

New perspectives in historical linguistics*

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This condensed review of recent trends and developments in historical linguistics proceeds from the empirical to the conceptual, from ‘what’ to ‘how’ to ‘why’. I begin with new findings about the origins, relationships, and diversity of the world’s languages, then turn to the processes and mechanisms of change as they concern practicing historical linguists, continue with efforts to ground change in the acquisition, use, and structure of language, and conclude with a look at ongoing debates concerning the explanatory division of labor between historical and theoretical linguistics and ways to unify historical and theoretical linguistics. The emphasis throughout is on current research rather than on established textbook knowledge.

1 Language relationships

1.1 The new look in historical linguistics

The most visible face of historical linguistics is the study of language relationships. It has been revitalized in the last few decades by a wealth of new linguistic, historical, anthropological, and genetic evidence, innovative methods of classification, and a better understanding of how languages disperse and change. New surprising relationships are being proposed, and the internal affiliations of already established families are being reassessed.¹ Binary tree models have been challenged by flatter “bush” or “rake” models that eliminate some formerly assumed subgroupings in favor of convergence between daughter languages, and the question of the families’ homeland, date, and dispersal has been reopened: see Sidwell (this volume) on Austro-Asiatic, Ehret 2001 on Bantu, McConvell & Bowern 2011 on Australian, Donohue & Grimes 2008 and Pawley 2011 on Oceanic, Häkkinen 2012a, 2012c on Uralic, Babel, Garrett, Houser & Toosarvandani 2009 on Western Numic, and Garrett 2006 on Indo-European, among many others.

Indo-Europeanists are continuing to debate two principal models of population spread: the farming model, which assumes expansion from Anatolia over a relatively long time span, implying an early date of 7000–6500 BCE for the protolanguage (Renfrew 1987, 1999, 2000, 2001), and a more rapid expansion by pastoralists from the Pontic-Caspian steppes beginning around 4500–4000 BCE (Mallory 1989). Renfrew’s theory has been popular among archeologists, but linguists have by and large preferred the pastoralist model, on the grounds that it fits the reconstructible vocabulary, that early contacts with Uralic support the more northerly homeland

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¹Uralic has a new look, with no “Finno-Ugric” branch and a new place for Saami (Häkkinen 1984, Itkonen 1998, Carpelan, Parpola & Koskikallio 1999, Salminen 2002, Häkkinen 2007, 2009). Häkkinen finds that Uralic first split into an eastern Ugro-Samoyedic and western Finno-Permic branch.

(Carpelan, Parpola & Koskikallio 2001), and that Greek and Indo-Iranian are linguistically and culturally too similar to have undergone as much as 5000 years of separate development (Darden 2001, Anthony 2007). Gray & Atkinson 2003 have weighed in with an argument for Renfrew's narrative based on applying computational cladistic methods from biology to the IE lexicon (see also Bouckaert et al. 2012, critiqued by Häkkinen 2012b). For now the question of the homeland, date, and dispersal of Proto-IE remains open.²

Understudied languages are being documented at an increasing rate, though not nearly fast enough in view of the rate of extinction. Provisional classifications based on typological or glottochronological criteria, or sometimes just on gut-level intuitions, are being tested by careful comparative work. Undoubtedly many as yet unsuspected affiliations remain to be discovered. Computationally enhanced lexicostatistic methods, if validated for relatively well-understood families such as Indo-European and Austronesian, may yet turn out to yield reliable shortcuts to establishing families and subgroups (Kessler 2013; Dunn, this volume).

Meanwhile more speculative efforts to join the known language families into larger macrofamilies and super-macrofamilies continue unabated. Candidates (some of them overlapping) include Almosan and the larger Amerind (Greenberg 1987), Altaic and the larger Ural-Altaic (Starostin, Dybo, and Mudrak 2003), Indo-Uralic and the larger Nostratic (Bomhard 2008), Austric (Reid 2005), Eurasiatic (Greenberg 2000), and Indo-Pacific (Greenberg 1971, criticized in Pawley 2009). Hypotheses of remote genetic relationship can never be definitively refuted, but none of the above are supported by solid linguistic evidence (Campbell and Poser 2008), nor, for the most part, do they conform with results of population genetics (Heggarty 2012). Ural-Altaic is undoubtedly a typological and areal grouping (Janhunen 2001, 2007), but not a genetic one as far as anyone has been able to show. Nostratic is the only one of these putative families that is claimed to rest on the comparative method.³ The equally famous putative macrofamily Amerind, comprising all American languages except for Na-Dene and Eskimo-Aleut (Greenberg 1987, Greenberg & Ruhlen 1992), is a priori plausible, and seems to fit well with population genetics and archeology, so it not surprising that many anthropologists and archeologists have readily taken the linguistic kinship on faith. Specialists on American languages, however, consider the Amerind family unsubstantiated on linguistic grounds because it is based only on the method of mass comparison, which is clearly flawed (Campbell and Poser 2008).

