American structuralism and European structuralisms: How they saw each other

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Abstract. *This paper examines how American structural linguists and their European counterparts saw each other from roughly the 1920s to the 1960s. American linguistics had deep roots in Europe, though by the late 1930s, most American structuralists had turned their back on the old continent. Attitudes towards the Europeans started to warm in the late 1940s and into the 1950s. Prague School conceptions had a major influence on generative grammar (at least as far as phonology is concerned) and on the nascent functionalist movement in the United States. From the European side, there was some, but not a great deal, of interest in American theorizing until the late 1940s. A real rapprochement was underway in the 1950s, which was derailed by the appearance of generative grammar, an approach that at the time most European structuralists rejected.*

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1. Introduction

This paper discusses the complex interactions between European and American structural linguists between the 1920s and the 1950s.[41] In fact, in this period a majority of the linguists in the world who identified as 'structuralist' were located in Europe. In Prague, Ge-

41. I would like to thank Stephen R. Anderson, Hans Basbøll, Julia Falk, Louis de Saussure, and Klaas Willems for their input on this paper. Errors are my own. The following abbreviations are used: *LSAB* (*Linguistic Society of America Bulletin*); LSAA (Linguistic Society of America Archives at the University of Missouri); TSA

neva, Copenhagen, Paris, London, and elsewhere there were major centers of structural linguistics, each embodied with its own distinctive traits and in some cases its own academic journal. However, the purpose of this paper is not to present, contrast and evaluate the various versions of European structuralism. A number of books have appeared that do just that, and I have no desire to repeat what they have had to say (I particularly recommend Lepschy 1972). Rather, I focus on the reciprocal relations between structural linguists in Europe and the United States. That is, I examine how practitioners of the two geographical varieties of structuralism saw each other, what their mutual influences (or lack of influences) were, and how all of this changed in the time period under discussion.

The paper is organized as follows. Section 2 provides the necessary background, by overviewing the state of linguistics in the United States in the mid-twentieth century.

Section 3 shows how indebted American linguists were to Europe in the early years of the Linguistic Society of America (LSA), while section 4 documents the Americans' increasing isolation in the following years. Section 5 reviews European attitudes towards work carried out in the United States and section 6 documents the increasing American appreciation of European theorizing that began in the late 1940s. Section 7 describes the European reaction to early generative grammar and section 8 the Prague School influence on American functional linguistics. Section 9 is a brief conclusion.

2. American linguistics in the mid-twentieth century

American linguistics in the late 1940s and early 1950s was dominated by the intellectual heirs of Leonard Bloomfield (1887–1949) and Edward Sapir (1884–1939). As far as synchronic studies are concerned, the majority of synchronic linguists based in the United States were 'structuralists', or 'structural linguists'. Put simply, their goal was

(Thomas Sebeok Archives at Indiana University); and RJA (Roman Jakobson Archives at MIT).

to elucidate the structural system at the heart of every language.[42] Bloomfield's classic work *Language* (Bloomfield 1933) set the tone for most mainstream American linguists in the mid-twentieth century. Bloomfield was by the 1930s quite anti-mentalist and was in touch with the logical empiricist philosophers of the Vienna Circle.[43] He contributed a monograph on linguistics to their *International Encyclopedia of the Unified Sciences*. This monograph, *Linguistic Aspects of Science* (Bloomfield 1939a), is the clearest statement in print on the intimate relationship between empiricist philosophy, behaviorist psychology, and structural linguistics. Bloomfield united all three in the following famous passage:

> If language is taken into account, then we can distinguish science from other phases of human activity by agreeing that science shall deal only with events that are accessible in their time and place to any and all observers (strict BEHAVIORISM) or only with events that are placed in coordinates of time and space (MECHANISM), or that science shall employ only such initial statements and predictions as to lead to definite handling operations (OPERATIONALISM), or only such terms that are derivable by rigid definition from a set of everyday terms concerning physical happenings (PHYSICALISM). (Bloomfield 1939a, 13)

Given such strictures, it follows that "the only useful generalizations about language are inductive generalizations" (Bloomfield 1933, 20). That in turn led Bloomfield to be sceptical that meaning,

42. Many American linguists at the time preferred the self-designation 'descriptivist' to 'structuralist'. Confusingly, however, not all linguists whose goal was to describe languages saw them as integrated structural systems. Franz Boas (1858–1942), for example, was a descriptivist, but not a structuralist.

43. Much earlier, however, his work was grounded in Wundtian (mentalist) psychology, which is reflected in his book *Introduction to the Study of Language* (Bloomfield 1914). Sources agree that his turn to behaviorism was in large part a result of discussions with his Ohio State colleague, the psychologist A. Weiss (1879–1931). Even in the 1930s, Bloomfield did not deny the existence of mental life or meaning or deny that linguistic forms have meanings. He felt, though, that an account of those meanings must necessarily involve an encyclopedic knowledge of the world that was quite inaccessible to linguistics. For discussion, see Anderson 2021, §12.2.

"the weak point in language-study" (p. 140), could play a central role in grammatical analysis. But Bloomfield felt quite conflicted here. While he was adamant that "The study of language can be conducted without special assumptions only so long as we pay no attention to the meaning of what is spoken" (p. 75), he felt that "as long as we pay no attention to meanings, we cannot decide whether two uttered forms are 'the same' or 'different'." (p. 77) and hence inevitably "phonology involves the consideration of meanings" (p. 78). Nevertheless, "linguistic study must always start from the phonetic form and not from the meaning" (p. 162).

Sapir was no less a structuralist than Bloomfield; indeed, papers such as Sapir (1925) and Sapir (1963 [1933]) probably did more to lay the foundations for structural linguistics in the United States than did Bloomfield's *Language*. In the opinion of Zellig Harris (1909–1992), one of the leading mid-century American linguists, "Sapir's greatest contribution to linguistics, and the feature most characteristic of his linguistic work, was [...] the patterning of data" (Harris 1951b, 292). Unlike Bloomfield, however, Sapir was not an empiricist. One has to describe him as more 'intuitive' than Bloomfield, whereby flashes of genius led him to a brilliant analysis of some linguistic phenomenon, but without some particular philosophy of science that gave that analysis a theoretical and methodological underpinning.[44] Sapir's bucking the empiricist tenor of the times and his untimely death at the age of fifty-five resulted in his having less influence than Bloomfield over the next generation of linguists.

The most influential tendency within American structural linguistics at mid-century followed Bloomfield's theoretical pronouncements, though often not his actual practice. Linguists customarily included in this group are Zellig Harris, George Trager (1906–1992), Bernard Bloch (1907–1965), Martin Joos (1907–1978), Henry Lee

44. Sapir and Bloomfield had deep respect for each other, but with certain reservations. Sapir admired Bloomfield's ability patiently to excerpt data and to file and collate slips until the pattern of the language emerged, but spoke deprecatingly of 'Bloomfield's sophomoric psychology'. Bloomfield was dazzled by Sapir's virtuosity and perhaps a bit jealous of it, but in matters outside of language referred to Sapir as a 'medicine man' (Jakobson 1979, 170).

Smith, Jr. (1913–1972), and (at least at as far as his earlier work is concerned) Charles Hockett (1916–2000).[45] Bloomfield's view of science, which members of this group adopted enthusiastically, pointed to linguistic descriptions that were essentially catalogues of observables and generalizations extractible from observables by a set of mechanical procedures: "The overall purpose of work in descriptive linguistics is to obtain a compact one-one representation of the stock of utterances in the corpus" (Harris 1951a, 366), that is, the requirement that all distinctive elements in a corpus be analyzed in the most efficient economical way. Given their subjective nature, informants' judgments were looked upon with suspicion, except perhaps for the judgment as to whether two words or utterances were 'the same' or 'different'. Analyses embodying underlying representations and derivations involving rule ordering were indeed mooted from time to time (see Swadesh & Voegelin 1939, Bloomfield 1939b, Wells 1949, and, for discussion, Newmeyer 2022, ch. 4), though they were never a popular view, given that they appeared to be incompatible with empiricist strictures. Charles Hockett wrote that he could not conceive of any meaning to 'ordering' but an historical one:

> If it be said that the English past-tense form *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? (Hockett 1954, 211)

For the most empiricist of the descriptivists, the idea was to arrive at a grammar of a language by performing a set of operations on a corpus of data, each successive operation being one step farther removed from the corpus. These operations, later called 'discovery

45. See Hymes & Fought (1981, 128; Murray 1983, 173; Hall 1987, 59; and Koerner 2002a) for an (often conflicting) breakdown of American structural linguists into various categories. The views of Hockett and many others evolved over the years, making it sometimes difficult to pigeonhole particular individuals as being in particular 'camps'.

procedures', aimed at the development of "formal procedures by which one can work from scratch to the complete description of the pattern of a language" (Hockett 1952a, 27). It followed then that the levels of a grammatical description had to be arrived at in the order: first, phonemics, then morphemics, then syntax, then discourse: "There is no circularity; no grammatical fact of any kind is used in making phonological analysis" (Hockett 1942, 20).[46] In actual practice, however, few if any linguists followed a set of (cumbersome) step-by-step procedures that were, in principle, necessary to arrive at a full grammar (for discussion, see Ryckman 1986, ch. 2). Rather, they presented analyses which, in retrospect examination, *could have been* arrived at by means of these procedures.[47]

The order of discovery of each level of the grammar was reflected, not surprisingly, in the number of publications devoted to each level. There were many more papers on phonemics than on morphemics, and many more on morphemics than on syntax or discourse. As Robert A. Hall, Jr. (1911–1997) explained: "Descriptive syntactic studies have also been rather rare; but, since they normally come at the end of one's analysis, the tendency is perhaps to hold them for incorporation into a more complete description" (Hall 1951–1952, 120).

Three groupings existed that were less influenced by a rigid empiricist methodology than the mainstream. One was made up of Sapir's students, most of whom were based at some distance from the American East Coast and were focused more on the description of indigenous languages than on debates about procedures. Morris

46. By the late 1940s it was widely recognized that phonemic analysis could be simplified by appeal to (higher level) morpheme and word boundaries. The problem was that while such boundaries were at times signaled phonetically (cf. *nitrate* and *night rate*), most of the time they were not (cf. *minus* and *slyness*). There was no general consensus on how to deal with this problem.

47. Appeals to meaning in phonemic analysis were commonplace: 'The basic assumptions that underlie phonemics, we believe, can be stated without any mention of mind and meaning; but meaning, at least, is so obviously useful as a shortcut in the investigation of phonemic structure — one might almost say, so inescapable — that any linguist who refused to employ it would be very largely wasting his time' (Bloch 1948, 5).

