In: Hans Boas and Marc Pierce, eds., New Directions in Historical Linguistics. Leiden: Brill, pp. 153-173.

CHAPTER 7

Historical Linguistics in the 50 Years Since Weinreich, Labov, and Herzog (1968)

Brian D. Joseph

1 Introduction

Some fifty years ago, in 1968, a volume was published with the papers from a 1966 symposium held at the University of Texas at Austin that had as its focus the theme of "Directions for Historical Linguistics" Symposium". Five papers were contained in the 1968 volume:

(1) Saussure's Dichotomy between Descriptive and Historical Linguistics, by W. P. Lehmann
(2) The Inflectional Paradigm as an Occasional Determinant of Sound Change, by Yakov Malkiel
(3) The Notion of Morpho(pho)neme, by J. Kuryłowicz
(4) Mutations of Linguistic Categories, by Émile Benveniste
(5) Empirical Foundations for a Theory of Language Change, by Uriel Weinreich, William Labov, and Marvin I. Herzog

These were all fine papers that each contributed to our field in its own way, but really only the final one, by Weinreich, Labov, and Herzog (hereafter WLH), can be said to have had a significant lasting effect (but see Thomason, this volume, for a slightly different view). One tangible, though admittedly rough, measure of impact is the number of citations in Google Scholar (as of 21 April 2019):

Saussure's Dichotomy …, by Lehmann	[19 citations in Google Scholar]
The Notion …, by Kuryłowicz	[36 citations in Google Scholar]
The Inflectional Paradigm …, by Malkiel	[50 citations in Google Scholar]
Mutations … by Benveniste	[220 citations in Google Scholar]
Empirical Foundations … by WLH	[2885 citations in Google Scholar]

As a contribution to this collection celebrating the golden anniversary of the publication of the volume containing WLH, I have been asked to address the following two questions:

 | DOI:10.1163/9789004414075_008

(1) How has the field of historical linguistics changed in the past 50 years?
(2) How have the ideas presented at the original "Directions for Historical Linguistics" Symposium hosted at UT Austin in 1966 influenced historical linguistics and sociolinguistics?

However, given the Google Scholar citation numbers, which accord with my own sensibilities as to the relative long-term impact of these papers, in answering these questions, I intend to focus here only on WLH.

In many ways, the publication of WLH was a total game-changer. The title in itself makes it clear that the focus of the article was to contribute to the study of language change. Labov (2017: 257; also in his contribution to this volume) describes what went into the development of the paper, and notes that Weinreich wanted to take part in the 1966 conference because, as he himself said, he felt he had (in his own words) "a distinctive approach to questions of linguistic change." Moreover, as the authors were settling on a title, Weinreich made the important change in the title from "for the theory of language change" to "… a theory …", signaling the interest he had in making a point about developing a general framework for studying linguistic change, not just tweaking, as it were, some existing framework.

So the title alone is telling, and makes clear where the focus of WLH lies, namely in the realm of the investigation of language change. Although a variationist approach to explaining language change was employed earlier, especially in Labov's own (1963) work on Martha's Vineyard vowels, WLH enunciated such a clear set of foundational principles that it set the tone for the next five decades of research into the why and how of language change. Indeed, to again turn to the matter of the origin of the title of WLH, Labov (2017: 258) observes that when the authors were trying to settle on the title, Weinreich said, in a letter to Labov, that "it might be advisable to get "empirical" into the title, because our interest in living evidence is perhaps as distinctive as anything else in our work." Thus, the methodological innovation of investigating linguistic change through the careful study of synchronic variation was reinforced in a powerful way by WLH (see also Labov, this volume, for further discussion).

By way of offering a perspective on how the field of historical linguistics has developed since 1968 and in particular how it has changed and how the ideas of WLH have been influential, I survey here several areas of current inquiry within historical linguistics to see how WLH has had an impact—or not—on their development.

It is suggested here that WLH is the sort of seminal work that comes along a few times at most in a century, a work that altered the course of the development of the field in important and lasting ways. In terms of its effect, WLH,

while substantial in length, being 100 pages in all, was more like a book than an article and actually compares favorably with various book-length works in our field that have defined an area of study for a generation and beyond of scholars, and have thereby created a paradigm for scientific investigation. In that respect, it can be compared, in my opinion, with such classics as Saussure (1916), with its view of language as a system and the contrast between synchrony and diachrony that it introduced and highlighted; Sapir (1921), with the way it presented linguistic typology and emphasized the importance of indigenous languages; Bloomfield (1933), with its setting forth of principles and methods of analysis leading to American Structuralism; and more recently, Chomsky (1957), with the generative program for linguistics that it set in motion; Chomsky and Halle (1968), with its establishing of methodology and principles for generative phonology for years to come; and even Smolensky and Prince (1993), for much the same reason as the preceding item, namely defining a paradigm for (mostly) phonological theory for over two decades. Among article-length works, there may well be competitors, so to speak, in terms of lasting effect; two that come to mind here in this regard are Chomsky (1970), with its development of a lexicalist approach to nominalizations, and Chomsky (1993), with its outlining of the principles that have fueled Minimalist syntactic study.[2]

Theoretical frameworks admittedly do seem to come and go, but solid methodological innovations can endure, and that may be a basis for singling out WLH within any discussion of seminal works. But it is in any case more important to acknowledge lasting value than to argue about which work has had greater impact, so that it is enough to place WLH among those works with a significant and enduring impact on our field.

