In this discussion, I focus on the contributions of Leonard Bloomfield and certain American linguists that have come after him. As a result, I do not comment specifically on those aspects of Professor Maltz's paper that deal with the pre-Bloomfieldian period in American linguistics. There is very little that I could, or would want, to add to his insightful commentary on that period. We may all be grateful for the fruit of his research.

Today's presentations by both Professor Maltz and Professor Uhlenbeck remind us that if we are really going to understand the history of modern American linguistics, we must come to grips with the impact of one person in particular: Leonard Bloomfield. This is not to deny the importance of Edward Sapir, Otto Jespersen, and others that contemporary American linguists single out as their pre-Bloomfieldian intellectual forbears, nor of the many scholars, prominent and obscure, who, as Maltz has shown, laid the foundations of linguistic science in America. What Bloomfield accomplished was the establishment of an autonomous American approach to the study of language. Part of his accomplishment was ideological—he gave American linguistics a solid empirical and positivist outlook—and part was inspirational—his descriptive and historical work, particularly on American indigenous languages, provided models that are still difficult to surpass today, even with our much greater theoretical sophistication. There is a certain irony in the fact that Halle and Chomsky (Halle 1959:13-14; Chomsky & Halle 1968:76) have singled out Sapir's phonological work as foreshadowing their own, since Bloomfield's paper 'Mesomorphophonemes' (1939), in my opinion, surpasses anything Sapir ever wrote.

1 This point has been most forcefully made by Hockett (1958, chapter 2; 1979). In preparing this discussion, I have greatly benefited from Hockett's 1st section of Bloomfield's career, and of his impact on his colleagues and students.

2 The importance of Bloomfield as a precursor of generative phonology is acknowledged by Biver (1993:487).

In phonology, and in its explicitness and rigor, practically achieves the status of a notational variant of generative phonology.

3 In Bloomfield (1939) a problem in Mesomorph phonology very much like the one that Hal's describes in Russian is noted. Among the long-sound phonemes /i, e, a, o, u/ it turns out that /i/ is non-conservative; it alternates with /a/ in environments in which /i/ alternates with /e/. However, /i/ and /e/ contrast, and hence must be separate phonemes. On the basis of past work, only in order to describe the alternation between /i/ and /e/ as morphophonemic, and least by the same rule that describes an alternation between /i/ and /e/ Bloomfield decides to treat /i/ as a separate phoneme. This he is not entirely happy with this solution is reflected in his decision to say that /i/ is not a "full phoneme". Even so, it is clear that Bloomfield, when faced with the option of reducing the phonemic inventory, was willing to sacrifice the generalization expressed by the symmetric phonological rules, the latter.
(1949), that although the two forms are notional variants, they make different claims about what constitutes automatic alteration. And, as Wells was able to show, with certain reasonable assumptions about the facts, the alteration that relates morphophonemic *pa-*tus in Latin to phonemic *pa-*tus is automatic (i.e., morphologically unconditioned) when viewed dynamically, but not automatic (morphologically conditioned) when viewed statically. This is an extremely important demonstration, even if its significance has gone largely unnoticed, because it shows that both theories can be notational variants, and yet make different claims about what is going on in a given language. With respect to the issue at hand, it reveals the superiority of the Sapir-Bloomingfield-Chomsky dynamic framework over the Saussure- Harris static one, since one would want to claim that the Latin alternation (given the factual assumption that Wells made) is an automatic one.

Although the study of phonology and morphology was the dominant concern of American linguistics under Bloomfield and his followers, a great deal of work was done on syntax; so much so, in fact, that the attitude prevailed that syntax was being done really for the first time in the history of linguistics (cf. Hockett 1968:31). Also, I cannot agree with Professor Uhlenbeck's assessment that there is a "conceptual discrepancy" between the theory of immediate-constituent (IC) analysis that was developed for syntax, and the distributional theories of phonemics and morphemics. One has only to examine Harris' paper "From morphemes to utterance" (1948b) and Wells' paper "Immediate constituents" (1947), to see that exactly the same distributional criteria for setting up phonemes and morphemes were used to set up morpheme-structure classes (or ICs). Any similarity between the techniques of IC analysis and traditional parsing procedures is either accidental, or a reflection that both procedures are basically correct. If reading these papers, particularly Wells', one cannot help but admire the degree of precision with which the theory is developed, and the concern for its possible inadequacies (for example in the matter of so-called "discontinuous constituents", which

4 Nida (1948:96) argued that distributional analysis is "only one of the principles which should be employed in determining sets of immediate constituents." However, other scholars developed all rules to distributional analysis.

5 Professor Jonas McCarver remarked from the floor, traditional parsing is formalizable in terms of dependency grammar (Harris 1952), whereas IC analysis is formalizable in terms of phrases to a grammatical structure (Chomsky 1957, Postal 1966). He observes that Bloomfield was aware of this difference.