Swadesh (1909–1967), Mary Haas (1910–1996), Charles Voegelin (1906–1986), and Stanley Newman (1905–1984) were part of this group. Morris Swadesh, perhaps the most brilliant of Sapir's students, saw "the evidences of a struggle between realistic fact and mechanistic [i.e., Bloomfieldian – (FJN)] fetishism: particularly between the fact that meaning is an inseparable aspect of language, and the fetish that anything related to the mind must be ruled out of science" (Swadesh 1948, 254).

Others approached linguistics as a tool to aid missionary work, and included such linguists as Kenneth Pike (1912–2000), Eugene Nida (1914–2011), and William Wonderly (1916–1988). For these linguists practical concerns typically outweighed theoretical ones, as is illustrated by the subtitle of Pike's book *Phonemics*, namely *A Technique for Reducing Languages to Writing* (Pike 1947b). Pike and his followers had no compunction about 'mixing levels' in a grammatical analysis, that is, appealing to morphological and syntactic information to arrive at a phonemicization of a particular language.

By the 1950s, there was also a considerable presence of linguists who had been members of the Prague School or influenced by it, including Roman Jakobson (1896–1982), John Lotz (1913–1973), Thomas Sebeok (1915–2001), and Paul Garvin (1919–1994). Jakobson had arrived in the United States in 1941 as a refugee from Europe and within ten years he had built a significant American following. The Prague School linguists were rationalist in their epistemology and not loath to base formal analysis to an extent on semantic criteria.[48] They advocated constructs that were shunned by the more empiricist-minded Bloomfieldians, such as universal categories, binary distinctive features, and markedness distinctions.

Historical studies, and in particular those of Indo-European languages, were far more prominent then than they are today. To illustrate, in the 1949 volume of the journal *Language*, over half of the articles dealt with diachronic themes. The journalist H.L. Mencken (1880–1956) even complained that *Language* devoted more

48. The first three groups of linguists discussed, though not those identified with the Prague School, were often referred to as 'post-Bloomfieldians' or 'neo-Bloomfieldians'.

space to Hittite than to American English![49] For the most part, American-based historical linguists were practicing neogrammarians (as had been Sapir and Bloomfield themselves). In brief, the neogrammarian position is that sound change is regular and operates on distinct classes of sounds (later called 'phonemes'):

> It can only be regarded as fortunate that the later work of wise and historically well-trained linguists like Hoenigswald of Pennsylvania, showed that neogrammarian formulations were closely similar to those of twentieth century structuralists, and that the consonant pattern of Grimm's law were a firm foundation for phonemic statement, instead of a merely happy intuition'. (Hill 1966, 4–5)

Furthermore, the positivist outlook of many leading linguists was deeply compatible with neogrammarian views, as well as the idea that one could make profound generalizations about language structure and history without taking into account the culture or other societal aspects of the speakers. Some descriptivists (including both Bloomfield and Sapir) applied neogrammarian assumptions to working out the historical development and genetic classification of the indigenous languages of the Americas.

The application of the results of linguistics to language teaching had been given a great impetus by the war (see Newmeyer 2022, ch. 1). By the early 1950s, American linguists had also started to branch out into subfields that had received very little attention in earlier years, such as sociolinguistics and psycholinguistics (two terms that had been coined in the 1930s, but were only just beginning to pass into current use), as well as information theory, discourse analysis, and translation theory (for discussion, see Carroll 1953 and Hamp 1961).

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49. The full Mencken quote is somewhat off-topic, but amusing enough to merit reprinting: "[T]he Linguistic Society has given a great deal more attention to Hittite and other such fossil tongues than to the American spoken by 140,000,000-odd free, idealistic and more or less human Americans, including all the philologians themselves, at least when they are in their cups or otherwise off guard" (Mencken 1948, 336).

3. The early American linguists' debt to Europe

Many American linguists in the 1920s and 1930 had European backgrounds (§3.1), and there were particular ties to the Prague School (§3.2) and the Geneva School and other linguists based in Switzerland (§3.3).

3.1 The European background of many American linguists

Until the mid-1930s or so, there was nothing particularly distinctive about American linguistics, as opposed to European linguistics. It is true that major figures in American linguistics, such as Boas, Sapir, and Bloomfield, were strongly focused on Amerindian languages,

Year	**President**	**Born**	**Studied**
1925	Hermann Collitz	Germany	Germany
1926	Maurice Bloomfield	Austria-Hungary	USA
1927	Carl Darling Buck	USA	USA, Greece, Germany
1928	Franz Boas	Germany	Germany
1929	Charles H. Grandgent	USA	USA
1930	Eduard Prokosch	Austria-Hungary	Austria-Hungary
1931	Edgar H. Sturtevant	USA	USA
1932	George Melville Bolling	USA	USA
1933	Edward Sapir	Germany	USA
1934	Franklin Edgerton	USA	USA, Germany
1935	Leonard Bloomfield	USA	USA, Germany
1936	George T. Flom	USA	USA, Denmark, Germany

Table 1: The European background of the first LSA presidents

but then, so were many Europeans.[50] In fact, all three linguists had strong European connections: The first two-named were born in Europe, and Bloomfield had been a student there. Indeed, as Table 1 illustrates, of the first twelve presidents of the LSA, nine had either been born in Europe or had spent some university time there:

Of the seven members of the LSA Executive Committee in 1936, five had studied in Europe: President George Flom (1871–1960; Copenhagen and Leipzig), Vice President Harold H. Bender (1882–1951; Berlin), Secretary and Treasurer Roland G. Kent (1877–1952; Berlin and Munich), Executive Committee Member Samuel E. Basset (1873–1936; Athens), and Executive Committee Member Albrecht Goetze (1876–1946; Munich and Heidelberg).[51]

3.2 American linguists and the early Prague School

Perhaps the most influential school of European structuralists was the Prague Linguistic Circle, known more often in English as the 'Prague School'. Its founders in 1926 included the distinguished linguists Roman Jakobson, Nikolai Trubetzkoy (1891–1938), and Vilém Mathesius (1882–1945), who was its first president. The former two were pioneers in structuralist phonological studies, the latter in functionally-oriented syntax. In April 1928 at the First International Congress of Linguists held in The Hague, the Prague linguists introduced a 'manifesto' outlining the tasks of phonology:

(1) To identify the characteristics of particular phonological systems, in terms of the language-particular range of significant differences; (2) To specify the types of such differences that can be found in general, and in particular to identify 'correlations', or recurrent

50. The supposed lack of interest on the part of European linguists in American indigenous languages has been greatly exaggerated. In 1924, the 21st International Congress of Americanists was held in two parts, in The Hague and in Göteborg. The organizers of the first part were all Dutch, and Americans were a fairly small part of the attendees. In fact, between the first Congress (in 1875 in Nancy) and 27th (in 1939 in Mexico City and Lima) only three were held in the United States.

51. Editor of *Language* George M. Bolling (1871–1963) completed all of his studies in the United States, but was awarded the gold cross of the Knights of the Redeemer by the Greek government in 1920 for his Homeric research (Hoenigswald 1964, 329).

differences that serve to characterize multiple pairs of elements (as e.g. voicing separates p from b, t from d, etc.); (3) To formulate general laws governing the relations of these correlations to one another within particular phonological systems; (4) To account for phonological change in terms of the phonological system (rather than the individual sound) that undergoes it, and especially to construe such changes as teleologically governed by considerations of the system; (5) To found phonetic studies on acoustic rather than an articulatory basis. Wording aside, this was a remarkably modern document (viewed from today's standpoint), going well beyond what American structuralists had specified in the late 1920s.

Taking into account the difficulties of inter-continental communications at the time, relations between early American structuralists and their Prague colleagues were cordial. Roman Jakobson has written:

> From the beginning there was a close connection between the Linguistic Society of America and the Prague Linguistic Circle. [...] N.S. Trubetzkoy's letters (Jakobson 1975) reveal some new data on the manifold ties between American linguists and the 'école de Prague'. At the end of 1931, Trubetzkoy, at that time immersed in the study of American Indian languages, emphasized that "most of the American Indianists perfectly describe the sound systems, so that their outlines yield all of the essentials for the phonological characteristics of any given language [...]". Trubetzkoy had a very high opinion of the American linguist whom he called 'my Leipzig comrade'. This was Leonard Bloomfield, who in 1913 shared a bench with Trubetzkoy and Lucien Tesnière [1893–1954] at Leskien's and Brugmann's lectures. Bloomfield (Hockett 1970, 247) praised "Trubetzkoy's excellent article on vowel systems" of 1929 and devoted his sagacious 1939 study on 'Menomini Morphophonemics' (Hockett 1970, 351–362) to N.S. Trubetzkoy's memory. (Jakobson 1979, 162)

Furthermore, Robert A. Hall, Jr. notes that George Trager addressed the Yale Linguistics Club in the early 1940s on various Prague School concepts and recalls overhearing the Americans Clarence E. Parmenter (1888–1982), a phonetician, and Manuel J. Andrade (1885–1941), an anthropological linguist, discuss the Prague concept of the phoneme in 1936 (Hall 1991, 160–161).

3.3 American linguists and the early Geneva School and Swiss linguists in general[52]

Switzerland is of particular interest because it was the home base of Ferdinand de Saussure (1857–1913), generally acknowledged as the major inspiration for structuralist studies of language. His posthumous *Cours de Linguistique Générale* had been published in 1916 (Saussure 1916) and by a decade later was being heralded as a landmark work of linguistic theory, at least in Europe. Saussure's colleagues and their students had established a major school of linguistics in Geneva (henceforth the 'Geneva School') and, elsewhere in Switzerland, linguistics was thriving as well.[53] The question is to what extent Swiss linguistic research was of interest to scholars in the United States. This question is in part addressed in a noteworthy historiographical study by Julia Falk (Falk 2004), in which the author documents the lack of impact that Saussure's book had among American researchers. While it is not my intention to dispute any of Falk's findings, one might be tempted, after reading her paper, to draw the conclusion that the work of the Geneva School was either unknown to or ignored by American practitioners. What follows is a corrective to that possible conclusion. Without wishing to exaggerate American interest, I show below that there was regular notice taken of the work of the Geneva School by American linguists in the interwar period.

A broader question also arises that is not addressed in the Falk paper: To what extent was Swiss linguistics *in general* of interest to American scholars in that time period. This question is complicated by the fact that there was not then, nor is there now, a homogenous school of linguistics in Switzerland, with uniform goals and methodologies. The linguists of Geneva did not have a great deal of contact with their co-federationists to the east. In the pe-

52. A much more detailed version of this section has been published as Newmeyer (2015).

53. For simplicity of exposition, I include the work of Saussure himself as part of the output of the 'Geneva School', even though the term was not coined (as far as I know) until after his death.

riod under consideration, French-speaking linguists in Switzerland were best known for their grammatical and stylistic studies, while German-speaking linguists built their reputation primarily around historical linguistics and dialectology. However, I need to stress that I use the terms 'Swiss linguists' and 'Swiss linguistics' in a purely national and geographical sense, not as a reference to a particular approach to the study of language. I hope to illustrate below that there was a remarkable degree of recognition accorded to Swiss linguists by their American counterparts.