2 WLH—Some Background and an Assessment

Turning now to the specific questions I was asked to address, let me start my answer to the first question with an anecdote. My mentor Calvert Watkins was fond of joking about talking about the reconstruction of Proto-Indo-European

1 ~~This work should be considered a book rather than an article, given that it is a "stand-alone" publication.~~

2 I am tempted to cite Kiparsky (1968) as well, but refrain as it was superseded in a short time by Kiparsky (1971), which did not have the same lasting effect as the works discussed here; for instance, it took aim at the issue of abstractness but that concern was only relatively short-lived as a key theoretical issue.

(PIE) in the first half of the 20th century, that no language changed more in 50 years than PIE, and one could point to the discovery of two new branches of the family in the early 20th century (Tocharian and Anatolian) and the confirmation of de Saussure's laryngeal theory[3] that Hittite afforded as key elements that led to reformulation of—that is to say, change in—PIE in those 50 years.[4]

So what might we say about the field of historical linguistics in the past 50 years? I would venture to say that the same could be said—not, however, in a joking manner—about our field; however, just as language change itself must be understood against a backdrop of stability in language—not everything changes at once, after all—so too is there some stability in our field in this period, as discussed in §4 below.

I see seven areas of inquiry within historical linguistics that show significant change over the past 50 years. I must point out that this is very much a personal take on the field; others might add or subtract to this list,[5] but I stand by it as one practitioner's view of things:

– quantitative sociolinguistics
– mathematical modeling
– large-scale corpus work ("big data")
– instrumental studies
– experimental studies
– language contact studies
– phylogenetic modeling

While it is true that of the first six, all but quantitative sociolinguistics had considerable history prior to WLH—for instance, the work of George Kingsley Zipf involved serious mathematical modeling—all can be shown to have been significantly affected by WLH. The seventh is distinct from the others in being a relatively recently emerging area that WLH could in no way have foreseen, but, it is argued here, it is one that is consistent with the rigor that WLH demanded of researchers into language change.

3 Laryngeal theory refers to the internally arrived at reconstruction by Saussure (1879) for consonantal elements in PIE that were largely reflected in their effects on adjacent vowels; as Kuryłowicz (1927) demonstrated, Saussure's reconstructions were confirmed by Hittite once it was shown to be an Indo-European language. Brief accounts of laryngeal theory can be found in textbooks such as Lehmann (1993: passim), Szemerényi (1996: passim), Fortson (2010: 56–58), Hock (1991: passim), and Mallory and Adams (2006: 48–50); more detailed presentations are to be found in Bammesberger (1988) and Lindemann (1987), and technical but very interesting treatments in the various branches of Indo-European are in Winter (1965).

4 I do not know if this quip was original to Watkins or something he picked up from someone else, and colleagues of his that I asked about this were not sure either; nonetheless, he is *my* source for it and thus I quote him here.

5 To judge, for instance, from the reviews I received of this paper!

As evidence of that rigor, consider that WLH enunciated five key "problems" (and attendant questions) about language change (§3.4, pp. 183–187 (with some discussion of some of these issues earlier in the article, pp. 170ff.)):

a. the constraints problem: What are the general constraints on change, if any, that determine possible and impossible changes and directions of change?
b. the transition problem: By what route does language change?
c. the embedding problem: How is a given language change embedded in the surrounding system of linguistic and social relations?
d. the evaluation problem: How do members of a speech community evaluate a given change, and what is the effect of this evaluation on the change?
e. the actuation problem: Why did a given linguistic change occur at the particular time and place that it did?

And, at the end of the paper, following on their consideration of these problems, they essentially lay out an agenda for the study of language change (§3.5, pp. 187–188):

– Linguistic change is not to be identified with random drift proceeding from inherent variation in speech. Linguistic change begins when the generalization of a particular alternation in a given subgroup of the speech community assumes direction and takes on the character of orderly differentiation.
– The association between structure and homogeneity is an illusion. Linguistic structure includes the orderly differentiation of speakers and styles through rules which govern variation in the speech community; native command of the language includes the control of such heterogeneous structures.
– Not all variability and heterogeneity in language structure involves change; but all change involves variability and heterogeneity.
– The generalization of linguistic change throughout linguistic structure is neither uniform nor instantaneous; it involves the covariation of associated changes over substantial periods of time, and is reflected in the diffusion of isoglosses over areas of geographical space.
– The grammars in which linguistic change occurs are grammars of the speech community. Because the variable structures contained in language are determined by social functions, idiolects do not provide the basis for self-contained or internally consistent grammars.
– Linguistic change is transmitted within the community as a whole; it is not confined to discrete steps within the family. Whatever discontinuities are found in linguistic change are the products of specific discontinuities within the community, rather than inevitable products of the generational gap between parent and child.