1.2 The comparative method

For now, the comparative method remains the gold standard (Weiss, this volume; Hale, this volume). The persistent superstition that it does not work for unwritten languages or for certain families has been refuted again and again — famously for Algonquian in the classic work of Bloomfield (1925, 1946), continued by Haas 1958, Goddard 1979, 1990, Garrett 2004, Berman 2006, and others. More recently the applicability of the comparative method has been questioned for Pama-Nyungan (Australian), but the work of Hale (1966) and many scholars since then should have laid the doubts to rest, see O'Grady and Hale (2004), Alpher (2004), and other articles in the same volume.

²The role of farming in dispersal is widely assumed for other language families as well but not unchallenged (Donohue & Denham 2010).

³E.g. Illič-Svityč's spectacular explanation of the three-way Indo-European contrast between plain velars, labialized velars, and palatals on the basis of the quality of the following vowel, preserved in Uralic (Dybo 1989). If the rest of Nostratic could be worked out to the same standards, the controversy would be over.

The comparative method requires first of all large amounts of reliable data. It works best when it can build on prior synchronic analysis and internal reconstruction (Joseph 2010), with as much partial reconstruction of subgroups and subfamilies as can be done first. When these foundations are in place, hypotheses about even remote genetic relationships can yield precise testable predictions. Recent promising work connects the Andaman Islands languages Jarawa and Onge to Austronesian (Blevins 2007), Chitimacha in Louisiana to Totonacan and Mixe-Zoquean (Brown, Wichmann, & Beck, to appear), and, most surprisingly, Na-Dene (Athabascan-Eyak-Tlingit, a family securely established by Krauss) to Yeniseian, whose sole surviving member is Ket, now spoken near Krasnoyarsk in south central Russia (Vajda 2010a, 2010b). Vajda has described how, struck by the resemblance between the areally unusual verb prefixes of Ket to the inner (“conjunct”) verb prefixes of Athabascan and Eyak, he discovered that the prefix positions in the reconstructed verb complex match up, except that Yeniseian has two extra slots, and marks aspect both directly after the auxiliary and after the root, whereas Na-Dene places the corresponding morphemes (presumably ancient enclitics) only after the root; the systematic character of this discrepancy actually makes the case for ancient affinity even stronger. What is more, many of the corresponding positions are filled by similar morphemes in Yeniseic and Na-Dene. The respective verb complexes, after Vajda (2010a: 38-40), are shown aligned in (1) and (2).⁴

(1) Reconstructed Proto-Yeniseian

OBJ.AGR.	SHAPE <i>n-</i> round <i>ʒ-</i> long <i>p^h-</i> flat	ANIMACY 3p. subj., <i>w-</i> inanim., <i>?dⁱ</i> anim.	AUX + ASP <i>s</i> <i>-n</i> PERF <i>qa</i> <i>-l</i> PROG	1/2 SUBJ. AGR.	PERF.-, STATIVE <i>jə</i> , IMP. <i>ʒ</i>	V-DERIV. <i>ʒ</i> (?also <i>ʔ</i>)	ROOT	PERF.-, STATIVE <i>-ej</i> , <i>-ŋ</i>
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(2) Athabascan (conjunct)

<i>Auxiliary complex</i>				<i>Root complex</i>					
OBJ.AGR.	SHAPE		AUX		1/2	PERF.-, STATIVE	CLASSIFIER (VALENCE)	ROOT	ASP
DEICTIC	* <i>n</i> round		* <i>s</i> (ə)		SUBJ.	<i>ni</i>	<i>ʔ, d, l, ∅</i>		<i>-ñ</i> PERF
PRON.	* <i>d</i> long * <i>qʊ</i> area		* <i>γə</i> * <i>nə</i>		AGR.				<i>-ʔ</i> PROG

The morphological parallelism and phonological similarities among corresponding affixes are suggestive, but the most compelling evidence for actual relationship comes from those sound correspondences which can be accounted for by independently motivated regular sound changes. (3) illustrates this with the aspectual morphemes. The consonantal difference between Proto-Athabascan-Eyak perfective **-ñ* and Ket *-n*, and Proto-Athabascan-Eyak progressive **-ʔ* and Ket *-l* is paralleled in other cognate pairs:

(3)	PA(E)		Ket		PA(E)		Ket	
	* <i>-ñ</i>	‘perfective’	<i>-n</i>	‘perfective’	* <i>-ʔ</i>	PROGRESSIVE	<i>-l</i>	PROGRESSIVE
	<i>de:ñ</i>	‘emit light’	<i>di:n</i>	‘emit light’	<i>hʊʔ</i>	‘club’ (Koy.)	<i>húùl</i>	‘club’
	<i>šwəñ</i>	‘black’	<i>sə'n</i>	‘dark blue/green’				
	<i>ʔañ</i>	‘many’	<i>o'n, òn</i>	‘many’				

⁴Athabascan has added an outer (“disjunct”) layer of prefixes, absent in the other branches. In the absence of a definitive reconstruction of Na-Dene, Vajda represents Na-Dene prefixes by a generalized model of the Athabascan conjunct prefixes.

In contrast to the Amerind hypothesis, this work adheres to the best practices of comparative historical linguistics.⁵ If it holds up, the relationship would be notable for the vast geographic distance that it spans. The Yeniseian homeland is believed to have been west of Lake Baikal, which implies a migration as astounding as the great voyage that brought Austronesian speakers from Borneo to Madagascar a millennium and a half ago (Dahl 1991, Kikusawa, this volume).

