DISCUSSION OF THE PAPERS BY YAKOV MALKIEL AND BY E. M. UHLENBECK

D. TERENCE LANGENDOEN

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 Malkiel'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 Malkiel 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.[1] This is not to deny the importance of Edward Sapir, Otto Jespersen, and others that contemporary American linguists single out as their pre-Chomskyan intellectual forbears, nor of the many scholars, prominent and obscure, who, as Malkiel has shown us, 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 solidly empiricist and positivist outlook – and part was inspirational – his descriptive and historical work, particularly on American indigenous languages, provided models that it is 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 'Menomini morphophonemics' (1939), in my opinion, surpasses anything Sapir ever wrote

[1] This point has been most forcefully made by Hockett (1968, chapter 1; 1970). In preparing this discussion, I have greatly benefited from Hockett's account of Bloomfield's career, and of his impact on his colleagues and students.

in phonology, and in its explicitness and rigor practically achieves the status of a notational variant of generative phonology.[2]

In applying the canons of positivistic methodology, Bloomfield had one major blind-spot, his belief that the features that distinguish the phonemes of a language have objective, measurable and invariant status in acoustic reality. This belief was brilliantly refuted by Twaddell in his monograph *On defining the phoneme* (1935). One consequence of this refutation is that there is no general warrant for the use of the principle of complementary distribution in establishing the phonemes of a language. In the short run, Twaddell's point had no impact; when Haugen and he repeated these objections in 'Facts and phonemics' (1942) against the particular phonemicization of English vowels developed by Trager and Bloch (1941), they were ignored, and Twaddell from that time on remained silent on the issue. But in the long run, the point was heeded, so that by the time of Harris' book *Methods in structural linguistics* (1951), one no longer spoke (that is, those who understood what was going on no longer spoke) of the phonemes of a language. Rather, one said that there are as many phonemicizations and corresponding sets of phonemes as there are decisions about whether or not to impose the criterion of complementary distribution. Accordingly, Halle's classic argument against the autonomous phonemic level in *The sound pattern of Russian* (1959) is fallacious, since it is based on the false premise that there is some particular phonemicization of Russian, required by phonemic theory, with respect to which a single phenomenon is treated in part as an alternation among phonemes, and in part as an alternation among phones. However, there are phonemicizations of Russian for which the alternation in question is entirely morphophonemic in nature. Thus, the claim that the interposition of an autonomous phonemic level necessarily leads to a loss of a generalization about Russian phonology is false.[3] Of course, the fact that phonemic theory

[2] The importance of Bloomfield as a precursor of generative phonology is acknowledged in Bever (1963, 1967).

[3] In Bloomfield (1939) a problem in Menomini phonology very much like the one that Halle describes in Russian is considered. Among the long-vowel phonemes /ī, ē, ǣ, ū, ō, ā/, it turns out that /ū/ is non-contrastive; it alternates with /ō/ in an environment in which /ī/ alternates with /ē/. However, /ī/ and /ē/ contrast, and hence must be separate phonemes. On the basis of pattern congruity, and in order to describe the alternation between /ō/ and /ū/ as morphophonemic, and hence by the same rule that describes an alternation between /ē/ and /ī/, Bloomfield decides to treat /ū/ as a separate phoneme. That he is not entirely happy with this solution is reflected in his decision to say that /ū/ is not a 'full phoneme'. Even so, it is clear that Bloomfield, when faced with the option of reducing the phonemic inventory versus saving a generalization expressed by the system of phonological rules, chose the latter.

should admit of so many alternative descriptions of the same phonomenon is as great a defect as the one that Halle thought he had found in it.

Before passing on to a consideration of the period following Bloomfield's major personal contributions, I should like to comment on Professor Uhlenbeck's list of what he sees as the five defining principles of phonemics, which he claims formed the basis of the extrapolative efforts he discusses. The principles he cites appear to be drawn from at least partially incompatible approaches to phonology, and there is no one, to my knowledge, who can be identified as having held all five. In particular, I know of no one who ever held the first principle – that of contrast and complementarity – and the fourth principle – that of psychological reality – simultaneously.