– Linguistic and social factors are closely interrelated in the development of language change. Explanations which are confined to one or the other aspect, no matter how well constructed, will fail to account for the rich body of regularities that can be observed in empirical studies of language behavior.

These agenda items, taken together, boil down to:

– the study of all aspects of language cannot be divorced from orderly variation that is the hallmark of language
– the study of change in language must be viewed in the context of the embedding of language in a social setting.

These are the important lessons of WLH for historical linguistics, and having reviewed them, I turn now to a brief elaboration of each of the subareas within historical linguistics mentioned above, along with some specifics about how they relate to WLH and/or where they were pre-WLH and where they have gone since WLH; in some instances, it is not so much WLH itself but the way that the work that it engendered and fostered has grown and moved in a particular direction. In that way, to some extent the influence has been indirect.

2.1 *Quantitative Sociolinguistics*

There was quantitative work in linguistics for many years prior to WLH, as seen for instance in the statistically based "dynamic philology" work of George Kingsley Zipf (e.g. Zipf 1935). But the giant step forward made by WLH—and, as an even better exemplar, Labov's work prior to that, e.g. on Martha's Vineyard—was to recognize that there was a quantitative side to variability that can be measured and charted and used as the basis for analysis, thus marrying quantitative studies with sociolinguistics, a hallmark of the Labovian (= WLH-ian) school ever since.

Post-WLH, there has been an explosion of work in quantitative sociolinguistics, as measured by such benchmarks as an enormously successful annual conference devoted to this line of research, NWAV, celebrating its 48th instantiation in October 2019, at the University of Oregon in Eugene; a similarly successful journal, *Language Variation and Change* (LVC) is dedicated to the WLH-ian model and its avatars, is in its 30th year as of 2019; courses specifically in quantitative sociolinguistics (as opposed to general sociolinguistics or sociology of language) are found now at many universities, including my own; and subfields within the WLH-ian paradigm even have emerged, specifically sociophonetics (as opposed the as-yet unlabeled, but no less real, study of the initiation, spread, and generalization of changes in other domains of grammar). The excellent research done by students of Labov's, e.g. Penelope Eckert, Gregory Guy, Shana Poplack, and John Rickford, and by students of theirs, e.g. Sali Tagliamonte (a student of Poplack's) and my colleague

Kathryn Campbell-Kibler (a student of Eckert's), and even by students of theirs, to go into another scholarly generation, e.g. Alexandra D'Arcy (a student of Tagliamonte's) and Katie Carmichael and Abby Walker (both students of Campbell-Kibler's), to name just a few in each case, deserves particular mention as carrying forward the WLH-ian imperative in impressive, though admittedly sometimes elaborated or altered, ways.

There is more to the field of sociolinguistics, to be sure, including research on language contact (see Thomason and Kaufman 1988, Winford 2003, Matras 2009, and see §2.6 below), language attitudes (see Preston 2004), language planning (see Kennedy 1982), conversational analysis (see Sidnell 2016), and numerous other areas where language and language use intersect with human interaction and human involvement. However, quantitative sociolinguistics counts as its own subfield, to be sure, and the indicators listed, along with other pieces of evidence that could be cited here (e.g. awards and grants won by those working in this area), point to a healthy and thriving field of inquiry.

2.2 *Mathematical Modeling*

WLH brought into the arena of linguistic investigation the mathematical modeling of sociolinguistic variation. It is perhaps something of a stretch to include it here, as among the areas where WLH had an effect. However, it may not be such a large stretch as it is clear that mathematical modeling of language in general and of language variation in particular has taken off since the application of mathematical testing in WLH. For instance, there was modeling work prior to WLH—Zipf's work can again be cited in this regard—but there is now work on frequency effects in change, e.g. by Bybee (2002) (see also the papers in Bybee and Hopper 2001), and the population-modeling approach of Baxter and Croft (2016) (on which see also §3).

