Where Was Historical Linguistics in 1968 and Where Is It Now?

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1   Introduction

Historical linguistics, especially historical Indo-European linguistics, was the dominant subfield of linguistics in nineteenth century Europe—for all intents and purposes, historical linguistics *was* linguistics at the time. The vast majority of contemporary linguistics studies engaged with historical topics, e.g. the pioneering work of Rasmus Rask (1818), Hermann Grassmann (1863), Karl Verner (1876), Hermann Osthoff & Karl Brugmann (1878), and Hermann Paul (1880; 5th edition 1920) addressed questions like the development of the Germanic consonant system and the loss of aspiration in Sanskrit and Greek, while also providing discussions of more general topics like the regularity of sound change. Paul (1920: 20) captured the role of historical linguistics at the time in his statement that:

> [e]s ist eingewendet, dass es noch eine andere wissenschaftliche Betrachtung der Sprache gäbe, als die geschichtliche. Ich muss das in Abrede stellen. Was man für eine nichtgeschichtliche und doch wissenschaftliche Betrachtung der Sprache erklärt, ist im Grunde nichts als eine unvollkommene geschichtliche, unvollkommen teils durch Schuld des Betrachters, teils durch Schuld des Beobachtungsmaterials. Sobald man über das blosse Konstantieren von Einzelheiten hinausgeht, sobald man versucht den Zusammenhang zu erfassen, die Erscheinungen zu begreifen, so betritt man auch den geschichtlichen Boden, wenn auch vielleicht ohne sich klar darüber zu sein.[1]

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[1] "It has been argued that there is a scientific approach to language other than the historical one. I reject this view. What some people declare to be an ahistorical but still scientific approach to language is basically nothing but an incomplete historical approach, an incompleteness for which partly the observer and partly the data are to blame. As soon as one goes beyond the mere observation of details, as soon as one tries to capture their connections, to understand the phenomena, one enters historical ground, maybe even without being aware of it" (translation by Peter Auer from Paul 2015: 45; one footnote was omitted).

Exactly what Paul (1920) meant by this statement remains disputed,[2] but the bottom line is simple: historical linguistics was the dominant area of the field in nineteenth century Europe, and retained that privileged position for a number of decades, through all the other shifts in the field, e.g. the emergence of synchronic approaches to linguistics driven by Saussure (1916) and the shift in North America from a field rooted in German intellectual history to a field focused largely on a more indigenous approach to linguistics that concentrated on the study of Native American languages.[3]

Events in the late 1950s and the following decade led to a time of tremendous upheaval in linguistics in North America. After a long period of relative homogeneity in the field under the framework of American Structuralism,[4] dominated by authors like Edward Sapir, Leonard Bloomfield, and Charles Hockett, 1957 saw the publication of Noam Chomsky's first major work, *Syntactic Structures*. This book, along with Robert B. Lees' long review of it in *Language* (Lees 1957), brought the new theoretical model of transformational/generative linguistics to the forefront of the field.[5] In addition, the Cold War and resulting political events in North America and Europe, especially after the launch of the Soviet satellite Sputnik in 1957, brought an enormous amount of American government money to linguistics, through organizations like the National Science Foundation and the US military. This funding was mainly directed towards generative approaches, which led to the increased application of such approaches at the expense of some more traditional areas of study.[6] At the same time, the field was expanding in new and exciting directions (including things like the development of sociolinguistics in works like Labov 1963). The end result of this was what some have seen as the marginalization of historical linguistics, as it was pushed by subfields like syntax and phonology out of its formerly central position in the field. Lehmann and Malkiel (1968: vii; page reference to 2017 reprint), for instance, point to what they saw as "the inadequate

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2   See Auer (2015) for a recent discussion of Paul's statement, and J. Blevins (2004) for a broadly similar view of sound change.

3   Pierce (2009) discusses this second development as it applies to Leonard Bloomfield.

4   We say "relative homogeneity" because a number of other theoretical approaches were also popular at the time, e.g. various versions of European structuralism, often brought to the USA by émigré scholars like Roman Jakobson.

5   We leave a number of details aside here, e.g. the role of scholars like Zellig Harris in the emergence of historical/ generative linguistics, the importance of Lees' review in bringing Chomsky (1957) to many scholars' notice, etc.

6   Although historical linguistics certainly benefited from this expansion of funding, it does not seem to have benefited as much as some other subfields, e.g. generative syntax and phonology.

attention to historical linguistics and the need to restore historical studies to their position of leadership among the primary linguistic disciplines."

One response to this marginalization was a 1966 symposium on historical linguistics at the University of Texas at Austin, organized by Winfred P. Lehmann, then chair of the Department of Linguistics there (among numerous other administrative responsibilities). The symposium aimed to help restore historical linguistics to its formerly central position in the field. To this end, it brought several speakers representing different areas of historical linguistics, who worked on different language families, to Austin in April 1966. Five papers were presented at the conference, with pre-presentation versions distributed to the approximately 100 conference participants. The papers addressed questions like the role of historical linguistics in the 1960s vs. its role in the 19th century, the relationship between the different components of the grammar, and potential new methods for the study of language change. The symposium was a seminal event in 1960s linguistics, not least because it brought major senior scholars like Jerzy Kuryłowicz and Émile Benveniste together with more junior linguists like William Labov, and led to fruitful intergenerational discussion.

A volume based on the symposium, *Directions for Historical Linguistics*, edited by Winfred P. Lehmann and Yakov Malkiel, appeared in 1968. It contained five papers, one each by Winfred P. Lehmann, Yakov Malkiel, Jerzy Kuryłowicz, Émile Benveniste, and Uriel Weinreich, William Labov, and Marvin I. Herzog. Although some of the papers have not found nearly as much resonance as the others (on which see e.g., Sarah Thomason's contribution to this volume), the impact of the original volume as a whole was enormous. In the case of the paper by Weinreich, Labov, and Herzog in particular, it is not overstating the case to say that most of the current work in historical sociolinguistics hearkens back to their paper, for instance (see the discussion of citation numbers of their paper in Brian Joseph's contribution to this volume).

After being long out of print, but available electronically on the website of the Linguistics Research Center at the University of Texas at Austin,[7] the original volume was reprinted by Brill in 2017 with a new foreword by Hans C. Boas and Marc Pierce (Boas and Pierce 2017). This reprinting formed the basis for a roundtable, co-organized by Hans C. Boas and Bridget Drinka, "New Directions for Historical Linguistics: Impact and Synthesis, 50 Years

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7 Here we note that Lehmann founded the Linguistics Research Center (LRC) at the University of Texas at Austin in 1961 and was its director until his death in 2007. The LRC was originally intended as a center for machine translation, but later moved in other directions. See Jonathan Slocum's brief history of the LRC, available at <https://liberalarts.utexas.edu/lrc/about/history/history.php> for details. The 1968 volume was (and still is) available at <https://liberalarts.utexas.edu/lrc/resources/books/directions/index.php>.

Later," at the 23d International Conference on Historical Linguistics, held in San Antonio, Texas, in August 2017. The roundtable aimed to discuss the evolution of historical linguistics in the 50+ years since the original Symposium in general and of the impact of the ideas from the 1966 Symposium and 1968 volume on historical linguistics and sociolinguistics in particular. Six prominent scholars of historical linguistics and sociolinguistics participated: Paul Hopper (Carnegie-Mellon University and a graduate student assistant at the original University of Texas at Austin Symposium), Brian Joseph (Ohio State University), William Labov (University of Pennsylvania and the only surviving author from the 1968 volume), Gillian Sankoff (University of Pennsylvania), Sarah Thomason (University of Michigan), and Elizabeth Traugott (Stanford University). The roundtable was one of the highlights of the conference—it was by far the best-attended session—and the enthusiastic response confirmed our intent to prepare the current volume.[8]

The remainder of the chapter is divided into four sections. Section 2 sketches the state of the art of historical linguistics in 1968 (going slightly beyond this chronological boundary in a few instances), while Section 3 provides the same type of sketch of the field as it currently stands. Finally, Section 4 outlines the contents of the remaining papers in the volume.[9]

2   Historical Linguistics in 1968

As discussed above, historical linguistics had been displaced from its once central position in the field by 1968. However, historical linguistics was by no means moribund in 1968. Instead, the middle and late 1960s saw a number of exciting developments in the field. Choosing the most important is therefore a difficult task, compounded by questions about the entire concept of "breakthrough" within linguistic historiography.[10] Here we address what we see as two of the most exciting such developments (others will no doubt have

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8   Videos of the talks are available at <https://liberalarts.utexas.edu/lrc/extras/ichl/index .php>. We thank Todd Krause for his work in posting them.

9   For feedback on an earlier version of this chapter we would like to thank Bridget Drinka, Pattie Epps, Danny Law, Robert Mailhammer, Na'ama Pat-El, and Cinzia Russi.

10   Some scholars contend that breakthroughs are relatively rare, while others argue that they are relatively common. See e.g. Hockett (1965) and Koerner (1999) as examples of the first position, and Joseph (1995) as an example of the second. Moreover, some scholars change their minds about what exactly is a breakthrough, e.g. Hockett (1965) endorses generative linguistics as a breakthrough, but Hockett (1968) disavows the approach, on the grounds that it cannot handle diachronic developments well.

different opinions). We focus on the emergence of sociolinguistic approaches to historical linguistics, exemplified by Labov (1963), and on the synthesis of generative approaches to language with traditional approaches to historical linguistics, as exemplified by Halle (1962), Kiparsky (1965), Postal (1968), and King (1969).[11] Moreover, we concentrate on sound change here, as that stands at the center of our own interests in this area.[12] The two developments just mentioned approach historical linguistics from two radically different perspectives: Labov's work aims at the surprisingly difficult question of the causes of sound change;[13] while generative approaches apply a new scholarly paradigm, developed largely on the basis of synchronic linguistic studies, to data and questions that had previously not been discussed within that paradigm. In what follows, we briefly sketch each of the approaches and then contextualize them within the field at that time. We begin with sociolinguistic approaches.