6 Professor Uhlenbeck could have strengthened his argument on this point by noting Chomsky was later so effective in using to reveal exactly what was wrong with the theory.

At the present time, interest in the study of kinship semiotics is high. The first chapter of this chapter of this book was written in 1949 in the Journal of the Research of Semiotics; seven years later in 1956, the journal appeared back-to-back to this book by Lounsbury (1956) and Goodenough (1956), setting forth in detail the computational-analysis (CA) theory of kinship semantics. Both papers emphasize that CA theory is based on the simple distributional techniques that proved so successful in the analysis of other domains of linguistics, and Goodenough further acknowledges that CA theory is basically just a modification of the semantic theory originally set out by Bloomfield.

Another important paper of the mid-fifties, Wells' "Meaning and use" (1954), suggests that the approach of the British ordinary-language philosophers is compatible with descriptive semantics, and to return the favor, Quine's theory of semantics can be seen as springing directly out of Bloomfield's conception of language. Thus, while some linguists may have viewed semantics as beyond the pale (a set that did not always include Johnson, who in 1958 published a very remarkable and quite insightful discussion of semantic theories in Tugra's "Studies in Linguistics," many others did not, and had the advent of generative grammar in the late fifties not wrecked the field of ordinary-language, or at least of generative grammar in the late fifties not wrecked the field of ordinary-language, or at least of the field of generative grammar and Bloomfield's style of semantic analysis might very well have become the standard for linguistic semantics today.

Let me conclude my remarks by considering briefly what Uhlenbeck says about the period of American linguistics. As the paper in "Chomsky's Syntactic Structures" in 1957 to the present. First of all, Uhlenbeck is correct in seeing the advent of generative grammar as both breaking with and continuing the Bloomfieldian line of development. However, I do not agree with his characterization of certain aspects of the current scene in American linguistics. I mention the minor ones first.

(1) Chomsky never claimed that what was wrong with previous theories of grammar was their lack of recursiveness, even finite-state grammars can be recursive. Rather, Chomsky was objecting specifically to the conception of grammar as a set of rules for selecting a fixed finite corpus of data, a view that was by no means held by all followers of Bloomfield. (2) I am not convinced by Uhlenbeck's argument that American linguistics had failed to develop a sufficiently explicit theory of deep structure. It may be that Chomsky did make everyone aware of the need for explicitness of Chomsky (1948) and Hockett (1958) have both argued that rigor and explicitness are two of the hallmarks of post-Bloomfieldian linguistics, and is it that in fact makes it possible to demonstrate that the theory is inadequate. Take the matter of discontinuous constituents. Without an explicit theory of IC analysis, it would not be possible to show that this notion is a contradiction in terms, and that a theory adequate to account for that notion requires two different levels of syntactic analysis, one of which the constituents can be defined, and the other at which their elements are discontinuous; in other words, a generative-transformational theory of the standard sort. My more serious objections to Uhlenbeck's account of the present state of the field concern his arguments against the way the competence-performance distinction has been used to generate grammar, and his characterization of the relation between semantics and the concept of deep structure.

Concerning competence and performance, Uhlenbeck argues that up to the advent of generative semantics, generative grammarians held that the study of competence should and could take place before and without paying attention to the data of performance. I do not believe that this is correct. Granted that the theory of competence is not a theory about linguistic performance (rather, it is a theory about sentence types, of which tokens may be manifested when people use language to talk to themselves or to one another), it is impossible to know how one could develop such a theory without knowing anything about what people actually say. The relationship between the theory of competence and the performance of a language is not a straightforward one, and it is not possible to develop a theory of competence without taking into account the way the language is actually used.

The data of performance is simply an indirect one, in which the performance data provide evidence for what we may conclude that people know about the structure of their language. These conclusions, what one might call the data of competence, are what the theory of competence is constructed to account for. In any event, I can see no advantage to be had in denying that there is a difference between competence and performance, or in affirming that there is no point to the study of competence.

Finally, concerning the relation of semantics to deep structure, Uhlenbeck contends that the concept of deep structure would not have been developed if the presence in sentences of meaning-marking units had been assumed. Quite the contrary. The concept of deep structure was developed under precisely that condition; Chomsky claimed in Syntactic Structures (1957) that one could and indeed must do syntax without semantics, but did not thereby deny that the elements of sentences were meaningful. Rather, he claimed, prophetically as it turned out, that should the syntactic descriptions resulting from completely inglorious semantics provide the most adequate basis for then, developing semantic descriptions, the syntactic theory so developed receives independent confirmation. If, on the other hand, one builds a theory that blends syntax and semantics together, ludicly, then one has a theory of neither domain. The semantic theory that has been developed by Katz (1962, 1970) on the basis of accepting the results of astoundingly schematic investigations hardly can be thought of as a failure.