Let us begin by re-examining Falk (2004). As we have seen, the two most important American linguistic theorists in the interwar period were Edward Sapir and Leonard Bloomfield. As Falk points out, "there is no evidence that Sapir was directly influenced by the *Cours*; he certainly never cited it in his work" (2004, 110). Nor, as far as I have been able to determine, did he cite any Geneva school linguists. Bloomfield, on the other hand, referred to the *Cours* on a number of occasions and even reviewed its second edition (though, again, there appear to be no citations to the work of other members of the Geneva School). One of Bloomfield's first references to the *Cours* was highly positive. In a review of Sapir (1921), Bloomfield (1924, 143) remarked that the *Cours* is a book "which gives a theoretic foundation to the newer trend of linguistic study, [...] in which restriction to historical work is [considered] unreasonable and, in the long run, methodologically impossible". However, as Falk notes, Bloomfield's review of the *Cours* was less inclined to attribute complete originality to the ideas expressed there:

> Bloomfield [in *An Introduction to the Study of Language* = Bloomfield 1914] wrote of the 'social character of language' and noted that a speech utterance "depends for its form entirely on the habits of the speaker, which he shares with the speech community. These habits are in a sense arbitrary, differing for the different communities [...]" (Bloomfield 1914, 17, 81–82). It should come as no surprise, then, that when Bloomfield reviewed the second edition of Saussure's *Cours de linguistique générale* in 1924, he was to say: "Most of what the author says has long been 'in the air' and has been here and there fragmentarily expressed (Bloomfield 1924, 318)". (Falk 2004, 108)

Falk then points out that this "seems to be the only review of the *Cours* published in any American journal until new editions were prepared in the second half of the twentieth century" (p. 109). She goes on:

> Bloomfield admired Saussure and on several occasions referred his readers to the *Cours*, but he did not adopt Saussurean terms. He viewed most basic Saussurean concepts as ideas that had been set forth by other, earlier scholars' (p. 111). Despite the above, in a postcard dated 15 January 1945 to J Milton Cowan (1907–1993), the Secretary-Treasurer of the LSA between 1941 and 1950, Bloomfield wrote that "[t]here is a statement going round that de Saussure is not mentioned in my *Language* text book (which reflects his *Cours* on every page)" (Cowan 1987, 29).[54]

Finally, Falk is certainly correct when she writes that "as in Bloomfield's own work after 1933 [the leading American linguists of the 1930s] rarely, if ever referred to Saussure or the *Cours*" (p. 112). In fact, Charles Hockett, arguably the most important American linguist between Bloomfield and Noam Chomsky, wrote to Falk that he "didn't read the *Cours* until after [he] retired from Cornell in 1982 [...]" (quoted in Koerner 2002b, 10).

Falk's claims are in need of a bit of nuancing, however. The most important American-written introduction to general linguistics in our time period, after Bloomfield's *Language* (Bloomfield 1933), was Louis H. Gray's *Foundations of Language* (Gray 1939). Gray (1875–1955), one of the preeminent Indo-Europeanists of the period, served as LSA President in 1938. In their lengthy review of this book, Zellig S. Harris and Donald C. Swanson (1914–*post* 1967) noted that "Gray speaks of three aspects of language (pp. 15–18), basing himself on the langue-parole dichotomy of de Saussure and many Continental linguists" (Harris & Swanson 1940. 228). Some years earlier, in the *American Journal of Philology*, Gray had written in a review of Louis Hjelmslev's (1899–1965) *Principes de grammaire générale* (Hjelmslev 1928) that "Adhering in general to the prin-

54. See Joseph 2019 for compelling arguments that Bloomfield's remark was not intended to be interpreted ironically.

ciples so brilliantly enunciated by the Franco-Swiss school of de Saussure and his followers, M. Hjelmslev has not only summarized everything of importance that had previously appeared upon his theme, but has made a very appreciable advance" (Gray 1931, 77). The same issue of *Language* in which the review of Gray appeared saw a review by Holmes (Holmes 1940) of *Mélanges Bally* (Faculté des lettres de l'Université de Genève 1939). His wording suggests that the readers of *Language* had at least basic familiarity with Geneva School contributions.

In fact, there were no fewer than 25 articles and reviews in *Language* between 1925 and 1940 that referred to Saussure. The majority concerned his contributions to historical linguistics, but more than a few noted the *langue-parole* distinction and other dichotomies found in the *Cours*. Saussure's synchronic work was cited in other American journals of language-related study from the period, including, as noted above, *American Journal of Philology*, and also *International Journal of American Linguistics* (Uhlenbeck 1927), *Modern Language Journal* (Bloomfield 1924, Zipf 1938), and *Modern Philology* (Field 1927). Other members of the Geneva School were not ignored in *Language*. For example, Henri Frei's (1899–1980) *La grammaire des fautes* (Frei 1929) was given a highly positive review by Reinhold Eugene Saleski (1890–1971) (cfr. Saleski 1930). Saleski informed readers that "the Geneva School (de Saussure, Brunot, Bally, Sechehaye) is interested not in the history of language as such but in the value of language to the individual speaker and hearer and no doubt to the society concerned" (p. 91).

Charles Bally (1865–1947) also received a mention in an article by Urban T. Holmes (Holmes 1931). Holmes (1900–1972) was Professor of Romance Philology at the University of North Carolina and was later to become a Chevalier de la Légion d'honneur. He wrote that "Charles Bally is not concerned with historical, only with psychological syntax, but he calls attention to a 'mentalité européenne' which would account for many resemblances [between Old French and Germanic]" (p. 195).

The fact that many American linguists had European backgrounds facilitated the transmission of ideas developed by European linguists to their New World counterparts. As one example, Alfred

Senn (1899–1978), who served on the LSA Executive Committee in 1939, was born in Switzerland and early in his career taught at the University of Lithuania, where he built a reputation as the world's leading Lithuanian dialectologist.[55] Senn moved to the United States in 1930. Given his Swiss roots it is not surprising to find an article written by him in a major American journal that begins with a reference to a member of the Geneva School (Senn 1937, 501).

We now turn to the significant degree of recognition accorded to Swiss linguists in the interwar period by the LSA. The highest recognition that the Society can give to a foreign scholar is that of 'Honorary Member'. Of the six chosen at the first election, two were Swiss. One was the Indo-Europeanist Jakob Wackernagel (1853–1938), who was born, spent most of his career, and died, in Basel. The other, Albert Debrunner (1884–1958), was also an Indo-Europeanist. He too was born in Basel and at the time of his election was a professor at Jena in Germany. However, he returned to his native country in 1935, teaching in Bern until his 1954 retirement. In 1936, the Indogermanische Gesellschaft, headed by Debrunner, was named an 'Associated Society' of the LSA. The only other society at the time to have received such recognition was the Société Linguistique de Paris.

The Second International Congress of Linguists (ICL) was held in Geneva from August 25th to August 29th, 1931. For the LSA and its members it was an important event. The Society was represented by three delegates: George M. Bolling, Carl D. Buck, and Franklin Edgerton (1885–1963; LSA President in 1934). Seven other members made the time-consuming trans-Atlantic journey: Kemp Malone (1889–1971; LSA President in 1944), Earle Brownell Babcock (1881–1935), David Simon Blondheim (1884–1934), William Edward Collinson (1889–1969), Sanki Ichikawa (1886–1970), Ephraim Cross (1893–1978), and a certain August Gunther.

Swiss linguists also played an important role in the 1931 LSA-sponsored Linguistic Institute, where Swiss dialectologists Jakob Jud (1882–1952) and Paul Scheuermeier (1888–1973) offered a course on the preparation of linguistic atlases. Another Swiss linguist on

55. In 1930 the University of Lithuania was renamed 'Vytautas Magnus University'.

the faculty at that Institute was Alfred Senn (see above), who gave courses entitled 'Church Slavonic' and 'Comparative Grammar of the Baltic Languages'. At the 1930 Institute he had been 'Docent in Indo-European Linguistics' (*LSAB* 6, 1930, 9) and at the 1931 Institute both he and Jud gave evening public lectures (*LSAB* 8, 1932, 15).

4. The American structuralists' turn away from Europe

By the early 1940s, American structural linguists had, by and large, stopped looking to Europe for intellectual inspiration. Contributing factors were the American structuralists' view of science (§4.1) and the effects of the Second World War (§4.2).

4.1 The American structuralists' view of science and its consequences

By the early 1940s, American structuralists had turned sour on the work of their European counterparts. As Einar Haugen (1906–1994) put it: "During the first quarter century of the LSA, there was a strong drift away from the European moorings" (Haugen 1979, 1). The main reason, at least at first, was the increasingly positivistic outlook of the former, leading to greater and greater divergence between the Americans and the Europeans. Never beholden to empiricist methodological constraints, the linguists of the Prague School, in particular Roman Jakobson, were developing an approach that had no reservations about hypothesizing any number of abstract constructs. They also laid the groundwork for functionalist approaches to language with the concept of 'functional sentence perspective'. To American linguists at the time all of this seemed hopelessly fuzzy. Years later, Joseph Greenberg (1915–2001) wrote:

> To a neophyte like me, American structural linguistics with its claims to rigorous scientific methodology and definitions of basic units of language without recourse to meaning, was naturally enough, enormously impressive. In contrast, Prague linguistics seemed impressionistic and lacking in scientific rigor. (Greenberg 1994, 22)

Not all of Prague linguistics was "lacking in scientific rigor", by any imaginable standards. After all, acoustic phonetic research was high on their agenda. But:

> Even the most patently 'scientific' (because highly technological) aspect of Jakobson's position – the appeal to data from acoustic research, which had progressed greatly by the end of the 1940s – was widely considered illicit [by Americans]. This was because of the use he made of it: in proposing a universal system of phonological description founded on properties that could be defined independent of particular languages, Jakobson threatened the position of presuppositionless, fundamentally agnostic analysis that many believed was essential to objective linguistic description. (Anderson 2021, 138)

But there was more to the American isolationism with respect to Europe than differing views of science (and, of course, the general isolationism that characterized America at the time). American linguists felt that they *didn't need* Europe, because they had worked out the basic principles of structural linguistics on their own:

> We do not know when the close-knit membership of the LSA – inhospitable to European theory – began to realize that Bloomfield had given them a wholly American and wholly explicit linguistic theory. We do, however, know that they could talk about nothing else at the half dozen Linguistic Institutes preceding World War II; and, more importantly, they could talk to Bloomfield who was present at every one of these LIs. (Voegelin & Voegelin 1963, 20)

American structuralists were baffled by what seemed to them as an almost mystical European obsession with the *langue-parole* distinction. To the Americans, *langue* was no more than the result of the set of operations that might be performed on *parole*, and therefore not of special interest:

> The separatism we are discussing [between US and European linguistics] dates from the spread of Saussure's influence in Europe, which was not matched in this country. For this there is a simple reason: we

> had our own giants, Boas, Sapir, and Bloomfield (Hockett 1952b, 86). Newer European contributions have been read with little sympathy and less understanding (p. 90) [...] The outstanding example [are debates over] the *langue* and *parole* problem. [...] The average American linguist is either unimpressed or else actively repelled [...] (Hockett 1952b, 90)

Hockett went on: "[t]he terms 'language' and 'speech' can well enough be used: 'speech' is behavior, 'language' is habits. Perhaps, indeed, this is what Saussure meant – but if not, it is what we should mean when *we* use the terms at all" (p. 99; emphasis in original).

