may in part be true, but Stephens' error is the ultimate source of Stoll's error.

Finally, no one would any longer suggest that Huastec may be the stem of all Mayan languages; in fact I have called into question the assumption that Huastec may have been the first to branch off from Proto-Mayan.[8]

Gatschet's classification is an important stage in the history of Mayan studies. His insights should not be forgotten, and his errors should be understood.

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A LEONARD BLOOMFIELD ANTHOLOGY, Charles F. Hockett, ed., xxix + 553 pp. Indiana University Press, Bloomington, Indiana 1970. $22.50

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This selection of Bloomfield's articles is a welcome volume indeed, and Charles Hockett is to be thanked for having prepared the material and for his many clarificatory comments. Much of the material is so valuable, and so apposite to the confusion of current linguistic discussion today, that I hope a paperback extract is published soon. Such a paperback should include certainly the Postulates paper (pp. 128–38 here), On recent work (173–90), A note on sound-change (212–13), the Ries review (231–6), Linguistic aspects of science (307–21), and the impressive structural articles Menomini morphophonemics (351–62) and Algonquian (440–88). It would also be quite educational to include the reviews of Jespersen, Hermann, Havers, and Swadesh, and the papers Sentence and word, Subject and predicate, Linguistics as a science, The structure of learned words, The stressed vowels of American English, Meaning, Secondary and tertiary responses to language.

Leonard Bloomfield was a major worker in comparative Germanics and in descriptive informant-based grammar (especially in Algonquian, but also in Tagalog, a Philip-

pine language). In addition, he and, separately, Edward Sapir were what one might consider the founders of modern linguistic theory. Bloomfield's fundamental contributions to theory (seen as 'The Bloomfield School') were misappreciated and opposed by many European linguists, and latterly by Americans; and it is his work in this area which will be reviewed here.

Sapir and Bloomfield were the final developers of the concept of the phoneme, which had grown out of the work of the English phoneticians and of the investigators of sound change, and whose recognition had been initiated in the work of de Saussure, Baudouin de Courtenay, Boas, and Trubetzkoy. Sapir was among the creators of the concept, in his Language (1921), and in his eophonemic Sound patterns in language (1925; in D. G. Mandelbaum, Selected Writings of Edward Sapir, p. 33); and he developed the use and methodology of phonemics in his later work and discussions (especially in his 1933 paper La réalité psychologique des phonemes, Mandelbaum, op. cit. p. 46, and in some of the ideas which appeared in Swadesh' The phonemic principle, Language 10 (1934) 117). Bloomfield, in turn, presented the first comprehensive view of phonemics in A set of postulates for the science of language (1926; pp. 128–38 of this volume) and in his epoch-making book Language (1933). Later, as a necessary correction on this, both men also developed the concept of the morphophoneme, going beyond the work of Baudouin de Courtenay and Trubetzkoy: Bloomfield in Menomini morphophonemics (1939, pp. 351–62 here); Sapir in the 1933 paper above and in Sapir and Swadesh, Nootka Texts, 1939.

Phonemics was the crucial advance; without what it yielded one could not move on to a science of language, toward which Bloomfield was consciously working (e.g. p. 93, 103). In the service of these more general interests, Bloomfield presented impressive arguments against the use of teleological explanations for language change,

[8] Campbell, op. cit., pp. 299–300.

such as the view that semantic convenience fosters change, or blocks change in particular words: cf. the reviews of Jespersen, Hermann, Ries, Havers. He also argued against the use of semantic explanations for syntactic phenomena (e.g. p. 403, 421 here, and the review of Swadesh): in the latter, for example, he discusses (p. 407) "the prescientific and indeed barbarous character" of "the notion that, beforehand and independently of any particular act of observation, one could formulate, by virtue of some sort of philosophic acumen, a realistic outline of the universe which would serve as a frame of reference for statements of the meaning of linguistic forms." And he gives obvious and telling arguments against the claim (later associated with the name of Benjamin Whorf) that the grammar of a language reflects or conditions the perceptions of its speakers (e.g. 285).