Like so many others, Uhlenbeck characterizes the period from 1940 to 1957 in American linguistics as 'post-Bloomfieldian', by which he means that a particular mode of analysis that he thinks of as 'Bloomfieldian' gained ascendancy over another that he characterizes as 'Sapirian'. The Bloomfieldian mode he identifies with the static, item-and-arrangement (IA) format; and the Sapirian mode with the dynamic, item-and-process (IP) format. It might, however, be more appropriate to call the former Saussurean and the latter Sapir-Bloomfieldian, since Saussure clearly identified in the *Cours de linguistique générale* (1916, Part I, Chapter 3) the static format with synchronic description and the dynamic format with diachronic description, whereas both Sapir and Bloomfield explicitly used and endorsed a dynamic format for synchronic description. The only essential difference between Sapir and Bloomfield was that Sapir ascribed psychological reality to the entities being described, whereas Bloomfield did not. The development of the IA format furthermore was due, not to Bloomfield, but to a student of Sapir's, Zellig Harris, in a series of review articles during the mid-forties (Harris 1944, 1945a, 1945c, 1947a, 1947b). Throughout this development, Harris was careful to point out that the two modes of analysis are notationally interchangeable, and that any decision to choose one over the other must be based on what the analyst wishes to highlight or focus on in his description. Exactly the same point was made by Hockett in 'Two models of grammatical description' (1954). Thus, I am in disagreement with Uhlenbeck's judgment that: 'From Hockett's "*Two models of grammatical description*" . . . one can conclude that the notion of process in a synchronic sense was difficult to appreciate for the author, as it was also for Harris and Wells.'

Not only did these three linguists have a deep appreciation for the IP format, but Wells was even able to show, in 'Automatic alternation'

(1949), that although the two formats are notational variants, they make different claims about what constitutes automatic alternation. That is, Wells was able to show that, with certain reasonable assumptions about the facts, the alternation that relates morphophonemic *pat + tus* in Latin to phonemic *pas + sus* is automatic (i.e. morphologically unconditioned) when viewed dynamically, but nonautomatic (morphologically conditioned) when viewed statically. This is an extremely important demonstration, even if its significance has gone largely unnoticed, because it shows that two 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-Bloomfield-Chomsky dynamic format over the Saussure-Harris static one, since one would want to claim that the Latin alternation (given the factual assumption that Wells makes) 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 theory of phonemics and morphemics. One has only to examine Harris' paper 'From morpheme to utterance' (1945b) 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-sequence classes (or ICs).[4] Any similarity between the technique of IC analysis and traditional parsing procedures is either accidental, or a reflection that both procedures are basically correct.[5] In 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 (1948:168) argued that distributional analysis is 'only one of the principles which should be employed in determining sets of immediate constituents.' However, these other principles all reduce to distributional analysis.

[5] As Professor James McCawley remarked from the floor, traditional parsing is formalizable in terms of dependency grammar (Hays 1965), whereas IC analysis is formalizable in terms of phrasestructure grammar (Chomsky 1957; Postal 1964). He observes that Bloomfield was aware of this difference.

Chomsky was later so effective in using to reveal exactly what was wrong with the theory).

As for semantics, I think it is an oversimplification to say that the subject was totally neglected by the post-Bloomfieldians. While it is true that in chapter 2 of *Language* (1933), Bloomfield effectively excluded semantics as a proper domain of linguistic study, at least given the then-available tools for its study, he also provided in chapters 9–11 of the same book a framework for a distributional theory of semantics. While the further development of such a theory languished during the way years (but cf. Bloomfield 1943), the postwar period saw a revived interest in it, particularly for the study of kinship semantics. The first surfacing of this interest is a short piece by Greenberg in 1949 in the journal *Philosophy of science;* seven years later, in *Language*, there appeared back-to-back papers by Lounsbury (1956) and Goodenough (1956), setting forth in detail the componential-analysis (CA) theory of kinship semantics. Both papers emphasize that CA theory is based on the same 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 Joos, who in 1958 published a very remarkable and quite insightful discussion of semantic theory in Trager's journal *Studies in Linguistics*), many others did not, and had the advent of generative grammar in the late fifties not wrenched the field of linguistics out of the course set by Bloomfield, Lounsbury-Goodenough-style 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 that dates from the appearance of 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.[6] However, I do not agree with his characterization of

[6] Professor Uhlenbeck could have strengthened his argument on this point by noting

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 accounting for just a fixed finite corpus of data, a view that was by no means held by all followers of Bloomfield.