Perhaps the most famous early expression of sociolinguistic approaches to language change is Labov (1963), based on his (1962) master's essay at Columbia, which was supervised by Uriel Weinreich (Labov 1963: 275 fn 9).[14] This incisive and groundbreaking study "concerns the direct observation of a sound change in the context of the community life from which it stems" (Labov 1963: 273), specifically "a shift in the phonetic position of the first elements of the diphthongs /ai/ and /au/ … [on] the island of Martha's Vineyard, Massachusetts" (Labov 1963: 273). Martha's Vineyard was particularly well suited to Labov's study, as (1) it is "a self-contained unit, separated from the mainland by a good three miles of the Atlantic Ocean.… [with] enough social and geographic complexity to provide ample room for differentiation of linguistic behavior" (Labov 1963: 275), and (2) data had been collected on the island in the 1930s for the *Linguistic Atlas of New England* (LANE; Kurath et al. 1939–1945), thus providing real-time comparative data. The choice of the first element of two

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11 We discuss Halle (1962) in somewhat less detail, as that work is not primarily concerned with diachronic considerations.

12 For contemporary applications of transformational/generative syntax to historical linguistics, see Klima (1964) and Closs [Traugott] (1965), among others.

13 Labov (1963) covers additional ground, e.g. the question of whether sound change could be observed while it was in progress. Bloomfield (1933) and Hockett (1958) contended that it was impossible; while Labov (1963: 293) takes a more optimistic position. We leave such questions aside. In fact, the following discussion must necessarily simplify much of the argumentation and discussion of the original studies. Readers are referred to the original works for details.

14 We focus on this study because it is one of the earliest such studies of which we are aware. Other studies that we could have addressed include Labov (1965, 1966) and Weinreich, Labov and Herzog (1968), from the original *Directions* volume, among a number of other possibilities.

diphthongs as the variable to be investigated over other potential variables like /r/ was motivated by considerations like the following: its saliency (for the linguist, if not for the average speaker); that it was "quite immune to conscious distortion"; and exhibited a nice "structural parallelism," as well as a simultaneous "great structural freedom in the range of allophones permitted by the system" (Labov 1963: 280). Moreover, there was also "the indication of a complex and subtle pattern of stratification" (Labov 1963: 280); it was "apparent that the present trend on Martha's Vineyard runs counter to the long-range movement of these diphthongs over the past two hundred years" (Labov 1963: 281); and "while this sound change is not likely to become a phonemic change in the foreseeable future, it operates in an area where far-reaching phonemic shifts have taken place in the past" (Labov 1963: 281).

Labov (1963: 273) contends that "[b]y studying the frequency and distribution of phonetic variants of /ai/ and /au/ in the several regions, age levels, occupational and ethnic groups within the island, it will be possible to reconstruct the recent history of this sound change; by correlating the complex linguistic pattern with parallel differences in social structure, it will be possible to isolate the social factors which bear directly upon the linguistic process." Additionally, he hoped "that the results of this procedure will contribute to our general understanding of the mechanism of linguistic change" (Labov 1963: 273). These are important questions, especially in light of some of the answers given to them by earlier historical linguists.[15]

Some scholars had in fact already suggested that it was impossible to pinpoint the causes of sound change. Leonard Bloomfield himself, the dean of American linguists between the two World Wars, had famously contended that "[t]he causes of sound-change are unknown" (Bloomfield 1933: 385). At the same time, however, Bloomfield did hint at some potential causes. He writes:

> The general direction of a great deal of sound-change is toward a simplification of the movements which make up the utterance of any given linguistic form. Thus, consonant-groups are often simplified. The Old English initial clusters [hr, hl, hn, kn, gn, wr] have lost their initial consonants, as in Old English *hring > ring, hlēapan > leap, hnecca > neck,*

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15   We note here that there are two perspectives on language change, that of the individual and that of the community. Historical linguistics has tended to focus on completed changes, while Labov's work brought the relationship between language variation and change to the forefront, in other words, an ongoing process that begins with the individual and spreads through the community. This distinction also led to a great deal of work on topics like incomplete changes and lexical diffusion (cf. Wang 1969, Labov 1981, etc.), and the issue remains important today. We thank Robert Mailhammer for helpful (electronic) discussion of this issue.

> *cnēow > knee, gnagen > gnaw, wringan > wring*. The loss of the [h] in these groups occurred in the later Middle Ages, that of the other consonants in early modern times; we do not know what new factors intervened at these times to destroy the clusters which for many centuries had been spoken without change.
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> BLOOMFIELD 1933: 370

This statement suggests that Bloomfield perhaps thought of "ease of articulation" as a potential causal factor for sound change (however that vague concept is to be defined), but later on the same page of *Language* Bloomfield notes that these clusters remain in the other Germanic languages. For instance, Icelandic allows the clusters that begin with [h]; German, Danish, Swedish, and some dialects of English allow [kn] as an initial cluster, etc. Bloomfield then contends that:

> As long as we do not know what factors led to these changes at one time and place but not at another, we cannot claim to know the causes of the change—that is, to predict its occurrence. The greater simplicity of the favored variants is a permanent factor; it can offer no possibilities of correlation.[16]
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> BLOOMFIELD 1933: 370–371

Somewhat later in the chapter, Bloomfield runs through a number of potential causes of sound change that had been proposed in the literature:

> Every conceivable cause has been alleged: "race," climate, topographic conditions, diet, occupation and general mode of life, and so on. Wundt [1901] attributed sound-change to increase in the rapidity of speech, and this, in turn, to a community's advance in culture and general intelligence. It is safe to say that we speak as rapidly and with as little effort as possible, … and that a great deal of sound-change is in some way connected with this factor. No permanent factor, however, can account for specific changes which occur at one time and place and not at another….[17]
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> BLOOMFIELD 1933: 386; quotation marks in original

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16   It is unclear to us when linguists first began to discuss the possibility of predicting sound change. Further historiographic research is necessary to pin this down.

17   Exactly what Bloomfield means by "permanent factor" is not entirely clear. Note also that Bloomfield discusses the possibility of a single factor causing language change, not multiple causation scenarios.

This early invocation of what would later become known as "the actuation problem" (Weinreich, Labov, and Herzog 1968) aside, in Bloomfield's view, because "no student has succeeded in establishing a correlation between sound-change and any antecedent phenomenon … the causes of sound-change are unknown" (Bloomfield 1933: 385, as partially cited above).

Some later scholars were equally, or even more, dismissive. Lehmann (1962: 200) had stated that "explaining" language change was in fact not even the responsibility of the linguist, writing that "[a] linguist establishes the facts of change, leaving its explanation to the anthropologist" (Lehmann 1962: 200). Moreover, Joos (1958: v) had memorably claimed that "[i]f the facts have been fully stated, it is perverse or childish to demand an explanation into the bargain." Such characterizations, however, do not address the questions of what exactly "explaining language change" means and what exactly counts as an "explanation."

At the same time, however, still other scholars were more optimistic. Andre Martinet's work in studies like Martinet (1952, 1955) had pointed to potential structural and/or functional forces driving language change. Martinet took a broadly functional view of language, and thus to sound change: since the goal of language is to communicate, sound change will take place in such a way as to enhance (or at least not to impede) communication. Martinet (1952: 5) suggests, for instance, that:

> The basic assumption of functionalists in such matters is that sound shifts do not proceed irrespective of communicative needs, and that one of the factors which may determine their direction and even their appearance is the basic necessity of securing mutual understanding through the preservation of useful phonemic oppositions.

This can also be linked to the concept of "functional load," which can be defined as follows (King 1967: 831; small caps in original):[18]

> The term FUNCTIONAL LOAD is customarily used in linguistics to describe the extent and degree of contrast between linguistic units, usually phonemes. In its simplest expression, functional load is a measure of the number of minimal pairs which can be found for a given opposition. More generally, in phonology, it is a measure of the work which two

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18   We cite King (1967) here because of the relatively short time span between his work and Martinet's. See also Mathesius (1929, 1931), Hockett (1955), or Martinet (1955) for roughly contemporary discussions of the concept.

phonemes (or a distinctive feature) do in keeping utterances apart-in other words, a gauge of the frequency with which two phonemes contrast in all possible environments.

In Martinet's view, sounds with high functional loads are more likely to be maintained, while sounds with lower functional loads are more likely to undergo change.

