The Role of Indo-European Studies in the XXIst Century*

H. Craig Melchert

1. Introduction

In order to assess accurately the position of Indo-European studies in the broader field of historical linguistics in the twenty-first century and evaluate its potential for contributing further to our understanding of language change, it is necessary first to summarize briefly the development of Indo-European linguistics through the nineteenth and twentieth centuries. Such an overview is particularly needed because some aspects of Indo-European linguistics have been the subject of misapprehension and in some cases misrepresentation. Furthermore, the true goal of traditional historical-comparative linguistics has often been misstated, even in handbooks written by those who practice it. The sketch presented here does not, of course, remotely purport to represent a history of Indo-European linguistics.[1] The focus will be on a few broad methodological issues that bear upon the overarching theme of this volume.

---

* I am indebted to Joseph Eska, Mark Hale, Don Ringe, and Michael Weiss for very helpful comments and suggestions, but the usual disclaimer applies with particular force: the views expressed here are solely my own, and none of them would endorse all of what follows.

[1] The nineteenth-century portion of this history is ably covered by Pedersen 1931. The history of Indo-European studies in the twentieth century remains to be written.

1.1 Typological Concerns

Contrary to the assertion of Johanna Nichols in her preface to the English translation (1995: xi), the highly original and important handbook of Gamkrelidze and Ivanov (1984) emphatically was not “the first whole-scale Indo-European reconstruction in which typology has played a major role.” Nor, despite the implication of Jakobson in the accompanying foreword (1997: xx), was there anything methodologically innovative in that “typological comparison renders salutary aid to comparative-historical procedures.”

A central role for typology in the reconstruction of Proto-Indo-European began with Franz Bopp, whose own chief interest in the comparison of verbal inflection among Indo-European languages was not in their role in proving genetic relationship (the aspect for which he is chiefly remembered), but rather in how the PIE system of verbal inflection came about. In opposition to a theory of Friedrich Schlegel, Bopp (1816: 147-51) argued that the PIE verbal endings were the result of agglutinated personal pronouns.

Furthermore, while this hypothesis has not enjoyed favor in the standard handbooks, it by no means disappeared and was vigorously revived by Seebold (1971), who also gives a survey of the varying reception of the proposal in the field through the century and a half between Bopp’s formulation and his own. I also stress that Seebold (1971: 188-9) explicitly appeals to plentiful cross-linguistic typological evidence for languages in which there *is* a transparent etymological relationship between the grammatical markers of person in the verb and those of personal pronouns in the nominal system. Deriving the reconstructed personal endings of the PIE verb from the same bases as the personal pronouns faces formidable—in the view of most, insurmountable—obstacles. That, however, is not the relevant point here: Bopp's *basic idea* is in typological terms quite modern—what goes around comes around. And in grounding his renewed, far more elaborated version in typology, Seebold was merely following common practice among Indo-Europeanists of all eras.

Recognition among Indo-Europeanists of the need to constrain linguistic reconstruction in terms of typological plausibility has not been confined to instances of what we would now term "internal reconstruction" of pre-Proto-Indo-European, as exemplified by Bopp's and Seebold's hypotheses about the verbal endings. Scholars in the field have always felt obliged to cite typological support for their reconstruction of features of Proto-Indo-European itself. When he first made the novel proposal that PIE had at the phonetic level syllabic sonorants (liquids and nasals), Brugmann (1876: 303) made sure to cite the findings of Eduard Sievers for modern languages like German and English to show that such sounds were natural and commonplace, nothing exotic, much less implausible.

Further examples of this kind could be cited *ad infinitum*, but I will add here only one more, since it concerns the much vexed and over-emphasized question of the manner of articulation of the PIE stop series. Again contrary to a widespread misconception, debate about the typological plausibility of the dominant reconstruction of the PIE stop system did not begin with the famous observation of Jakobson (1958: 23):

> To my knowledge, no language adds to the pair /t/ – /d/ a voiced aspirate /dh/ without having its voiceless counterpart /th/, while /t/, /d/, and /th/ frequently occur without the comparatively rare /dh/, and such a stratification is easily explainable; therefore theories operating with the three phonemes /t/ – /d/ – /dh/ in Proto-Indo-European must reconsider the question of their phonemic essence.

Nor did alternative proposals to address this conundrum begin with the "glottalic theory" of Hopper (1973) and Gamkrelidze and Ivanov (1973 *et alibi*).

Although many twentieth-century handbooks did follow Brugmann (1886: 1.263) in reconstructing four series of stops for PIE (voiceless, voiceless aspirated, voiced, and voiced aspirated), Brugmann himself (1886: 1.406-8) conceded the extreme sparseness of the evidence for the voiceless aspirates, and more than a few major Indo-Europeanists rejected their reconstruction from the start. Eduard Prokosch (1918-19), citing the typological unnaturalness of a three-way opposition of voiceless, voiced, and voiced aspirated stops, suggested a reanalysis of the PIE voiced aspirates as voiceless fricatives. Holger Pedersen (1951: 14-16), basing his argument on the rarity of PIE **b* and the typological argument that it is *p* that typically is lacking in a stop series boldly proposed that the three PIE stop series **T, *D, *Dh* reflected a pre-PIE system **D, *T, *Th*. The failure of these alternatives to win wide acceptance was due to the same factor that has led most Indo-Europeanists to reject the newer glottalic theory: all such attempts eliminate the typological implausibility of the *synchronic* system posited for PIE at the cost of having to assume equally serious typological implausibilities in the *diachronic* developments to the attested descendant languages.[2] The continued acceptance by some of the reconstruction of four series, including the voiceless aspirates, through the twentieth century may also hardly be attributed merely to the "authority" of the Brugmannian reconstruction. The more important motivation has been precisely that positing the voiceless aspirates preserves the diachronic typological advantages of the traditional reconstruction while removing the synchronic typological problem posed by the absence of the voiceless aspirates. This motivation is made quite explicit by Elbourne (1998: 7 and 26-7) in his unconvincing attempt to revive the voiceless aspirates for PIE.

In sum, Indo-European linguistics has always acknowledged what is now termed the "uniformitarianism principle": we must assume that the human language faculty has not changed in nature in the time span reached by our reconstruction techniques, and therefore we must reconstruct protolanguages that conform to the typology of attested ones and posit prehistoric changes that conform to those observed to have occurred within the historical period of attested languages (cf. Ringe and Eska: 2013: 4 and 25). As shown by the quotation from Brugmann (1897: 1-2) cited by Labov (1994: 22), the fundamental importance of this principle, especially promoted by William Dwight Whitney, was explicitly recognized by the "Neo-Grammarian establishment." Brugmann emphasized the diachronic aspect in the quotation, but we have seen that he and others

[2] See on these problems among others Joseph and Wallace 1994. For a recent attempt to account for the existence but rarity of the problematic voiceless, voiced, voiced aspirated (breathy) system see Weiss 2009a.

have applied it to the synchronic reconstruction of PIE as well. As illustrated by the debate over the PIE stop series, the problem is not that Indo-Europeanists have failed until recently to apply typology to reconstruction and historical change, but rather that they have not yet found a way to resolve the tensions between the demands of synchronic and diachronic typology. We will return below to the question of how much they can contribute to the resolution of this conflict.