(2) I am also not convinced by Professor Uhlenbeck's argument that American linguistics had failed to develop a sufficiently explicit theory of linguistic structure, and that it was Chomsky who made everyone aware of the need for explicitness. Chomsky (1964) and Hockett (1968) have both argued that rigor and explicitness are two of the hallmarks of post-Bloomfieldian linguistics, and it is these that in fact make 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 have been possible to show that this notion is a contradiction in terms, and that a theory adequate to account for that notion requires two distinct levels of syntactic analysis, one at 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 in generative 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 inconceivable how one could develop such a theory without knowing anything about what people actually say. The relationship between the theory of competence

Footnote 6 continued

the relation of generative-transformational grammar to Harris' taxonomic conception of transformational grammar (Harris 1957).

and 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-bearing units had been assumed. Quite the contrary. The concept of deep structure was developed under precisely that condition; when Chomsky claimed in *Syntactic structures* (1957) that one could and indeed must do syntax without semantics, he 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 ingoring 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, indiscriminately, then one has a theory of neither domain. The semantic theory that has been developed by Katz (1966; 1972) on the basis of accepting the results of autonomous syntactic investigation 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.[7]

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[7] A point argued in some detail in Bever (1974); see also Bever, Katz and Langendoen 1976.

REFERENCES

Many of the papers referred to here have, conveniently, been reprinted in one or more of the following three anthologies. Hence I give only the date of the original appearance of those papers that have been so anthologized; page numbers refer to the reprinting.

Harris, Zellig S. 1970. *Papers in structural and transformational linguistics*. Dordrecht, D. Reidel Publishing Company.

Joos, Martin (ed.) 1966. *Readings in linguistics I*. University of Chicago Press.

Makkai, Valerie Becker (ed.) 1972. *Phonological theory: evolution and current practice*. New York, Holt, Rinehart & Winston.

Bever, Thomas G. 1963. 'Theoretical implications of Bloomfield's *Menomini morphophonemics*'. *Quarterly progress report of the Research Laboratory of Electronics* 68:197–203.

Bever, Thomas G. 1967. *Leonard Bloomfield and the phonology of the Menomini language*. Unpublished doctoral dissertation, Massachusetts Institute of Technology.

— 1974. 'The ascent of the specious, or there's a lot we don't know about mirrors.' In David Cohen (ed.), *Explaining linguistic phenomena*, New York, Halsted Press, pp. 173–200.

Bever, Thomas G., Jerrold J. Katz, and D. Terence Langendoen (eds.) 1976. *An integrated theory of linguistic ability*. New York, T. Y. Crowell.

Bloomfield, Leonard 1933. *Language*. New York, Holt.

— 1939. 'Menomini morphophonemics', Makkai 58–64.

— 1943. 'Meaning', *Monatshefte für deutschen Unterricht* 35:101–106.

Chomsky, Noam 1957. *Syntactic structures*. The Hague, Mouton.

— 1964. *Current issues in linguistic theory*. The Hague, Mouton.

Chomsky, Noam and Morris Halle 1968. *The sound pattern of English*. New York, Harper & Row.

Goodenough, Ward S. 1956. 'Componential analysis and the study of meaning'. *Language* 32:195–216.

Greenberg, Joseph 1949. 'The logical analysis of kinship'. *Philosophy of science* 16:58–64.

Halle, Morris 1959. *The sound pattern of Russian*. The Hague, Mouton.

Harris, Zellig S. 1944. 'Yokuts structure and Newman's grammar'. Harris 188–208.

—1945a. 'Emeneau's Kota texts'. Harris 209–16.