Another concept important to Martinet's work is that of "phonological space"[19]—since no two phonetic events are exactly alike, and sound change is meant to enhance the communicative possibilities of the language, not damage them, in

> a homogeneous speech community it is probable that the normal range of dispersion of every phoneme in a given context will not be contiguous to those of its neighbors, but that there will be a margin of security in the form of a sort of no man's land.
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> MARTINET 1952: 4

If this "margin of security" is reduced, then sound change can occur:

> We shall reckon with a sound shift as soon as the normal range of a phoneme (in a given context—from now on this shall be understood) is being ever so little displaced in one direction or another, whereby the margin of security which separates it from its neighbors increases or decreases.
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> MARTINET 1952: 5

Martinet (1952, 1955) further conceptualizes this in the form of "push chains" and "drag chains." "Push chains" occur when a phoneme moves away from another to avoid merger, thus encroaching on the "range of dispersion" of another phoneme, which itself may then move into the "range of dispersion" of a third phoneme. In other words, the first phoneme is "pushing" the others before it. "Drag chains," on the other hand, occur when a phoneme moves into a "range of dispersion" where there is no chance of a merger, and can thereby "drag" additional phonemes along with it to fill the gaps in the system. These

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19   See Moulton (1962) for an application of this idea to Swiss German dialectology. This work is cited approvingly by Labov (1963: 274 fn. 4), who states that "The empirical confirmation of many of Martinet's ideas to be found in Moulton's investigation of Swiss German dialects has provided strong motivation for some of the interpretations in the present essay." Moulton (1962: 23–26) also gives an excellent concise review of the history of the concept of "phonological space."

concepts of "push chains" and "drag chains" have proven especially useful in analyzing chain shifts like the Great Vowel Shift of Early Modern English or the Northern Cities Shift in Modern English.[20]

The bottom line here is that while some linguists argued that the causes of language change could not be determined, or were too vague to be of any real use, others like Martinet argued that language change could be traced to largely functional causes.

Labov's 1963 article, however, while grounded in work by Martinet and others, drew attention to the social, i.e. external, non-linguistic, factors driving sound change. In 1961 and 1962 he collected data from 69 "native island speakers" (Labov 1963: 284), focusing on the two diphthongs mentioned above. These speakers are to be distinguished from "the summer people," i.e. the non-natives of the island who vacation there in the summer. At the time, /ai/ was "well centralized," while /au/ showed "phonetic variation on a truly impressive scale" (Labov 1963: 282). Labov (1963: 290–291) noted the effects of a number of linguistic factors on centralization, including stress, stylistic considerations, and the phonetic environment of the diphthongs. He then points out that the "[c]entralization of /ai/ and /au/ appear to show a regular increase in successive age levels, reaching a peak in the 31 to 45 age group" (Labov 1963: 291), and argues, based on the data reported on in LANE, that this is a genuine sound change, and not a "change in speaking habits with age" (Labov 1963: 293–294).

The next problem Labov tackles is the possible explanation of the change. Here he points to "some striking social correlations which are not easily explained away" (Labov 1963: 295). These include geography (centralization is higher in what were then more rural areas of the island), occupation (fishermen show more centralization than farmers), and ethnic groups (English and Native American speakers show more centralization than Portuguese speakers). More important, though, is "strong resistance to the incursions of the summer people" (Labov 1963: 297): those who show this strong resistance also centralize these diphthongs more. The highest degree of centralization was in fact found among rural fishermen. The two speakers who centralize the most are a rural father and son pair; the father is described as "perhaps the most eloquent spokesman for the older Vineyard tradition," while the son is "a college graduate who tried city life, didn't care for it, came back to the island, and built up several successful commercial enterprises on the … docks" (Labov 1963: 300). The role of these social factors in the case of the son's speech is underscored by a comment made by his mother that he "didn't always speak that

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20   For more on chain shifts like the Northern Cities Shift, see e.g. Kirchner (1996), Labov, Ash, and Boberg (2005), or Labov (2011), among numerous other studies.

way ... it's only since he came back from college. I guess he wanted to be more like the men on the docks" (Labov 1963: 300). Other age groups show similar patterns; among high school students, for instance, those who intended to stay on the island centralized far more than those who intended to leave the island, if the latter centralized at all (Labov 1963: 300). On Martha's Vineyard in the early 1960's, then, "the immediate meaning of [centralization] is 'Vineyarder'" (Labov 1963: 304). That is, "the meaning of centralization, judging from the context in which it occurs, is *positive orientation towards Martha's Vineyard*" (Labov 1963: 306; italicization in original). Language change, in this view, may be driven by social factors.

Thus, to sum up this part of the discussion, while some earlier scholars like Bloomfield (1933) had argued that it was impossible to explain sound change, and other earlier scholars like Martinet (1952, 1955) had linked sound change to functional concepts like phonetic space, Labov (1963) demonstrated convincingly that social factors may also play a role. This does not mean that social factors are the only factors motivating sound change; we find the position taken by some scholars in this regard too extreme. Postal (1968: 283) exemplifies this view:

> It seems clear to the present writer that there is no more reason for languages to change than there is for automobiles to add fins one year and remove them the next, for jackets to have three buttons one year and two the next, etc. That is, it seems evident within the framework of sound change as grammar change that the 'causes' of sound change without language contact lie in the general tendency of human products to undergo 'nonfunctional' stylistic change. This is of course to be understood as a remark about what we might call 'primary change,' that is, change which interrupts an assumed stable and long-existing system.
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> quotation marks in original

Ultimately, in fact, sound change is driven by multiple causes, both linguistic and non-linguistic. Labov's work, specifically his demonstration of the role of social factors, thus represents an enormous step forward in scholarly understanding of language change (and a firm rebuttal of Bloomfield's views on determining the causes of sound change).

We now turn to the second innovation in the time period around 1968, namely the emergence of generative approaches to language change, as exemplified by Halle (1962), Kiparsky (1965), Postal (1968), and King (1969). We concentrate here on two issues: (1) the status of rule insertion, i.e. can phonological rules be added at any point in the phonology, or must they be added to

the end?;[21] and (2) is sound change purely phonetically/ phonologically conditioned, or can morphological factors also play a role? We begin with rule insertion, starting with Halle (1962).

In Halle's view, "the primary mechanism of phonological change is the addition of rules to the grammar with special (though not exclusive) preference for the addition of single rules at the ends of different subdivisions of the grammar" (Halle 1962: 67), in line with his view that synchronic rule ordering normally corresponds to diachronic relative chronology.[22] However, this is not always the case (as "though not exclusive" indicates in the quotation just given). Halle argues, for instance, that certain vocalic developments in Middle English are best accounted for by means of rule insertion. That is, in some dialects of Middle English, "tense (long) /ǣ/ and /ā/ became /ē/ simultaneously with tense (long) /ɔ̄/ becoming /ō/" (Halle 1962: 68). Halle argues that rule insertion enables a more elegant analysis. He concedes that there may be chronological issues, since

> Some scholars believe that the change /ā/ → /ē/ was later by 50 years than the changes /ǣ/ → /ē/ and /ɔ̄/ → /ō/. If they are right, my example is a hypothetical, rather than an actually attested instance. This does not affect its validity, however, since the example does not violate any known constraints on the structure or on the evolution of language.
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> HALLE 1962: 68 fn. 17

The idea of rule insertion was also endorsed by Kiparsky (1965), who illustrated his argument with a discussion of Lachmann's Law in Latin. According to this sound law, first proposed by Karl Lachmann in 1850,[23] vowels are lengthened

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21   King (1973: 551) offers the following definition of rule insertion: "It has been claimed that phonological change can take place by the addition of a rule in the 'middle' of a grammar- i.e., the addition of a rule that alters not the terminal phonetic representations of a previous grammar, but the non-superficial intermediate or underlying representations of that grammar. Within the theory of generative phonology current through 1968, a change of this type would have to be described as the addition of a new rule, ordered before a phonological rule present in an earlier grammar. For convenience I will call this RULE INSERTION, in order to distinguish it from the more commonplace and less controversial kind of rule addition in which a rule is added 'at the end of a grammar'—i.e., without crucial ordering before any phonological rule in a previous grammar" (small caps in original).

22   A full discussion of these issues would address questions like: how exactly rules are added to the grammar, who actually adds them, and so on. The phrase "different subdivisions of the grammar" reflects the Chomskyan view that grammar is modular.

23   Because Lachmann's own formulation of the law is "distressingly imprecise" (Collinge 1985: 105) and in Latin to boot, we rely on the secondary discussions of Kiparsky (1965),

before clusters of voiced stop + voiceless obstruent, e.g. *agō* 'I drive, lead' ~ *āctum* 'having been driven, led', or *regō* 'I rule' ~ *rēctum* 'having been ruled'. This rule interacts with another, by which underlyingly voiced obstruents become voiceless when preceding a voiceless obstruent. However, Lachmann's Law is a Latin, or at the earliest Italic, innovation, while the voicing assimilation rule is Indo-European (Kiparsky 1965: 29, Jasanoff 2004: 405). While earlier scholars like Saussure (1885) had attempted to account for this discrepancy within a typically Neogrammarian scenario of sound change interacting with analogical leveling, Kiparsky (1965: 29) argued instead that the best account of this was to have Lachmann's Law apply before the voicing assimilation rule. In Kiparsky's view, the best solution was to take Lachmann's Law as an example of rule insertion not at the end of the phonological component of the language.

Postal (1968: 253–260) also defends the idea of rule insertion first proposed in Halle (1962), arguing that a rule of vowel epenthesis in Mohawk and a rule of accent shift in Oneida are both examples of rule insertion, as well as a potential example from Old English (Postal 1968: 263). In the case of Mohawk vowel epenthesis, for instance, an epenthetic [e] breaks up some, but not all, sequences of underlying /kw/, e.g., [kewíʹstos] 'I am cold' with epenthesis, but [raʹkwas] 'he picks it' without epenthesis (examples cited from Postal 1968: 247; transcription as in original). Postal argues that there are several reasons for this, including morphologically conditioned sound change (as discussed below) and rule insertion. That is, his rule inserting [e] between sequences of a consonant followed by a resonant "operated *not* on phonetic or autonomous phonemic representation, but on the much more abstract systematic phonemic representation ... [T]he rule of epenthesis would operate on the systematic representation of forms directly only if it were added as the first phonological rule, which was certainly not the case" (Postal 1968: 253; italics in original), i.e. it was not inserted at the end of the phonological component, and is thus an example of rule insertion.