To conclude: I do not disagree with Professor Uhlenbeck's conclusion that full investigations of the way language functions in a speech community will be necessary to provide answers to such questions as what the structure of language really comprises, and how universal and individual characteristics are interrelated. I only caution that such investigations will only be successful if they are related to conceptually rich and articulated theories of both language structure and language function.
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discussion of papers by malakel and unhelke


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READER BY E. M. UHLBRECHT:

Regarding Dr. L. Lonsengeen's discussion of the book "The analysis of grammatical continuums," I will first take up some differences of opinion occasioned by expressing the views of my opponent, given the fact that the book in question was written many years ago. I am not, of course, familiar with the contents of my paper for about fifteen years. Now that the symptom is finally overcome, however, I may be able to analyze the situation. However, my critical analysis may be useful in some way to the problem. Dr. Lonsengeen's book has been treated by me in an altered and more refined way. In the following sections, I will first take up some differences of opinion concerning the period before 1957.

In a second paragraph I will briefly go over some issues of Dr. Lonsengeen's book, and then offer a more extensive and critical view of the development of generative grammar and of the present situation in the United States. In a short first paragraph I will present some general observations about my paper.

In my paper I stressed the importance of Bloomfield's adoption of the behavior of the brain, as seen this book from the structural linguistics apart from other factors. Dr. Lonsengeen is silent on this crucial point, but implicitly grayed by reinterpreting a book which I believe that Bloomfield had used major field for the behavior of the brain as philosophy of the brain.

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Bloomfield's views were little interest in linguistic theory, let alone in general and explicit description. No concept of language universal, no less so as to topic did not attract any attention in the field which was given and so preeminent. Many linguists still adhered to Bloomfield's opinion that "the only valid generalizations about language are declarative generalizations." It is certainly true that the notion that linguistics should develop and accept rigorous procedures was widespread, but as Bickel explicitly stated, "most modern theory of structural description has come into existence; nor can Harris's Account in structural linguistics, one of the major theoretical notions of the period, qualify as such.

(3) Langendieck does not agree with my contention that generative grammarians have assumed that the study of competence should and could take place before and without paying attention to the data of performance. According to Langendieck it is inadmissible that a theory of linguistic competence could develop without knowing anything about what people actually say (page 130). We have here a (familial) case in which the theory is not in harmony with actual linguistic practice. There are inescapable anomalies in it in the transformations the general literature about the presence of competence over performance and about the fact that competence encompasses the primary subject of linguistics, and Langendieck must be familiar with them. House's deplorable practice of assigning a linguistic competence without any serious attempt to examine the propositional wall of the phenomena is examined by my contention in this way.

Langendieck's reaction focused me to consider the text of his paper. In rejecting his objections I do not want to give the impression that I consider my characterization of the last half century of American linguistics as uninterpretable and beyond all criticism. I respect that important scholars such as Bickel, Harris, Householder and Pike have not been given the opportunity to express views in one of the recent papers by Bickel in this important and seminal work, Harris as a scholar who forms a bridge between the Post-Bloomfieldian and generative grammar, Householder because of his insightful review of Harris's "measurability is not and because of his positions against Chevallier and Halle, and Pike in an early article of Post-Bloomfieldian doctrine, as a planner in the study of language, a "level of language" and of the level of language." There may be other shortcomings. It may be that I have concentrated too much on professors and on the importance of the interpersonal aspect of the development of American linguistics. There might be other aspects of contributions of the remarkable size of American linguistics in the present century needs the space of a book.

For references, see pp. 148-49.

D. TERENCE LANGENDIECK:

In this sentence, I comment briefly on the specific points made in Professor Uehleke's remarks and then summarize what I take to be the major differences between us. My numbering of sections follows his.

(1) I do not believe that Bloomfield's adoption of the radical behaviorism of "Weiss seriously influence the development of American linguistics. In fact, I do not feel that radical behaviorism affected once Bloomfield's linguistic work (what he said in his polemical articles and review is another matter). American linguists on the whole followed, rather, the more traditional behavior of writers like Tolman and Hall, etc. For an interesting discussion of these matters, see Slobin, 1946. The differences between American and European linguistics of the 1930s and early 1950s, of course, are not so much differences of theory, that rather come of emphasis. For example, although studies of paradigmatic relations, the notion that different combinations of words, sentences, etc. are used, is a point of emphasis in America, where they have not been of the same concern in Europe. For an illuminating discussion of this matter, see Slobin, 1946. The differences between American and European linguistics are to be seen in the loose coupling of theory of the universe of human behavior. There may be other shortcomings. It may be that I have concentrated too much on the importance of the interpersonal aspect of the development of American linguistics. There might be other aspects of contributions of the remarkable size of American linguistics in the present century needs the space of a book.

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