—1945b. 'From morpheme to utterance'. Harris 100–25; Joos 142–53.

—1945c. 'Navaho phonology and Hoijer's analysis'. Harris 177–87.

— 1947a. 'Structural restatements I'. Harris 217–34.

— 1947b. 'Structural restatements II'. Harris 235–50.

— 1951. *Methods in structural linguistics*. University of Chicago Press.

— 1957. 'Cooccurrence and transformation in linguistic structure'. Harris 390–457.

Haugen, Einar and W. Freeman Twaddell 1942. 'Facts and phonemics'. Makkai 91–98.

Hays, David 1965. 'Dependency theory: a formalism and some observations'. *Language* 33:283–340.

Hockett, Charles F. 1954. 'Two models of grammatical description'. Joos 386–99.

— 1968. *The state of the art*. The Hague, Mouton.

— (ed.) 1970. *A Leonard Bloomfield anthology*. Bloomington and London, Indiana University Press.

Joos, Martin 1958. 'Semology: a linguistic theory of meaning'. *Studies in linguistics* 13:53–70.

Katz, Jerrold J. 1966. *The philosophy of language*. New York, Harper & Row.

— 1972. *Semantic theory*. New York, Harper & Row.

Lounsbury, Floyd 1956. 'A semantic analysis of Pawnee kinship usage'. *Language* 32:158–94.

Nida, Eugene A. 1948. 'The analysis of grammatical constituents'. *Language* 24:168–77.

Postal, Paul 1964. *Constituent structure*. Bloomington and London, Indiana University Press.

Saussure, Ferdinand de 1916. *Cours de linguistique générale*. Paris, Payot.

Trager, George L. and Bernard Bloch 1941. 'The syllabic phonemes of English'. Makkai 72–89.

Twaddell, W. Freeman 1935. *On defining the phoneme*. Joos 55–79 (extract).

Wells, Rulon S. 1947. 'Immediate constituents'. Joos 186–207.

— 1949. 'Automatic alternation'. *Language* 25:99–116.

— 1954. 'Meaning and use'. *Word* 10:235–50.

REJOINDER BY E. M. UHLENBECK:

While listening to Dr. Langendoen at the symposium – the author had not sent me his text beforehand – I became painfully aware of the futility of scholarly exchanges, if not sustained by a sincere wish to understand a point of view different from one's own. I must confess that I have little hope that what I am going to say in this rejoinder will have much impact upon the views of my opponent, given the fact that he had been able to familiarize himself with the contents of my paper for about three months before the symposium was held. However, for other readers the following notes may be useful as they may clear up some confusion due to the necessary briefness of my paper or to the uncomprehending remarks made by Dr. Langendoen. While the text of my paper is identical with the version orally presented (except for some minor stylistic alterations), this rejoinder was written after Dr. Langendoen's revised text had been forwarded to me. It is organized in the following way. First I will take up some differences of opinion concerning the period before 1957. In a second paragraph I will briefly go into some of Dr. Langendoen's objections against my view of the development of generative grammar and of the present situation in the United States. In a short final paragraph I will present some general observations about my paper.

I

(1) In my paper I stressed the importance of Bloomfield's adoption of the behaviorism of Weiss, as this sets his form of structural linguistics apart from other forms. Dr. Langendoen is silent on this crucial point, but implicitly plays down its importance by stating that Bloomfield had one major blind spot namely his belief in the acoustic invariance of phonemes. If a blind spot, it was a minor one in comparison with his acceptance of behaviorism, and still more important, one which did not play a role during the period under review, as Dr. Langendoen himself admits. Bloomfield's views concerning the acoustic reality of phonemes were not so different from those of other linguists, and Twaddell's monograph of 1935 was not specifically directed against Bloomfield but was intended to show that neither a psychological nor a physical or physiological definition of the phoneme was possible. Since Twaddell was convinced of the usefulness of the notion of the phoneme, he concluded that it was best to consider the phoneme as a fictitious unit. What Twaddell did not understand at the time, however, was that phonemes are LINGUISTIC units and cannot as such be defined solely either in psychological or in physical terms.

