are not encoded linguistically (Bowerman, 1976). The complex Russian system of verbs of motion discriminates between round trips and trips with stops; this information is encoded in different verbal forms in Russian and is absent in English, although speakers of English are clearly able to discriminate between such events.

The preceding discussion covers much of the material presented in Chapters VI-VIII. However, VDG also addresses stress placement and the form of output in one- and two-word utterances in Chapters VIII and IX. He argues that one- and two-word utterances are the result of output constraints under which the child operates. The child produces a single-word utterance by selecting the rightmost element in the logical tree structure; a two-word utterance is the result of the child's selecting the two rightmost elements of the logical tree structure. Turning to stress assignment in two-word and multiword utterances, VDG proposes a model that accounts for this phenomenon in terms of stress placement and the juxtaposition of old and new information in the adult input. An analysis of these arguments would require detailed discussion of VDG's theory of stress assignment and ellipsis presented in Part I of CC and is therefore beyond the stated scope of this review.

A highlight of CC is VDG's treatment of pragmatic errors in his son's early speech productions (Chapter X). For the sake of simplicity, the English glosses provided by VDG will be presented here to illustrate the phenomena. Mark, his son, produced the utterances under A with the apparent meanings under B.

A	B
1a. *I have to sit down.*	1b. *I want to sit down.*
2a. *I don't get anything else.*	2b. *I don't want anything else.*
3a. *Am I just doing this?*	3b. *Can I just do this?*

The author explains these errors as too literal adaptations of the adult input sentences. Hence, 1a seems to be derived from (adapted from) the adult sentence 1c *You must sit down* in that it "serves the same ultimate effect"–Mark will sit down (p. 237). Likewise, 2a is the adaptation of 2c *You don't get anything else* and 3a of 3c *Will you just do this?* Apparently Mark had encoded the basic pragmatic function he wanted to express but had not yet determined "whether it was the speaker or the addressee who desired the ultimate effect represented in the propositional structure" (p. 237).

Similar speaker-addressee errors were found in Mark's use of *please* for *thank you, I* for *you,* and *yours* for *mine.* In each case, Mark had identified the correct set of words used in a particular pragmatic situation without having worked out the details of which form was associated with the speaker and which with the hearer. Further data on pragmatic errors in child language should suggest what the essential properties of rules of language use might be.

A few observations on the technical quality of CC must be included. The volume suffers from poor editing; it was not rare to find sentences like the following:

> In my *Early child grammars: an evaluation* I argued that analysing child language data by writing grammars fails. One reason this fails is that, because it neglects the fact that in linguistics grammars are written to make linguistic intuitions explicit, rather than to describe the products of linguistic abilities, a mismatch arises between the purpose of grammars in linguistics in its narrow sense and the use of child grammars in the field of developmental psycholinguistics, which latter are designed to account for the child's productions. (p. 1)

CC was produced by offset reproduction, a practical and inexpensive means of avoiding production delays. While it is likely that more publishers and authors will choose this method of printing, it appears to have contributed to technical problems in the volume. For example, typos were occasionally corrected in handwriting and letters, words, and phrases sometimes rose above the type line. Occasionally typos led to my inability to decipher the meaning of a sentence. Finally, references were occasionally left out of the bibliography: for example, "O'Donnell" and "Van Der Geest *et al.* (1974)."

Despite these infelicities, it is clear that a tremendous amount of thought went into developing the ideas presented in CC. Certain issues raised by VDG such as the role of nonverbal behavior in models of communicative competence deserve further consideration in future research (see Keller-Cohen *et al.,* 1977). Subsequent investigations by Bruner (1974/1975), Bates *et al.* (1975), and Dore (1975), among others, have contributed to increasing our understanding of what the child can be said to know about the process of communication during the earliest stages of development. We must ultimately come to grips with systematically integrating both the verbal and nonverbal features of the child's behavior into a theory of the child's communicative competence. Van Der Geest's work is a worthy introduction.

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