King (1969: 43–45) discusses two cases of rule insertion, Lachmann's Law in Latin and a change of [xs] to [ks] in "the history of German." His discussion of Lachmann's Law hews closely to that of Kiparsky (1965)—King (1969: 43) labels Lachmann's Law "[o]ne of the more certain cases" of rule insertion"—and is therefore not discussed here. As for the change of [xs] to [ks], King (1969: 44) cites examples like *oxs > oks* 'ox', *zexs > zeks* 'six'; but indicates that "[x] remained when separated from [s] by a morpheme boundary: *maxst* 'you make' ... *laxst* 'you laugh'" (phonetic transcription as in original). If this were to be

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Collinge (1985), Drinka (1991), and Jasanoff (2004) here. Again, we leave a number of considerations to one side here, that these and other studies treat in more or less detail.

accounted for with a rule added to the end of the phonological component, it would have to take the form "*xs > ks* only if no morpheme boundary intervenes" (King 1969: 44), which in his view "violates the apparently valid empirical hypothesis (Chomsky and Halle 1968: 364) that processes operating within morphemes normally also apply across morpheme boundaries." The best resolution, in King's view, is that this rule "was added as a morpheme-structure constraint—not, in other words, at the end of the phonological component, but as a condition on the configuration of morphemes" (King 1969: 45). That is, this rule is another example of rule insertion.

To summarize, then, these generative scholars argued that a handful of phonological phenomena in languages like Latin, Middle English, and Mohawk, are best accounted for if the rules that motivate them were added somewhere other than at the end of the phonology.

This idea was not universally accepted, however, and a great deal of controversy arose as a result. Leaving aside the sizable body of literature on Lachmann's Law from the 1960s and 1970s (see e.g. Strunk 1976, Collinge 1985, and Jasanoff 2004 for overviews), much of which sought to account for Lachmann's Law without using rule insertion, the more general topic of rule insertion came under fire from scholars like Jasanoff (1971). In his view, "[u]nfortunately, it is doubtful that such RULE INSERTION actually explains the Latin facts correctly …; given the absence of other convincing examples, it would seem that this kind of change, if it occurs at all, is extremely rare" (Jasanoff 1971: 76; capitalization as in original).[24] Others took a more neutral position: Campbell (1971: 195) largely sidesteps the issue, saying only that if some sound changes can be shown to be morphologically conditioned (which was an idea he seems to accept), then "Lachmann's Law need not be viewed as a case requiring the addition of a rule at some deeper level in the hierarchy of rules." Within a few years, though, King himself had disavowed his earlier position, contending that "[r]ule insertion is a fiction…. Grammars change by rule addition at the end of the phonological rules" (King 1973: 577). This rejection aside, it cannot be denied that the idea of rule insertion spurred a good deal of generative work on historical phonology, and as such it merited discussion here. A complete historiographical treatment of this topic remains a desideratum.

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24   A more informal rejection of rule insertion came from Winfred P. Lehmann. In his copy of King (1969), now in the possession of Marc Pierce, he added quotation marks around "more certain" in King's description of Lachmann's Law as the result of rule insertion, and also wrote "WRONG," in all capital letters, in the margin of the page (King 1969: 43).

To conclude this section, we address the idea of morphologically conditioned sound changes.[25] While Neogrammarians like Paul (1920) and structuralists like Bloomfield (1933) had argued that sound change was exclusively conditioned by phonetic/phonological factors, some generativists, e.g. Postal (1968), rejected this claim, contending that sound change could also be conditioned by morphological factors. The clearest statement of Neogrammarian view on this topic is found in Paul (1920: 69):

> Wenn wir daher von konsequenter Wirkung der Lautgesetze reden, so kann das nur heissen, dass bei dem Lautwandel innerhalb desselben Dialektes alle einzelnen Fälle, in denen die gleichen lautlichen Bedingungen vorliegen, gleichmässig behandelt werden. Entweder muss also, wo früher einmal der gleiche Laut bestand, auch auf den späteren Entwicklungsstufen immer der gleiche Laut bleiben, oder, wo eine Spaltung in verschiedene Laute eingetreten ist, da muss eine bestimmte Ursache und zwar eine Ursache rein lautlicher Natur wie Einwirkung umgebender Laute, Akzent, Silbenstellung u. dgl. anzugeben sein, warum in dem einen Falle dieser, in dem anderen jener Laut entstanden ist.[26]
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> PAUL 1920: 69

Furthermore, the Neogrammarians also invoked a variety of other devices to account for sound changes that seemed to admit exceptions, or not to be phonetically conditioned, e.g. analogy, dialect borrowing, the later operation of other sound changes, and the exclusion of typically sporadic types of sound change like metathesis from their hypothesis.[27]

Later scholars like Bloomfield (1928, 1933) also endorsed the Neogrammarian position. Bloomfield (1928: 99), for instance, in his discussion of data from

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25   See e.g. Labov (1981), N. W. Hill (2014), or Pierce (2016) for additional discussion of these issues.

26   "Accordingly, in referring to the consistent operation of sound laws we can only mean that in phonetic change within a dialect every single case in which the same phonetic conditions exist is treated uniformly. Therefore, there are just two possibilities: (1) where the same sound occurred at an earlier time, the same sound remains at later stages of development; or (2) the sound splits into different sounds, in which case there must be a specific cause that explains why different sounds have developed in different environments. These causes must always be of a purely phonetic nature; for example, the influence of neighboring sounds, accent, and syllable position" (translation by Robert W. Murray from Paul 2015: 78).

27   These topics are discussed in more detail in numerous handbooks of historical linguistics (e.g. Hock 1991, Campbell 2013, Bowern and Evans [eds] 2014, etc.) and are thus not discussed further here.

Swampy Cree, states that "sound change goes on regardless of meaning and is therefore subject to phonetic conditions only (and is not affected by frequency, euphony, meaning, etc. or words and other forms)."[28] Bloomfield (1933: 353–354) reaffirms this idea, writing that "[t]he limitations of these conditioned sound changes are, of course, purely phonetic, since the change concerns only a habit of articulatory movement; phonetic change is independent of nonphonetic factors."

While many of the structuralists took the same position as Bloomfield in this area,[29] his most famous contemporary, Edward Sapir, took a different tack. In Sapir's view,

> Every linguist knows that phonetic change is frequently followed by morphological rearrangements, but he is apt to assume that morphology exercises little or no influence on the course of phonetic history. I am inclined to believe that our present tendency to isolate phonetics and grammar as mutually irrelevant linguistic provinces is unfortunate. There are likely to be fundamental relations between them and their respective histories that we do not yet fully grasp. After all, if speech-sounds exist merely because they are the symbolic carriers of significant concepts and groupings of concepts, why may not a strong drift or a permanent feature in the conceptual sphere exercise a furthering or a retarding influence on the phonetic drift? I believe that such influences may be demonstrated and that they deserve far more careful study than they have received.
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> SAPIR 1921: 52

Sapir is somewhat less confident in his position than Bloomfield is in his, but Sapir's openness to the idea of morphologically conditioned sound changes is clear.

Some generativists, most prominently Postal (1968) and King (1969), rejected the strongest version of the Neogrammarian hypothesis. Postal (1968) argues, in his discussion of the Mohawk epenthesis rule mentioned above, that this rule is also morphologically conditioned. In his view, [e] is not inserted within [kw] clusters when "the [k] was the first person morpheme and the [w] the first element of the plural morpheme" (Postal 1968: 247). Postal is cautious about this interpretation, however, stating that this happens "[i]rregularly, and

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28   Bloomfield (1928: 100) also endorses the idea that sound changes are exceptionless, labeling it "a tested hypothesis: in so far as one may speak of such a thing, it is a proved truth."

29   Postal (1968: 236–239) conveniently gathers and presents a number of similar statements from Structuralist works.

for reasons which are inexplicable" (Postal 1968: 247; this is in fact the beginning of the statement quoted above). In Postal's view, then, "[s]ome regular phonetic changes take place in environments whose specification requires reference to nonphonetic morphophonemic and/or superficial grammatical structure" (Postal 1968: 240). That is, some sound changes are in Postal's view morphologically conditioned.

King (1969) endorses Postal's account of Mohawk, and then argues that final schwa deletion in Yiddish was also morphologically conditioned (see Jacobs 2005 for a somewhat different assessment of the Yiddish material). Yiddish has generally lost final schwas, e.g., *teg* 'days', *erd* 'earth', *gib* 'I give', from Middle High German *tage, erde*, and *gibe*, respectively, but retains them sometimes, "principally when the [schwa] is an adjective inflectional ending" (King 1969: 123), e.g. *dos alte land* 'the old country', *di groyse shtot* 'the big city', etc. King (1969: 123) further states that "[a] few other final unaccented [schwas] are retained, erratically, but these too are confined to specific morphological environments, e.g. *gésele* 'little street', where -(*e*)*le* is the diminutive suffix." King argues that things like differences in phonetic environments and analogy cannot account for the Yiddish data, arguing instead that schwa deletion in Yiddish "is a case pure and simple of phonological change that cannot be stated in terms of purely phonetic features" (King 1969: 123–124). In King's view, then, "[c]ases are not uncommon of changes that occur across the board except in certain morphological environments" (King 1969: 123). That is, morphologically conditioned sound change occurs, and it is not uncommon.