1.2 Explanatory versus Descriptive Approaches

The attempts by Bopp and Seebold cited above to derive the inflectional endings of the PIE verb from originally agglutinated personal pronouns not only depended on typological arguments, but also clearly aimed at *explaining* how the PIE system arrived at by comparative reconstruction had come about. In keeping with the primary goal of comparative linguistics (see 2.2 below), most Indo-Europeanists have contented themselves with trying to account for, in as much detail as possible, the facts of the attested languages by positing PIE as their common source and tracing the developments from it to the individual descendant languages. They have regarded the prehistory of the reconstructed PIE as something beyond the reach of scientific method and hence not within their purview.

Nevertheless, there has never been an absence of Indo-Europeanists prepared to carry out internal reconstruction on virtually all aspects of PIE. See the references cited by Seebold (1971: 185-6) to competing accounts of the origin of the PIE verbal inflections, to which should be added Kuryłowicz (1964: 148-57). Kuryłowicz (1964:

179-206) and Haudry (1970) and (1982) present very different hypotheses regarding the prehistory of the PIE nominal case system. Attempts to explain in phonetic terms the origin of PIE morphologically conditioned "ablaut" (the functional vowel alternations still seen in English 'sing', 'sang', 'sung') are too numerous to mention. I do not mean to overstate the impact on the field of such explorations into pre-PIE. As noted by Seebold (1971: 186-7), they have for the most part either been dismissed as mere "glottogonic speculation" or passed over in respectful silence. I merely wish to stress that in this respect, as in so many others, the widespread perception of Indo-European studies as a monolithic and largely static field is a myth.

Indo-Europeanists have also not hesitated to venture explanations for the changes observed in languages during the historical period and for those posited to have occurred between PIE and their first attestation. Paul (1880: 56-7) sought the cause of "regular" phonetic change in the "principle of least effort" applied to speakers' articulation. However, as described in Melchert (1975), since the beginning of Indo-European linguistics, continuously through the period of the promulgation of the principle of "regularity of sound change," many scholars have argued that other factors play a role in sound change. Namely, some sound changes are attributed to the *frequency* of use of the words or expressions in which they occur, but by a popular competing hypothesis it is the weakening of *function* in such frequent items that causes the changes, and conversely some regular sound changes are impeded or blocked in morphemes that bear a heavy functional load. It is true that in most instances these factors are invoked to account for a limited range of examples—often those where regular sound change or morphological change do not seem applicable. However, a much larger role in sound change has been proposed for both frequency (Mańczak 1969) and functional effects (Martinet 1952, 1953, 1959). Once again, one should not overstate the role of such studies in the field. It has been and remains common practice to motivate specific historical analyses largely in terms of their ability to account for the observed facts and, when necessary, to buttress the plausibility of the changes assumed by citing typological parallels.

1.3 Disconnect

I have shown in the two preceding sections that neither the systematic application of typology nor explanatory approaches to aspects of language change are recent innovations in Indo-European linguistics. There are nevertheless good reasons for the widespread view—within the field as well as outside it—that Indo-European linguistics and the traditional approach to historical linguistics that it represents somehow became isolated during the twentieth century from the synchronic study of language, remaining to its detriment largely untouched by the dramatic advances in the latter. For this reason we have heard for some time and continue to hear calls for the need to “reintegrate” Indo-European studies and traditional historical-comparative linguistics more generally with current synchronic theoretical approaches (see e.g. Hale 2007: xi and the subtitle of Ringe and Eska 2013).

There is no doubt that there was in fact a significant “disconnect” between Indo-European studies and synchronic linguistics in the period from the 1920s to the 1960s. Remarkably, Prokosch (1918-19: 15.621) in his study of the PIE stops cited in 1.1

already used the concept of the phoneme, *avant la lettre*. By 1930 the phoneme was fully established, and Jakobson (1931) already set forth the principles of phonemic change, that is, the effects of sound change on the synchronic phonological system of a language, as distinct from phonetic change. Bloomfield (1933: 351) also made clear that sound change means changes in phonemes, not just phones, and describes reconstructed systems in terms of phonemes (1933: 302-5). One might have expected that this fundamentally new way of viewing sound systems and sound change would at once be widely adopted by Indo-Europeanists and used to recast the description of PIE and to reexamine sound changes from PIE to attested languages. In fact there was no such paradigm shift (see e.g. the traditional treatment of Krahe 1943 and subsequent editions). It was not until Lehmann 1952 that structuralist phonemics was systematically applied to PIE. It is also symptomatic that the influential version of Jakobson's article was the French translation of 1949, while the standard American exposition is that of Hoenigswald (1946)—all post World War II.

A number of factors likely contributed to the relative lack of influence of either European or American structuralism on Indo-European linguistics. It surely did not help that Saussure, who viewed language change as fortuitous and unsystematic, in at least some of the remarks attributed to him appears to deny any direct meaningful relationship between diachrony and synchrony (1983: 85): "Any notion of bringing together under the same discipline facts of such disparate nature would be mere fantasy. In the diachronic perspective one is dealing with phenomena which have no connexion with linguistic systems, even though the systems are affected by them." While insisting that phonetic change affects phonemes, the ten chapters in Bloomfield's *Language* devoted to language change are cast largely in traditional terms (as was his own historical-comparative study of Algonquian languages). There is no chapter describing types of phonemic change analogous to Jakobson 1931 or Hoenigswald 1946.

One should not discount the effects of how academic disciplines were organized in European and American universities. In Europe in the period in question those pursuing the new field of structuralist descriptive linguistics tended not to be in departments with Indo-Europeanists, who were typically associated with language departments, especially of the classical languages, or belonged to independent institutes specializing in Indo-European (see the remarks of Matthews 2001: 52). This situation also applied to a number of Indo-Europeanists in the United States: e.g., Hermann Collitz held a chair in Germanic, and so did George S. Lane until the 1960s. However, even in cases such as Yale University, where Edgar H. Sturtevant and Edward Sapir were colleagues from 1931-1939, with Sturtevant being joined in the 1940s by Leonard Bloomfield and Bernard Bloch, the coexistence of Indo-European and structuralist descriptive linguistics seems not to have led to much influence of the latter on the former.