(2) It is simply not the case, as Langendoen states (page 146), that by 1951 one no longer spoke of the phonemes of a language. As a matter of fact most linguists keep doing this to this very day. It is true that Dr. Langendoen makes a restriction by adding between parentheses: that is, those who understood what was going on. However, since no names are mentioned, one remains completely in the dark to whom Dr. Langendoen is referring.

(3) Langendoen's comments about the extrapolations from notions in phonology are partly irrelevant as they have no relation to what I actually wrote, and partly incorrect. I did not speak of defining principles of phonemics. I did not express the opinion that my list of five notions came from one and the same approach to phonology, although it is not difficult at all to mention linguists in Europe as well as in the United States who simultaneously held to the notion of opposition and to the requirement of psycholinguistic reality (Jakobson and in general the members of the Prague school, Sapir, and Dutch linguists such as Pos, De Groot and Reichling).

(4) Langendoen misinterprets my characterization of the pre-Chomskyan period by applying the terms 'static' and 'dynamic' in a way not found at all in my paper. He gives the impression that I characterized 'the Bloomfieldian mode' as static, something I never did. He also gives the impression that I ascribed the development of the IA-model to Bloomfield, which I did not do either. Furthermore, Dr. Langendoen is in error when he states that Hockett and Wells considered the IA and the IP descriptive models as notational variants. This will become clear to whoever takes the trouble to read Hockett's 'Two models of grammatical description' and Wells's article on 'Automatic alternation'. Hockett wrote (1954:213): 'There is partial translatability between IP and IA but the results of translation are apt to seem somewhat strange', and Hockett's final conclusion was: 'In other words, what we have is two main types of model, neither completely satisfactory. We must have more experimentation as much with one model as with the other – and with the devising of further models too, for that matter – looking towards an eventual reintegration into a single more nearly satisfactory model, but not forcing that reintegration until we are really ready for it' (1954:233). As to Wells's article, the author was not even concerned with the question whether or not IA and IP are notational variants. Moreover, the static and dynamic way of describing automatic alternation, as defined by Wells, 'lead to different results in theory and in practice'. (1949:101) and elsewhere he discusses in a separate paragraph (1949:110–11) the non-equivalence of the two conceptions.

(5) My conclusion that Hockett as well as Harris and Wells experienced some difficulty in appreciating the notion of process was based on certain passages found in articles of these three linguists, listed in my bibliography. Hockett for instance wrote in his 'Two models' article: 'For example, if it be said that the English past tense from *baked* is "formed" from *bake* by a "process" of "suffixation", then no matter what disclaimer of historicity is made, it is impossible not to conclude that some kind of priority is being assigned to *bake*, as against either *baked* or the suffix. And if this priority is not historical, what is it? Supporters of IP have not answered that question satisfactorily' (1954:211). In his article on 'Automatic alternation' Wells connected 'the dynamic conception' with the 'predominantly historical interest of most linguists', and concluded that 'for purely descriptive purposes . . . the dynamic conception has the disadvantage of requiring that for every morpheme that has two or more morphs, one of these be treated as basic to all the others' (1949:113).

(6) I am really surprised at Langendoen's contention that in the pre-Chomskyan period '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'. Langendoen backs up this statement by referring to a page in Hockett's *State of the art*. But this is surely not adequate. On page 31 of his book Hockett admits that 'very little had been published on the syntax of American Indian languages, with which so many of us had served our apprenticeships'. He points out the existence of vast quantities of excellent data on Latin, Greek, and Sanskrit, but were they collected by American scholars? The only syntactic study by an American scholar mentioned by Hockett, is Bloomfield's *Tagalog texts with grammatical analysis* (1917). It happens that as a student of Indonesian languages, I am quite familiar with this valuable volume. It consists of a collection of texts with translation followed by a grammatical analysis largely based on these texts. The part of the

grammatical analysis which is devoted to syntax (pp. 146–209) consists of a series of remarks, valuable for our knowledge of Tagalog, but certainly not innovative as far as syntactic theory is concerned. The syntactic information given in brief numbered paragraphs is very much ad hoc: it serves to explain certain constructions and other syntactic features found in the texts. The theoretical framework is traditional, and does not deviate in any major way from what we find elsewhere in Europe at the time Bloomfield was writing. As Langendoen neither receives factual support for his claim from Hockett, nor, on his own, mentions important syntactic studies made during the period 1940–57, one cannot but conclude that Langendoen has made a statement for which apparently he can not present any evidence.