Responses to this idea were mixed. The non-generativist Raimo Anttila (1972), in his widely read handbook, endorsed the idea enthusiastically, and extended the idea to include semantic factors. In Anttila's view, "[b]ecause language is one organic whole … where everything depends on everything else …, it is logically thinkable that some sound changes would start from the grammar" (Anttila 1972: 77). He goes on to trace a number of sound changes to morphological conditions, e.g. the loss of word-final nasals in Karelian, which occurs across the board, except in the genitive singular (compare illative *vete-hen* > *vedeh* 'into water' with loss of the nasal, with the genitive singular *venehen* 'of a boat' with retention of the nasal); the retention of nominal endings that have otherwise been lost in English, e.g. (archaic) *whilom* 'in former days', from OE *hwīlum*; incomplete phase formation in Rotuman; and vowel lengthening in Sanskrit. Taking a wider view of "morphological conditioning," Anttila argued that even the Neogrammarians had allowed for morphologically conditioning sound changes, as in his view word boundaries are also morphological, and the Neogrammarians accepted the idea that word boundaries could condition sound change.

Others were more cautious. Campbell (1971) took a middle ground, rejecting Postal's analysis of Mohawk in favor of that proposed by Chafe (1970), who had argued that native speakers of Mohawk might parse these forms differently from Postal, suggesting that a morphologically conditioned analysis might not be necessary. At the same time, though, Campbell was happy with the idea that sound changes could be morphologically conditioned, and, as noted above, his acceptance of the idea helped him to sidestep the problem of rule insertion— since Campbell viewed Lachmann's Law as a morphologically conditioned sound change, he did not have to engage with the rule insertion question in that case.[30]

Still others rejected the idea out of hand. One of the more rigorous rejections came from Jasanoff (1971). Although Jasanoff accepted the idea that some synchronic phonological rules could be morphologically conditioned, he rejected the idea of morphologically conditioned diachronic sound changes (a position also taken by Bloomfield himself; compare Bloomfield 1930 on the dorsal fricatives in Modern German with his position on the same topic in Bloomfield 1933). He accepted Chafe's analysis of the Mohawk example discussed by Postal and King; and further dismissed King's account of Yiddish.[31] Hock (1976) argued against the idea on different grounds, contending that alternative accounts not involving morphologically conditioned sound changes were available for the alleged cases described by Anttila (1972). He argued, for instance, that there are some clear phonetic differences between the Karelian forms (in length and syllabification, for instance), meaning that the differences were not necessarily due to morphological conditioning. In Hock's view, then, at best "unambiguous instances of grammatical conditioning are very rare and difficult to find."[32]

The issue remains controversial, as demonstrated by the remark in Campbell's textbook of historical linguistics that "[w]hether sound change can be morphologically conditioned is disputed and remains an empirical question" (Campbell 2013: 263). In fact, the issue remains controversial in

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30   Campbell was a student of Anttila at UCLA, which is presumably one reason for his acceptance of the idea in 1971.

31   Briefly put, Jasanoff (1971) agreed with King that retained final schwas were not the result of analogy, but he argued that deleted final schwas went back to Middle High German schwas and that retained final schwas went back to Middle High German long vowels that had been regularly reduced to schwa.

32   In later work, Hock (1991) admits that there are problems with the Neogrammarian hypothesis, but ultimately rejects the idea of morphologically conditioned sound change in favor of the traditional Neogrammarian theoretical devices of regular sound change and analogy.

Campbell's textbook itself, as in a later part of the book he discusses several "well-known (putative) examples of morphological conditioning of sound change" (Campbell 2013: 326), and ultimately draws a conclusion close to that of Sapir's quoted above: "At this stage of our understanding, we cannot ignore any potential causal factor…. It will only be through further extensive investigation of the interaction of the various overlapping and competing factors that are suspected of being involved in linguistic changes that we will come to be able to explain linguistic change more fully (Campbell 2013: 335). This is certainly true, and underscores our point that the emergence of this idea is one of the most important developments of historical linguistics in the 1960s.

3   Historical Linguistics 50 Years Later

Since the late 1960s, historical linguistics has seen a number of major advances, some of which we briefly summarize in this section before giving an overview of the remaining chapters in this volume in Section 4. More specifically, we discuss (1) the phonetics of sound change, (2) the role of language contact in language change, and (3) computational and phylogenetic approaches. Other developments that we find worthy of discussion, but are compelled to leave aside due to space considerations, include the continuing application of Labovian sociolinguistic methods and generative ideas to the study of language change (on which see Labov 2011, Dresher 2015, and Holt 2015, among other studies), the emergence of large-scale corpora for the study of language change (cp. Joseph, this volume), and research on connections between genetics, archaeology, and linguistics.[33]

One major advance in our understanding of language change is a better understanding of the mechanisms of sound change. In this context, Weinreich et al. (1968: 102) formulated, as noted above, the actuation problem, one of the major puzzles in language change (more generally) more than 50 years ago as follows: "Why do changes in a structural feature take place in a particular language at a given time, but not in other languages with the same feature, or in the same language at other times?" A number of theoretical studies, such as Jespersen (1894), Martinet (1952), Kiparsky (1968), King (1969), and Vennemann

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33   The last of these shows that historical linguistics is not an isolated discipline, but instead interacts with other disciplines. We are inclined to treat such evidence with caution, in light of some of its flaws (perhaps most famously the earlier incorrect classifications of the languages of Africa, many of which relied on genetic or cultural traits, rather than linguistic evidence, on which see Greenberg 1966). We do, however, find it interesting and worthy of additional consideration elsewhere.

(1993), suggest, among other hypotheses, that sound change improves communication or that it is implemented by altering the grammar. Ohala (2003: 683–684) criticizes these teleological accounts for a "lack of rigor in marshaling the evidence" and instead offers a phonetically based account of sound change that offers the possibility of rigorous testing in the laboratory.

In a number of studies, Ohala (1993, 1995a, 1995b) investigates the phonetics of sound change, in particular the phonetic preconditions for sound change (as opposed to their actual trigger or subsequent spread and transmission).[34] Following earlier research on the instrumental study of speech (e.g. Rousselot 1891, Meyer 1896–7, Hombert et al. 1979), and drawing on technical improvements not available in the 1960s, Ohala focuses on the only factors common to all languages at all periods of time, namely the physical phonetic properties of the speech production and perception systems, which are apparently subject to a significant degree of variability: "One of the major discoveries of phonetics over the past century is the tremendous variability that exists in what we regard as the 'same' events in speech, whether this sameness be phones, syllables, or words" (Ohala 1993: 239). This observation leads Ohala (2003: 671) to conclude that the "relatively short list of allophones given in conventional phonemic descriptions of languages is just 'the tip of the iceberg'," and that "the variation is essentially infinite, though generally showing lawful dependency with respect to the phonetic environment, speech-style, or characteristics of the individual speaker."

With respect to the role of phonetic variation and sound change, Ohala proposes that a great deal of synchronic phonetic variation, including that found in present-day speech, resembles diachronic variation. While the parallels between synchronic phonetic variation and diachronic change are difficult to overlook, Ohala also notes that pronunciation somehow remains relatively stable over time despite the great variation observed in everyday speech. If synchronic variation were in fact "sound change observed 'on the hoof'," then sound change should progress much more quickly than it actually does, according to Ohala (2003: 674). This raises the question of why this is not the case.

Ohala thus proposes that it is the listener that has the lead role in sound change, not the speaker. According to Ohala, data from perceptual studies show that even though the speaker produces the phonetic variation in speech,

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34   Other important studies in this area, some of which dissent from Ohala's conclusions, include Hockett (1958), J. Blevins (2004), Beddor (2009), and Bybee (2015). As Ohala's analyses seem to be the best known, we focus on them here. See also E. Hill (2009), who argues that such theories of sound change cannot account for "conspiracies," i.e. situations in which a number of sound changes (or phonological rules) have the same end result (e.g. the creation of certain types of syllables, etc.).

the listener is typically capable of parsing the variation to its proper sources. In the end, then, "[v]ariability created by the speaker makes the speech signal ambiguous to the listener, but it is the listener who inadvertently makes the error in (re)constructing the pronunciation norm" (Ohala 2003: 684). In this view, sound change at its very initiation is the result of an inadvertent error on the part of the listener.[35] It does not serve any purpose at all and it does not improve speech in any way, nor does it make speech easier to pronounce, easier to hear, or easier to process, according to Ohala. This view also sheds light on the parallels between synchronic and diachronic variation: "One of the most important aspects of the comparative method is establishing likely paths, that is, sound changes, between one posited state of a language and another. Phonetics can assist in evaluating alternative paths" (Ohala 2003: 684). We thus see Ohala's approach to the phonetics of sound change as a milestone in historical linguistics over the past 50 years, because it allows linguists to study sound change in the laboratory using empirical methods that can better elucidate the parallels between variation in centuries past and variation in speech today.[36]

Another major advance in historical linguistics over the past half century comes from insights about the role of language contact in language change. This topic was already the subject of considerable debate in the 1960s, brought to the forefront by Weinreich's (1953) seminal work, as well as Malkiel (1968) and Weinreich, Labov, and Herzog (1968), among other publications (see also Thomason, this volume). One of the major publications on contact-induced change that appeared post-1968 was Thomason and Kaufman (1988), which makes a principled distinction between borrowing and language shift (defined briefly below).[37] Thomason and Kaufman (1988: 37) define borrowing as "[t]he incorporation of foreign features into a group's native language by speakers of that language: the native language is maintained but is changed by the addition of the incorporated features." In language contact situations, non-basic lexical items are typically borrowed first, followed later by other lexical items as well as structural features, depending on the length and intensity of contact.