Given that Indo-Europeanists played a leading role in the founding of the Linguistic Society of America in 1924 and in its governance for years thereafter, it is unsurprising that articles on Indo-European topics actually dominate the early issues of *Language* and remain prominent through the 1950s. However, with extremely rare exceptions (Moulton 1948 and Twaddell 1948—NB again post World War II) the analyses of Indo-European problems are entirely traditional in methodology and have no commonality with the structuralist descriptive articles that appear beside them. There obviously was mutual respect and recognition of both fields as legitimate branches of linguistics, but there is little visible evidence of meaningful dialogue.

Perhaps the impact of structuralism on Indo-European studies was limited by the fact that Indo-Europeanists were preoccupied in the period beginning roughly in 1930 with the daunting task of integrating into the reconstruction of PIE the new facts of Hittite and Tocharian, whose remains had only been discovered early in the twentieth century and whose textual material only became widely accessible in the late 1920s and the 1930s. This new evidence, especially that of the very anciently attested Hittite, raised serious questions about the validity of not only specific features, but also the overall received model of PIE. The process of meeting this challenge, further delayed by World War II, continued through the 1950s into the 1960s. Since the task was principally one of reapplying the comparative method with the inclusion of new data, to which the relevance of the structuralist model was minimal, the field as a whole inevitably turned its focus inward.

There was also some time lag in the application of the generative model in Indo-European linguistics, but it was relatively short. By the 1970s synchronic phonological rules were being posited for PIE and subsequent language stages, and diachronic changes formulated in terms of rule change (see e.g. Schindler 1976 with refs. and 1977). One version of those for PIE was systematically incorporated into the handbook of Mayrhofer 1986. There was predictably also a surge of interest in both synchronic syntax, that of PIE and its most ancient descendants, and diachronic syntax. One may cite as early examples the seminal article of Watkins 1963 and the monographs of Lehmann 1974 and Friedrich 1975.

However, the application of contemporary formal models in Indo-European linguistics over the past several decades has been very uneven. Their use to elucidate problems involving the syllable or accentual phenomena has only recently become commonplace (see further below 3.1). Analogy in its traditional form is still routinely used in accounting for morphological changes, despite awareness of its very serious flaws and limitations. It is true that no fully satisfactory replacement for analogy has yet been found (see below 3.2.2), but one might minimally expect more frequent acknowledgement that something better is needed.

As an active participant in the field for forty years, I can attest that the slow and sometimes fitful progress in this regard is due only in part to active resistance to such innovations by some (mostly more senior) colleagues. A significant contributing factor is the wide range of competing theoretical models and the relative rapidity with which they evolve. These features make it very challenging for any single individual to master fully the methods of Indo-European linguistics with the requisite philologies and also adequately control and stay current in phonological, syntactic, or discourse theory. There are, however, promising signs that some in the newest generation of Indo-Europeanists are prepared to meet this challenge.

The ability of contemporary and future Indo-Europeanists to contribute to our understanding of language change faces a further obstacle: the fundamental difference in the goal of traditional historical-comparative linguistics and that of modern, theoretically grounded study of the implementation and causes of language change. It is to this issue that we now turn.

2. Current Issues and Non-Issues

Recent efforts to “(re)integrate” historical-comparative linguistics into theoretically oriented contemporary linguistics, such as Hale 2007 and Ringe and Eska 2013, have helpfully resolved some issues and eliminated pseudo-problems, but they have not fully confronted the fact that not only some of the methodology of historical-comparative linguistics, but also its principal goal, differs from that of modern study of language change.

2.1 The Nature of Language Change and Its Impact on Comparative Reconstruction

There has been a long-running debate within historical-comparative linguistics about the “reality” of linguistic reconstructions achieved by the comparative method. The “idealist” view has professed to see reconstructions as merely formulas that express the structural relationships among related languages and help make explicit the changes that have taken place in each (representative is Pedersen 1931: 268-9). The “realist” view has regarded reconstructions as at least near approximations to the grammar of a once existent prehistoric language.[3] Since reconstructions are by definition hypotheses, framing the

[3] While this viewpoint has been widely and unjustly parodied with reference to August Schleicher’s reconstruction of a PIE “fable” (Schleicher 1868), it has in practice been the

problem in terms of their “reality” is misguided and unhelpful (cf. the remarks of Hale 2007: 244-52).

As recognized, but not resolved, by Tremblay (2005: 92-3), it is also self-contradictory to define the result of comparative reconstruction as an essentially ahistorical system of correspondences among historically attested languages (Meillet 1934: 47) and then take this reconstruction as the starting point for an historical account of the individual languages (Meillet 1934: 17). As argued by Hale (2007: 226-9 and 253), the way out of this dilemma is to recognize that “language change” is change in individual grammars (see also Ringe and Eska 2013: 32-44) and that a reconstructed protolanguage is, due to limitations in both our methods and data, only a partial grammar, or more accurately, a *set* of grammars that agree in all of their recoverable features, but potentially vary in unknown ways in the features that we cannot recover. Thus what is relevant is not the “reality” of our reconstructions, but how much of the prehistoric grammar we can recover. We must openly concede that there are many things we cannot and never will be able to explain, but in the favorable cases we can constrain the set of possible grammars to a scientifically interesting degree. That is, for Proto-Slavic or Proto-Romance, and to a lesser degree for Proto-Indo-European, we can recover enough features to show that only a relatively narrow range of prehistoric grammars could have

---

dominant approach in the field. Otherwise, there would never have been the vigorous and long-running debate about the typological naturalness of features of reconstructed PIE (those cited in 1.1 above and many others).

led to the respective sets of attested languages, excluding the possibility that most conceivable natural language grammars could have done so.

What in my view needs to be more explicitly acknowledged is that the same principle applies to the historical aspect of the historical-comparative model. We must likewise admit that the posited series of historical events leading from the protolanguage to the attested languages also stand for a _subset_ of the changes that would produce the observed results.[4] For example, Indo-Europeanists devote much discussion and debate to the matter of “relative chronologies”, the order in which certain posited historical changes occurred. In some cases the nature of the changes allows us to recover both them and the relative chronology with reasonable assurance: e.g., the simplification of _*ss_ after long vowel in Latin _uīsus_ ‘seen’ < _*uīssos_ must be later than the change of intervocalic single _*s_ > _r_, else the result would be _*uīrus_ (with Weiss 2009: 151).