(7) As to immediate constituent analysis, I am not the only one who has noticed its parallelism with traditional parsing methods. In his *Constituent structure*, Postal wrote (1964:67): 'The conceptions represented by P-markers are really traditional. The earlier grammatical analysis which spoke of parsing a sentence etc. utilized in essence just these ideas. For example, on the highest level the sentence was not only divided into two parts, but these were named "Subject" and "Predicate". Similarly on a lower level, each word was assigned to one or more labelled constituents called "parts of speech" etc.'

(8) As to the study of semantics, Langendoen considers it 'an oversimplification [apparently on my part] to say that the subject was totally neglected by the Post-Bloomfieldians', and then goes on to mention in support of this view a short article by Greenberg of 1949 and the two *Language* articles by Lounsbury and Goodenough of 1956, all three on kinship semantics, Wells's well-known piece on 'Meaning and use' in *Word* of 1954, and finally Joos's article in *Studies in Linguistics* of 1958. First of all I must point out that in my paper I never made the oversimplification that semantics was 'totally neglected'. In fact, when one has to survey a large body of literature belonging to a field as extensive as linguistics, there is little room for sweeping general statements, and in my paper I carefully observed that 'in the early fifties the reluctance to discuss "meaning" was somewhat lessening' (p. 130 of my paper). Apparently Langendoen has little use or feeling for nuance. Nevertheless I consider it an undeniable CHARACTERISTIC of linguistics as practised by the Post-Bloomfieldians that little attention was given to semantics. The evidence for this is simply overwhelming. Some of it has been presented in my paper. Even the articles mentioned by Langendoen corroborate my point. This is particularly true of Wells's interesting paper on 'Meaning and use' which, by the very cautious, not to say gingerly, way in which he approaches his topic, clearly shows how uncertain at that time many American linguists were about the relationship between semantics and linguistics.

II

Against my treatment of the development of transformational generative grammar Dr. Langendoen has raised four objections, two minor and two major ones. I will take up all four, in the order in which he presents them.

(1) On page 131 of my paper I was concerned with the 'five interrelated' arguments on which the claims of the superiority of generative grammar were based. One of the superior features of such a grammar was the feature of recursive rule application. It cannot be denied that, especially in the early years, recursiveness played a fundamental role. Language was viewed as 'a set (finite or infinite) of sentences, each finite in length and constructed by concatenation out of a finite set of elements' (Chomsky 1962). Accordingly a grammar which had the task of enumerating and describing all the sentences of the language was viewed as a set of rules that '(in particular) recursively specify all the sentences of the language'.

(2) I regret that I have not been able to convince Langendoen that among the Post-

Bloomfieldians there was little interest in linguistic theory, let alone in general and explicit theories. The concept of language universals, nowadays so fashionable a topic, did not attract any attention in the fifties which were in general not prone to speculation. Many linguists still adhered to Bloomfield's opinion that 'the only valid generalizations about language are inductive generalizations'. It is certainly true that the notion that linguists should develop and apply rigorous procedures was widespread, but as Bloch himself explicitely stated, 'no unified theory of structural description had come into existence'; nor can Harris's *Methods in structural linguistics*, one of the major theoretical treatises of the period, qualify as such.

