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ED 020 509 AL 001 278

THE INSUFFICIENCY OF THE TRANSFORMATIONAL APPROACH TO CHILD LANGUAGE.

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PUB DATE APR 68

EDRS PRICE MF-$0.25 HC-$0.48 10P

DESCRIPTORS- *LANGUAGE DEVELOPMENT, *CHILD DEVELOPMENT, VERBAL DEVELOPMENT, TRANSFORMATION GENERATIVE GRAMMAR, *TRANSFORMATION THEORY (LANGUAGE), *PSYCHOLINGUISTICS, LANGUAGE RESEARCH, CHILD LANGUAGE,

THE AUTHOR FEELS THAT TO APPROACH CHILD LANGUAGE TRANSFORMATIONALLY IS TO USE A TECHNIQUE SUITED TO PROVIDING ADDITIONAL INSIGHT INTO A WELL-KNOWN LANGUAGE FOR TREATING AN UNKNOWN, OR AT BEST LITTLE KNOWN LANGUAGE. SHE MAKES THE FOLLOWING CRITICISMS OF TRANSFORMATIONAL ANALYSIS OF CHILD LANGUAGE--(1) NOTHING CAN BE DIRECTLY INFERRED WITH REGARD TO THE PROCESS OF LANGUAGE ACQUISITION FROM TENTATIVE INSIGHTS INTO THE MECHANISM OF NATURAL LANGUAGE, (2) THE HUMAN BRAIN DOES NOT WORK LIKE A COMPUTER, AND (3) CONCENTRATING ON SYNTAX (THE CORNERSTONE OF TRANSFORMATIONAL RESEARCH) IN CHILD LANGUAGE ANALYSIS IS "TOTALLY MISLEADING." FURTHERMORE, THE CHILD'S FIRST VERBAL EXPRESSIONS ARE DESIDERATIVES, WITH INTERROGATIVES APPEARING VERY EARLY. ACCORDING TO GENERATIVE THEORY, THE PRODUCTION OF THESE FORMS REQUIRES MORE THAN ONE TRANSFORMATION. THE AUTHOR ALSO POINTS OUT THE CONSTRUCTIVE ROLE OF THE GENERATIVE APPROACH--(1) IT HAS CONTRIBUTED TO A MORE PRECISE FORMULATION OF THE MANIFOLD PROBLEMS OF FIRST LANGUAGE ACQUISITION, (2) BY BRINGING TO LIGHT THE FRUITLESSNESS OF USING MODELS BEFORE STRUCTURING DATA, IT HAS LEFT LINGUISTS FREE TO TRY OTHER ROUTES, AND (3) IT HAS GIVEN IMPETUS TO NEW RESEARCH ON CHILD LANGUAGE. THIS PAPER WAS PRESENTED AT THE UNIVERSITY OF KENTUCKY FOREIGN LANGUAGE CONFERENCE, LEXINGTON, IN APRIL 1960. (DO)

THE INSUFFICIENCY OF THE TRANSFORMATIONAL APPROACH TO CHILD LANGUAGE

Paper Delivered at the 21st University of Kentucky
Foreign Language Conference, April, 1968

by

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As we still know very little about human language, any approach towards its analysis may contribute something to our understanding of the subject. Transformationalism, for example, helps by focusing sharply upon certain grammatical relationships within a language. It is also useful as a pedagogical device, and I have successfully employed it myself to elucidate some of the difficulties of the French subjunctive. There is always the danger, however, that new developments, like penicillin, may be pushed beyond their area of effectiveness.

To approach child language transformationally is to use a technique suited to providing additional insight into a well-known language, for treating an unknown, or at best little known, language. Moreover, at times there appears to be a confusion between the nature of language, the psychology of speech, and the process of language learning -- even though the generativists do not claim that their theory automatically provides a heuristic means of discovery. Transformationalism posits the rules that govern language and not necessarily those of language behavior. This is a concept of language in the manner of the medieval natura naturans. On the other hand, according to Chomsky, the native speaker is supposed to have an intuitive

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awareness of the laws governing his language. These intuitions cannot be explained by one who does not himself process them; but Chomsky also believes that by becoming fluent in a foreign language, one can gain a near-native intuition. How can the latter be possible except through language performance? Transformational sequences must therefore be part of both competence and performance. If these permutations are inherent in the covert process of speech programming, they are either inborn or acquired. As no grown-up is a native or near-native speaker of child language, we can never find out anything beyond trivial superficialities. And if that's so, we ought to give up on it all together. But maybe all we have to do is rephrase Marcel Cohen's distinction between langue bébé and langue adulte by calling the former "pretransformational language". Then, what remains is to explain how, at a certain stage of ~~its~~ his linguistic maturation, the child suddenly employs transformations. Except for Minerva's birth from Zeus, there are no incidences in nature of instantaneous change. Consequently, transformations must have developed gradually during the period of langue bebe, and our vicious circle is thereby closed.