However, it is well-known that in many instances where there is more than one possible way to formulate the changes, one formulation requires one relative chronology, and another formulation the other. We would in many such cases do well to spend less time and effort debating at length the merits of one account versus the other and to merely state the most viable alternatives, conceding the remaining indeterminacies. As

[4] It is for this reason that I am skeptical of Hale’s assurance (2007: 253) that the proper recognition that we can reconstruct only sets of possible grammars “changes very little…in the day-to-day working methodology of those who do linguistic reconstruction.”

per above, the crucial factor is our ability to characterize both the protolanguage and the changes that lead to the attested languages uniquely enough to eliminate *significantly different* scenarios.

2.2 The Goal of Historical-Comparative Linguistics

Any discussion of the "(re)integration" of historical-comparative linguistics and its potential contribution to the study of language change must be based on a recognition of the true goal of the former. Unfortunately, there is a popular misconception that this is to reconstruct protolanguages, and even some handbooks of Indo-European and historical linguistics perpetuate this error. For example, Ringe and Eska (2013: 230-1) state: "The ideal aim of comparative reconstruction is to recover the entire grammar and lexicon of a prehistoric language that has left multiple descendants."

This is a highly infelicitous formulation. The true goal of historical-comparative reconstruction is to explain how a set of attested languages came to be the way they are. The features of the attested languages are *the only facts* and the facts to be explained. Reconstructed protolanguages are merely part of the means of explaining the features of the attested languages, and the other part of the task, delineating the specific changes that have taken place between the hypothetical protolanguage and the attested languages, is arguably more important and occupies more of the time and effort of historical linguists. This is certainly true for a well-established family such as Indo-European.

Traditional historical-comparative linguistics is, given sufficient data and the requisite ingenuity and persistence of scholars, very good at achieving its main goal.[5] However, its primary focus has been and continues to be *descriptive*: to account for *how* (by what steps) a set of attested languages come to both resemble *and* differ from each other in terms of various grammatical features. As described above in section 1.2, individual Indo-Europeanists have not hesitated to offer hypotheses as to *why* certain kinds of changes have taken place (and more broadly, why some changes appear to be commonplace, while others are rare or non-existent), but this activity has always been ancillary to the main task of elucidating the history of specific attested languages and language groups. And the techniques employed to achieve the latter goal have no direct connection to theoretical models of language change.

Hale (2007: xii) concedes that contemporary linguistic theory and the methods of traditional historical-comparative linguistics appear to be unrelated. He suggests two possible explanations for the fact that both appear to be productive and insightful

[5] I heartily recommend as an impressive example of such an achievement Goddard 1998, which distills the results of the efforts of many Algonquianists to account for the very divergent appearance of Arapaho vis-à-vis other Algonquian languages. One could cite similar examples from the history of Indo-European studies involving subfamilies such as Celtic, Armenian, or Tocharian, where cumulative historical changes led to attested systems far less transparently derived from PIE than those of Indo-Iranian, Greek or Latin.

approaches to the study of language: either they are not as unrelated as they appear, or two fundamentally distinct approaches to the study of the same object can be equally productive. He finds the latter possibility unlikely and seeks to show that the practices of traditional historical linguistics are in fact grounded in a recognized form of contemporary linguistic theory (2007: xiii).

After an able and frank description of the considerable conceptual distance between the textual artifacts that form the main basis for diachronic study of the Indo-European family and the individual mental grammars that he takes to be the locus of innovations, Hale nevertheless concludes (2007: 26) that:

> The ultimate focus of diachronic linguistic research concerns the relationship between the *grammars* in figure 2.6. It is important to understand that this is not *my* claim about what diachronic linguistics should be about – it is an observation about what such work has always, to my knowledge, been about *in practice*. The similarity of the (inexplicit) grounding assumptions of diachronic work are strikingly similar to those of contemporary synchronic linguistic theory.

I must emphatically reject this claim. As Hale has made amply clear in his very lucid exposition in Chapter 1 (2007: 3-18), language change from the viewpoint of contemporary theory must be conceived as change in grammars, and this refers to grammars in the sense of "internalized language" (I-language). But Indo-European handbooks, whether they describe "the grammar" of the synchronic stage of a given language or changes from one synchronic stage to another, refer only to some form of "externalized language" (E-language).[6] This characterization applies not only to Indo-European synchronic and diachronic grammars of the past, but also to the very latest exemplars: e.g., Hoffner and Melchert 2008 (synchronic), Ringe 2006 and Weiss 2009b (diachronic). I challenge anyone to cite any Indo-European handbook that is based on the premises of I-language grammar (individual mental grammars).

I must therefore conclude that, despite Hale's skepticism, two different approaches to the study of language change can both be valid and insightful. The reason is, I must again insist, is that contrary to his formulation, traditional historical-comparative linguistics and theoretically informed study of language change (change in I-language grammars) do *not* primarily study the *same* object. I repeat one more time: the primary goal of historical-comparative linguistics has been and to a large extent remains to account for the history of particular languages and sets of languages, *not* for the mechanisms by which language change—in the sense of change in individual mental grammars—takes place. The validity

[6] By which I mean that they describe "a" grammar based on generalizing across a sampling of texts produced by many individual grammars. Such a grammar is comparable to Standard American English, a construct which corresponds to no individual speaker's internal grammar. Obviously, just as SAE bears a strong resemblance to the respective true individual grammars, a well-done description of New Hittite or Classical Latin surely captures many of the features of the grammars of individual speakers. But we are not dealing with I-language.

of the methods of traditional historical-comparative linguistics need not be "grounded" in linguistic theory. Their efficacy has been empirically demonstrated repeatedly in application to languages from all over the world. Certainly, these methods must be based on assumptions about language change that are somehow fundamentally accurate (Hale 2007: xi), but I hardly need demonstrate here how difficult it has proven to be to relate notions like the "regularity of sound change" to what we know so far about how changes in I-language grammars take place (see the concession by Hale himself 2007: 144 that "many mysteries remain," despite his own efforts, those of Labov 1994: 419-543, and many others).

I do not mean to deny that the research program of traditional historical-comparative linguistics, in particular as exemplified by Indo-European studies, and that of theoretically informed investigation of language change share significant commonalities. Nor do I claim that they cannot and should not inform each other. Recent steps in this direction have proven fruitful (see further below), and even more active collaboration is called for and to be highly encouraged. However, we must be realistic in our expectations and recognize that very real differences in both primary goals and methods place limits on what can be achieved.

3. Indo-European Studies and Contemporary Theory (Synchronic and Diachronic)

3.1 Contributions of Contemporary Theory to Indo-European Linguistics

There is no doubt that informed application of contemporary theoretical models to problems of Indo-European linguistics, synchronic and diachronic, can lead to new and superior solutions. As described briefly above in section 1.3, such applications have been made since the 1970s, but it is only in the last decade that they have become an integral part of discussion in the field.