1.3 Language contact

Thanks to advances in archeology, geochemical fingerprinting, and genetics (Heggarty, this volume, Pakendorf, this volume) prehistoric population movements are becoming better understood, putting contact-based explanations for language change on a firmer footing. Consequently substratum and superstratum hypotheses, long considered a somewhat disreputable last resort (Lass 1997, Honti 2007), are becoming empirically falsifiable and are being advanced more confidently (Matras & Sakel 2007, Lucas, this volume). Campbell (1997, Ch. 9) identifies 21 linguistic areas in the Americas alone; fine-grained areal analyses have been worked out for South Asia (Masica 1976), Mainland Southeast Asia (Enfield 2005), the Balkans (Friedman 2006), and Amazonia (Aikhenvald 2002), among others. Even the possibility of “excavating” entire layers of lost prehistoric languages through their substratum effects in attested languages has been explored for India (Witzel 1999) and northern Europe (Salmons 1992, Schrijver 1999, Aikio 2004, 2006).

Phonological contact effects come predominantly from substratum (non-prestige) languages. Schrijver 2009 explains several sound changes of English on the basis of a Celtic substrate, curiously enough of the Irish rather than British Celtic type. Schrijver 2011 argues that the High German Consonant Shift in the Rhineland “is the result of speakers of Gallo-Romance switching to Germanic and replacing Germanic aspirated voiceless plosives by voiceless affricates *but only in the phonetic positions in which these affricates occurred in Gallo-Romance*” (p. 243). Fennists are increasingly inclined to accept Posti’s old theory that certain Proto-Finnic sound changes were caused by contact with Baltic and Germanic (Kallio 2000).⁶

Substratum languages can not only be the source of borrowed features, but also the cause of retention of conservative features. For example, Fenno-Swedish has preserved the quantitative contrasts of Germanic under Finnish influence (Kiparsky 2008). And the area where these contrasts were retained in medieval Swedish and Norwegian coincided approximately with the then extent of Saami, a language with a similar quantitative system. Contact with Saami and Finnish, which themselves have no pitch accents, seem to have contributed to retaining the prosodic system that was the precursor of the Scandinavian two-peak pitch accent systems (Kiparsky 2012b) — a more unusual role for contact in tonogenesis than what we are familiar with from Southeast Asia and elsewhere.

Contact-induced change has become more respectable in syntax and semantics as well. Western Uralic innovations such as finite *that*- and *Wh*-clauses, SVO word order, prepositions, and even (in some languages) articles are very likely due to contact with Indo-European; North Russian syntax is influenced by Finnish. Kroch, Taylor, and Ringe 1995 attribute basic word-order changes in

⁵But see the critique by Campbell 2011 and the reply in Brown, Wichmann & Beck, to appear.

⁶This would be an unusual case of superstrate influence in phonology. But it might be understood in the context of the ancient Germanic practice of making the elite of conquered populations send their children to be raised in the courts of the conquerors as hostages (*gīsl*, Finnish *kihla*), eventually returning to powerful positions in their native country as fully acculturated speakers of the rulers’ language. Moreover there is population-genetic support for prehistoric language shifts from Germanic to Finnic (Sajantila & Pääbo 1995).

early English to Scandinavian influence on Northern dialects. Filppula 2013 summarizes evidence for the Celtic source of syntactic innovations such as *it*-clefting. Persistent contact on a large scale in bilingual communities can even change the whole typology of a language. Stilo 2004 insightfully treats Iranian as a “buffer zone” of Arabic and Turkic traits imposed on an Indo-European base. Ross (1996, 2001) has assembled a dossier of what he calls METATYPY, his most impressive case being the massive structural borrowing by Takia (Oceanic) from Waskia (Papuan). A favorite example in the contact literature is the moribund Greek of some Cappadocian communities, with noun morphology refurbished in agglutinative style under Turkish influence (Janse 2009).⁷ Drawing on Andersen 1988, Ross 2003 distinguishes three sociolinguistic/demographic types of speech communities (closed, open tightknit, open looseknit) which differ in the kinds of contact-induced and endogenous linguistic change they typically undergo (cf. Greenhill, this volume, and Heine & Kuteva 2005).

1.4 Historical linguistics in science journals and the media

Although firm results on long-range relationships have been elusive, general science journals acknowledge the popular appeal of the topic by allotting more space to it than to any other kind of other historical work, let alone to theoretical linguistics. The most far-reaching hypotheses about language origins and change even make it all the way into the popular media — just about the only kind of linguistics that does. This turns out to be a mixed blessing. While the relatively generous amount of this coverage is welcome, its quality is haphazard. Historical linguistics, like all linguistics, suffers more than its share of uninformed, sometimes downright unserious reporting. In the case of Amerind, the journals, followed by the media, covered the original proposal more or less faithfully, but essentially ignored the subsequent lively methodological and empirical discussion, thereby passing up a golden opportunity to present the problems, principles, and challenges of comparative linguistics to a broader public. The most solid work (such as Vajda’s) tends to get the least attention.