It is true that some European linguists attempted to minimize the differences in world view between the Americans and the Europeans at that time. For example, the Geneva School linguist Robert Godel (1902–1984) wrote that there is "no reason to contrast 'Saussurean linguistics' with 'American linguistics'" (Godel 1966, 480). I tend to agree with the following rebuke to Godel: "Intellectual influence and a common ground there certainly is, but there has also been conscious opposition. A contemporary observer, Harold Whitehall (1905–1986), referred to '[...] the depressing and sometimes hysterical conflict between the 'Americanist' and 'Prague' schools [...]' (Whitehall 1944, 675)" (Hymes & Fought 1981, 14).

By the mid-1940s, "It can almost be said that there was no desire to know Europeans. The Americans had been hurt in their pride by the European supremacy in certain domains. The world being split in two by the war, they took advantage of the opportunity to ignore for years the existence of European thinking and to assert their independence from Europe" (Martinet 1974, 222). Along these lines, Martinet offered the opinion:

> The Americans obstructed everything; they were very happy that there was a war that prevented the Europeans from coming and pestering them. One must say that the Europeans in America were insufferable; they arrived and they considered the Americans wretched, and the Americans were understandably not happy about that. They were very happy to be free from the Europeans, from European pressure. There were all these émigrés, in general European Jews, Germans, who had

> every reason to get out. (quoted from an interview in Chevalier and Encrevé 2006, 57–58)[56]

The anti-European feeling among American structuralists accelerated with Bernard Bloch taking over the editorship of *Language* in 1940. Bloch's editorial practice was to favor American-style (post-Bloomfieldian) structuralism and neogrammarian historical linguistics over alternative approaches, in particular those emanating from Europe. Roman Jakobson submitted two papers to *Language* and both were rejected. In 1940, when he was in exile in Sweden, he sent Bloch an article entitled "Les lois phoniques du langage enfantin et leur place dans la linguistique générale". Bloch's 3 x 5 note card on the submission reads as follows:

> Rec. 17 Dec. 40 (via Sergius Yakobson, c/o Dr. Friedland, Woodbine, N.J.). – Not a member – Read 4 Jan 41: utter drivel! Sent to G.L. Trager same day, with letter q.v. – Trager concurs fully: balderdash; E.H. Sturtevant suggests that I return the MS with a general statement that it is not according to the taste of the American Public. – MS back 14 Jan. – Returned 19 Jan. (LSAA)

This paper was a shorter version of what was to become his celebrated book *Kindersprache, Aphasie und allgemeine Lautgesetze* (Jakobson 1941), a pioneering work that attempted to relate child language, speech disorders, and principles of phonology. And a few years later, "Bloch had rejected an article about poetry (written by Jakobson) insisting that such poetic study was not within the science of language' (Pike 1994, 39–40).[57] In fact, no article by Ja-

56. In the original French: "Les Américains bloquaient tout; ils étaient contents d'avoir une guerre qui empêchait les Européens de venir leur casser les pieds. Il faut dire que les Européens en Amérique étaient insupportables; ils arrivaient, il considéraient les Américains comme de pauvers types; et les Américains n'étaient pas contents, à juste titre. Ils étaient très contents d'être libérés des Européens, de la pression européenne. Il y avait tous ces émigrés, en general des Juifs européens, allemands, qui avaient toutes sortes de raisons de s'en aller de chez eux."

57. However, no submission meeting this description appears on any of Bloch's 3 X 5 cards. Perhaps Bloch discouraged Jakobson from even sending the paper to him.

kobson appeared in *Language* until 1966, the year William O. Bright (1928–2006) took over as editor.

Reviews of European work continued to appear in *Language*, but they were overwhelmingly negative. For example, Zellig Harris in 1941 reviewed Trubetzkoy's *Grundzüge der Phonologie* and wrote

> The Prague School terminology [...] has two dangers: First it gives the impression that there are two objects of possible investigation, the Sprechakt (speech) and the Sprachgebilde (language structure), whereas the latter is merely the scientific arrangement of the former'. (Harris 1941, 345)

That same year George Trager reviewed Louis Hjelmslev's *La catégorie des cas* and wrote that he couldn't understand what a "general category of case might be", since his operationalist methodology wouldn't allow the idea of any universal categories (Trager 1941, 172).

Even Leonard Bloomfield, who, along with other American linguists of his generation, tended to respect European scholarship, could not help making a thinly-veiled barb at the European practice of forming 'schools' of thought:

> It may not be altogether wrong to say that the existence of the Linguistic Society has saved us from the blight of the odium theologicum and the postulation of 'schools'. When several American linguists find themselves sharing some interest or opinion, they do not make it into a King Charles's head, proclaiming themselves a 'school' and denouncing all persons who disagree or who merely choose to talk about something else. (Bloomfield 1946, 3)

Hockett (1952b) agreed, claiming that no American journal could conceivably be called the "Leonard Bloomfield Bulletin", analogously to the Swiss publication *Cahiers Ferdinand de Saussure.* I can easily imagine the outrage among European structuralists on reading these assertions by Bloomfield and Hockett. In their eyes their American colleagues were far more dogmatic and closed-minded than they were.

Negative attitudes toward European scholarship in general continued to be expressed until well into the 1950s, as the following quotations illustrate:

> [...] socially biased value judgments which European scholarship has inherited from the aristocratic, theological background of mediaeval and Renaissance intellectualism. (Hall 1946, 33–34)[58]
>
> Thus we hear the term *scientific* applied to unprovable speculation; e.g. "[...], to much of European structural studies (with their concepts of neutralization in their analyses of concepts of cases)" (Smith 1950, 5).
>
> [This book] exhibits the usual kind of European philosophizing on the basis of insufficient evidence. (Trager 1950, 100)

As one further sign of the changing times, as mentioned in section 3.1, in 1936 five of the seven LSA Executive Committee had been born in Europe and five had studied there. There were eight members of the Executive Committee in 1946. Not a single one was either born in Europe or had studied there.

4.2 The Second World War and the two-dollar bill conspiracy

Refugee scholars from Europe started arriving in the United States in the 1930s, an influx which continued after the outbreak of the war. Their arrival had short term effects, which were largely negative in terms of the relationships between American and European linguists. At first, American academics saw the Europeans as a threat to their own well-being:

> However, the strong anti-European feeling of many American linguists in the 1930s and 1940s had its main roots in often-times bitter personal experiences. Not a few young Americans saw, and frequently more than once, positions (for which they had been trained and were eminently qualified) snatched from under their noses and given to European refugees. Such a reaction, though by no means generous, was easily un-

58. In a reply to Hall, Leo Spitzer (1887–1960) accused Hall of wanting to set up an 'Academic FBI' (Spitzer 1946, 499).

> derstandable in the days of the depression when any job at all was hard to come by, especially since American scholars, then as now, were not protected by citizenship requirements of the kind prevailing in virtually all European university systems. A frequent remark heard from [many leading American linguists] was "We'll show those Europeans we have something they never dreamed of". (Hall 1969, 194)

A few years later, Jakobson strongly rebuked Hall:

> Bloomfield particularly despised chauvinistic protectionists, who launched quasi-ideological arguments in order to repress the competition of foreign linguistics and to gain for native Americans academic positions which might otherwise be "snatched from under their noses and given to European refugees" as was so bluntly avowed by Robert A. Hall, Jr. in order to justify "the strong anti-European feeling" of his comrades. (Jakobson 1973, 17–18)

Allan Walker Read (1906–2002) was later to write that "We felt that we were carrying on an American-based linguistics and were not cordial to the intrusion of certain refugee scholars. This was resented by some of them, who felt that they were superior to American scholarship. Especially difficult to deal with was Roman Jakobson, who seemed to us at that time to be overbearing and self-aggrandizing" (Read 1991, 282).

Jakobson's arrival in New York City in 1941 triggered the most despicable incident in the history of American linguistics, namely 'the two-dollar bill conspiracy'. John Kepke (1891–1965), a minor figure in American linguistics, was one of the linguists based at 165 Broadway in New York at the Language Section of the United States War Department. The task of this group was to prepare instructional materials in languages that were deemed vital to the war effort. Kepke passed around some two-dollar bills to his colleagues there, that banknote having long been considered to bring about bad luck to the bearer.

> The two dollars were to be a contribution towards paying the fare of Jakobson and the others back to Europe on the first cattle boat *after the*

> *war was over*. Kepke went around the office with the two-dollar-bill, but without great success. I refused to sign it, and no-one in the 'Reverse English' section was willing to do so, nor (as far as I can gather) were many others outside of Kepke's small clique. When I saw the bill, it had perhaps five or six names on it. This was in reality a minor office prank, in extremely bad taste, and not representing the attitude of the 165 Broadway linguists as a group. [...] [H]owever, it contributed to intensified ill feeling on the part of Jakobson and his followers. (Hall 1991, 162; emphasis in original)

One of the signers on the two-dollar-bill was Charles Hockett. He attempted to justify his signing in the following way:

> In after-hour bar sessions and evening get-togethers of our group, the resentment [against Jakobson and other émigré scholars] came to be concretized, some time early in 1943, in the form of a two-dollar bill club. Each 'member' had a two-dollar bill, on which all 'members' signed their names; the avowed 'purpose' was to pay for Jakobson's return to Europe on the first available cattle boat. I should not really have to add that all of this was intended purely for internal consumption. It was a metaphor designed as a basis for communion and mutual commiseration. Anyone in the group would have stood aghast at the notion of really delivering anyone into the clutches of the Nazis. That was so obvious to all of us that it never had to be said. I will not name 'members' of the club other than myself (most of the others are dead by now). [...] To the best of my belief, neither our 165 Broadway group nor anyone of those in or close to it was at any time in any position either to promote Jakobson's search for a decent academic appointment in this country or to stand in the way of such an appointment. (Hockett to Morris Halle, 22 February 1989; TSA)

Thomas Sebeok was cc'ed on Hockett's letter to Halle. In a reply to Hockett, after dismissing the latter's outrage at various interpretations of forty-five-year-old events in the field, Sebeok concluded his letter by writing: "Where was your moral indignation when you, in the uniform of the U.S. Army, signed John Kepke's notorious 'two-dollar bill'?" (Sebeok to Hockett, 3 March 1989; TSA). Another signatory of the two-dollar bill was Norman McQuown (1914–2005): "Michael Silverstein, of the University of Chicago, in an email mes-

sage (of 28 February 2004) referring to Hall's 'lurid details of the $2.00-note incident,' added that 'Norman McQuown showed the artefact around our Monday lunch table some years back, by the way; it's the genuine article'." (Dixon 2007. 439).