The earlier absence of theoretically informed approaches to problems of PIE accent has now begun to be filled: Keydana 2013 and Kim 2013 may serve as examples of a burgeoning trend. Likewise, earlier theoretical treatments of problems of syllabification such as Steriade 1982 focusing on Ancient Greek (and subsequent responses, for which see the references in Kiparsky 2003) or the long-running and lively debate regarding "Sievers' Law" in Germanic (see e.g. Drescher and Lahiri 1991, Riad 1992, Kiparsky 1998, and the references in Pierce 2011) had very limited impact on the field at large. Recently, however, debate over the proper formulation of syllabification in PIE and its most ancient attested descendants has become mainstream: see e.g. Byrd 2010 with references to Keydana 2004 and Kobayashi 2004. A conference of July, 2013, produced no less than four competing formal accounts of the pre-PIE change known as "Szemerényi's Law", traditionally stated in the problematic form **-*VRs#* > *-*V̄R#* (a word-final sequence of vowel+sonorant+*s* leads to loss of the -*s* and compensatory(?) lengthening of the vowel). Treatment of syntactic issues from a variety of theoretical perspectives is also now commonplace: one may compare the very different approaches but largely convergent solutions to problems of Hittite interrogative and relative clauses by Goedegebuure 2009 (functional discourse grammar) and Huggard 2011(minimalism). Again these examples could easily be multiplied, and others could be substituted for those given.

One may notice that most of the examples I have cited in the preceding paragraph actually directly address synchronic problems, not diachronic (though synchronic includes PIE itself). My choices are not accidental in this regard, and this distribution raises a note of caution. A number of articles published over the last twenty years that have purportedly applied theoretical approaches of various kinds to diachronic problems in Indo-European languages have proven to be premature, their diachronic claims being false because they were based on seriously flawed analyses of the synchronic material. Formal analyses of synchronic data rarely achieve the level of “definitive” solutions, so it would be unrealistic and probably counterproductive to expect scholars in the field to wait indefinitely to venture new theoretically informed historical accounts. However, scholars should refrain from the all too frequent practice of leaping to major claims about historical changes (especially sweeping generalizations) without first taking reasonable steps to make sure that they are building on sufficiently rigorous synchronic analyses.

3.2 Contributions of Indo-European Linguistics to the Study of Language Change

For the reasons discussed in section 2.2 above, I seriously doubt that most current synchronic and diachronic analyses by Indo-Europeanists will have a direct impact on the broader study of language change, *in the form in which they are presented*. This does not mean, however, that Indo-European studies cannot inform both contemporary synchronic theory and the study of language change.

3.2.1 Indo-European Linguistics and Synchronic Theory

While some typological surveys of world languages do include them (e.g. Haspelmath et al. 2001), many theoretical linguists tend to ignore the ancient corpus languages available in the Indo-European family, even large ones like Sanskrit, Ancient Greek, and Latin, when making broad, even universal claims regarding features of attested natural languages. In so doing, they cut themselves off from both further supportive evidence and counterevidence. I cite here two illustrations of the former and one of the latter.

Hittite belongs to the set of languages that has no temporal conjunction ‘before’. To express anteriority at the clause level, it must use a combination of the temporal conjunction *kuitman* ‘while, until’ plus the adverb *nāwi* ‘not yet’ (see briefly Güterbock and Hoffner 1980-1989: 422-3). As shown by Goedegebuure (2002-2003, especially 21-4), Hittite not only has a three-way system of deixis based on the speech act participants (proximal/speaker associated, medial/hearer associated, and distal/other), but also shows the “negative emotional” or disassociative use of the distal deictic pronoun also attested in some modern languages. Sharvit (2003: 673ff.) claims that there are no natural languages that do not have embedding under propositional attitude verbs and that therefore can only express a sentence such as “John decided that he would say to his mother that he missed her” by direct quotation: “John made the following decision: ‘I will say to my mother, "I will miss you".'." But Sanskrit certainly is such a language (see Delbrück 1888: 529-34).[7]

More active engagement of Indo-European scholars with theoretician colleagues would surely help the latter to better exploit the rich source of natural language data provided by the ancient Indo-European corpus languages.

3.2.2 Indo-European Linguistics and the Study of Language Change

Many Indo-European language families are attested over centuries, even millennia, some of them in quite extensive textual corpora, and most are continued by modern languages still being spoken. Although by the well supported hypothesis of Proto-Indo-European, they all spring from a common source, they have significantly diverged in many typological respects. For various historical reasons, analysis of much of this wealth of data on language change—in terms of traditional historical-comparative methodology—is particularly well developed for the Indo-European family. The numerous case studies of Indo-European linguistics provide a virtual laboratory for those seeking to develop and test theoretical models for all aspects of language change. For a selection of these involving phonological and morphological change I refer readers to Chapters 5 through 8 of Ringe and Eska 2013, whose discussions I will not replicate here. Additional analyses

[7] Sanskrit does not, of course, lack embedding in general (see e.g. clauses with *yáthā* in the sense '(so) that'; Delbrück 1888: 595-6).

dealing with syntactic and morphosyntactic change are to be found in Jonas, Whitman, and Garrett 2012. See also the case studies in Part IV of this volume.

I do call attention to the fact that the selections in the works just cited include not only putative changes from PIE (or intermediate protolanguages) to attested languages, but also many changes that took place within the historical period of various Indo-European languages. One must be careful in using posited prehistoric changes as the basis for study of language change, since these are by definition hypothetical. Even in the case of changes within the historical period we do not always have nearly as much data as we would prefer (our grasp of the sociolinguistic situation is often particularly limited), so previous descriptions and analyses should not be taken as established fact. However, we can in such instances at least be reasonably sure that a given change *did* take place, and in one way or another it must ultimately be accounted for in any viable theory of language change.

I do wish to sound a cautionary note about two issues regarding potential contributions of Indo-European studies to the understanding of language change. First, it is not only theoretical models that evolve, due to improved analyses of existing data or discovery of new data. Both diachronic accounts of changes in Indo-European and the synchronic analyses on which they are based also are subject to change. The tired witticism that no attested language changes as fast as reconstructed Proto-Indo-European retains it full force; if anything, the pace of change has accelerated.