Part of the problem is that science editors usually have little expertise in linguistics, and even the most prestigious journals fail to subject linguistics articles to adequate peer review. Such journals become platforms for speculative hypotheses that would not pass muster in most other fields (Sproat 2010). For example, Atkinson’s 2011 claim that average phonemic diversity is greatest in Africa and decreases with distance from there, supposedly confirming the view that humans came from Africa 50,000–70,000 years ago (which is firmly back by other evidence, of course), was launched in *Science*, but by the time discussion of it by specialists had returned a devastating negative verdict,⁸ the journal had moved on. Also first published in a high-profile journal and quickly picked up by the news media is the idea that all languages of the world are derived from a proto-language with Object-Verb order, which shifted to Verb-Object order in half of the extant daughter

⁷The Cappadocian situation is usually presented in overly broad strokes, overlooking the relationship of large-scale morphological borrowing to reduced radius of communication and language death (for which see Simpson, this volume). As Dawkins 1916 takes care to point out, the Turkicized morphology was confined to a few communities (Ferték, Ulaghátsh and Semenderé) where Greek was on the verge of extinction, its use being mainly confined to women and children. In Ferték the Greek men already spoke Turkish with each other, but still understood the dialect (though “not very freely”) and at Ulaghátsh even women were talking Turkish to their children. The other Cappadocian dialects of Greek described by Dawkins 1916 were threatened not by Turkish, but by standard Greek, which for the most part replaced them in the Cappadocian communities that settled in Greece after 1922. It is also worth noting that even in its terminal state the Ulaghátsh dialect faithfully preserved the archaic medieval clitic system and features of the clausal syntax lost in the majority of dialects including the standard language (Condoravdi and Kiparsky 2004).

⁸Lieberman 2011, Hunley, Bowerman, and Healy 2012, and several articles in *Linguistic Typology* Vol. 15, 2011.

languages (Gell-Mann & Ruhlen 2011) (“Early human language like Yoda sounded”, *CBS News*) — with no reason given either why proto-world language should have been OV nor why its daughter languages would have unidirectionally reversed the order. Another such claim was that the Greenbergian word-order generalizations are lineage-specific rather than universal, so that word order is primarily the result of cultural evolution shaped and constrained by the existing linguistic system (Dunn, Greenhill, Levinson, & Gray 2011). Since these articles raise fascinating questions, one would happily trade some of their slick graphics for more cogent argumentation.⁹

2 Mechanisms and trajectories: how languages change

This section surveys the basic empirical landscape of interest to the working historical linguist, leaving some theoretical issues for sections 3 and 4.

2.1 Contact-induced change

Some items are more likely to be borrowed than others, depending on their category and place in the system (Curnow 2001, Aikhenvald 2006, Matras 2009). This is important to know in assessing the evidence for genetic relationships. Comparativists value shared irregularities and exceptions as reliable indicators of common descent, for they are relatively rarely borrowed. Loanwords, on the other hand, are the most frequent type of borrowed item (for a typology, see Haspelmath & Tadmor 2009), though some languages are unreceptive to them, because of structural incompatibility or for sociolinguistic/ideological reasons. Bound morphemes are more rarely borrowed (Johanson & Robbeets 2012) — derivation more often than inflection (Gardani 2008), and number more often than case (Gardani 2012). The reason derivational morphology appears to be comparatively easily borrowed may be simply that it can be imported through loan vocabulary that contains it, and can then spread from there into the native layer of word formation. This is not strictly speaking borrowing of morphemes, of course. Dozens of suffixes such as *-ess*, *-ee*, *-esque*, *-ette*, *-oid*, *-nik* that have come into English with loanwords from Romance and other languages are now productively added to native words. Hindi-Urdu freely extends the suffix *-dān* ‘container’ from its Persian loans to native words, e.g. *pīk-dān* ‘spittoon’. Among grammatical markers, it is naturally the ones closer to the stem, such as number and gender, that piggy-back on loanwords in this way. The plural Persian (ultimately Arabic) ending *-āt* has been nativized in Urdu, so that it can now be added even to words of Indic origin, e.g. *janglāt* ‘forests’. Case morphemes, on the other hand, are normally imported by bilingual code-switchers in more intimate contact settings.¹⁰

The stratified morphology-phonology association posited in Lexical Phonology and Morphology and Stratal OT predicts a parallel hierarchy for phonology. Much of English stem-level (morpho)phonology has been smuggled into English in Romance loanwords. The Romance stress rule penetrated even the native vocabulary in Early Modern English (Dresher & Lahiri 2005, Dresher 2013). Finnish, with fixed word-initial stress, has a productive system of quantitative alternations

⁹In a forthcoming study I provide what I believe is a more promising alternative account of the important word order generalizations that the latter two studies are concerned with.

¹⁰Under exceptional sociolinguistic situations, two languages can become ‘intertwined’. Michif (Cree verbs and French nouns) was spoken by descendants of native American women married to French fur traders (Bakker 1997). Mednyj Aleut (Aleut with Russian finite verb inflection) resulted from intermarriage between Russian men and Aleutian women (Thomason & Kaufman 1988).

that reflect the donor languages' stress alternations, as in *politiikka* 'politics', *poliitikko* 'politician', *poliittinen* 'political'. The introduction of such phonological contraband with loanwords is more common than borrowing of low-level (word-level and postlexical) phonology, which typically occurs in a substratum situation.