2.3 *Large-scale Corpus Work* (*"Big Data"*)

By drawing on data from a corpus such as the Yiddish Atlas (Herzog et al. 1992ff.; see also https://library.columbia.edu/locations/global/jewishstudies/lcaaj.html) for some of the empirical side of the presentation in WLH, the authors were continuing a long tradition of work based on dialect atlases. But it was in a sense a precursor—before anyone could really know that large, readily searchable storehouses of data would become available—of work that was to come in the 1990s and 2000s up to the present day, as large databases became available with massive storage capabilities that give access to a wealth of information about the usage of individuals and of groups, possibly—if the data is rich enough—defined along different social parameters (age, gender, socioeconomic class, etc.), in different geographical locations, and at different

historical stages. In this regard, the work done by members of the Historical Sociolinguistics Network (HiSoN, https://hison.sbg.ac.at/) and North American Research Network in Historical Sociolinguistics (NARNiHS) deserves mention, with its emphasis on corpus-driven micro-social history of language use and language users.

In fact, it is hard to imagine doing serious work on variation and change these days without access to the sort of "big data" that corpora offer, and fortunately, there are many corpora that can be used for research of this sort, so many that it is difficult to list them all or to do them all justice. However, English corpora such as the Corpus of Contemporary American English (COCA, https://corpus.byu.edu/coca/), the Corpus of Historical American English (COHA, https://corpus.byu.edu/coha/), the Helsinki Corpus of English Texts (www.helsinki.fi/varieng/CoRD/corpora/HelsinkiCorpus/) can be noted, and there are numerous national corpora as well, e.g. the British National Corpus (for British English, www.natcorp.ox.ac.uk/).[6] Moreover, there are corpora that include, instead of the approximations of pronunciation that written records afford, real samples of speech, e.g. as digital recordings, that are eminently usable for historical sociophonetic research on actual pronunciation; the Buckeye Speech Corpus housed at The Ohio State University (https://buckeyecorpus.osu.edu/) and the Switchboard corpus (https://catalog.ldc.upenn.edu/ldc97s62) are two examples of such corpora.

Many of these corpora are specifically geared towards historical work by offering data from different historical periods, but even narrow synchronic "slices", such as those in the Buckeye Speech Corpus and the Switchboard corpus or COHA, can afford a glimpse at trends over short stretches of time indicative of temporally very localized spread of innovations. We may never be able to witness the exact point of origination of an innovation, but being able to chart how an innovation fares within a narrow window of time and space can be revealing; Russ (2013), for instance, using data from Twitter, pinned down the locus of diffusion for the innovative intensifier *hella* to northern California, and thus confirming with hard data what was believed impressionistically to be the case.

In any case, all of this "big data" allows for research into linguistic variability and change that goes beyond the traditionally examined geographic basis for variation, i.e. traditional dialectology, and in that way goes beyond anything envisioned in WLH. But the highly empirical basis of such research is entirely within the spirit of WLH.

6 See Traugott (this volume) for further discussion and mention of other relevant corpora.

2.4 *Instrumental Studies*

There was instrumentation and thus instrumental studies before WLH and there is plenty of instrumental work now that is not directly connected to WLH, but in the aftermath of WLH, instrumentation and instrumental studies have been deeply embedded in socio-historical linguistic work, as even the most cursory glance at an NWAV program or a table of contents for LVC shows. Moreover, sociophonetics, combining several areas of investigation discussed here but preeminently involving instrumental analysis, is a recognized specialization within graduate research programs and typically now is a panel theme in general meetings of the field such as the annual meeting of the Linguistic Society of America.

2.5 *Experimental Studies*

What is said in §2.4 regarding instrumental studies can also be said for experimentally based studies in the aftermath of WLH. If experimentation is defined a way of acquiring data about language in controlled conditions,[7] then WLH was a precursor to this approach as well, even if it did not do experimentation in other senses. That is, by emphasizing the empirical imperative regarding the study of language, WLH opened the doors for bringing other kinds of controlled data collection into the realm of the study of language change.

2.6 *Language Contact Studies*

WLH made it clear that Herman Paul's notions about language and language change led to viewing a speech community as a collection of individual grammars, of idiolects, and by arguing against that view, WLH essentially said that one has to factor the interaction among speakers into any analysis of variation and change. Moreover, their "evaluation problem" (see (d) above in §2) means that someone has to do the evaluating and someone has to produce a word or an utterance or a piece thereof that can be evaluated, and this means that change is not just something in an isolated individual but involves at least two people. It is inherently social in nature, as a result, and requires contact between speakers.[8]

2.7 *Phylogenetic Modeling*

The one sub-area of historical linguistics and the study and modeling of change, or the effects of change, that is divorced and unrelated to the agenda and goals of WLH is the modeling of language evolution via computational phylogenetic