I cite as one example recent developments in the study of relative clause syntax in PIE and its oldest descendants. Until recently there had been a broad consensus that PIE and the oldest stages of the attested languages do not have embedded relative clauses of the type “To the runner who wins you give the prize” or “The man whom you seek is here”, but show rather adjoined correlative structures of the type “Whoever obeys the gods, they listen to him as well.” Embedded relatives have been assumed to develop out of the adjoined correlative type in the individual languages.[8] However, Probert (2006) has demonstrated that Old Hittite has embedded as well as adjoined correlative relative clauses and that the former then disappear in later Hittite. While she prudently leaves open the question of whether PIE had embedded relative clauses, her findings have serious consequences for the diachrony of relative clauses: (1) not only can adjoined relatives be reanalyzed as embedded relatives, but also vice-versa; (2) the coexistence of the two types in Old Hittite raises the question of why a language would have both and what factors govern their use. Furthermore, Hale (1987ab) and Garrett (1994) argued that both Vedic Sanskrit and Hittite show overt wh-movement, a striking agreement suggesting that this was also true for PIE. However, these conclusions for Hittite were based almost entirely on the evidence of relative clauses, due to the dearth of data for wh-interrogatives. Recent investigation of Hittite wh-interrogatives by Goedegebuure (2009) and Huggard (2011) based on more extensive data that has since become available has shown that Hittite in fact lacks overt wh-movement. The question of whether PIE had

[8] Opinions differ regarding the PIE status of other structures, such as postposed relative clauses or appositional relative clauses. Compare e.g. Fritz apud Meier-Brügger 2000: 229-30 and Fortson 2010: 163-4.

overt wh-movement is thus at a minimum reopened. For that matter, by no means all questions regarding relative and interrogative syntax in Hittite itself have been settled. The clear lesson from this case is: if we are to successfully exploit the very extensive evidence provided by historical-comparative analysis of the Indo-European family for purposes of modeling language change, there must be increased collaboration or at least consultation between Indo-Europeanists and theoreticians.[9]

A second prerequisite for successful integration of Indo-European linguistics and theoretically oriented study of language change is an open acknowledgement of the wide diversity of viewpoints regarding the nature and mechanisms of language change (this diversity is on full display in the volume edited by Jonas, Whitman, and Garrett cited earlier). One cannot expect a historical linguist to rehearse every possible theoretical approach when analyzing a particular case of language change, but must allow the author to choose what seems to him or her the most insightful. It is likewise perfectly in order that Hale (2007) and Ringe and Eska (2013) in their broad surveys of the topic take firm stands on methodological issues that they then consistently follow.

However, I find it distinctly unhelpful to the cause of promoting the role of Indo-European and traditional historical-comparative linguistics in the study of language change to ignore without any mention opposing views regarding broad fundamental

---

[9] The ever-changing "consensus" about reconstructed PIE also affects the vigorous ongoing debate regarding application of phylogenetic models to language reconstruction. See for merely one example Nakhleh, Warnow, Ringe and Evans 2005.

issues. As I already complained in my review (Melchert 2009: 205), it is quite unacceptable that in a 269-page book on theory and methods of historical linguistics Hale (2007) does not even mention, much less discuss the problem of “analogy” in language change. Ringe and Eska (2013: 153-65 and also 194) totally reject analogy as an explanatory principle and deny that paradigms are part of grammar. They have every right to hold such a view, but it is improper not even to acknowledge the existence of the very different approach of Albright 2008 *et alibi*. The inadequacies of traditional analogy are not in question, but it is far from decided how the phenomena it was intended to describe are best to be accounted for in an adequate model of language change, and the fact that there are competing proposals for solving this problem should not be suppressed. For a balanced and helpful review of the topic see Miller 2010: 1.97-122.

Ringe and Eska (2013: 213) imply that they agree with Hale that syntactic change is not change in syntactic rules, but change in the features of functional heads. However, they then proceed (2013: 214-8) to describe a change in Greek word order in terms of changes in parameter settings and statistical analyses, following Anthony Kroch’s Grammars in Competition Hypothesis. They fail to acknowledge that Hale (2007: 161-72 and 172-80) emphatically rejects such approaches to syntactic change. My point is again not who is right or wrong on this matter. Rather, it is that while Hale in this case not only acknowledges, but also discusses at length his reasons for rejecting other views, those reading Ringe and Eska’s discussion (2013: 213, including footnote †) would wrongly infer that Hale’s approach is similar to that of Lightfoot, Longobardi, Roberts and others cited. Their very laconic phrasing also is misleading in that it tends to minimize the by no means non-trivial differences in the viewpoints of the other scholars listed. In sum, this is not an adequate representation of the great diversity of opinion regarding syntactic change. For a broader review of the problem see Miller 2010: 2.33-66.

I stress that the fact that Hale and Ringe and Eska respectively adopt and consistently apply particular methodologies is not only entirely proper, but also enhances the coherence and lucidity of their presentation of the major issues facing the study of language change. I am not demanding extensive argumentation against every extant opposing view on every topic, merely more consistent and accurate recognition that there are alternative accounts for the various problems treated that interested readers may want to investigate and evaluate for themselves.

4. Conclusion

The reservations and caveats expressed in the preceding sections are by no means meant to leave a discouraging or pessimistic impression—quite the contrary. The study of language change in its modern conception—i.e., as innovations in individual mental grammars mostly due to first-language acquisition but in part to language contact, followed by sociolinguistically governed diffusion—is by any measure a very young subfield of linguistics.[10] We may be sure that the debate regarding how best to model the

[10] The primary role of first-language acquisition in language change was already clear to Paul (1880: 34), but this recognition had no significant impact on the historical-comparative study of Indo-European languages.

implementation of language change will continue for the foreseeable future, and the need for relevant case studies that can help to test competing analyses will likewise remain acute (Ringe and Eska repeatedly and rightly end discussions of particular topics with a call for more research—2013: 123, 135, 209 and *passim*). For reasons given above in the first part of section 3.2.2, Indo-European linguistics is singularly suited to providing the basis for such further research. It is emblematic that the overwhelming majority of the case studies in Jonas, Whitman, and Garrett 2012 are taken from Indo-European languages (NB analyzing changes that took place within the historical period)—likewise those in Part IV in this volume. If Indo-Europeanists not only welcome, but also actively seek out engagement with their theoretical colleagues, they have every opportunity both to improve their accounts of the history of the attested Indo-European languages (their traditional and abiding goal) and to contribute to enhancing our understanding of the mechanisms of language change (viewed as change in grammars and diffusion).

References

Albright, Adam. 2008. Explaining universal tendencies and language particulars in analogical change. In Jeff Good (ed.), *Language Universals and Language Change*, 144-81. New York: Oxford University Press.

Bloomfield, Leonard. 1933. *Language*. New York: Holt, Rinehart, Winston.

Bopp, Franz. 1816. *Über das Conjugationssystem der Sanskritsprache*. Frankfurt am Main: Andreas.

Brugmann, Karl. 1876. Nasalis sonans in der indogermanischen Grundsprache. *Studien zur griechischen und lateinischen Grammatik* 9.285-338.

