

Letters to the Editor

ON THE DEVELOPMENTAL THEORY OF LANGUAGES

Sir:

I read Doctors Werner and Kaplan's paper in the current issue of the *AMERICAN ANTHROPOLOGIST* with the greatest interest, the more so that I am working on the developmental approach to cognition, and use language and culture data in my approach. I found this paper very remarkable and certainly indicating that we may expect interesting results from further research on the part of the authors. At the same time, I would like to mention certain points on which I disagree.

The first of these is the factual question of the law of evolution of languages. It is true that the majority of the so-called primitive languages follow Jespersen's descriptions. At the same time, there are many exceptions to it, for instance Mazateco or Mixteco in Mexico, or again Ibo in West Africa. On the other hand, Smith and Trager have shown that there is a formation in English of clusters with a single semantic value such as "go-down" or "down payment"; these may be written as two words but are pronounced as one with a primary stress on the first syllable and a plus juncture. Something similar is happening in Chinese, although there clusters appear primarily as a defense against homonyms and their semantic value is unclear.

Another point concerns the interpretation of such evolution as may be said to exist. While I agree with Dorothy Lee's interpretation of Trobriander, I would strongly disagree with any interpretation which would place flexed languages (*cantavisset*) somewhere in between languages like Trobriander and languages characterized by free association with invariant forms. The principal conceptual significance of synthetic languages may not be that of necessary clusters of meaning, but on the contrary it may have the significance of a compulsory subdivision of a main concept into more precise subconcepts. I have given an example to that effect in regard to Polish ("Semantic Difficulties in International Communication." Etc., Spring 1954).

A third point may be put in parallel with the first one. From the point of view of conceptualization, the tendency to differentiation which the authors describe as a direction of evolution is reversed in the case of such philosophies as those of Hegel and even that of Whitehead, not to speak of the voluntarism of Fichte and of some characteristics of totalitarianism and nationalism in general.

On the basis of these remarks, it seems to me that it is at least premature to speak of any direction of evolution. We might instead have one or several cyclic phenomena. It is also dangerous to insist particularly on a single polarity (which might even be called a paradigm). For instance, in addition to the paradigm which I would rather call association-disassociation so as to avoid the use of the value-laden word "primitive," there may be the universal-particular polarity first suggested by Pribram or the esthetic-theoretical polarity suggested by Northrop. In the last respect, Northrop places Chinese culture on the side of the esthetic continuum which has some points of resemblance with the authors' pole of participation or association; this in spite of the fact that Chinese is a highly analytic language. Or is it so in fact? While it appears to be so in its spoken form, it is highly synthetic in its writing.

To sum up, I definitely agree with the basic developmental thesis, according to

which cognition must be studied as a genetic and evolutionary process. I also agree with the thesis that language is but one manifestation of a general symbolizing function (vide Cassirer and Langer). At the same time, I find it dangerous to reduce evolution to something which resembles progress, without taking into account the possibility of cyclical or even haphazard transformations, and also to reduce the genetic and evolutionary description to a single criterion.

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REPLY

Sir:

We are grateful to Mr. Glenn for his critical comments on our recent paper (1956), and to the editors of the AMERICAN ANTHROPOLOGIST for affording us the opportunity to amplify the major points germane to these comments. We limit ourselves to the general criticisms raised by Mr. Glenn, since an answer to questions of detail, e.g., the formation of "clusters" in English, would involve a lengthy excursion into specific propositions and experiments in general perception as well as developmental theory.

We feel that much of Mr. Glenn's criticism is directed toward views we do not hold and derives from the persistence of ambiguities we had sought to expose and eliminate in our original article. It may be worthwhile to cite certain paragraphs from this article, before further evaluating Mr. Glenn's remarks.

On p. 866 of our paper, we introduce the orthogenetic principle of developmental theory and characterize it as follows: "Wherever development occurs, it proceeds from a state of relative lack of differentiation to a state of increasing differentiation, articulation and hierarchic integration. . . . Though itself not subject to empirical test, it (this principle) is valuable to developmental psychologists in directing inquiry and in determining the actual range of applicability with regard to the behavior of organisms."

On p. 868, in exposing the confusion between the temporal and logical criterion of primitivity, we remark: "From the viewpoint of developmental psychology, the developmental progression is defined not by chronological sequence, but by the principle of increasing differentiation and hierarchic integration. It is empirically true that the processes emerging in the actual time sequence frequently conform to the developmental sequence; what occurs earlier in time often involves a greater lack of differentiation than what occurs later. This empirical relationship, however, does not entail the proposition that temporal order of emergence and developmental sequence are of the same logical character."

In light of the above remarks, one may question Mr. Glenn's suggestions that we espouse either (a) the general thesis that all changes over time (evolution, in his usage) are in any single direction, or (b) the specific thesis that such "evolutionary" changes are in the direction of increasing progress. These suggestions rest on the supposition that we identify the developmental progression with changes over time, an identification Mr. Glenn apparently maintains but one that we expressly repudiate in our article.

Also, in light of the quotations from our original article, it should be clear that Mr. Glenn's contention that we describe the direction of evolution (*qua* temporal change) in terms of a tendency to differentiation is again based on a misinterpretation of our viewpoint. First, we do not describe any general direction of changes over time. Moreover, we do not even characterize developmental progression solely in terms of differentiation; we explicitly define the orthogenetic principle as involving both increasing differentiation and hierarchic integration, i.e., integration of articulated components in