7 I owe this formulation to my colleague, Professor Emerita Mary Beckman.

8 See Thomason (this volume) for more on language contact studies in the aftermath of WLH.

methods, largely borrowed from biology. Research of this sort has to a great extent focused on the question of genealogy, that is relationships and subrelationships holding among sets of languages, and the proper modeling thereof, e.g. in terms of family trees, as in such studies as Dyen et al. (1992), Ringe (1992), Ringe et al. (2002), Nakhleh et al. (2005), Gray and Atkinson (2003), Bowern and Atkinson (2012), Chang et al. (2015), among others.[9] However, the methodology is not limited in its application to just such questions, as shown by Bowern et al. (2011) with its investigation of lateral transmission (i.e., borrowing) in hunter-gatherer societies. To some extent, this mode of research is an outgrowth of mathematical modeling and the need for computational means of dealing with "big data" of a historical nature such as that which arises when a large number of features is crossed with a large number of languages and the resultant data matrices are of considerable complexity (though see Czekanowski (1928) and Kroeber and Chrétien (1937) for pre-computer-era statistical investigation of relatedness issues in much the same vein).

3 An Area of Lesser Impact for WLH

For all the impact that WLH has had, either directly as with the birth of variationist sociolinguistics or indirectly as with dealing with "big data", there is one very important idea in WLH that has not taken hold as strongly as it might have. This has to do with the transmission of language and its relevance for language change. WLH (p. 188) states that:

> Linguistic change is transmitted within the community as a whole; it is not confined to discrete steps within the family. Whatever discontinuities are found in linguistic change are the products of specific discontinuities within the community, rather than inevitable products of the generational gap between parent and child.

This notion runs counter to the claim made in early generative models of language change, e.g. that enunciated in Halle (1962) (though, for later instantiations of this view, see also Lightfoot 1997, 1999; Hale 2007), that, as Labov 2007: 346n.4) puts it, "linguistic change is the result of children's imperfect learning … that late additions to adults' grammars are reorganized by children as a

9 Forster and Renfrew (2006), with its 16 chapters covering different language families and different aspects of this general methodological approach is an excellent resource; Greenhill et al. (~~2019~~) is an enlightening overview as well.

simpler model, which does not exactly match the parents' original grammar." Crucially, however, Labov (2007: 346n.4), while recognizing this generative viewpoint, dismisses this stance, noting that "Although Lightfoot (1997, 1999) argues for this model as a means of explaining completed changes, such a process has not yet been directly observed in the study of changes in progress." That is, the generative take on the role of child language acquisition in language change has no empirical basis, according to Labov.

Nonetheless, this assumption about the role of children is ingrained in many linguists' consciousness and practice, even in the absence of empirical confirmation, as I experienced with regard to an innovative gerundial/participial form of *have to* that occurs for at least central Ohio speakers. That is, instead of *ha*[v]*ing to, ha*[f]*ing to* can be heard, as in *There I was, at the checkout counter, ha*[f]*ing to pay for my groceries but without a penny or a credit card in my pocket!* My first encounter with this form was in the early 1980s, when I heard it from an adult (and then from other adults, and eventually from children). I assumed it was an analogical form based on the [f] in the surface form of *have to* ([hæftu]), and a reanalysis of [hæf] as the root of the verb. With that in mind, I wrote about it in Joseph (1992). Having heard it mainly from adults, I also assumed that this was an innovation by those adult speakers; there was no evidence to the contrary, though admittedly no positive indication either. However, when I mentioned this form to colleagues, many said that these speakers must have carried out the analogy/reanalysis as children and that the form was never "corrected", as it were; they assumed this form arose in the language learning process that speakers engaged in.

Conceptually, however, I see no reason to restrict such innovations to children, even if we can observe them in child language. That is, adults—and, importantly, *monolingual* (though also multilingual) adults—are subject to the same pressures as children caused by a network of related linguistic forms (leading to "analogical change"); and, because they know more words, adults have the potential for greater analogical pressure on particular forms, and with adults, there are memory issues to reckon with too, as the retrieval of infrequent and/or irregular forms may simply be harder for adults, allowing analogical formations to slip into their usage. Also, adults' production can be affected ~~also~~ by social pressures associated with using particular forms; moreover, they have greater awareness of other dialects and, typically, more exposure to a wider range of styles and varieties than language-learning children.

Thus, it should come as no surprise that adults can innovate linguistically, as WLH made clear. Moreover, studies that have been carried out of change over the lifetime of individuals, most notably Sankoff and Blondeau (2007), though Baxter and Croft (2016) is relevant here as well, show that adults can indeed

innovate. It is probably time, therefore, in keeping with the approach in WLH, to put to rest once and for all the empirically uncorroborated view of the role of child language acquisition in language change.