5. European views of American linguistics

An interesting question is what European linguists thought about what was going on in the United States during the heyday of American structuralism. If we are talking about the period up to about 1950, the answer is "probably not much, though there are conflicting views on the question". In the 1930s and 1940s citations in European work to American structuralism were few and far between and were mostly references in passing to Sapir and Bloomfield. Koerner (1984, xxi), for example, is aware of only three European reviews of Sapir's *Language*. Charles Bally, a leading member of the Geneva School, first published his *Linguistique générale et linguistique française* in 1932, though revised editions kept appearing until the late 1940s (see, for example, Bally 1965 [1932]). Even in the later editions, no American linguists are mentioned. Hall (1951–1952) asserted that Bloomfield was not just unknown, but also untranslated in Europe. This assertion is reinforced by the comment that "Jakobson thinks that Trubetzkoy probably never read Bloomfield's *Language*; he did not read it himself before coming to America" (cited as a personal communication in Kilbury 1976, 126).

On the other hand, Eramian (1988) has documented at length the considerable degree to which the Prague School linguists were familiar with the work of Edward Sapir. For example, as early as 1926 Vilém Mathesius wrote approvingly of Sapir's 'Sound patterns in language' paper (Sapir 1925), noting Sapir's "theory about the special grouping of sounds which is individual for each language and which depends not on their phonetic similarity, but on their function in a given language" (Mathesius 1926, 39). Trubetzkoy maintained a lengthy and productive correspondence with Sapir, though most letters have unfortunately not been preserved. However, some appear in Jakobson (1975), where the deep respect that the two linguists had for each other is made evident.

By and large European linguists were dismissive of the post-Bloomfieldian zeal for attempting to construct theories of grammar where meaning was considered peripheral, if it was considered at all. Zellig Harris, in particular, was the subject of scorn. The following passage from a historiography of linguistics conveys a very typical European sentiment:

> Some American linguists on the other hand have gone much further and indulged in speculations that are divorced from reality. The analytic method of Z.S. Harris for example is a logico-mathematical construction lacking firm foundation. He deliberately restricted his research to questions of distribution, [...] thereby eliminating the meaning of words from his analysis, as B. Bloch and G. Trager had done before him. One wonders what happens, with this purely mechanical procedure, when the criterion of distribution is considered to be the only relevant one, to the expressive, stylistic, and other variants that are of prime importance in communication amongst human beings. (Leroy 1967 [1963], 80)

But little by little 'international relations' among linguists began to change. A watershed event in 'European-American relations' was the publication of Roman Jakobson's 'Russian conjugation' paper in 1948 (Jakobson 1971 [1948]). His debt to Bloomfield is explicit throughout the paper. For example:

> In the stimulating chapter 'Morphology' of Bloomfield's *Language*, the way has been indicated: "When forms are partially similar, there may be a question as to which one we had better take as the underlying form, and ... the structure of the language may decide this question for us, since, taking it one way, we get an unduly complicated description, and, taking it the other way, a relatively simple one" (13.9). Following Bloomfield's suggestions, we would say that "the simple and natural description is to take as a starting-point" the non-truncated stem from which we can easily infer the truncated alternant as well as the use of each. If, on the contrary, we took the truncated stem as our basic form, we would be unable to predict the corresponding full-stem and we "would have to show by elaborate lists" what phonemes are added. (Jakobson 1971 [1948], 166–167).

Jakobson's 'Russian conjugation' paper, like Bloomfield's 'Menomini morphophonemics' (Bloomfield 1939b), presents a set of rules mapping a morphophonemic representation onto a phonemic one, although unlike in Bloomfield's, no special morphophonemic elements are posited. Jakobson derives most of the superficial complexity of the Russian conjugation system by positing a single underlying stem for each verb along with a set of rules that allow each surface stem and desinence to be derived. Jakobson's endeavor was a more modest one than Bloomfield's in two crucial respects. While Bloomfield posited rules *for the Menomini language,* Jakobson's were focused on a circumscribed subpart of Russian. As a result, he did not state them with full generality. For example, several rules that he discusses, such as the vowel / zero alternation, substantive softening, and bare softening, occur elsewhere in Russian, yet they are stated in their verbal environments only. Furthermore, 'Russian conjugation' lacks the attention to rule ordering of 'Menomini morphophonemics'. There are instances, for example, of one rule being presented after another, even though the correct derivation demands its prior application.

The most noteworthy feature of 'Russian conjugation', from the point of view of linguistic historiography, is its 'un-Jakobsonianness'.[59] For one thing, Jakobson seems to have had in general little interest in morphophonemics, and when he did treat such phenomena it was as a subpart of morphology. Only in his work on Gilyak (Jakobson 1971a [1957]) do we find anything resembling the rule-centered analysis presented in 'Russian conjugation'. Jakobson had little interest in rule systems in general – to him, categories and their contrasts were paramount in language. The indirect evidence points to Jakobson not considering the paper very important. While it triggered a dozen imitations from his students – one for each Slavic language – Jakobson himself gave the paper only a couple of brief published references in the remaining 34 years of his career. Jakobson's own student, Michael Shapiro, felt the need to criticize the paper for ignoring the principles that he had learned from his teacher. Shapiro

59. An overview of the history of morphophonemic theory states that the Russian conjugation paper 'is not properly Jakobsonian' (Kilbury 1976, 127).

condemned 'Russian conjugation' for valuing "descriptive economy [as] a legitimate surrogate for explanation" (Shapiro 1974, 31).

It is worth asking why Jakobson wrote the 'Russian conjugation' paper. Halle 1988 suggests that it arose from discussions that Jakobson carried on with Bloomfield between 1944 and 1946. Jakobson was impressed with Bloomfield's *Spoken Russian* text, produced for the war effort (Lesnin, Petrova & Bloomfield 1945), and suggested that the two collaborate on a Russian grammar. Bloomfield was constantly in contact with Jakobson for comments, suggestions and examples. He tried hard to get Jakobson to write a descriptive grammar of Russian, perhaps in part to help establish credentials that would get him a job. While the grammar never materialized, the 'Russian conjugation' paper shows the unmistakable imprint of Bloomfield's influence. Bloomfield is the only linguist Jakobson refers to in the paper (other than himself).

Given that the paper grew out of the idea of a pedagogical grammar of Russian, it is not surprising that its goals seem more applied than theoretical. This interpretation seems to be supported by Jakobson's remarks in the conclusion. Rather than summarizing its theoretical import, he focuses entirely on the paper's relevance for pedagogy. The paper concludes: "The rules formulated above allow the student [...] to deduce [the] whole conjugation pattern [...]. And these rules could be presented in a popular form for teaching purposes' (162–163).

By the 1950s, European knowledge of (if not approval of) American work had grown by leaps and bounds. The British phonetician-phonologist Daniel Jones (1881–1967) in his overview of work on the nature of the phoneme (Jones 1950) showed himself to be quite knowledgeable about American contributions, while Jean Cantineau (1952) gave a very extensive survey of American structuralist research. The encyclopedic overview of structuralism by André Martinet asserted that "It is interesting to note that, in spite of profound theoretical divergences. There is a considerable amount of practical agreement among structuralists, [...]" (Martinet 1953, 575), and took the position that the three major structuralist schools were located in Prague, Yale (the home base of Bernard Bloch and many others), and Copenhagen.

The leading British linguist in this period was J.R. Firth (1890–1960). His collected papers from 1934 to 1951 (Firth 1957) contain only scattered references to American work. He was critical of both Sapir's and Bloomfield's approach to meaning and of the latter's behaviorism. He saw American linguistics as developing out of the need to study indigenous languages and mentioned "Boas, Sapir, Hoijer, and others" for their Amerindian work (p. 172). The Swedish structuralist Bertil Malmerg (1913–1994) discussed the internal diversity within American structuralism (Malmberg 1964 [1959]), and the Dutch structuralist E.M. Uhlenbeck (1913–2003) castigated Martin Joos for not giving any space in his edited volume (Joos 1957) to the less positivist approaches within American linguistics (Uhlenbeck 1959). Finally, the Danish linguist Knud Togeby (1918–1974) cited American work extensively in his *Structure immanente de la langue française* (Togeby 1965 [1951]).

Each of the European structuralist schools had its own journal or one that it published in regularly, including *Travaux du Cercle Linguistique de Prague* (1929–1939; Prague), *Acta Linguistica* (1939-present; Copenhagen – now *Acta Linguistica Hafniensia*), *Cahiers Ferdinand de Saussure* (1941-present; Geneva), *Bulletin de la Société Linguistique de Paris* (1869-present; Paris), and *Transactions of the Philological Society* (1854-present; London). It is interesting to review their pages to see how much American work was presented and how it was treated.[60] Let's begin with *Cahiers Ferdinand de Saussure*. No American work was cited before 1945. In that year the journal published an article by Thomas Sebeok on Finnish vowel assimilation (Sebeok 1945). In 1946 Thomas Godel reviewed a book of his (Sebeok 1946), noting that he was inspired by Roman Jakobson. A few years later, an article by the British linguist C.E. Bazell (1909–1984) (Bazell 1949) cited several post-Bloomfieldian publications, including Bloch (1947), Pittmann (1948), and Nida (1948). American work was cited regularly in the *Cahiers* after that year.

60. I would very much have liked to present the material in *Travaux du Cercle Linguistique de Prague*. However, COVID-19 restrictions prevented me from leafing through its pages in my university library and I have been unable to locate online versions of the journal.