In place of these non-controllable semantic characterizations, which he considered all too easy to make in order to obtain an apparent result, Bloomfield developed conceptual tools for a purely morphological and syntactic characterization of language entities. These tools included the concept of partial similarity among sentences as a basis for segmentation and for classification of the segments. They included the concept of selection (co-occurrence preferences) between the individual words (or morphemes) within classes which 'occur' together. A particularly valuable tool of analysis was the relation of being immediate constituent. This relation served as a single property which one could use recursively in decomposing a sentence down to its elementary segments. These concepts gave the intellectual equipment, if not quite the methods, for describing a system of 'syntactic' relations among the elements of a sentence— something which was to be known later as structural linguistics. For Bloomfield, this body of concepts was the basis for a specification of descriptive linguistics, as distinct from historical and comparative linguistic

considerations, and without appeal to semantics. With these tools he built the descriptive section of his book Language, which set the stage for linguistic thinking for a whole generation.

Just as the phoneme was defined with the aid of the relative occurrence (complementary, free-variant, contrastive) of phones, so Bloomfield was here defining syntactic elements by their relative occurrence, which linguists somewhat confusingly called distribution. Although Sapir, and indeed the Boas students generally and all makers of satisfactory grammars of 'native' languages, worked explicitly on such grounds, it was a new and rather courageous claim on Bloomfield's part to say that distribution and not meaning was the criterion for syntactic analysis. Even Bloomfield stopped short of using, or seeing, the distributional relation in cases where the phonemic difference between complementary or free-variant morphs was too great. Thus he accepted, in addition to the distributional criteria, also such 'processes' as suppletion (although suppletion is nothing more than a complementary alternation between morphs which have no appreciable phonemic similarity). In a somewhat similar way, the early phonemicists balked at putting into one phoneme two phones which had no phonetic similarity to each other. These were, however, only the hesitations of novelty. Bloomfield himself noted that there was no essential syntactic difference between morphemes and intonation or contrastive stress; that things which were expressed by intonation in one language might be expressed by a morpheme in another. The generality of his views may be gauged from the fact that when I once asked Bloomfield whether he would agree that the distribution of an element was or should be the sole criterion for the syntactic characterization of that element, he answered yes. Perhaps it is relevant that on another occasion Bloomfield said to me that these ideas—his ideas—were hard for him to un-

derstand fully, and that he expected they would be easy only for the next generation.[1]

Bloomfield's work in syntactic theory, then, was the establishing of the basic concepts for a science of syntax. From the vantage-point of the later developments in transformational analysis, his work has been attacked as inadequate, in effect for not having reached transformations. One wonders at such an odd activity as attacking the past for not being the present. The history of science is that each scientist builds, directly or indirectly, upon the results of his predecessors, whether he is adding observations posed in their terms or is erecting a new system of relations to fill out what they had not reached. It is true that in some fields such as philosophy, where instead of a progression we may find certain types of views recurring (in new form) from time to time, there is a tradition of one school of thought attacking its predecessor. And, of course, we have such attacks in the politics of competition for power. In science, however, that is not to the point.

It is true that Bloomfield did not have the concept of transformations, as can be seen in many places, e.g. in his discussions here on pp. 68 (on compounds) and 76 (on the passive, etc.). Neither did Sapir have this conception; his ideas in this area were not in terms of a grammatical method, but were a more sophisticated version of the general recognition that there were such forms as passive by the side of active, etc. Both men died relatively young: Sapir at 57, Bloomfield cut off from work by a stroke at 59; we do not know what they might have yet discovered. But if they did not have transformations, neither could transformations have been developed without their work and especially that of Bloomfield. The discovery of the transformational relation came in fact in a quite conscious search to refine the distributional 'Bloomfieldian' linguistics in order to reach beyond the gross sentence-structures where Bloomfield's methods stopped. It was found quite soon that if we take as the object of investigation not a single sentence (regarded as wholly independent of anything outside it—including neighboring sentences) but discourses as they occur in nature, then the successive sentences of the discourse are not wholly independent of each other. But the dependence between them is not in terms of the syntactic structure—the constituent expansion of word-classes, etc. Rather, it was in the selection of the particular morphemes within the classes. The dependence, or preservation, of selection turned out to be a general relation which was called a system of (partial) transformations on the set of sentences.