4 Stability

As a counterbalance to all of this discussion of change in the field, it is important to mention some aspects of historical linguistics that show stability over the past 50 years. In this regard, I have to confess I am somewhat of a traditionalist but I say this based on the fact that many of the findings, methods, and analytic tools developed within the past 200 years are as valid today as they were in the 20th century and, I daresay, in the 19th century; this may make linguistics somewhat unusual among 21st-century sciences, but so be it. The particular stable features that I have in mind that characterize historical linguistic investigation today, and have characterized it over the past 200 years, are both methodological in nature and foundational on a conceptual and practical level:

– the importance of the Comparative Method: Calvert Watkins (1995: 4) has called the Comparative Method "one of the most powerful theories of human language put forth so far and the theory that has stood the test of time the longest". When it was first developed by Franz Bopp in (1816), and later refined throughout the 19th century, it provided the best means and the best measure for both the determination of language relationships and the development of a clear picture of the state of a proto-language for a set of related languages. That is, it served not only the goal of working out language relationships but also the goal of reconstructing prior linguistic states. And, in allowing for reconstruction, it fed—and was fed by—knowledge gained about the nature of language change. Some scholars have expressed some mild misgivings as to the efficacy of this method; Clackson (2007: 20), for instance, states that "The (partly) successful operation of the comparative method over a non-restricted field of (open-class) vocabulary does not furnish proof that two languages are genetically related, rather the comparative method is used to reconstruct the parent language of any two languages which are already hypothesised to be related." However, in my view, the Comparative Method has not been surpassed,[10] especially for proto-language reconstruction but also for confirming hypotheses of genea-

10 See also Joseph (2016) for an extolling of the virtues of the Comparative Method, an article in a special journal issue celebrating the bicentennial of Bopp's work.

logical relatedness, even if other, though not necessarily better, methods for investigating relationships have emerged (see §2.7 above).

– Interest in determining language relationships: this interest entails both investigations at a macro-level involving basic genetic/genealogical classification and those at a micro-level involving subgrouping within a set of related languages. It must be admitted that WLH is not completely irrelevant here as there are sociolinguistic issues to consider in connection with investigations of relatedness among languages, such as how a particular speech community for the examination of relatedness is to be constituted. Also, as an outgrowth of WLH influence and the discussion in WLH of how language is necessarily embedded in a social setting, one might legitimately ask if different sociolinguistic strata in a speech community could show different sorts of genealogical relationships.
– Affirmation of the Neogrammarian view of sound change: the Neogrammarians posited the regularity of sound change and the availability of only phonetic conditioning to guide how sound change emerges in a language and despite the development of a school of thought regarding lexical diffusion (see, e.g., Phillips 2006, [illegible]), a direct challenge to Neogrammarian regularity, the validity of the view that sound change is regular, affecting all candidates for a given change that meet the conditioning environment for the change, has been reaffirmed (cf. Labov 1981, for instance). I cannot resist putting in a plug here for a terminological nuance that I favor, namely talking about Neogrammarian-style regular sound change as "sound change proper" or "sound change in the strict sense", or even "Neogrammarian sound change", to distinguish it from changes in pronunciation that have a non-phonetic origin, such as those arising by a sociolinguistically driven "adaptive" rule like the t ==> p change seen in certain words in "tetak" dialects of Czech as discussed by Andersen (1973),[11] or such as any changes in the sound shape of a word or morpheme caused by analogical pressure from a related word or morpheme.
– Philology and the data we work with: the 19th century western European scholars who got the enterprise of historical linguistics rolling with their

11 The "tetak" dialects of Czech are those that with the loss of contrastive palatalization in Old Czech assigned original palatalized (sharpened) *p* ([pʲ]) to /t/, based on the acoustics of [pʲ], as opposed to the "petak" dialects that assigned it to /p/. Petak speakers were associated with economic and political power while the tetak speakers were generally residents of more rural areas, and they learned to "adapt" their usage when interacting with petak speakers, adjusting their dialectally native /t/ to /p/ in those words from original /pʲ/ (e.g. *tivo* / *pivo* 'beer') to avoid stigma. Such "adaptive" processes are quintessentially sociolinguistically driven.

> study of the history of particular Indo-European languages and the development of the whole language family were Classical philologists, scholars of Greek and Latin, the western European classical languages. With the recognition that Sanskrit and Old Persian were relevant for understanding how Greek and Latin (and other languages of Europe) developed, they moved to being Comparative Philologists but their text-based empirical foundation carried over into their study of these newly discovered languages. Good historical linguistics, like good linguistics and really any science, depends on good data, and philology, with its careful study and analysis of written texts, was and still remains a primary source of data upon which historical inferences are based. While our notion of "text" may have been expanded beyond 19th century conceptions to include oral recordings and more recently materials deriving from social media (Twitter, and the like), the basic empirical underpinnings of the field remain the same. We know that texts cannot be taken at face value but must be subjected to philological analysis to ensure quality and accuracy of data. We can ask more sociolinguistically informed questions now, post-WLH, such as who was writing the texts and whose language is represented in them, so in that sense, WLH did have an effect here. Finally, lest the term "comparative philology" seem antiquated, it can be noted that it was the name of the department at Harvard into at least the 1950s, and that it is still the name of a master's degree program at University of Oxford: the "MSt in General Linguistics and Comparative Philology".[12]

These are all aspects of the study of historical linguistics that were an important part of the field in the pre-WLH days, in some instances dating back into the 19th century, and which remain part of the field even to this day. Moreover, post-WLH, they could remain part of the field more or less intact and unchanged, as they represent aspects of historical linguistic research that were not really affected by, nor really targeted by, WLH, despite its critique of the view of the speech community offered by Herman Paul, a consummate Neogrammarian.