The general point to keep in mind is that the borrowability of an item depends not only on its category (whether it is morphology, phonology, and so on) and on the contact situation but also on its place in the linguistic system, such as its regularity, productivity, and its lexical or postlexical status. For this reason contact hypotheses have to be anchored in solid grammatical analyses.

Loanword adaptation is a balancing act between PHONETIC APPROXIMATION and PHONOLOGICAL/MORPHOLOGICAL INTEGRATION.¹¹ In order to preserve as much as possible of a loanword's phonological content, phonotactically difficult sequences are dealt with preferentially by epenthesis rather than deletion, unless epenthesis would require additional repairs, in which case deletion is resorted to (Paradis & LaCharité 1997). For example, Finnish preserves word-final consonants by epenthesis, but deletes all but the innermost consonant in initial clusters, e.g. Swedish *skruv* [skrʉ:v] 'screw' → *ruuvi*. The reason is that epenthesis into initial clusters would produce either exceptional non-initial stress (**sekrúuvi*, **eskerúuvi*) or, if the stress were moved, a further distortion of the original's phonological content (**sékruuvi*, **éskeruuvi*).

In time, loanwords are integrated by making their stems inflectable and morphophonologically regular. In the earliest literary Finnish, foreign words ending in consonants were usually left unchanged, or adopted with various added final vowels, but the modern language generally nativizes them with final *-i*, even when they end in coronals, which are licit word-final consonants in the native vocabulary: *Japani* 'Japan', *tunneli* 'tunnel', *Inkeri* 'Inger'. Although **Japan*, **tunnel*, **Inger* are pronounceable (cf. the native *hapan* 'sour', *kannel* 'Finnish harp', *penger* 'ledge', 'embankment') -V stems are preferred in nativization because they are not subject to the morphophonological alternations of -C stems.

The closer the contact, the more systematic the rendering becomes, and the more morphophonology and morphology overrides simple phonetic similarity. The way in which borrowed items are adopted can therefore provide clues to the date and degree of contact and layering of loanwords. For example, early Germanic loanwords neutralized voicing (Finnish has no voiced stops), while later ones transposed the voicing contrast into the Finnish gemination contrast, e.g. /d/ → /t/, /t/ → /tt/ (Steinitz 1964). Another ingenious pattern of phoneme substitutions which reveals extensive contact is seen in the adaptation of foreign /f/ in Finnish. Depending on the phonological context it is rendered as:¹²

- (4) a. *hv* intervocalically within a foot: *kaffe* → *kahvi* "coffee", *biff* → *pihvi* "steak", *soffa* → *sohva* "sofa", *giráff* → *kirahvi* "giraffe".
 b. *h* before a consonant: *saffran* → *sahrami* "saffron", *saft* → *sahti* "table beer".
 c. *v* elsewhere: (1) word- and foot-initially: *fiská:l* → *viskaali* "prosecutor", *färg* → *väri* "color", *unifórm* → *únivòrmu* "uniform", *ingefá:ra* → *ínkivääri* "ginger", and (2) after a consonant: *konfékt* → *konvehti* "candy", *asfalt* → *asvaltti* "asphalt".

Heinämäki (1976) noted that *f*'s basic replacement is *hv*, which unpacks its features into two adjacent segments, and that its other replacements are regular accommodations of *hv* to the phonotactic

¹¹For overviews of loan phonology see Kang 2010 and Uffmann 2013.

¹²I give Swedish source words, in standard orthography except that vowel length and non-initial stress are marked.

constraints of Finnish. These exclude tautosyllabic clusters and foot-final *-h. So, with the nativized stress in place and final codas supported by an epenthetic vowel, *hv* is syllabified as a coda and onset to the extent that these constraints allow, and any unsyllabifiable consonant is dropped: *konhvehti, sahyrami, uni hvormu*.¹³

2.2 Sound change

All empirical and theoretical work on historical phonology must come to grips with the questions in (5).¹⁴

- (5) a. *The constraints problem*: are sound changes always natural, or can they be arbitrary? Is the direction of sound change predictable?
- b. *The regularity problem*: Is sound change always regular, or can it be sporadic?
- c. *The implementation problem*: is sound change abrupt or gradual? What is the role of lexical frequency in sound change?

To be convincing, the sound changes posited in historical analyses should be natural, which is not always easy to achieve (Blust 2005, and see Blevins 2007a, 2008b). Intuitions about naturalness go only so far, so they must be grounded in typology, phonetics, and phonology. Naturalness has been a central theoretical concern of Natural Phonology (for a recent overview, see Nathan & Donegan 2013), later pursued also in formal generative phonology by markedness, feature geometry, and models of the phonetics–phonology interface. Interest in the typology of phonological processes grew further with Optimality Theory, whose commitment to the perceptual and articulatory grounding of constraints and to intrinsic typological predictions attracted many phonologists to phonetic issues under the heading of Laboratory Phonology. From the wealth of recent empirical and theoretical work let us mention Cho 1999, Cser 2003, Barnes 2006, Smith 2005 and Kümmel 2007 (consonant fortition and lenition), Hajek 1997, Labov 1994 and Walker 2011 (vowel shifts, harmony, metaphony), Lahiri, Riad & Jacobs 1999, Lahiri 2013 (stress shifts), Hombert, Ohala, & Ewan 1979, Thurgood 2002, Riad 2003, Ratliff 2013 (tonogenesis), Blevins & Garrett 2004 (metathesis), Blevins 2008b (consonant epenthesis), Blevins 2008c (overview).