This could mean the end of my paper, were it not that science is cumulative; and indeed generative grammar has contributed importantly to a more precise formulation of the manifold problems of first language acquisition. It has given impetus to new investigations on child language and nowadays more scholars are engaged in this kind of research than in the early fifties. What disturbs this encouraging picture is the arbitrariness of

some of the research. Dogmatism has replaced unprejudiced investigation - or, not to idealize the past, former biases were not so prescriptive as present ones. The outlook, however, is not all negative, because this aprioristic approach has engendered cogent criticism which has shed light on facets of linguistic analysis which might otherwise have gone undetected.

I would now like to turn to some specific criticisms of transformational analysis of child language, criticism which I certainly intend to be constructive.

Fundamentally, it seems to me, if by positing transformations, some additional tentative insights into the mechanism of natural language may accidentally be gained, nothing can ipso facto be inferred from these speculations with regard to the process of language acquisition. An ordered sequence of rules advancing by binary choice is the conventional algorithm for computers, but unless biological evidence forces us to concede that all that is man-made bears a one-to-one relationship to natural phenomena, we cannot conclude that the human brain works like a computer. Although it is likely that man first thought of building airplanes from observing birds, it is clear that birds do not fly by fueled motors. What is more, neurologists view our brain as "a multi-channel nervous system". Psychologically, Alex Liberman has furnished good evidence for his assertion that it is "the parallel processing that makes the speech code so efficient". With regard to phonology, the concurrent working of motor impulses has been demonstrated by Wickelgren and myself. The syntax of a natural language is

still best described as a network of relationships. I believe it may ultimately turn out to be more productive if we concentrate our efforts on trying to discover how these relationships gradually build up in the verbal behavior of the child. I question the primacy of syntax, which, of course, is the cornerstone of transformational research. If concentrating exclusively on syntax is a flagrant oversimplification of the analysis of adult speech, applied to child language it is totally misleading. With Slobin, "I do not see why grammatical linguistic marking of learnable categories should be afforded special status".

In addition, along the lines of Vygotsky, my own limited research suggests that congruence of exponency is an important stage in child language development, but certainly initially the conematic and the pleromatic levels follow avenues of their own. Robins is probably correct in "treating the word as a basic unit of grammar". As Halliday puts it, "language is systemic"; and I agree completely with Bolinger that "intonation should be the first of a subsystem of language to develop."

This leads us to concrete details of child language acquisition. One of the more baneful influences of transformationalism on the study of child language arises from the effort to isolate and magnify the onset of the two-word period. The isolation is practiced on all levels. The two-word period is described as if it were the beginning of the child's verbal behavior, when in reality it follows the holophrastic stage, with some initial overlapping. It is reasonably safe to assume that an infant's first verbal

expressions are to be classified as desideratives and that the interrogative appears very early. According to generative theory, the production of these forms requires more than one transformation. It is not impossible but difficult to explain why the affirmative statement, which is supposed to be the simplest form, should emerge in the child's overt speech later than the more derived forms. McNeill's assertion that "transformations appear relatively late in a child's career" does not tell us when they start, nor why they have to start at all.

Likewise, explaining early negatives as pretransformational vocabulary items begs the question. Positing an amorphous purport and skipping the base structure in the analysis of speech programming, as I do in the treatment of child bilingualism, is of course outside the generative approach, especially because it runs counter to the centrality of syntax. As long as we are trapped in *a priori* explanation of grammar, we will never come to understand semantics which is complex enough even when not masked by a filter of doubtful usefulness and, certainly, of no proven reality. Fodor's non-sentence underlying form is reminiscent of the junkshop theory. The negation in substandard English can be derived transformationally only if one goes so far as to postulate a covert past negative. The American ghetto child has no grammatical marker for the positive past, whereas the negative past and present are morphologically distinct: *He wash* today and *He wash* yesterday, but *He don't wash* today and *He ain't wash* yesterday. Taxonomics would speak of a zero marker and of a lexical versus a morphological system.

Informationally, there is less redundancy. This is a simple, matter of fact statement without causal explanation, but neither can the less documentable generative interpretation explain any causes.

Grammaticality is no problem in a substandard dialect, which by definition has a grammatical structure of its own. But, when raised within a family of speakers of standard English whose correct forms show occasional free variations with substandard forms, is the child capable of abstracting the "correct" grammar, or does he too come up with utterances like <u>says I</u>? To categorize such utterances either as alternate grammatical forms or as syntactic freezing is too easy a way out, once we postulate the child's learning in the form of an active search for grammatical rules.