(3) Langendoen 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 Langendoen it is inconceivable that a theory of linguistic competence could develop 'without knowing anything about what people actually say' (page 150). We have here a (familiar) case in which the theory is not in harmony with actual linguistic practice. There are innumerable statements to be found in the transformational generative literature about the precedence of competence over performance and about the fact that competence constitutes the primary subject of linguistics, and Langendoen must be familiar with them. Hence the deplorable practice of assuming that a linguist could arrive at judgments about sentences just by some kind of introspective activity. Now it has finally been understood (as recently by McCawley; see his interview with Parret 1974) that this activity consists of trying to construct a situation in which the sentence under review could be used. If such a situation is found, the sentence is considered to be 'grammatical'. Therefore, unwittingly, generativists have somehow had to take into account data of performance, BUT this is something quite different from an intentional and systematic study of the factors involved in the act of speech. It is one of the most obvious weaknesses of transformational generative research up till about 1970 that no expertise was developed on this crucial point.

Before I can turn to the last of the four objections raised by Langendoen I have to insist that I never stated that there is no difference between competence and performance, nor that there is no point in the study of competence, as Langendoen seems to intimate on page 151 of his reply. What I want to stress is that the relationship between competence and performance is not at all a simple one and is still in need of further clarification. I expect that the way Chomsky distinguishes between the two will prove to be more of a hindrance to linguistic theory than anything else; for a similar opinion, compare Jacques Bouveresse in Parret 1974).

(4) Langendoen's few critical remarks concerning my conclusions about the reasons why the concept of deep structure came into existence prove that he has not yet grasped what I had to say about the linguistic sign (see page 135 of my paper). He still has confidence in the semantic theory of Katz, without, however, refuting or even paying attention to the fundamental criticisms made against Katz' views on semantics by Bolinger, Lyons, Cohen and others.

III

Langendoen's reaction forced me to reread and rethink the text of my 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 unassailable and beyond all criticism. In retrospect I regret that important scholars such as Bolinger, Harris, Householder and Pike have not been given the place they ought to occupy in any account of the recent past: Bolinger as a fundamental and persistent critic of generative grammar, Harris as a scholar who forms a bridge between the Post-Bloomfieldians and generative grammar, Householder

because of his insightful review of Harris' *magnum opus* and because of his polemic against Chomsky and Halle, and Pike as an early critic of Post-Bloomfieldian doctrine, as a pioneer in the study of intonation and also because of his far-ranging 'unified theory of the structure of human behavior'. There may be other shortcomings. It may be that I have concentrated my attention too much on presenting an overall picture of the historical development of only the dominant groups in American linguistics, but I found that the characterization of the main lines of this development was my first task on this festive occasion. I am fully aware that a complete, balanced and well-articulated account of the remarkable rise of American linguistics in the present century needs the space of a book.

For references, see pp. 140–44.

SURREJOINDER BY D. TERENCE LANGENDOEN:

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

I

(1) I do not believe that Bloomfield's adoption of the radical behaviorism of Weiss seriously affected the development of American linguistics. In fact, I do not find that radical behaviorism affected even Bloomfield's linguistic work (what he said in his polemical articles and reviews is another matter). American linguists on the whole followed, rather, the mediated behaviorism of writers like Tolman and Hull – see, for an interesting discussion of these matters, Schlauch 1946. The differences between American and European linguists of the 1940's and early 1950's, therefore, are not so much differences of theory, but rather ones of emphasis. For example, although studies of paradigmatic relations, one of the primary concerns of European linguistics during this period, did not flourish in America, neither did they totally languish; one need only cite Bloch's work on Japanese morphology (Bloch 1946a) to see that they did not.

Concerning Twaddell and the phoneme, I would maintain that Twaddell did see the phoneme as a linguistic unit. Twaddell's point was that the only way of saving the notion of the phoneme for linguistics was to view it as an abstract entity.

(2) I had in mind particularly Harris and Hockett. The following observation by Miller (1969:xxv) also bears testimony to my claim.

The phonemics article [Bloch 1950] ... probably represents the apogee of American descriptivist phonemic theory and practice. It was the last such major paper to appear in *Language*, and in the years immediately following the problems and methods with which it concerned itself – as well as the full-blown "phonemics paper" itself as a genre for linguistic publication – would begin to hold less and less interest for many linguists.