The other main reality check on sound changes is that they should be regular. An even stronger requirement, the NEOGRAMMARIAN HYPOTHESIS, is that sound changes are exceptionless, and conditioned only phonologically. The best argument for the neogrammarian hypothesis is that phonemes don't split spontaneously. Rather, new contrasts arise when the conditioning environment of allophones is obscured by other sound changes (SECONDARY SPLIT). As Bloomfield noted, if the hypothesis were false, then languages would have huge incoherent phonological inventories, littered with stray sounds and clusters left over from sporadic or non-phonetically conditioned sound changes at various stages of their history, e.g. laryngeals, pitch contrasts, [ç], [β], [b^h], [œ], /d^hg^{wh}-/, /mn-/, /kn-/ in English.¹⁵

¹³There is no reason to believe that *hv* was first added in full and later reduced to accommodate the syllable structure requirements. Rather, *hv* is an abstract target that the language approximates as best it can. When this strategy does not produce a syllabifiable sequence, the labial is minimally modified to permit syllabification. *-fs-* cannot be accommodated as **vs* or as **hs* since neither cluster is permissible; hence *tofs* → *tupsu* 'tuft'.

¹⁴For more on these questions, and on sound change in general, see Salmons 2010 and the chapters by Garrett and Hamann in this volume.

¹⁵Irregular sound developments can arise through spelling pronunciations (Sloos 2013), effects of word associations, and taboos (Newman 1996).

The neogrammarian hypothesis nevertheless continues to be questioned on the basis of two sorts of phenomena. The first is based on variable phonetic realization. The second is based on word by word phoneme replacement. We take them up in turn.

The more common a word or phrase, the more reduced its pronunciation. The reduction can be an imperceptible phonetic effect of a few milliseconds, or neutralization to a categorically distinct pronunciation, as in the often cited example of English vowel syncope (Bybee 2007):

- (6) High frequency word: *every* [∅]
- Mid frequency word: *memory* [∅ ~ ə]
- Low frequency word: *mammary* [ə]

SPEAKER-BASED explanations for such frequency effects hold that articulatory targets become more automatized through use (Bybee 2001, Pierrehumbert 2001, 2002). LISTENER-BASED explanations say that frequent words are more predictable, so speakers can put less effort into their articulation without risk of being misunderstood (Jurafsky et al. 2001).

In addition to frequency, such variation is sensitive to morphological and phonological factors, style, social class, gender, etc. All this is entirely compatible with the neogrammarian hypothesis. Structured variation is not in itself sound change; it can persist for centuries and even millennia.

As a sound change, syncope dates back to Old English, where it was phonologically conditioned by stress and syllable weight, conforming perfectly to the neogrammarian hypothesis (Sievers-Brunner 1942: §158-159, Campbell 1983). Once we look at the Old English change itself, rather than at the synchronic variation which it bequeathed to Middle English, and which remains productive in Modern English, we see that far from falsifying the neogrammarian hypothesis, it strongly supports it. To test the neogrammarian hypothesis one needs philologically interpreted textual material from the relevant period, or sociolinguistically aware field work on ongoing sound change (Labov, Rosenfelder & Fruehwald 2013). After a thousand years, a variation pattern does not necessarily look like the sound change that originally caused it.

Structure-preserving processes can yield apparent counterexamples to the neogrammarian hypothesis because their isolated outputs can become lexicalized. Many syncopated trisyllabic words which had no synchronic morphological analysis (*marshal, parchment*), or lost it (*poultry, butler*) are now underlying disyllables. This is still compatible with the neogrammarian hypothesis, for lexicalization of reduced forms is not sound change, as has always been recognized. In transparently suffixed words, on the other hand, such as *mammary, cursory, generative, temporal, cidery, buttery, cobblery, clownery, cookery*, the morphology gives evidence of their medial vowel even if it is deleted. Their trisyllabic underlying form can be acquired (“analogically restored”) even by speakers who have only heard the syncopated form, and remain subject to variable syncope indefinitely.

A related challenge to the neogrammarian hypothesis is LEXICAL DIFFUSION (Chen & Wang 1975). Its status remains controversial. Many of the instances of lexical diffusion cited in the literature are frequency effects on variable synchronic reduction processes similar to syncope, and can be explained the same way. Phillips (2001, 2006, 2013) argues that there are also sound changes that conversely affect the least frequent words first, as well as sound changes that affect the members of some word class first. Importantly, these are not reduction processes, and appear to be always discrete and structure-preserving. Such word-by-word redistribution of phonemes in the lexicon of a language is what is meant by lexical diffusion in the narrower sense (Labov 1994: 542,