Few transformationalists nowadays still hold to the 1957 innateness theory because a selective forgetting device runs counter to whatever else we can observe in learning procedures. That human language is species specific, if once doubted by a handful of scholars, is no longer seriously questioned. It is the task more of the philosopher than of the linguist to discover the balance between what is inborn and what is acquired. The discussion of learning theories belongs to the psychologist. In the long run, the linguist will contribute much more to an understanding of the complexity of the language learning process if he limits himself to collecting raw data, to be quantized and subjected to empirical validation, insofar as possible. A little experiment I conducted on a seven-year old as well as a

longitudinal study of an infant during his first year of life would incline me to favor learning-without-awareness. More recently, Cazden reached a conclusion similar to mine concerning the extremely limited applicability of both the reinforcement and the expansion theories. One of the lasting albeit indirect merits of transformationalism consists in its having promoted research which has helped dispel the fallacy of equating first and second language acquisition.

For those who correlate transformational rules in language with Piaget's findings on the conservation of amount, I can only point out that the two developments occur at completely different ages in childhood.

The psychopharmacologists will eventually reveal the workings of our memory. Meanwhile, the linguist might contribute more to the solution by refraining from speculation and providing instead verifiable data on the quantization of linguistic symbols. I have in mind especially the valuable work done at the Haskins Laboratories.

In a way, the generativists are correct in saying that before figuring out how language is acquired, we should know what language is. Memory and learning are complex phenomena. Before testing them on language - of which we know so little and which is so extraordinarily complex - we had better test them on simpler forms of human behavior. It may well be, however, that the only way to figure out the essential nature of language, without postulating an arbitrary philosophical set-up, is by

observing how it is acquired. Just one thing is sure so far: much hard work still lies ahead.

Oftentimes generative analysis of child language is based on a transcription into adult English orthography, where the suprasegmentals fall by the wayside. On the level of supra-segmentals, disregard for prosodic features creates one of the pitfalls of transformational pedolinguistics by positing an enormous gulf between competence and performance. The simple experiment conducted by Tappolet, and others after him, including myself, all through the twentieth century, shows quite clearly that the adult listener may presume in the child knowledge of vocabulary and grammar, when, in fact, the infant understands the message in a manner as gross and linguistically unquantized as he himself produces it by crying, or by cooing modulations. To say that competence exceeds performance is tautological. In a closely related domain, Leopold uses the term "incubation period" for the interval between passive and active language. The fact, by the way, that passive language precedes active language, was documented by Grammont as far back as 1902.

In much of present-day research little consideration is given to the immediate extra-linguistic situation - not to speak of the broader environment of culture. The material so presented is susceptible of whatever grammatical interpretation one wishes, without the possibility of verification. Furthermore, by neglecting to incorporate the context of conversation, what the child has been asked and what answer he is given, many trans-formationalists end up by subscribing to such old wives' tales

as "naming" and "concrete objects." A more serious consequence of this approach is that the investigator cannot distinguish between linguistic items internalized by rote and those meaningfully assimilated. The necessity for differentiating between rote and non-rote memory cooling was confirmed by my study of a child's translating process, where the problem of memory storage is compounded by the mixing of the levels of linguistic analysis. Rules of expectancy cannot adequately cover a variety of phenomena.

Another side of the isolation of the linguistic data under investigation is represented by the tendency to postulate the sentence as a self-contained unit of meaning. Anaphora, to cite only one example, is surely universal in language. Indeed, except for rare cases, I would agree with Malinowski that there are no ambiguous sentences. Katz and Fodor are correct in stating that "a speaker can disambiguate parts of a sentence in terms of other parts," but obviously I do not share their concern about the weakness of expanding "other parts" to the "socio-physical environment and the rest of the written or spoken utterance." Co-variance of form and meaning is probably among the basic constants of language, if not its actual raison d'être.

By the way, if one asks a child for a clarification, he never comes up with a transformation and, I am told by an anthropological linguist, neither do informants. Of course, this is no conclusive evidence against transformations within the unconscious, and perhaps all we need is a Freudian device to bring them into the open. Or am I haplessly mingling

operational and external adequacies?

To conclude on a hopeful note: For the study of child language the generative approach has played a constructive role by bringing to light the fruitlessness of using models before structuring data, thus leaving us free to try other routes. My personal inclination would be in the direction of quantitative linguistics, information theory, and semantics, somewhat along the lines of Martinet's functionalism. There is no reason, as far as I can see, why we should not use the distributional abstractions of taxonomics before postulating hypothetical underlying forms.

From the psycholinguistic standpoint, it is fortunate that not all psychologists have abandoned research on the mother-infant relationship, and the influence of environmental factors, in favor of the easy philosophy of total innateness. My own research confirms the findings of Spitz. It is difficult to fathom how his findings can be explained in terms of an innate syntactically-based cognition system. The only logical solution within the transformationalist premises is that the essence of language is related neither to language acquisition nor to speech programming.

Hic sunt leones.