1949 turned out to be a critical year for European interest in American theorizing. That year the journal *Lingua* was founded by the Dutch linguists Anton Reichling (1898–1986) and E.M. Uhlenbeck. It was almost as if the mission of *Lingua* was to acquaint Europeans with American research. The first volume had no less than eight articles that cited American linguists. And the second volume contained an article by Eli Fischer-Jørgensen (1911–2010), whose entire basis was glowing praise for Kenneth Pike's analysis of English intonation (Fischer-Jørgensen 1949). In the same issue appeared an article by the Norwegian-American linguist Einar Haugen (Haugen 1949) and one by Uhlenbeck on the structure of the Javanese morpheme that showed profound knowledge of work carried out on the other side of the Atlantic:

> In the United States, finally, morphonology has during the last ten years become the centre of the attention of those linguists who have been strongly influenced by Bloomfield. In a series of articles in the journal *Language* several linguists who for the most part seem to have been in close contact with one another, have tried, starting from Bloomfield's definition of the morpheme, to develop a theory of morpheme-analysis which was more satisfactory than what could be found about this in *Language*. On the whole they confined themselves to working out and systematizing Bloomfield's views, at the same time removing a few inconsistencies. (Uhlenbeck 1949, 246).

Aside from Bloomfield, the article cited Harris, Hockett, Bloch, Voegelin, Wells, and Nida. From the 1950s on, *Lingua* has regularly published work by American scholars, including (after 1957) articles devoted to generative grammar.

6. The American rediscovery of European linguistics

Beginning in the mid-1940s, American linguists began to warm up to work carried out in Europe. The presence of European refugee linguists in the United States led to more familiarity with European theorizing (§6.1) and, eventually, to a greater appreciation of this theorizing (§6.2).

6.1 Roman Jakobson, the École Libre des Hautes Études, and the founding of Word

Not all American linguists were opposed to Roman Jakobson's presence in the United States. Indeed, some went out of their way to welcome him and help him to find work. Foremost of these was Franz Boas, who wrote to Bloomfield: "I am very much disgusted but it seems quite impossible to find any position for Roman Jakobson. [...] What annoys me the most, I heard indirectly that he had been turned down at Yale because it was feared that his method, being different from the Yale method, might be detrimental to the students. [...]" (Boas to Bloomfield, 28 September 1942; quoted in Swiggers 1991, 283). Since Boas was to die three months later, he was not there to give Jakobson further support.[61]

Both Zellig Harris and Leonard Bloomfield supported Jakobson's presence in the United States, despite their disagreements with his theoretical stance. Harris, who was teaching at the University of Pennsylvania, did his best to secure a position for Jakobson there, possibly with support from the American Council of Learned Societies (ACLS):

> Dear Dr Jakobson,
>
> It is with great regret that I have to tell you that our plans for you here at the university have apparently come to nothing, though I think other possibilities exist. What happened was this: Drs [name illegible] and Metro both spoke with [Mortimer] Graves [of the ACLS]. Graves assured them that the ACLS would really do the best it could as soon as any university would request it for money for you. Then we tried to get a request from our university. Not only our department, but also another group interested in Slavic put through a strong joint request for you. Knowing that our university, which is one of the poorest, had

61. A number of web pages claim that "When the American authorities considered 'repatriating' [Jakobson] to Europe, it was Franz Boas who actually saved his life". No citations are ever given. It seems quite implausible to me that Boas, a German immigrant and a Jew, would have had any influence with the 'American authorities'. And again, Boas died the year after Jakobson's arrival in the United States.

a deficit, we did not request a straight appointment [...]. Now I have just learned that the university administration refused the Dean because they said that they could not take on anything for which they could not pay themselves. We will still try to reopen the matter, but there is a very small chance. [...] I am afraid the question now is to find anew a school which will request such courses with you. Do you have any suggestion? I am writing Boas, who wrote me recently asking if he can think of any possibility.

Regards etc. [Harris to Jakobson, 2 August 1942; RJA][62]

A follow up letter from Harris a few weeks letter was both more personal and more on the subject of the differences of approach between Jakobson and his American colleagues:

Dear Dr Jakobson,

I am certainly glad to have received your last letter and to have your questions, because I can imagine that the series of unjustifiable disappointments must make you wonder about the attitude or the status of scientists in America.

First, I must say that I did not know about Edgerton's letter.[63] Also that that letter cannot be responsible for some of your disappointments, certainly not the one at Pennsylvania. The Pennsylvania case, and perhaps some of the others, are ordinary examples of what faces many scientists, both refugees and Americans (though perhaps Jews more than others). The whole attitude toward scientific work is commercial and often derogatory, and appointments often depend on family connections and having the right kind of friends.

But since there is nothing we can individually do about that, it is more relevant for me to say how I understand the difference between your work and that of American linguists. You know from the Trubetzkoy review that I, and most American linguists, disagree with the philosophical approach of many European linguists (and other scientists), including that of the Prague Circle. [...] It seems that the above opinions have made some American linguists feel that much of European linguistics can be disregarded. Sapir, whom I knew well, did not feel

62. That same day Harris wrote to Boas imparting the same information and the same sentiment (see Swiggers 1991).

63. There was no letter from William Franklin Edgerton (1893–1970) in the RJA.

that way, nor do I – for perhaps two reasons: First because Prague has contributed so much to modern linguistics [...] that they have demonstrated their productivity. Second, because every piece of work which is formal is of use some place or other, and most Prague work has been [...]. Especially since speaking with you I have the opinion that work like yours, precisely because it has different points of departure from ours but is still formal, and because it is so original, can give us new ideas and important suggestions. Perhaps only after you have published more in America will more Americans realize this. [...] And, of course, I am still looking for other possibilities instead of Pennsylvania. [...]

Cordially,
Zellig S. Harris [Harris to Jakobson, 28 August 1942; RJA]

Bloomfield went to bat for Jakobson soon after his arrival in the United States, writing to Boas: "Of course I have been in touch with Jakobson, and I know that Edgerton and Sturtevant also have him in mind, but so far we have not found any opening" (Bloomfield to Boas, 20 October 1941; cited in Swiggers 1991, 282). A few years later Bloomfield wrote directly to Jakobson:

Dear Jakobson,

[...] Miss Petrova has spoken of you in a way that disquieted me and in fact has made it painful to write. She said that you were hard up for a job and were placing your hope in Yale. Of course, I imagine that any such report is inaccurate, but even without being told, I can see the basis. I can see it especially as I read yesterday a shocking story of how you had been treated in connection with your coming to this country. This too may have been inaccurate, but even if it is half true, it is bad enough. Therefore it is painful to have to tell you about the situation here; had I written to you even a week ago, I might have not felt it necessary to mention it. There is no possibility here of an appointment in Slavic languages. [...]

As ever,
Leonard Bloomfield [Bloomfield to Jakobson, 28 March 1944; RJA]

Jakobson was one of many European refugee scholars who entered the United States from the late 1930s to the end of the war. Some

ended up in New York City, teaching at the École Libre des Hautes Études, which was founded in 1941, inaugurated on February 14, 1942 and housed by the New School for Social Research. It was a sort of a university in exile for European refugees, offering its courses in French, and supported by the Rockefeller Foundation, the Belgian and Czech governments in exile, and the Free French government. Jakobson was there from the beginning. According to Testenoire (2019), linguistics courses were offered both at its Institut de Philologie et d'Histoire Orientale and at its Institut de Sociologie. Five linguists gave courses at the École Libre: Giuliano Bonfante (1904–2005), teaching comparative Indo-European linguistics; Jakobson, teaching Russian and general linguistics; Wolf Leslau (1906–2006), teaching Semitic languages; Henri F. Muller (1879–1959), teaching history of the French language; and André Spiré (1868–1966), teaching French versification.

The École Libre was located a bit uptown from 165 Broadway, where so many post-Bloomfieldian linguists were based. At the beginning, the relations between the two groups were tense:

> Between the group at 165 Broadway and that at the École, therefore, one might have hoped that good relations and profitable intellectual exchanges could have prevailed – if times had been normal. Unfortunately, however, by the early and mid 1940s, they were not normal. In many fields, including linguistics, there was hostility between American scholars, especially the younger generation, and refugees who had come to America beginning in 1933. [...] In the resultant clash between '165 Broadway' and the linguists at the École, not all the members of either group were involved. It was, rather, a conflict centered on the dislike of Trager and Jakobson for each other, with theoretical disagreements between European and American structuralists in the background. [...] Some 165-Broadway-ites attended Jakobson's lectures at the École, but reports differ concerning the latter's relations with the '165 Broadway' group. There was a certain amount of tale-bearing and gossip relayed by members of the secretarial staff who frequented the École, in at least

> two instances with harmful results in the post-war picture of American linguists that prevailed in Europe. (Hall 1991, 161–162)[64]

With respect to the antagonism between Jakobson and Trager, Stephen R. Anderson has offered the view:

> This in part goes back to the history of the Army Language manuals for Russian [see §5]. That was first assigned to Trager, who claimed expertise in Russian on the basis of what he felt he had learned as a child. He produced a set of materials that were full of inaccuracies, at least with respect to the standard language. Bloomfield sent this to Jakobson for comment, and Jakobson wrote a devastatingly negative critique of what Trager had done. Trager insisted he was right, although all the native speakers they could consult said his materials were full of mistakes. Jakobson's critique was never published, but the task of writing the Russian materials for the army was taken over by Bloomfield (with constant reference to Jakobson). Trager was not pleased. (p. c., 8 April 2021)

Despite all of this, my feeling is that the presence of the two groups of linguists in the same city at the same time was, in the long run, positive. Even though there were personal and professional animosities, linguists from each camp came to better understand the other's orientation and motivating influences. Even Charles Hockett wrote that "before very long I was attending Jakobson's lectures at the École Libre des Hautes Études, benefitting from them greatly, and coming not just to respect but to admire the man even when I disagreed with him" (Hockett to Halle, 22 February 1989; TSA).

In any event, a year after the war ended, Jakobson secured full-time employment in the United States, being named Thomas G.

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64. Hall remarks in a footnote: 'According to some accounts, efforts to have Jakobson give one of the talks at the after-hours linguistic meetings at 165 Broadway were received coldly and were not acted on. Others report that he often attended these meetings and alienated other scholars' sympathies by his virulent hostility to American linguistics and by behaving like "a boor and a bore." The two accounts are not incompatible. In any case, personal antipathies were certainly involved' (p. 162).

Masaryk Professor of Czechoslovak Studies at Columbia University in New York, a position which, despite its name, allowed him to devote most of his energies to linguistics. He moved to Harvard University in 1949, where he remained until his death in 1982. Thanks to Jakobson and other European scholars who remained in the United States after the war, elements of the Prague School approach to linguistics had become, while perhaps not mainstream, at least a major pole of attraction for American students entering linguistics in the 1940s and 1950s.