Kiparsky 1995). It characteristically eliminates marked values of marginally contrastive features, attributable to a learners' bias to simplify weakly entrenched contrasts in lexical representations (Bermúdez-Otero 2007). In that respect, it is arguably more akin to analogical change than to sound change, albeit not necessarily to analogy of the proportional kind (Kiparsky 1995). The spread of “diatonic pairs” from nouns such as *tórmènt* and *rébel* (derived from *tormént*, *rebél*) to new cases, such as *áddict* from *addíct*, often cited as a case of lexical diffusion, is obviously ordinary proportional analogy. The spread of accent retraction in *nonderived* nouns like *mustache*, *garage*, *massage*, *cocaine*, which does not extend an alternation, but simply regularizes the word's stress pattern, is a *nonproportional* counterpart of the same analogical process. For example, after [mə'stæʃ] is replaced by ['mʌs,tæʃ], its final stress need no longer be registered in its lexical entry, which simplifies the word's lexical representation. Except for being nonproportional, it is no different from the generalization of accent retraction to deverbal nouns like *áddict*. Both instances of the change remove individual exceptions to the rule that nouns bear main stress on a heavy penult, just as the morphological regularization of *kine* to *cows* removes an individual exception to the plural rule.

2.3 Analogical change

This approach to lexical diffusion fits hand in glove with a conception of analogical change as a process which eliminates arbitrary complexity from the grammar — as grammar optimization, or simplification if you will. This represents a break with the received view inherited from the neogrammarians, who introduced four-part proportional analogy as a way of doing both synchronic and historical morphology without having to posit anything as abstract as morphemes or grammatical rules. This project did not work out (Morpurgo Davies 1978, 1998): proportions sometimes have to be built on “grammatical abstractions” rather than on actual word forms, some analogical processes are not representable by proportions at all (“non-proportional analogy”), and even with these enrichments proportions predict impossible analogies and fail to account for some actual ones. Kuryłowicz solved some of the problems by stipulating that the terms of proportions have to be stripped of “redundant” morphemes, that the effects of automatic phonological rules have to be undone, that they can be categories rather than particular items, that the left hand side must contain *formes de fondation* and the right hand side the corresponding *formes fondées*, as well as several other “Laws of Analogy”. It was also found that proportions can operate on distinctive feature representations (Garey 1959) and that several proportions must be allowed to interact in a single analogical change (Leed 1974). See further Albright 2008, Garrett 2008.

The outcome of these revisions was that the proportions themselves were left with little work to do, and the substance of the theory came to reside in the constraints imposed on them. When generative research showed that language structure and use is based on rules rather than on proportions, a natural move was to try eliminating proportions as a mechanism of analogical change as well, and see it as a process that eliminates unmotivated complications. Coupled with some reasonable assumptions about the mental representation of grammar, this turned out to predict the effects of Kuryłowicz' extensions and to unify proportional and non-proportional analogy. It also provided a plausible acquisitional mechanism for analogical change: complications are eliminated by failing to be acquired by learners at some point. Moreover, the natural morphologists' principles of preferred morphological organization (system-congruity, uniformity of inflection classes, avoidance of allomorphy, compositionality, and coincidence of formal marking with morphose-

mantic markedness, Wurzel 1989) could all be seen as characterizations of morphological simplicity, hence as targets of natural morphological change (Kiparsky 2000a).

It has been claimed that the idea that analogy is simplification or optimization implausibly requires that learners must “first correctly acquire the target grammar (so they can evaluate its complexity), and then . . . replace the acquired grammar with a simpler one . . . despite the fact that some of the speakers in the environment do have such grammars” (Reiss 2003: 150). Actually it just requires, like all learning theories, that learners have a preference ranking of grammars and that they move from simpler grammars to more complex ones to the extent needed to home in on the target language. Change occurs when some aspect of the target language is never acquired.

The optimization approach does face a real obstacle, however. Changes which in the end simplify the language can pass through quite messy intermediate stages. For example, the reduction in Sanskrit of a complex and arbitrary morphological subsystem of phrasal co-ordination to ordinary compounding passes through some *even more complicated* intermediate stages before reaching its simple goal (Kiparsky 2010). Such “bumpy rides” pose a challenge for the proposal that analogy is a simplifying or regularizing process, and indeed for almost *any* causal theory of change. Many of them can be explained by the assumption that a complex subsystem of grammar cannot be dismantled in one fell swoop, but only in minimal steps. Formally, this idea can be reconstructed in Optimality Theory by positing a modular level-ordered organization in which morphology and phonology are interacting subsystems. Change can then be modeled as the promotion of constraints within grammatical subsystems through a series of local optima. Although this works well for many cases of long-term drift which follow a similar “complex → even more complex → simple” path, there remain recalcitrant cases to be accounted for.

2.4 Grammaticalization and semantic change

GRAMMATICALIZATION is morphosyntactic and semantic change that is endogenous, but which, unlike analogy, is not based on any pre-existing patterns in the language, and gives rise to new grammatical categories (for a recent survey see Narrog & Heine 2011). These categories can express functional content previously not expressed in the language, such as new tenses or moods, and they can be new formal categories, possibly for old functional content, as when postpositions turn into case endings, or fixed word order replaces morphology as the mark of grammatical relations. I’ll refer to the two kinds of grammaticalization as FUNCTIONAL ENRICHMENT and FORMAL RENEWAL, respectively. They often go hand in hand (new form for new function), as when free pronouns turn into pronominal clitics and end up as agreement affixes.