True, one might argue that the transformational relation might have been discovered independently of this residual problem of Bloomfieldian linguistics, by a direct raising of the question: In what general way can one sentence be said to have grown out of another (aside merely from constituent expansion). But this is not what happened, and there is no reason to think that anyone would have raised such a question—which can be regarded as the basic question of transformational linguistics—out of the blue. In any case, such a question would have led to the discovery of transformations only if it rejected as solutions all merely semantic paraphrases, and had sought only solutions (derivational 'growth' relations) which could be expressed strictly in formal terms without dependence upon meaning. It was Bloomfield who established this latter approach as a necessary condition for the solution of a grammar-making problem.

And now, a word about Bloomfield. There was, of course, no 'Bloomfield School'. There was perhaps a Bloomfield method, more precisely a Bloomfield view of language structure. There were no Bloomfield followers. There were a very few students of his who adhered closely to his views in detail. Mostly, people learned from him certain

[1] In a similar vein, the great algebraist, Emil Artin, once said to me that modern notions like mapping, which he had helped develop, were still somehow hard for him, although they were second nature to the students to whom he himself had taught the ideas.

general methods and approaches; and they learned from his rigorousness and the esthetic precision of his writing, clean as it was of any special pleading (see for example the beautiful two-page A note on sound change). At the points where his ideas were arbitrary or not justifiable—and to whom does this not happen—people did not follow him. Thus, valuable as were his somewhat simplistic exhortations against 'mentalism' (in the terms of those days), Bloomfield's espousal of a particular current school of Behaviorist psychology as an interpretation of linguistics was arbitrary and not supportable. This was clear to almost everyone from the start, and neither his students nor his readers took it up. (Nor did it lower his value or his standing in linguistics.) Even within linguistic theory, there was at least one extraneous and not well-founded thing that he proposed; this was rarely used by anyone, and simply ignored. I am referring to his chapter on Sememes in his book Language: I had once told him that the ideas in this chapter seemed questionable to me, and he answered that he didn't quite know why he put that chapter in and didn't put much stock in it.

I have to refer to one other misconception. I am sometimes asked if there was any antagonism between Bloomfield and Sapir. These were two age-mates with very different backgrounds, who did not know each other until each had appeared on the scene as major theoreticians at the birth of a science. The question is posed from the competitive values of this society. The answer is no. Publicly and in print they always spoke with great respect about each other, and praised and used each other's work. Privately, they knew each other very little and had no particular warmth for each other—their styles and personalities were indeed exceptionally different—but each one, and his students, spoke with respect for the other and above all with appreciation for the other's linguistics. I was close to each of them in their last years and never heard a derogatory comment.

Neither competed, or saw his scientific achievement as a matter of personal aggrandisement. And this was not for lack of a sense of history about their work. Both men knew that they were creating—or rather participating centrally in the creation of—a science. There was an excitement around them, in their ideas, among their students and colleagues. Each of them pushed for his ideas—Bloomfield by incisive argument, Sapir by brilliant exposition—though without seeking to pre-empt the field. Each was, to the good fortune of those who knew them and I hope of themselves, an extremely decent person of high integrity; each had utter and explicit contempt for the posturings and status in this society as well as for its vast injustice and inequality. They were people not with ambition, least of all with ambition in the terms of this society, but rather with satisfaction in what they were producing. Those who remember Bloomfield and Sapir know this about them.

While it is pleasant, indeed touching, to remember them as people, this is not what is relevant to the development of linguistics. The work of Bloomfield can be looked at as paving the way for the later methods of transformational analysis. But his work is not only of historical relevance. It created the apparatus for a certain type and degree of linguistic analysis, and a body of analytic concepts which are a necessary part of any theory of grammar.

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LINGUISTIC VARIABILITY & INTELLECTUAL DEVELOPMENT. By Wilhelm von Humboldt. Translated by George C. Buck and Frithjof A. Raven†. Miami Linguistic Series No. 9. University of Miami Press: Coral Gables, Florida, 1971. Pp. XXII, 296. $15.00.

W. KEITH PERCIVAL

Readers of IJAL will welcome the appearance of the first full-length English translation of Wilhelm von Humboldt's celebrated treatise on language diversity.[1] Humboldt's

[1] Ueber die Kawi-Sprache auf der Insel Java,