5 Concluding Remarks

WLH invented neither the field of historical linguistics nor the field of sociolinguistics, but it—and they—did play a major role in making the latter

12 See https://www.ox.ac.uk/admissions/graduate/courses/mst-general-linguistics-and-comparative-philology?wssl=1).

an integral part of the overall field of linguistics and in intertwining the two fields. The result is that now it is generally recognized that the study of sociolinguistics entails some attention to a consideration of language change and the study of language change necessarily entails a consideration of the role of synchronic variation and of the social factors governing its distribution.

Regarding the importance of sociolinguistics per se and how its role in the field at large has changed, I can point personally to the fact that when I was in graduate school, 1973–1978, in a fine graduate program (Harvard University), there was no course in sociolinguistics available to us, whereas now it would be unthinkable for such to be the case at a major top-flight program. Moreover, the same holds, of course, with regard to undergraduate curricula in linguistics around the country, which, in contrast to the situation 50 years ago, now typically include several sociolinguistically oriented classes; in fact, in the mid-1990s, as we at The Ohio State University were building up our undergraduate offerings, we focused on developing courses in sociolinguistics (Language and Social Identity, Language and Gender, Language, Race, and Ethnicity in the US, *vel sim.*), reasoning that students "live" and experience sociolinguistics on a daily basis in their interactions with others, in ways that they do not "live" and experience syntax or phonology.

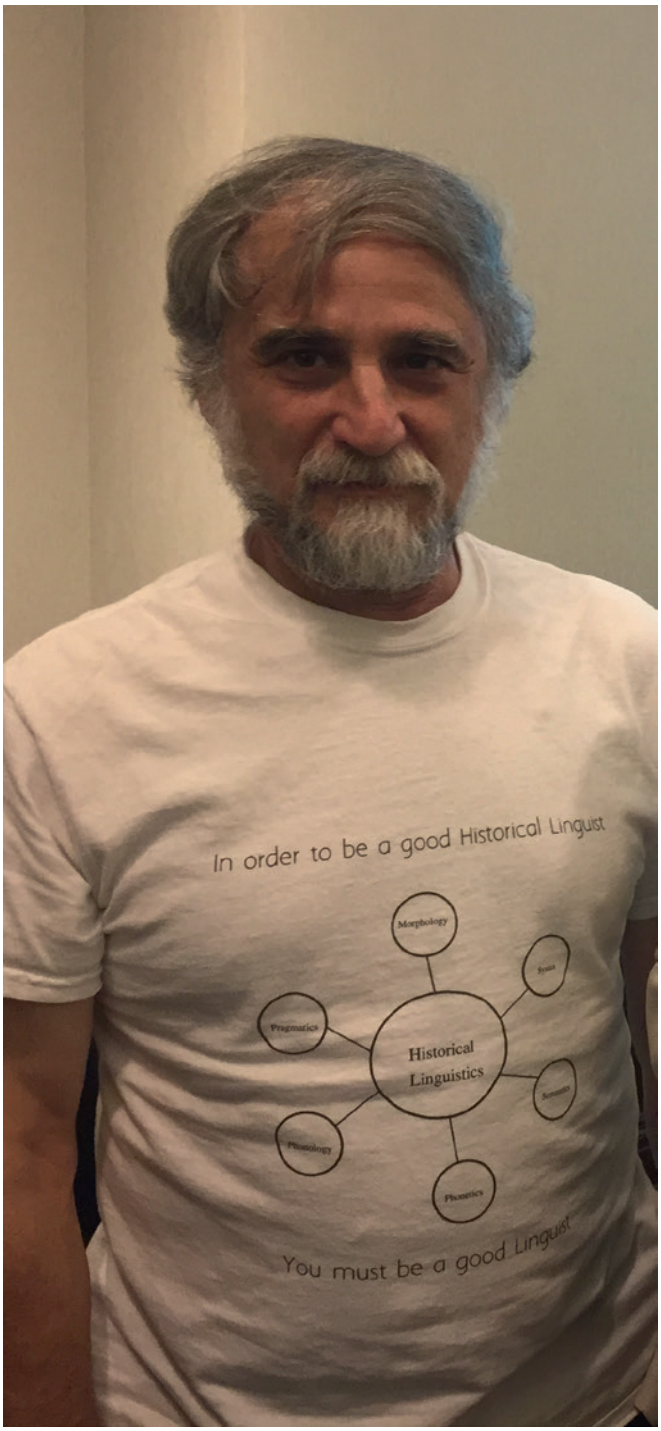
As for the point that the study of language change needs to incorporate insights from (Labovian quantitative) sociolinguistics, I offer a few thoughts of my own that show how a sociolinguistic sensitivity might be injected into Indo-European linguistics, an area of historical linguistic scholarship that has not generally been amenable to serious sociolinguistic investigation. For instance, we have a reasonably good idea about stratification in Proto-Indo-European society, based on lexical analysis that shows words for rulers and underlings, for gods as opposed to humans, and for the organization of the kinship system (see Benveniste 1969), but there is very little sense of what the linguistic correlates to that stratification might have been (see Shields 1980 on this), other than evidence of a distinct Indo-European poetic style (on which, see Watkins 1995). But the existence of two reconstructible lexemes for 'father', both *pə$_2$ter- (as in Sanskrit *pitar-*, Latin *pater*, Greek *patér-*, and English *father*) and *at- (as in Hittite *attas* and Old Church Slavonic *отьсь*), taken together with the fact that there is a Greek form answering to *at-, namely *átta*, which is a term of address used for elders, admits of an interpretation in which one is the ordinary word for 'father' (*pə$_2$ter-) and the other (*at-) is a nursery word, elevated into ordinary usage in some traditions; that is, we perhaps have here a glimpse into a socially determined stylistic distinction in PIE. Similarly, based on references in various Indo-European traditions, as