——. 1886. *Grundriss der vergleichenden Grammatik der indogermanischen Sprachen*. Drei Bände. Strassburg: Trübner.

——. 1897. *The Nature and Origin of the Noun Genders in the Indo-European Languages*, translated by E. Y. Robbins. New York: Scribner's.

Byrd, Andrew. 2010. Motivating Sievers' Law. In Stephanie W. Jamison et al. (eds.), *Proceedings of the 21st Annual UCLA Indo-European Conference*, 45-67. Bremen: Hempen.

Delbrück, Berthold. 1888. *Altindische Syntax*. Halle: Verlag der Waisen Hauses. Reprint, Darmstadt: Wissenschaftliche Buchgesellschaft, 1968.

Drescher, B. Elan, and Aditi Lahiri. 1991. The Germanic foot: metrical coherence in Old English. *Linguistic Inquiry* 22.251-86.

Elbourne, Paul. 1998. Proto-Indo-European Voiceless Aspirates. *Historische Sprachforschung* 111.1-30.

Fortson, Benjamin W. Fortson IV. 2010. *Indo-European Language and Culture: An Introduction*. Second edition. Malden MA/Oxford: Wiley-Blackwell.

Friedrich, Paul. 1975. *Proto-Indo-European Syntax: The Order of Meaningful Elements*. Butte MT: Journal of Indo-European Studies.

Gamkrelidze, Tomas V., and Ivanov, Vyačeslav V. 1973. Sprachtypologie und die Rekonstruktion der gemeinindogermanischen Verschlüsse. *Phonetica* 27.150-56.

——. 1984. *Indoevropejskij j͡azyk i indoevropejcy: rekonstrukcij͡a i istoriko-tipologičeskij analiz praj͡azyka i protokul'tury*. Tbilisi: Izdatel'stvo Tbilisskogo universiteta.

——. 1995. *Indo-European and the Indo-Europeans: A Reconstruction and Historical Analysis of a Proto-Language and a Proto-Culture*, translated by Johanna Nichols. Berlin/New York: Mouton de Gruyter.

Garrett, Andrew. 1994. Relative Clause Syntax in Lycian and Hittite. *Die Sprache* 36.29-69.

Goddard, Ives. 1998. Recovering Arapaho Etymologies by Reconstructing Forwards. In Jay Jasanoff, H. Craig Melchert, and Lisi Oliver (eds.), *Mír Curad: Studies in Honor of Calvert Watkins*, 183-200. Innsbruck: Institut für Sprachwissenschaft der Universität Innsbruck.

Goedegebuure, Petra. 2002-2003. The Hittite 3rd person/distal demonstrative *aši* (*uni*, *eni* etc.). *Die Sprache* 43.1-32.

——. 2009. Focus Structure and Q-word Questions in Hittite. *Linguistics* 47/4.945-69.

Güterbock, Hans G., and Harry A. Hoffner, Jr. 1980-1989. *The Hittite Dictionary of the Oriental Institute of the University of Chicago.* Volume L-N. Chicago: Oriental Institute of the University of Chicago.

Hale, Mark. 2007. *Historical Linguistics: Theory and Method*. Malden MA/Oxford UK: Blackwell.

Hale, Mark. 1987a. Studies in the Comparative Syntax of the Oldest Indo-Iranian Languages. Ph.D. diss., Harvard University.

——. 1987b. Notes on Wackernagel’s Law in the language of the Rigveda. In Calvert Watkins (ed.), *Studies in Memory of Warren Cowgill (1929–1985)*, 38-50. Berlin/New York: de Gruyter.

Haspelmath, Martin, et al. 2001. *Language Typology and Language Universals: an International Handbook*. Berlin/New York: de Gruyter.

Haudry, Jean. 1970. L’instrumental et la structure de la phrase simple en indo-européen. *Bulletin de la Société de Linguistique de Paris* 65.44-84.

——. 1982. *Préhistoire de la flexion nominale indo-européenne*. Lyons: Institut d’études indo-européennes de l’Université Jean Moulin (Lyon III).

Hoenigswald, Henry. 1946. Sound Change and Linguistic Structure. *Language* 22.138-43.

Hoffner, Harry A. Jr., and H. Craig Melchert. *A Grammar of the Hittite Language*. Part I: Reference Grammar. Winona Lake IN: Eisenbrauns.

Hopper, Paul. 1973. Glottalized and murmured occlusives in Indo-European. *Glossa* 7/2.141-66.

Huggard, Mattyas. 2011. On Wh-(Non)-Movement and Internal Structures of the Hittite Preposed Relative Clause. In Stephanie W. Jamison, et al. (eds.), *Proceedings of the 22nd Annual UCLA Indo-European Conference*, 83-104. Bremen: Hempen.

Jakobson, Roman. 1931. Prinzipien der historischen Phonologie. *Travaux du Cercle Linguistique de Prague* 4.247-67.

——. 1949. Principes de phonologie historique. In N. S. Troubetzkoy, *Principes de phonologie*, translated by J. Cantineau, 315-36. Paris: Klincksieck.

——. Typological Studies and Their Contribution to Historical Comparative Linguistics. In *Proceedings of the Eighth International Congress of Linguists*, 17-25. Oslo: Oslo University Press.

——. 1995. Foreword. In Gamkrelidze and Ivanov 1995, xix-xxi.

Jonas, Dianne, John Whitman, and Andrew Garrett (eds.). *Grammatical Change: Origins, Nature, Outcomes*. Oxford/New York: Oxford University Press.

Joseph, Brian, and Rex Wallace. 1994. Proto-Indo-European Voiced Aspirates in Italic: A Test for the ‘Glottalic Theory’. *Historische Sprachforschung* 107.244-61.

Keydana, Götz. 2004. Silbenstruktur und Phonotaktik im Indogermanischen. In Maria Kozianka, Rosemarie Lühr, and Susanne Zeilfelder (eds.), *Indogermanistik—Germanistik—Linguistik: Akten der Arbeitstagung der Indogermanischen Gesellschaft, Jena 18.–20.09.2002*, 163-92. Hamburg: Kovač.

——. 2008. Hohe Sonorität in der Koda: Indogermanische Lösungen für ein phonetisches Problem. *Historische Sprachforschung* 121.54-64.

——. 2013. Proterokinetische Stämme, Akzent und Ablaut. In Keydana et al. 2013, 31-62.

Keydana, Götz, Thomas Olander, and Paul Widmer. 2013. *Indo-European Ablaut and Accent*. Copenhagen: Museum Tusculanum.

Kim, Ronald. 2013. Metrical grid theory, internal derivation, and the reconstruction of PIE nominal accent paradigms. In Keydana et al. 2013, 63-105.