American structural linguistics continued its diversification in a European direction with the founding of the journal *Word* in 1945, which was an indirect product of the École Libre. In 1943, several linguists connected with the École Libre, most notably Jakobson, founded the Linguistic Circle of New York (LCNY). Henri F. Muller, a historian of the French language, was its first president. Two years later, the first issue of the LCNY's journal *Word* appeared, under the editorship of Pauline Taylor of New York University. The first editorial board was about half recent arrivals from Europe, but also linguists born and trained in the United States, such as the structural linguist Charles C. Fries (1887–1967), the Indo-Europeanist Robert A. Fowkes (1913–1998), the orientalist Louis H. Gray, and the historian of the English language Albert C. Baugh (1891–1981). Sapir's student Morris Swadesh edited the second volume. From 1947 and for the next two decades the journal was edited by the structuralist André Martinet, who had arrived from France to take a position as full professor and department chair at Columbia University in 1946.[65] In an editorial statement in the first issue, Muller

65. Martinet remained at Columbia until 1955, at which point he returned to France. His stay at Columbia (as well as most of the rest of his life) is documented in his fascinating quirky memoir Martinet (1993), which was the subject of a controversy within the LSA in 1994. Some members wanted the Society to condemn Martinet for the following remark, which was deemed to be anti-Semitic: "[Jakobson] s'inclinait devant Troubetzkoy, car en face du prince, il était tout de même le Juif muscovite" ["Jakobson deferred to Trubetzkoy, because in the presence of the prince, he was still the Moscow Jew"]. No action was taken against Martinet, an LSA Life Member, by the way. It is worth pointing out that in the same book, Martinet brags about his efforts on behalf of the Yiddish Studies program at Columbia.

emphasized how ecumenical the new journal would be, taking it as self-evident that the unit 'word' was a construct that all approaches shared:[66]

> Why 'Word'? Because the word, in its various aspects, is a focal point of the science of language. Linguists of diverse schools are in agreement here. Ferdinand de Saussure says: "Le mot, malgré la difficulté qu'on a à le definir, est une unité qui s'impose à l'esprit, quelque chose de central dans le mecanisme de la langue". Edward Sapir stresses "the definitely plastic unity of the word", which is "the existent unit of living speech, an integral whole, a miniature bit of art", and opposes it to the smaller units "abstracted as they are from the realities of speech". Viktor Vinogradov, the outstanding linguist of New Russia, states: "The word, the laws of its life, its historical development, its role in the history of material culture are the basic subjects of modern linguistics". Not only linguistics, but also sociology, anthropology, psychology, and logic deal with the word. With the title WORD we intend to emphasize the multiform natural structure of linguistic reality and the necessity for studying language in all the fullness of its various functions and relations. (Muller 1945, 4)

From the beginning, *Word* presented a more diverse picture of linguistics than did its well-established rival *Language*. Not surprisingly it featured a number of papers by linguists associated with the Prague School. But notably it published papers by American structuralists who were outside of the militantly positivist mainstream. For example, the first issue featured a paper by the missionary linguist Eugene Nida (Nida 1945) and the second volume by linguists who were more followers of Sapir than of Bloomfield (Newman 1946, Sapir & Swadesh 1946, Haas 1946). The third volume was extremely diverse, with articles by Fred Householder (Householder 1947) and Rulon Wells (Wells 1947), two linguists who were close to the American mainstream, as well as an important paper

66. But it was not self-evident: "Neo-Bloomfieldians did not deal with 'words' at all. For them, 'word' was not a technical term, nor a focus for analysis. 'Words' were the victims of metaphysical amateur etymology in contrast to 'morphemes,' which were the object of hardboiled professional scientific research" (Murray 1994, 215).

by Kenneth Pike, in which he laid bare his differences with majority opinion over whether grammatical information was legitimate input to phonemic analysis (Pike 1947a). By the mid-1950s even the orthodox post-Bloomfieldians were publishing in *Word*, as is illustrated by the appearance there of a paper by Charles Hockett (Hockett 1954). In other words, *Word* was both a product of, and a contributor to, a growing rapprochement among the diverse schools of structural linguistics.

Even *Language* editor Bernard Bloch came to terms with Jakobson and the new journal with which he was involved. Despite his earlier summary rejection of two of Jakobson's submissions, he wrote to the latter:

> Dear Mr Jakobson,
>
> I have read your article on Russian conjugation [(Jakobson 1971 [1948])] with great interest and pleasure; your exposition is so clear that even my ignorance of the language did not prevent me from following it. I have, however, one regret: that you did not send the article to me for *Language*. Since it includes a detailed criticism of a paper which had appeared in *Language* [(Cornyn 1948)], I believe it would have been appropriate to publish this new treatment of the same subject in the same journal. The fact that it appeared in *Word* may give some readers the false impression that there is some kind of rivalry or bad feeling between the two journals; you will agree with me, I know, that we ought to do everything we can to suppress that misconception. There is plenty of room for two American periodicals devoted to linguistic science; the existence of *Word* side by side with *Language* does not mean – as a few poorly informed persons possibly suppose – that American linguistics is divided into opposite camps. [...]
>
> Sincerely yours,
> Bernard Bloch (Bloch to Jakobson, 29 March 1949; RJA)

Jakobson sent a gracious reply to Bloch, remarking: "I am glad you liked my paper and I fully agree with the conclusion of your letter. Orally and in letters, I always emphatically fight against the false idea of two would-be linguistic factions. And I am deeply convinced that there are not." (Jakobson to Bloch, 18 April 1949;

RJA). Jakobson went on to note that the starting point of his paper was an approach developed by Bloomfield (Bloomfield 1939b, I imagine) and suggested that they get together for a personal talk.

6.2 Increasing American appreciation of European linguistics

By the late 1940s, the times were changing with respect to the desire of American structural linguists to understand European work. A sign of the changing attitudes is reflected by a letter that Kenneth Pike wrote to Thomas Sebeok in 1949, just before (what I believe was) his first trip to Europe. Pike told Sebeok: "As the semester goes by I still hope as much as ever to get to Europe in the not too distant future and so I am proceeding with a note asking for information of the type you so generously offered to provide me. Which are the descriptive linguists in England, France, Netherlands, Germany, Czechoslovakia, Denmark, Hungary, Finland, and Russia whom you think I would be interested in meeting?" (Pike to Sebeok, 16 March 1949; TSA). Sebeok's reply, excerpts of which are provided below, is of great historical interest:

> Denmark. The dominant figure in linguistics is Professor Louis Hjelmslev, who is also editor of *Acta Linguistica*, the journal devoted entirely to structural linguistics. There are also some excellent phoneticians, notably, a girl [*sic*], Eli Fischer-Jørgensen.
>
> England. Professor J.R. Firth you will probably have met last summer at the Linguistic Institute, and you undoubtedly know all about Daniel Jones. You should not miss Ida Ward.
>
> Netherlands. There are at least two first-rate linguists in Holland, namely Anton Reichling and A.W. de Groot. These two edit *Lingua* and are quite aware of American linguistics.
>
> Czechoslovakia. You are, of course, acquainted with the publications of the Cercle Linguistique de Prague. The Cercle has broken up pretty completely since the war, but one outstanding, brilliant, young linguist remains: Joseph Vachek. Vachek is not a Communist at all, but must watch his step carefully. Give him an opportunity to speak to you in private, where no one can overhear you.

> Hungary. There is only one structural linguist in Hungary: he is professor Gyula Laziczius. He is a bitter lonely old man.
>
> Finland. We have a Visiting Professor from Finland this year at Indiana University, Professor Lauri Posti. He will introduce you to all of the Finns.
>
> Germany. I cannot give you any further information about the present whereabouts of the people I used to know before the war.
>
> France. Here it is best, of course, to contact everybody through the offices of the Société Linguistique de Paris. (Sebeok to Pike, 5 April 1949; TSA)

While neither Pike nor Sebeok were in the dominant positivist wing of American structuralism, their letters manifest a new openness to an exchange of ideas between the two continents (as does the fact that Firth had been invited to teach at the 1948 Linguistic Institute).

The most dramatic testimony to renewed American interest in Europe is provided by Einar Haugen's LSA Presidential Address in 1950 (published as Haugen 1951). Haugen began his address by observing that

> Linguistic science is today in every sense of the word an international science. Few disciplines can lay better claim to this term than ours, in view of its universally and specifically human subject matter, as well as its bearing on the interrelationship and communication of nations. Even within our generation a vast expansion of linguistic study has taken place when compared with the preceding one. It is characteristic that around 1930 contributions to phoneme theory were being made by men as widely scattered as Trubetzkoy in Austria and Yuen Ren Chao in China. This was already a forward step over the much narrower field of Rask and Grimm, but we have seen a still more intense effort in the last two decades. (Haugen 1951, 211)

Haugen went on to deplore that fact that "Rarely does one see a reference in American writings on linguistic theory to the works of de Saussure, Trubetzkoy, or other European writers, although they were the thinkers who gave us the instruments with which we work"

(p. 211). He attributed this fact to the increasing terminological gulf between the Americans and Europeans and went on to explain how, to a significant degree, and terminology aside, the views of the Dane Louis Hjelmslev coincided with those of American structuralists, even those in the more empiricist camp. Hjelmslev, in fact, was on the faculty at the 1952 Institute and his countrywoman Eli Fischer-Jørgensen was there as a visitor for a month (Hill 1991, 71).[67]

The same year that Haugen's address was published, Charles Hockett published a remarkably positive review (Hockett 1951) of André Martinet's book *Phonology as Functional Phonetics* (Martinet 1949). In Hockett's words: "This booklet should be widely read; it ought to be read in this country, with a more open mind than we sometimes grant our European colleagues" (Hockett 1951, 334). In a review of another important structuralist work published in Europe, Daniel Jones's *The Phoneme: Its Nature and Use* (Jones 1950), Fred Householder (1913–1994) made some astute comments about the differences between the various 'national' approaches to structural linguistics:

> Every American linguist is aware that phoneme theory and practice have been more or less independently developed in three places: the United States, England, and continental Europe. [...] The three areas, while agreeing in essentials, differ mostly in philosophical background and primary aims. The philosophical background of the British linguist is largely that empiricism and logic of terms which is most familiar to us in the works of Bertrand Russell; much more given to skepticism (in the philosophical sense) and gentlemanly moderation than either the United States or the continent. The United States background is, in the main, Deweyan pragmatism, with a strong shot of behavioristic metaphysics and a bias toward logical rigor and methodology imparted originally by Leonard Bloomfield, but carried much farther by the younger Amer-

67. Fischer-Jørgensen had written to the LSA on 6 December 1949: "In Copenhagen we are very interested in American linguistics and we have often discussed American books in the Cercle Linguistique. I think that there is a certain relationship between the methods of American linguistics [...] and Hjelmslev's theories, so that a discussion of the undoubtedly existing differences would be fruitful [...]" (*LSAB* 26, 1950, 443).