seen specifically in Sanskrit, Greek, Hittite, Old Norse, and Old Irish, Watkins (1970) reconstructs a PIE distinction between language of gods and language of men, a distinction which Colvin (1999: 44) labels as "an example of recognized linguistic diversity", e.g. for the Greeks; since gods and men were distinct social groups, of a sort, we have again a glimpse of a social distinction with a linguistic realization. Also, although a word for 'pig', based on Greek *hūs*, Avestan *hū-* Albanian *thi*, and other forms, can be reconstructed as *sū-, with a long vowel that points to an earlier sequence of *u + H (a laryngeal consonant), in longer forms such as compounds, the word has a short vowel (cf. especially Greek *hu-phorbós* 'swineherd'); this pairing of *suH- in the free form but *su- in at least some compounds, invites an interpretation by which there was a fast-speech rule in PIE that dropped laryngeals (alternatively, shortened long vowels), thereby offering a bit of insight into a possible phonological correlate of a certain speech style. Finally, in Joseph 2011, I suggest that if Germanic *bringan (English *bring*) reflects an admittedly unusual PIE blend of two roots for 'bear, carry' (*bher- and *Hnek-) that were suppletive in at least Greek, Albanian, and Celtic, perhaps the rarity of this type of compounding by root-blending gives a basis for thinking of this as a PIE expressive word-formation, perhaps with the same sort of stylistic "edginess" that celebrity-couple blends like *Brangelina* (= Brad Pitt + Angelina Jolie) have in contemporary American English usage. All of these examples are not quantitative, of course, and are highly speculative, but they are reflective of ways in which thinking sociolinguistically might open up new avenues within even as traditional a scholarly enterprise as Indo-European studies; indeed, they can be taken to suggest possible stylistically and/or sociolinguistically conditioned variation within PIE.

The growth of sociolinguistics documented here and the utility of sociolinguistic insights discussed here come despite the fact that Labov himself has expressed concern at the use of the term "sociolinguistics". That is, Labov has said (1972: xiii): "I have resisted the term 'sociolinguistics' for many years, since it implies that there can be a successful linguistic theory or practice which is not social".[13]

One way of recasting Labov's concern is to recognize a lasting legacy of WLH, namely that the study of language cannot be divorced from the study

13 Thanks to Rich Janda and Dennis Preston for helping me find the exact source of this interesting and telling sentiment. As an aside, I have often wondered whether mathematical/algebraic linguistics, with its focus on the mathematical foundations of linguistic theory would qualify as an asocial "linguistic theory or practice". I leave this as an open question.



Brian Joseph sporting a historical linguistics T-shirt

of language change, and vice versa—the study of language change cannot be divorced from the detailed study of language itself. That is, as I tell my students every chance I get, to be a good historical linguist one has to be a good linguist, a statement which my former M.A. student (now a Ph.D. student at the University of Michigan), Yourdanis Sedarous,[14] memorialized on t -shirt which I proudly wear annually now at the end of my introduction to historical linguistics class:

And, by way of concluding, it is fair to say that WLH embody this sentiment perfectly, as all three were/are excellent historical linguists and, accordingly, excellent linguists!

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14 Yourdanis is the author of *Studies in nominal modification in Bohairic Coptic*, her (2016) Ohio State M.A. Thesis, co-supervised by Peter Culicover and me.

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