Kiparsky, Paul. 1998. Sievers' Law as Prosodic Optimization. In Jay Jasanoff et al. (eds.), *Mír Curad: Studies in Honor of Calvert Watkins*, 345-60. Innsbruck: Institut für Sprachwissenschaft der Universität Innsbruck.

——. 2003. Accent, syllable structure, and morphology in Ancient Greek. In Elizabeth Mela Athanasopoulou (ed.), *Selected Papers from the 15th International Symposium on Theoretical and Applied Linguistics*, Thessaloniki. Available at http://www.stanford.edu/~kiparsky/Papers/opacity2.pdf (consulted 8/20/2013).

Kobayashi, Masato. 2004. *Historical Phonology of Old Indo-Aryan Consonants*. Tokyo: Fujiwara.

Krahe, Hans. 1943. *Indogermanische Sprachwissenschaft*. Berlin: de Gruyter.

Kuryłowicz, Jerzy. 1964. *The Inflectional Categories of Proto-Indo-European*. Heidelberg: Winter.

Labov, William. 1994. *Principles of Linguistic Change: Internal Factors*. Cambridge MA: Blackwell.

Lehmann, Winfred. 1952. *Proto-Indo-European Phonology*. Austin: University of Texas Press and Linguistic Society of America.

——. 1974. *Proto-Indo-European Syntax*. Austin: University of Texas Press.

Mańczak, Witold. 1969. *Le développement phonétique des langues romanes et la fréquence*. Kraków: Nakł. Uniwersytetu Jagiellońskiego.

Martinet, André. 1952. Function, structure and sound change. *Word* 8.1-32.

——. 1953. Concerning the preservation of useful sound features. *Word* 9.1-11.

——. 1964. *Économie des changements phonétiques; traité de phonologie diachronique*. Berne: Francke.

Matthews, Peter. 2001. *A Short History of Structural Linguistics*. Cambridge UK/New York: Cambridge University Press.

Mayrhofer, Manfred. 1986. *Indogermanische Grammatik I.2: Lautlehre* [Segmentale Phonologie des Indogermanischen]. Heidelberg: Winter.

Meier-Brügger, Michael. 2000. *Indogermanische Sprachwissenschaft*. 7., völlig neubearbeitete Auflage. Berlin/New York: de Gruyter.

Meillet, Antoine. 1934. *Introduction à l'étude comparative des langues indo-européennes*. 7e édition. Paris: Hachette.

Melchert, H. Craig. 1975. 'Exceptions' to Exceptionless Sound Laws. *Lingua* 35.135-53.

——. 2009. Review of Hale, 2007. *Language* 85/1.203-6.

Miller, D. Gary. 2010. *Language Change and Linguistic Theory*. Oxford/New York: Oxford University Press.

Moulton, William G. 1948. The Phonemes of Gothic. *Language* 24.76-86.

Nakhleh, Luay, Tandy Warnow, Don Ringe, and Steven N. Evans. 2005. A Comparison of Phylogenetic Reconstruction Methods on an Indo-European Dataset. *Transactions of the Philological Society* 103.171-92.

Nichols, Johanna. 1995. Translator's Preface. In Gamkrelidze and Ivanov 1995, xi-xviii.

Paul, Hermann. 1880. *Prinzipien der Sprachgeschichte*. Halle: Niemeyer.

Pedersen, Holger. 1931. *The Discovery of Language. Linguistic Science in the Nineteenth Century*, translated by John W. Spargo. Cambridge MA: Havard University Press.

——. 1951. *Die gemeinindoeuropäischen und die vorindoeuropäischen Verschlusslaute*. Copenhagen: Munksgaard.

Pierce, Marc. 2011. The status of the Onset Principle in Early Germanic. In Stephanie W. Jamison et al. (eds.), *Proceedings of the 22nd Annual UCLA Indo-European Conference*, 193-208. Bremen: Hempen.

Probert, Philomen. 2006. Clause Boundaries in Old Hittite Relative Sentences. *Transactions of the Philological Society* 104.17-83.

Prokosch, Eduard. 1918-19. Die indogermanischen "Mediae aspiratae". *ModernPhilology* 15.621-28, 16.99-112, 325-6, 543-52.

Riad, Tomas. 1992. Structures in Germanic Prosody. Ph.D. diss., University of Stockholm.

Ringe, Don. 2006. *A Linguistic History of English*. Vol. I. *From Proto-Indo-European to Proto-Germanic*. Oxford/New York: Oxford University Press.

Ringe, Don, and Joseph F. Eska. 2013. *Historical Linguistics: Toward a Twenty-First Century Reintegration*. New York: Cambridge University Press.

Saussure, Ferdinand de. 1983. *Course in General Linguistics*, translated and annotated by Roy Harris. London: Duckworth.

Schindler, Jochem. 1976. Diachronic and Synchronic Remarks on Bartholomae's and Grassmann's Laws. *Linguistic Inquiry* 7.622-37.

——. 1977. Notizien zum Sieverschen Gesetz. *Die Sprache* 23.56-65.

Schleicher, August. 1868. Fabel in indogermanischer Ursprache. *Beiträge zur vergleichenden Sprachforschung auf dem Gebiete der arischen, celtischen und slawischen Sprachen*. 5.206-8.

Seebold, Elmar. 1971. Versuch über die Herkunft der indogermanischen Personalendungenssystem. *Zeitschrift für Vergleichende Sprachwissenschaft* 85.185-210.

Sharvit, Yael. 2003. Embedded Tense and Universal Grammar. *Linguistic Inquiry* 34.669-81.

Steriade, Donca. 1982. Greek Prosodies and the Nature of Syllabification. Ph.D. diss., Massachusetts Institute of Technology.

Tremblay, Xavier. 2005. Grammaire comparée et grammaire historique: quelle réalité est reconstruite par la grammaire comparée? In Gérard Fussman et al. (eds.), *Āryas, Aryens et Iraniens en Asie Centrale*, 21-346. Paris: Boccard.

Twaddell, W. Freeman. 1948. The Prehistoric Germanic Short Syllabics. *Language* 24.139-59.

Watkins, Calvert. 1963. Preliminaries to a historical and comparative analysis of the syntax of the Old Irish verb. *Celtica* 6.1-49.

Weiss, Michael A. 2009a. The Cao Bang Theory: Some Speculations on the Prehistory of the PIE Stop System. Available as PowerPoint file at: http://linguistics.cornell.edu/people/Weiss.cfm (consulted 8/11/2013).

——. 2009b. *Outline of the Historical and Comparative Grammar of Latin*. Ann Arbor/New York: Beech Stave.