> ican linguists. The continental background is more complex: a strong element of idealism and grandiose system building such as we associate with Hegel is present, but also strong are the influences of Comptean positivism and Gestalt psychology. [...] The European asks "Is it true?", the American "Is it consistent?", the Englishman "Will it help?" But in spite of these differences in background and purpose, the agreement on fundamentals among followers of the three schools is considerable, and Jones is in many ways closer to American theory than to continental. (Householder 1952, 99–100)

And a year later, in his overview of the field of linguistics, John B. Carroll (1916–2003) wrote that "In the last year or two, there have been signs of a necessary and well-justified rapprochement, after a temporary lapse beginning in the thirties, between American and European linguistics" (Carroll 1953, 22). Furthermore, the International Congress of Linguists, held in Oslo in 1957, "had more American members than any of the previous ones" (Mohrmann, Sommerfelt & Whatmough 1961, 9)

The most vivid indicator of the cross-Atlantic rapprochement was the election of Roman Jakobson as LSA President in 1956. Hymes & Fought (1981, 175) go so far as to suggest that if a knowledgeable person were queried in the early 1950s as to who was the most prominent linguist in the United States, the answer would likely be 'Roman Jakobson'.

7. The European reaction to early generative grammar

The era of good feeling between American and European structuralists would have continued uninterrupted for many years had it not been for an event that would turn world linguistics upside-down: the publication of Noam Chomsky's *Syntactic Structures* (Chomsky 1957). There is not much obvious European influence in that book, given that Chomsky mainly cites American structuralists and formal philosophers. But it was his joint work with Morris Halle (1923–2018) in phonology where Prague School influence – especially Jakobson's – became evident. Halle had been one of Jakobson's leading students. In fact, his work in generative phonology started out as a restatement of Jakobson's 1948 'Russian conjugation' pa-

per. Chomsky and Halle's *The Sound Pattern of English* was mostly written in 1962, but not published until six years later (Chomsky & Halle 1968). The book was dedicated to Roman Jakobson for a good reason, namely that the influence of the Prague School is evident throughout. Notions like universal phonetic and phonological elements, underlying forms, binary distinctive features, and markedness all go back to Jakobson and Trubetzkoy (for discussion, see McCawley 1977). And by the early 1960s Chomsky was asserting that the problem of the correct theory is intimately tied to the problem of child language acquisition, just as Jakobson had done in 1941.

One would think, then, that early work in generative grammar would have brought American and European linguists even closer together. Unfortunately, just the opposite happened. Many European structuralists (and their co-thinkers in the United States) were appalled that Chomsky appeared to continue the post-Bloomfieldian idea that semantics is not central to grammatical theory. In particular, they found the *Syntactic Structures* advocacy of the autonomy of syntax especially troubling. The critique was led by none other than Roman Jakobson, who, true to his Prague School roots, argued that grammatical form could not be dissociated from meaning. He asserted that "Chomsky's [...] ingenious attempt to construct a 'completely non-semantic theory of grammatical structure'" was a "magnificent argumentum a contrario" (Jakobson 1959, 144), and went on to argue that import of Chomsky's classic sentences "Colorless green ideas sleep furiously" and "Golf plays John" was precisely the opposite of that intended by Chomsky. And in a situation that I find somewhat ironic, the European attack on Chomsky was led by two Dutch linguists associated with the journal *Lingua*, Anton Reichling and E.M. Uhlenbeck, who from the start had opened that journal up to American descriptivists. Reichling stressed that hermetically sealing off syntax made the process of sentence understanding intractable:

> Native speakers do not exclusively understand each other by means of their language as a closed system; the linguistic means in a natural language are always used in conjunction with data supplied by the situation [...] (Reichling 1961, 16).

Uhlenbeck's criticisms echoed those of Reichling:

> Language is not a self-contained system. Its structure is founded on the assumption that it will be used not in vacuo. It functions in its setting, but as soon as a speech-utterance is observed by the linguist outside of its situational setting and as soon as the frame of reference of the speaker is taken into account, the utterance becomes for him uninterpretable, that is it becomes ambiguous. (Uhlenbeck 1963, 11–12)

André Martinet summed up the European reaction in commenting on a 1950s submission by Chomsky to the journal *Word*:[68]

> [Chomsky's submission is] a reaction against the self-imposed limitations of the Bloomfieldian approach, but one retaining all of its formalistic prejudices with a few additional ones. [...] Actually, my impression was one of utter drabness unrelieved by any glint indicating some hidden awareness of what a real language is. (André Martinet, quoted in Murray 1980, 77)

Many European linguists did adopt generative grammar in later years. That, in fact, will be the subject of a chapter of Newmeyer (forthcoming).

8. The Prague School influence on American functional linguistics[69]

The Prague Linguistic Circle was officially disbanded by the Stalinist regime after the war. Those in Prague who continued to do linguistic work were mainly involved in developing the idea of 'functional sentence perspective', namely that grammatical (in particular, syntactic) properties of language are a product of the communicative

68. Though in the early 1960s, *Word* published what was perhaps the most important journal article of the decade in generative syntax: Charles Fillmore's "The position of embedding transformations in a grammar" (Fillmore 1963).
69. For a more extensive discussion of American functionalism and the Prague School, see Newmeyer (2001).

setting in which language is used. This work was a forerunner to modern functional linguistics, rather than anything that generative grammarians were doing. Not just in Prague (Daneš 1964; Firbas 1965), but also in London (Halliday 1961), Paris (Martinet 1962), Amsterdam (Dik 1968), and elsewhere in Europe, functionalist studies eclipsed generative ones for several decades.

There is strong evidence pointing to the conclusion that the pioneers of American functionalism not only were familiar with the central writings of the Prague School, but found them intellectually inspiring. I will demonstrate this point by reference to the work of Dwight Bolinger (1907–1992), Joseph Greenberg, Wallace Chafe (1927–2019), and Susumu Kuno (1933-).

Bolinger had begun to refer to the work of Prague School linguists as early as 1965. A book published in that year (Bolinger 1965a) reprinted some of his early papers and contained some never published ones as well. In a new preface to one of the former (Bolinger 1965 [1952]), he remarked that when he wrote the article, he "was not aware of the earlier work of V. Mathesius and the recent work of Jan Firbas on what Firbas calls 'functional sentence perspective ..." (p. 279) and went on to cite a paper of Firbas's and to characterize the (rather minor) differences between their respective positions. In a new paper in that same volume (Bolinger 1965b), he expressed his debt to a 'cautious statement' (p. 167) in Daneš 1957 regarding stress-timed rhythm in English that had helped to shape his thoughts on the matter. And in his popular 1968 introductory text, *Aspects of Language*, Bolinger noted:

> A group of Czech linguists refers to this tendency of many languages to put the known first and the unknown or unexpected last as 'sentence perspective' [a footnote here cites Firbas 1964]. They point out that, in order to communicate the sentence dynamism that has been partially lost by the stiffening of word order, English must resort to other stratagems, and these are among the things that give the language its distinctive syntactic appearance. (Bolinger 1968, 119–120)

Bolinger continued to cite Prague School work until the end of his career. For example, we find in Bolinger (1986) and Bolinger

(1989) some discussion of the approach to accent prominence taken in Daneš 1960.

The influence of the Prague School permeates every page of Joseph Greenberg's seminal paper "Some universals of grammar with special reference to the order of meaningful elements" (Greenberg 1963). Indeed, by Greenberg's own acknowledgement (Greenberg 1963, 104), the paper was written in response to Roman Jakobson's call for an 'implicational typology' of language universals (Jakobson 1971b [1957]). Prague School terminology is also rampant in the Greenberg paper, as is evidenced by the frequent description of one order of elements as being 'more marked' or 'less marked' than another.

In his 1970 book, Chafe notes that "the basic role played by semantic structure in the structure of language [...] has been seriously neglected by the mainstream of linguists" (Chafe 1970, 210). To this remark he adds in a footnote:

> It has not been totally neglected, however. Some members of the 'Prague School' have given it considerable attention, beginning with Vilém Mathesius and continuing now with, especially, the work of Czech linguists such as Jan Firbas (see Firbas 1966 and numerous other publications). (Chafe 1970, 210)

Kuno bestowed upon the Prague School a signal honor – he named one of his papers 'Functional sentence perspective' (Kuno 1972), and began the acknowledgement footnote with the following remark:

> I am most grateful to Jan Firbas for discussing with me the theme-rheme (or predictable information vs. unpredictable information) interpretation of *wa* and *ga* in Japanese. The reader will find that I have been greatly influenced in my analysis by the Prague School notion of functional sentence perspective. (Kuno 1972, 269)

We have the personal testimony of the 'second generation' of functionalists, as well, that their mentors, Chafe and Kuno, valued the work of the Prague School enough to call their attention to it:

> Wally Chafe's work in the 1960s was an important influence on my thinking, and it was Chafe who got me to reading the Prague School work [...]. I heard Chafe give lectures in the 1960s in which he referred to FSP, and spoke of it as the basis of his ideas. Pre-war names like Mathesius were often mentioned, so this wasn't merely the newer Prague School. (Paul Hopper, personal communication, 20 January 1999)
>
> Incidentally, the person who pushed Prague School ideas on information flow the most here at Berkeley during the 70s was Wally Chafe, who of course was a major force behind the formation of the functionalist school first here and later at Santa Barbara. (George Lakoff, *Funknet* posting, 11 February 1999)
>
> At least by the early 1970s, Kuno was indeed talking about the Prague School. I remember reading Mathesius and Firbas on his recommendation at that time. (Ellen Prince, *Funknet* posting, 16 February 1999)

In short, there can be no question that the American functionalist movement, as it took form in the early 1970s, was shaped to a significant degree by the conceptions of the Prague School.

9. Conclusion

This paper has examined how American structural linguists and their European counterparts saw each other from roughly the 1920s to the 1960s. American linguistics had deep roots in Europe, though by the late 1930s, most American structuralists had turned their back on the old continent. Attitudes towards the Europeans started to warm in the late 1940s and into the 1950s. Prague School conceptions had a major influence on generative grammar (at least as far as phonology is concerned) and on the nascent functionalist movement in the United States. From the European side, there was some, but not a great deal, of interest in American theorizing until the late 1940s. A real rapprochement was underway in the 1950s, which was derailed by the appearance of generative grammar, an approach that at the time most European structuralists rejected.

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