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35   Robert Mailhammer (p.c.) reminds us that this error could also be an "unconscious optimization of perception," which would then tie the process back to the view that sound change is in fact driven by an effort to improve communication.

36   In this context, note also the new focus on sociophonetics in works like Foulkes et al. (2010), as well as the attention paid to the role of the listener in various works by Elizabeth Traugott.

37   Another major publication in this area (and from the same year) is Van Coetsem (1988). See Van Coetsem (1989) for a comparison of his approach with that of Thomason and Kaufman (1988).

Borrowing typically occurs in situations of "full" bilingualism. Moreover, Thomason and Kaufman argue compellingly that any linguistic feature can spread through contact, i.e. that anything can be borrowed in the right circumstances, and thus that language change is socially conditioned and that there is essentially no upper limit on language contact.

A number of recent studies therefore address contact-induced or contact-reinforced change.[38] Examples from our own recent work on Texas German, which has been in contact with English for over 150 years, include numerous lexical borrowings, from nouns like *Schulyard* 'schoolyard' and *County Commissioner* to discourse markers like *well* and *so* (Boas and Pierce 2011, Boas 2018); structural borrowings like verb-second position in subordinate clauses (see Dux 2017, 2018, as well as Fuchs 2018), changes in relative clauses (Boas et al. 2014), and the English-origin progressive marker *-ing* (Guion 1996 and Blevins 2018); as well as some phonological and/or phonetic influence from English (Boas 2009, Pierce, Boas, and Roesch 2015, Pierce and Fingerhuth 2018).[39] Based on the amount of borrowing from English into Texas German, current Texas German can be characterized as stage 3 (more intense contact) on Thomason and Kaufman's 5-point borrowing scale.[40]

In contrast to borrowing, Thomason and Kaufman define shift as follows: "in this kind of interference a group of speakers shifting to a target language fails to learn the target language (TL) perfectly" (1988: 19). In such cases, which Thomason and Kaufman also label shift-induced interference, features from syntax and morphology are most commonly transferred. Cases of shift-induced interference are more difficult to determine because multiple processes may be at play, according to Thomason and Kaufman. Thomason (this volume: 118)

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38   The changes discussed in this section are not always motivated solely by language contact, hence the hedge "contact-reinforced" here.

39   This work has been undertaken under the auspices of the Texas German Dialect Project (TGDP; www.tgdp.org) at the University of Texas at Austin. Project members have been resampling Gilbert's data set from the 1960s (when the recordings for his 1972 *Linguistic Atlas of Texas German* were originally made) since 2001 (see Boas et al. 2010). So far, the TGDP has recorded more than 700 speakers of Texas German. This has created a rich pool of real-time data.

40   The five stages of Thomason & Kaufman's (1988: 74–76) borrowing scale are as follows: Stage 1: Casual contact (lexical borrowings only); Stage 2: Slightly more intense contact (slight structural borrowing; conjunctions and adverbial particles); Stage 3: More intense contact (slightly more structural borrowing; adpositions, derivational suffixes); Stage 4: Strong cultural pressure (moderate structural borrowing [major structural features that cause relatively little typological change]); Stage 5: Very strong cultural pressure (heavy structural borrowing; major structural features that cause significant typological disruption); see also Winford (2003) for discussion.

characterizes the complex interplay of different processes in shift-induced interference as follows:

> Shifting speakers may carry over features from their L1 into their L2; these innovative features become part of the shifting group's version of the target language (TL): let's call this version TL2. Second, the shifting speakers may also fail (or refuse) to learn certain features of the TL, and these instances of imperfect learning also form part of TL2. These two processes are distinct in principle and often in fact, but some innovations in the target language almost certainly result from both processes. (…) The third possible process in shift-induced interference arises if the shifting group and the original group of target-language speakers—that is, speakers of TL1—merge into a single speech community. In that case, speakers of TL1 may borrow a subset of innovative features from TL2, yielding a third "melded" version of the target language, TL3.

The various complexities involved in shift-induced interference make it more difficult to establish contact as a cause of language change than in situations of borrowing discussed above. According to Thomason (2003: 691), knowledge about the presence versus the absence of full, or at least extensive fluency in the recipient language is the key sociolinguistic variable that helps us with determining whether the people who introduce the interference features speak the language into which the features are introduced or not.[41] In other words, the nature of the interference process is at least partially determined by imperfect language learning. However, to determine whether imperfect language learning was involved in any contact situation requires the relevant data about members of the speech community. Unfortunately, this is not always available—relatively little is known about the sociolinguistic setting of Gothic-Greek contact, to give an older Germanic example. As Thomason (2003: 710) notes, "[i]n this respect contact-induced language change is no different from other subfields of historical linguistics: inevitably incomplete

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41   Thomason's (2003) point about incomplete information is an important one, especially when it comes to determining the role of nonlinguistic factors such as social influences in motivating linguistic change. More broadly speaking, it is important to highlight the close ties between historical linguistics and sociolinguistics, especially when it comes to determining the nature of language change. The pioneering research by Labov and his associates discussed in Section 2 above provides an empirically based model of the dynamic processes in language variation and change. For an overview see Wolfram & Schilling-Estes (2003).

information all too often makes it impossible to tell just what occurred at some distant past time, and why."

To return to Texas German, having access to information about fluency in the recipient language has also proven helpful in our own research on its development. Gilbert (1972) presents a short biographical sketch for each of the speakers he recorded for the 1960s for his *Linguistic Atlas of Texas German*. While the biographical information differs in length and level of specificity from speaker to speaker, it is often possible to glean information about a speaker's fluency in Texas German. This information is often indicative of whether imperfect language learning played a role or not as it helps to put the linguistic data recorded by Gilbert into context. More recently, the Texas German Dialect Project (Boas et al. 2010) has systematically collected biographical information about the more than 700 speakers it has recorded since 2001. An 11-page long written questionnaire covers a broad range of biographical information, including information about speaker and language attitudes and information about how much English and German the speakers used throughout their lives in different private and public domains. The biographical information is used as the speaker metadata in the Texas German Dialect Archive (http://www.tgdp.org), so that each recording in the database is linked to its metadata. The combination of linguistic and non-linguistic information in the database allows researchers to systematically study whether certain sociolinguistic variables may have influenced a specific contact-induced change in Texas German (or not). Preliminary research on the role played by some sociolinguistic variables suggests that speaker attitudes towards Texas German and English (Boas 2009, Boas and Fingerhuth 2017), religious identification (Boas 2015), and knowledge of Standard German (Boas and Fuchs 2018) may influence contact-induced change in Texas German.

In sum, research on language contact and its influence on language change has not only informed numerous studies in historical linguistics.[42] It has also laid the ground for investigating more general mechanisms of contact-induced interference, such as code-switching (Poplack 1980, Myers-Scotton 1993, Clyne 2003, Backus 2005). Much of this research, in turn, has formed the basis for more recent cognitively-oriented models of language contact such as Cognitive

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42   In fact, it does not go too far to say that there has been an explosion of publications (we thank Danny Law for suggesting this formulation to us) in this area since 1988, including the creation of (at least) one new journal focused specifically on language contact (*Journal of Language Contact*). Somewhat at random, we list the following such studies: Bisang (2006), Epps (2012), Joseph (2013), Drinka (2017), and Dworkin (2018). Drinka (2017) won the LSA's Leonard Bloomfield Book Award, indicating the importance of this area of inquiry.

Contact Linguistics (Zenner et al. 2019) and Diasystematic Construction Grammar (Boas and Höder 2018). Although it is not completely clear how this subfield will develop in the future, we see it as one of the most important and exciting areas of historical linguistics today.

Technological advances over the past 50 years have led to much faster and powerful computers with enormous storage capabilities. These changes have not only spurred the development of computational linguistics and corpus linguistics as emerging research fields, but have also led to new insights in historical linguistics. One of these areas is more technical, concerning corpora, databases, storage, i.e. new electronic resources, and is therefore left aside here. The other advance concerns methodology, more specifically the adoption of Bayesian phylogenetic models from biological systematics for linguistic research (see Bowern 2017, Garrett 2018, as well as the additional references given below, for additional discussion).

The best-known application of this methodology to historical linguistics involves the origins and dispersal of the Indo-European languages. This discussion has focused on two hypotheses. The first of these, the so-called steppe model proposes that Indo-European has its origin in the steppes north of the Black and Caspian Seas around 4500 BCE and was later spread by pastoralists who knew how to build wheeled vehicles and breed horses (Mallory 1989, Anthony and Ringe 2015). In contrast, the co-called Anatolian model proposes that Proto-Indo-European developed in Anatolia some 3,000 years earlier and that it expanded into Europe with the spread of agriculture (Renfrew 1987, Heggarty 2015). The steppe model and the Anatolian model differ significantly with respect to the time and place of origin of Proto-Indo-European.