(3) I see that I did misunderstand Uhlenbeck's point in his listing of five prominent notions of the phonological theories of the period; I also stand corrected on the matter of the existence of those who held the notions of contrast and complementarity, and that of psychological reality simultaneously, although I believe that Sapir's name should be dropped from the list, since he explicitly rejected what later came to be called the principle of complementary distribution (Sapir 1924, note 2).

(4) I derived my alleged misinterpretation primarily from the two paragraphs on pp. 129–30. By 'notational variants', a technical term not found in the literature under discussion, I meant 'notations in which one can express the same properties and relations for a given

class of phenomena, but which do not necessarily classify any given phenomenon the same way'. I am sorry for the misunderstanding that my failure to define this notion in my discussion gave rise to.

(5) With this clarification, I withdraw my disagreement with Uhlenbeck's judgment concerning Hockett's, Harris's, and Wells's difficulties in appreciating the notion of process in a synchronic sense.

(6) I present as evidence such well-known and major studies on syntax as Nida (1943), Harris (1945b), Bloch (1946b), and Wells (1947).

(7) The issue is not whether IC-analysis has its roots in traditional syntax, but whether there is any discrepancy between it and the distributional analysis that was proposed for phonology and morphology. Uhlenbeck has not presented any substantiation for his claim that such a discrepancy exists.

(8) My objection to Uhlenbeck's discussion of post-Bloomfieldian semantics centers on the last part of the following passage (p. 130).

It may be true ..., that in the early fifties the reluctance to discuss 'meaning' was somewhat lessening, but it is hard to find signs of a serious attempt to ANALYZE [emphasis his] the semantic facts of even to study what had been done in semantics so far.

The semantic studies I cited represent serious attempts during that period to analyse semantic facts, which do contain considerations of what had been done in the field previously.

II

(1) What Uhlenbeck says here is true, but concerns, perhaps, another point. Recursiveness is implicit in IC-analysis.

(2) Here, we simply disagree.

(3) I accept Uhlenbeck's protestation that he does not wish to do away with the competence-performance distinction. It is still not clear to me, however, that Uhlenbeck wishes to make the distinction, which I take to be crucial with respect to the development of an adequate competence-based theory of semantics, between 'what is said' and 'what is meant'. For example, an utterance of 'Can you pass the salt?', on normal occasions of its use around a dinner table, literally inquires about the addressee's ability to pass the salt. This is what must be represented as the meaning of the sentence. The fact that such tokens of the sentence-type communicate the speaker's desire to have the salt passed to him can be accounted for in a performance theory that considers the semantic representation of that sentence, together with principles of language use (pragmatic principles) that are not themselves incorporated in the grammatical system.

(4) Again, we simply disagree. This is not the place to go into a detailed consideration of semantic theory.

III

My reading of the recent history of American linguistics differs from Uhlenbeck's primarily in that I do not recognize that radical behaviorism had any important impact on the development of linguistics in America, and that work in American linguistics was almost exclusively devoted to phonology and morphology. Rather, I hold that linguistic theory in America, as it applied to phonology, morphology, syntax, and semantics, developed organically during this period into a full-fledged theory of language. By the mid-fifties that theory included the notion of syntactic transformations, and the rise of generative grammar shortly afterwards should be viewed as a reinterpretation of that theory, not as a replacement of it.

ADDITIONAL REFERENCES:

Bloch, Bernard 1946a. 'Studies in colloquial Japanese I. Inflection;' 'III. Derivation of inflected words'. *Journal of the American Oriental Society* 66:97–108; 304–15.

— 1946b. 'Studies in colloquial Japanese II. Syntax'. Joos 154–84.

— 1950. 'Studies in colloquial Japanese IV. Phonemics'. Joos 329–48.

Miller, Roy Andrew (ed.) 1969. *Bernard Bloch on Japanese*. New Haven. Yale University Press.

Nida, Eugene A. 1943. *A synopsis of English syntax*. Doctoral dissertation. University of Michigan.

Sapir, Edward (1924). 'Sound patterns in language'. Joos 19–25.

Schlauch, Margaret (1946) 'Early behaviorist psychology and contemporary linguistics'. *Word* 2:25–36.