The past two decades have seen the emergence of phylogenetics as a major method to shed more light on this question. Instead of using the traditional approach, i.e. relying on the comparative method to assess language relationships and linguistic family trees, a phylogenetic analysis uses a set of linguistic traits such as basic vocabulary,[43] typically including very large informative data sets that are then reduced to numerical characters that are used for further modeling and analysis. According to Garrett (2018: 31), two other crucial parts are involved:

> A second is a model of trait evolution, expressing assumptions about how linguistic traits change over time; a third is a clock model, expressing assumptions about how rates of change vary across the whole language tree and from trait to trait. The clock model is crucial if a goal is chronology:

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43   In fact, phylogenetic approaches tend to rely mostly on basic vocabulary.

to hypothesize when the ancestral language was spoken, assumptions about rates of change are needed. Finally, there are hard constraints, for example based on historical evidence—that Old Irish was spoken from 700 to 900 CE, or that Classical Latin was spoken in the first century BCE.

Based on these various types of inputs, computer programs use Bayesian phylogenetic methods to evaluate very large numbers of possible language trees automatically for likelihood, which then allow researchers to identify a set of probable language trees (Garrett 2018, Bowern 2017). One of the major and perhaps also controversial proposals about the origins of Proto-Indo-European based on Bayesian phylogenetic models can be found in Bouckaert et al. (2012). In this work, the authors argue against the steppe model and in favor of the Anatolian model of Proto-Indo-European with a date of about 6000 BCE (see also Gray and Atkinson 2003 and Heggarty 2015 for discussion). This proposal was quite controversial, as it argued against the widely held steppe model that is based on qualitative evidence from reconstructed vocabulary and correlations with archeological data (see Bybee 2011, Campbell 2013, and Verkerk 2017 for discussion). Some historical linguists therefore rejected it outright; e.g. Pereltsvaig and Lewis (2015: 8) contend that traditional approaches "deserve to be cherished, not tossed away on the rubbish bin of intellectual history by overly eager, would-be paradigm shifters."[44] Others were more open, e.g. Garrett (2018: 32), who writes that his "own approach has been to try to understand these methods and work out why they produce the results they do."

Ultimately, the issue requires extensive consideration, as works like Chang et al. (2015) and Garrett (2018) demonstrate. Garrett (2018), building on the discussion in Chang et al. (2015), lays out the issues clearly and carefully. He notes that Chang et al. (2015) "first replicated the results of … [Bouckaert et al. 2012] … using the same data set and methodologies" (Garrett 2018: 33). Their results closely reflected those of Bouckaert et al. 2012, which led them to raise the question of whether historical linguistics have "simply been wrong about … Indo-European?" (Garrett 2018: 33). Closer inspection, however, indicated some flaws in the work of Bouckaert et al. (2012), specifically that their phylogenetic analysis did not account for "innovations that occur independently in related languages but were not present in their common ancestor" (Garrett 2018: 35), e.g. vocabulary developments in Romance and Celtic. Specifically, the Romance

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44   See Bowern (2017) for an insightful review article of Pereltsvaig and Lewis (2015). Pereltsvaig and Lewis's comment resembles numerous earlier remarks, e.g. the lament of Markey (1986: 678 fn. 7) about scholars who use "an unprincipled invocation of known typologies from the present to discredit deductively presumed reconstructions."

languages, as well as Modern Irish and Scots Gaelic, replaced the cognate Latin and Old Irish terms for 'man, male person' with "cognate words that originally meant 'human being'" (Garrett 2018: 35). Although these were separate, parallel developments, they were apparently interpreted within the phylogenetic model as being present in Proto-Indo-European, which "stretche[d] the overall tree like an accordion" (Garrett 2018: 35). This interpretation moreover necessarily imputes a greater time depth to the proto-language, in this case making it look like the Anatolian model fit the chronological parameters of the study better than the traditional steppe model (Garrett 2018).

Garrett (2018: 35) further points out that this issue can be resolved by "hardcod[ing] known relationships." Once Chang et al. (2015) had added these "known relationships" to the mix, then they "were able to infer overall rates of change, from which [they] could infer rates of change over the entire tree" (Garrett 2018: 35), with the end result of not stretching the tree, i.e. obtaining more reliable chronological results, in this case getting a result of 4000 BC, not 6000 BC for the time of PIE, which fits the traditional steppe model, not the newer Anatolian model. Chang et al. (2015: 233) therefore conclude that "statistical phylogenetic analysis strongly supports the steppe hypothesis of IE origins, contrary to the claims of previous research."

We have focused on the Indo-European situation here to illustrate the controversial nature of phylogenetic analysis. Note, however, that although the phylogenetic analysis of IE origins led to incorrect results in our view, phylogenetic analysis did yield correct results in other situations, e.g. for Austronesian (Chang et al. 2015: 233). While this indicates that it remains a potentially powerful and useful tool for historical linguistics, it still needs to be treated with caution. Its successes so far have essentially replicated what was already known, and, despite considerable work in phylogenetic methods, it has not been demonstrated that traditional methods of linguistic classification like the comparative method are unreliable. If nothing else, the entire debate has showed how complicated the entire issue is, from the application of the methods to the interpretation of the results. We therefore conclude here that, although it has spurred considerable discussion in the scholarly literature, at best the methodology should not be dismissed out of hand, but instead carefully analyzed and then either employed or not.[45]

We conclude this section with a very brief general summary. We contend that historical linguistics has evolved in a very positive way since the 1968 publications of the original volume. There are numerous new insights, numerous new and exciting lines of research, institutes involving historical linguistics

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(the Max-Planck-Institutes in Leipzig and Jena), large conferences on histori-
cal linguistics (e.g. the International Conference on Historical Linguistics and
the International Conference on English Historical Linguistics), and public
interest in historical linguistics (as noted above, the LRC at the University of
Texas at Austin focuses on historical linguistics; its website draws between
30,000 and 40,000 visitors monthly). On the negative side, historical linguistics
remains marginal in some departments (especially in language departments),
and there are very few jobs available, with many historical linguists in recent
years being hired because of their expertise in some subfield beyond histori-
cal linguistics. But despite this, we contend that historical linguistics, although
it has not reclaimed its central position within the field, remains strong and
vital, with a number of promising areas for future research,[46] nearly 240 years
after Sir William Jones proposed the most famous early version of the Indo-
European hypothesis.

4   The Chapters in This Volume

The chapters in this volume are based on presentations given at a roundtable
"New Directions for Historical Linguistics: Impact and Synthesis" organized
by Hans C. Boas and Bridget Drinka at the 23rd International Conference on
Historical Linguistics, held in San Antonio in August 2017. The roundtable
aimed to discuss the evolution of historical linguistics in the 51 years since
the original symposium in general, as well as the impact of the ideas from the
1966 Symposium at the University of Texas at Austin and the 1968 volume on
historical linguistics and sociolinguistics in particular. The chapters in this
volume each touch on the impact of the original symposium and the edited
volume since the 1960s and they discuss how historical linguistics has evolved
since then.

   The first chapter, by William Labov, *What has been Built on Empirical
Foundations*, discusses how the original paper *Empirical Foundations for a
Theory of Language Change* by Weinreich, Labov, and Herzog came to be pro-
duced and its impact on linguistic theory since its publication more than a
half century ago. In the first part of the paper, Labov provides a chronological

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46   These include the study of numerous lesser-described languages in traditionally under-
      studied geographic areas, which lends new insights into language change, e.g. the role of
      social structures and sociolinguistic practices that are not generally encountered in areas
      like Europe and North America, and the question of broader typological patterns that
      may result from processes of change that differ from those encountered elsewhere. We
      thank Patience Epps for helpful comments on this topic.

overview of events from the time when Weinreich first received an invitation from Winfred Lehmann and Yakov Malkiel to participate in the 1966 symposium at the University of Texas until the time of the symposium itself. Labov provides a number of interesting quotes from the correspondence between Weinreich, his co-authors, and the organizers of the symposium. In the second part, Labov addresses the role of variable behavior, arguing that the last half century has seen an ever-increasing amount of studies focusing on linguistic variation (also enabled by more sophisticated computational resources). Part three evaluates the role of the notion of idiolect in Weinreich, Labov, and Herzog (1968) and how it has been investigated since the 1960s. Labov draws a direct connection between Weinreich's close reading of important works by 19th century linguists such as Hermann Paul to "Empirical Foundations" and to ongoing current research on the relationships between individual speakers in speech communities. The remainder of the paper focuses on how current quantitative methods have advanced the proposals in Empirical Foundations about linguistic change. More specifically, Labov examines a particular sound change from below, that of the raising of /eyC/ in Philadelphia, as an example of a sound change that "lies well below the level of awareness for the general public and phoneticians alike." Based on a comparison of data from the 1930s and 1940s with data collected by members of the class of "The Study of the Speech Community" at the University of Pennsylvania from 1973 to 2012, Labov discusses various developments regarding the advance of /eyC/ across several generations of speakers, highlighting the role and status of the notions of idiolect and speech community.

Gillian Sankoff's chapter, *Building on Empirical Foundations: Individual and Community Change in Real Time*, begins with a discussion of the role of variation in language change, one of the key topics of Weinreich, Labov and Herzog's (1968) paper. To this end, Sankoff addresses the role of two main concepts from that foundational paper, the Transition Problem and the Embedding Problem, in variationist sociolinguistics. The second part of her chapter provides a discussion of the apparent time concept, including Labov's (1963) Martha's Vineyard Study, his (1966) New York City study, and how psychologists view the role of so-called "optimal periods" in language acquisition. After a short overview of the role of transmission and diffusion in part 3, Sankoff draws on evidence from second-language and second-dialect acquisition to discuss inter-individual variability in acquisition outcomes for adults that differ from what is typical for children and adolescents. Reviewing a number of studies of linguistic behavior of long-term adult residents in areas they did not grow up in leads Sankoff to conclude that "Labov's distinction between transmission and diffusion has a clear basis in maturationally-determined language-learning

abilities." Sections 5 and 6 discuss studies of individual trajectories in real time and individual trajectories in language change, respectively. Sankoff proposes three different possible trajectory types that adults may follow in speech communities in which changes are ongoing: They may either retain their earlier-acquired grammar, or they may become more conservative as they age (adopting the linguistic practice of their elders), or they may, under the influence of increasing numbers of younger speakers who are spearheading the change, participate in the change themselves. Following an in-depth discussion of various studies on each of the three types of trajectories, Sankoff concludes, among other points, that structured variability is a normal property of speech communities and that so-called "structured heterogeneity" is something that language users are apparently well equipped to deal with.

Paul Hopper's chapter, *Timely Notes on Saussure and Hermann Paul after 1968*, provides a number of observations on one major theme of the later 20th century and beyond, namely temporality, which Hopper calls "the flux of time." Hopper first discusses how different contributions in the original 1968 volume address ideas by Saussure and Paul, in particular the idea that the notion of time should play a crucial role in any empirical study of language structure. More specifically, Hopper shows how Saussure's distinction between synchronic and diachronic study of language in the *Cours de linguistique générale* was seen by Lehmann (1968) and Weinreich et al. (1968) both as important but also somewhat limited. This leads Hopper to a more extensive discussion of Saussure's influence on its structuralist descendants throughout the 20th century and a reevaluation of Saussure's *Cours* in the context of the discovery of new sources. More specifically, Hopper shows how scholars such as Rastier (2006) and Jäger (2010) hold different views of Saussure's proposals in his original *Cours* in the light of Saussure's handwritten unpublished manuscripts discovered in the Saussure family mansion in 1996, which were eventually published as *Écrits de linguistique générale* (Saussure 2002). Hopper then compares the terms and concepts *état de langue* (Saussure) and *Sprachzustand* (Paul), showing that the former played a much more important role in Saussure's *Cours* than the latter did in Paul's (1920) *Prinzipien*. The remainder of Hopper's chapter discusses how (1) Paul's (1920) psychological view of language influenced Weinreich, Herzog, and Labov's (1968) thinking of the role played by the individual as the primary carrier of language, (2) the notion of time has played a crucial role in different subdisciplines of linguistics, and (3) how different ideas from the European continent moved into the English-speaking academic world in the decade after the 1966 symposium. Hopper finishes his chapter with a discussion of the contrasting ideologies of Saussure's synchronic *langue* in the *Cours*

and Hermann Paul's historicism in the *Prinzipien vis-à-vis* the emergence of temporality as an important concept in the study of language.

Sarah Thomason's chapter, *Historical Linguistics since 1968: On Some of the Causes of Linguistic Change*, begins with the observation that Weinreich, Labov, and Herzog's (1968) paper has had by far the most influence on developments in historical linguistics and sociolinguistics since 1968. In Thomason's view, though, Malkiel's (1968) paper *The Inflectional Paradigm as an Occasional Determinant of Sound Change*, published in the same volume, should receive more attention than it has, not only because Malkiel amplifies some of the observations made by Weinreich, Labov, & Herzog, but also because Malkiel sketches some important proposals for new directions for post-1968 historical linguistic research. The main part of Thomason's chapter provides a discussion of some of Malkiel's (1968) proposals and how they have influenced historical linguistics in the past five decades. She first summarizes some of Malkiel's proposals and then shows how these proposals have been addressed by different researchers over the past 50 years. Thomason begins with a discussion of Malkiel's question *Can morphological analogy lead to sound change?*, which leads her to look into the problem of multiple versus simple causation in linguistic change. In her view, some of the most pressing questions in this respect are the following: "How common is multiple causation, as opposed to a single cause for a single linguistic change?" and "Can multiple causes be identified for some, many, or even most changes?" Thomason then reviews how different researchers over the past 50 years have investigated interference of nonlinguistic factors with language history, a point made by Malkiel (1968) and based on earlier research by Weinreich (1953). To this end, she provides an in-depth discussion of the concepts of borrowing and shift-induced interference as proposed by Thomason and Kaufman (1988) and concludes that "the distinction between borrowing and shift-induced interference is a social distinction that correlates robustly with distinct sets of linguistic outcomes, and this is just one of many contexts in which historical linguistics and sociolinguistics are intimately interconnected." The remainder of Thomason's chapter is concerned with reviewing another "pressing problem" formulated by Malkiel (1968), namely allowances for the purposefulness of language change. Thomason concludes by providing an outlook of what the next fifty years might bring to research in historical linguistics, in particular the idea that historical linguistics and sociolinguistics will continue to benefit from each other's insights: "If we can expand the common ground and resolve our methodological differences, we should be able to arrive at a deeper understanding of how and why languages change."

In *Precursors of Work on Grammaticalization and Constructionalization in Directions for Historical Linguistics*: Elizabeth Closs Traugott discusses research on morphosyntactic change since 1968. More specifically, she shows how some of the ideas articulated in Benveniste's (1968) paper on auxiliation and Kuryłowicz's (1968) paper on analogy as a mechanism of sound change laid the groundwork for some later research in historical linguistics, particularly grammaticalization and constructionalization. Traugott first discusses the technological advances of the 1970s and 1980s, which led to historical linguists for the first time gaining broad access to large quantities of empirical data in searchable electronic data bases and corpora such as the Helsinki Corpus of English Texts, which in 1991 became the first larger text corpus available for research. According to Traugott, such corpora enabled the quantitative turn that historical morphosyntax took in the last twenty years, allowing researchers such as Heine (2002), Diewald (2002), and Bybee (2010) to investigate systematically a number of important issues raised in the 1968 *Directions* volume: (1) usage of speakers/writers (Lehmann 1968: 15; Malkiel 1968: 30), (2) context (Benveniste 1968: 90), (3) gradualness, in a dynamic, "fluid view of change" (Lehmann 1968: 14), and (4) micro-history, and the "minute shifts" evidenced in texts (Lehmann 1968: 14). Traugott next reviews some milestones of 20th century research on grammaticalization (some of which was foreshadowed by the *Directions* volume), especially the idea that grammaticalization involves a shift from lexical to grammatical status, the hypothesis of unidirectionality and possible counterexamples to it, the context-dependency of grammaticalization, and the role of analogy in grammaticalization. This discussion leads Traugott to the main part of her chapter, which deals with constructionalization (Hilpert 2013, Traugott and Trousdale 2013), an approach "that emerged only in the late 20th century," developed within the framework of Construction Grammar (see Hoffmann and Trousdale 2013 for an overview). Adopting the main idea that the basic linguistic unit is a pairing of form with meaning (a construction), historical linguists working within Construction Grammar adopt a usage-based perspective that incorporates information about the context in which a construction is used as well as the role of frequency to aim to understand how morphosyntactic change happens over time. In the remainder of her chapter, Traugott compares how different concepts such as unidirectional reduction and expansion have played a role in research on grammaticalization and constructionalization. Finally, she highlights several exciting emerging research programs on language change, including quantitative approaches, ideas about complex adaptive systems, psycholinguistics (including experimental research shedding light on the psychological reality of cognitive mechanisms presupposed in historical work), and evolutionary game theory.

Finally, Brian Joseph's chapter, *Historical Linguistics in the 50 years since Weinreich, Labov, and Herzog 1968*, evaluates the influence of Weinreich, Labov and Herzog (WLH 1968) on historical linguistics and sociolinguistics in the past 50 years. Joseph begins by pointing out that WLH has 2885 citations in Google Scholar as of April 2019 and sets the stage for the remainder of the chapter by posing the following two questions: (1) How has the field of historical linguistics changed in the past 50 years?, and (2) How have the ideas presented in the original "Directions for Historical Linguistics" Symposium hosted at UT Austin in 1966 influenced historical linguistics and sociolinguistics? Given the large number of citations of WLH, Joseph's goal is to answer these two questions with respect to WLH. In Section 2, Joseph lists what he sees as the major developments in historical linguistics over the past 50 years, including quantitative sociolinguistics, mathematical modeling, large-scale corpus work ("big data"), language contact studies, and phylogenetic modeling. He then reviews the five major "problems" in language change listed by WLH (1968: 183–187), including the "constraints problem" (how is language change constrained?), and the actuation problem (why do language changes occur in some languages at some times, but not in other language at other times?), as well as of the "agenda for the study of language change" put forth by WLH (1968). From there, he discusses these major developments, with attention to their connection to WLH (1968), e.g. that "WLH brought into the arena of linguistic investigation the mathematical modeling of sociolinguistic variation," etc. In section 3, Joseph briefly discusses the "transmission of language and its relevance for language change," i.e. the question of whether change arises in the process of language acquisition, as numerous generative studies have held, or if adults can "innovate linguistically" (Joseph argues strongly, following WLH 1968 and subsequent work, that adults can indeed do so). Section 4 addresses the notion of stability in historical linguistics, i.e. features of historical linguistics that "characterize historical linguistic investigation today and have characterized it over the past 200 years." This list includes the comparative method, language relationships, (some version of) the Neogrammarian hypothesis of the regularity of sound change, and what Joseph refers to as "philology and the data we work with." Section 5 discusses the connections between sociolinguistics and historical linguistics by way of conclusion, arguing that "a lasting legacy" of WLH (1968) is the acknowledgement that neither can language change be studied independently of language, nor can language be studied independently of language change, thus demonstrating that historical linguistics must remain an integral part of linguistics.

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