Like sound changes and analogical changes, grammaticalization processes have a characteristic directionality: “lexical categories become grammatical and grammatical categories become more grammatical” (Kuryłowicz 1965, Hopper & Traugott 2003:xv). Functional enrichment depletes lexical items of their semantic and interpreted features and eventually reduces them to purely functional elements with only uninterpreted features (Roberts and Roussou 2003, van Gelderen 2011, this volume). Formal renewal results in reduced segmental content and/or tighter prosodic bonding. These generalizations amount to the famous unidirectionality hypothesis. Changes in the reverse direction obviously occur, such as the change of genitive *-s* from an ending to a clitic in English and continental Scandinavian. Some researchers dispute their relevance to the unidirectionality hypothesis, on the grounds that they don’t create new categories but analogically generalize already existing constructions of the language (Plank 1995, Kiparsky 2012). Others consider them

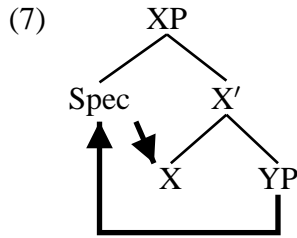
“degrammaticalizations” that weaken the unidirectionality hypothesis or falsify it outright (Norde 2009, Harris and Campbell 1995: 336-338, Joseph 2001, Newmeyer 2001); see Börjars & Vincent 2011 for discussion of the issue.

The exciting thing about grammaticalization is that it reveals the language faculty at work. Formal renewal engenders new categories that conform to cross-linguistic generalizations regardless of their source. For example, complementizers work like complementizers whether they come from prepositions, pronouns, or verbs. Functional enrichment follows particular pathways (surveyed in Heine & Kuteva 2002 and Narrog & Heine 2011) which invite semantic and pragmatic explanation. In SUBJECTIVIZATION processes, extensively studied by Traugott, deontic modals (such as *will*, *must*) acquire epistemic uses, as do verbs (*it promises to rain*), and expressions for objective facts become subjective descriptions of beliefs, attitudes, perceptions and evaluations, *apparently* ‘plainly, openly’ → ‘to all appearances’ → ‘as far as I can tell’, *surely* ‘certainly’ → ‘I should think so’, (Traugott 1989; Schwenter & Traugott 2000, Traugott & Dasher 2002). Also remarkable are the pathways to tenses (Nicolle 2012). *Go*-type verbs are recruited to mark intentional/agentive future, and *come*-type verbs for unintentional/nonagentive future, both of which may then be generalized into ordinary futures (Hilpert 2008). Another trajectory goes from location to present tense: expressions denoting location are the source for focalized progressives (e.g. Finnish *olin lukemassa kirjaa*, literally ‘I was in-reading the book’), which denote a point of time, and therefore are incompatible with stative predicates, and cannot be modified by phrases denoting extent of time. These become ordinary progressives, as has happened in English. Progressives in turn become imperfectives, and these finally end up as present tenses. The denotation at each stage is a superset of the denotation of the preceding stage (Deo 2009, 2012, this volume). In a parallel trajectory, resultative markers generalize to markers of perfective aspect, perfect tense, and finally past tense (Bybee, Pagliuca & Perkins 1994; Dahl 1985, 2000, Condoravdi & Deo 2008).

A unified explanation for grammaticalization, especially for functional enrichment, has proved a tall order. Several avenues, partly competing, partly addressing different aspects of the phenomenon, are being pursued. *Pragmatic* approaches were prefigured by Meillet’s idea that grammaticalization is due to the renewal of the expressiveness of speech forms, which has an intrinsic direction because it is a constant factor in ordinary language use. Eckardt (2006, 2011) sees grammaticalization as a type of reanalysis in which hearers/learners who correctly grasp the intended meaning of an utterance arrive at a novel compositional derivation of it, either by redistributing the semantic content over its constituents, or by reassigning the force of a pragmatic inference of the original state to an overt linguistic element in the innovative state, first by way of conventional implicature, then as semantic content. The scenario seems plausible, but would need to be enhanced with an account of which bits of pragmatic interpretation get semanticized and why, and why grammaticalization is unidirectional. A *semantic* approach is taken by Condoravdi and Deo (2008), who show that the resultative-to-past and progressive-to-present paths are characterizable as successive stages of semantic generalization. Kiparsky (2012) proposes that semantic generalization and phonological depletion are instances of simplification driven by the learner’s search for the optimal grammar. *Syntactic* accounts were pioneered by Roberts & Rousou’s (1999, 2003) parameter resetting model, according to which a learner faced with ambiguous evidence for a parameter setting reverts to its default value, causing a lexical category to raise to a functional head position and to become reanalyzed as base-generated there.¹⁶ In the same spirit, van Gelderen (2011) postulates that Late Merge drives elements to higher functional heads, either

¹⁶For developments of this approach see the articles in Batllori et al. 2005 and Galves et al. 2012.

from a lexical head or from a lower functional head,¹⁷ and Feature Economy requires minimizing semantic and interpretable features in the derivation, motivating the reanalysis of specifiers as heads. For example, pronouns can lose their interpretable person and number features and turn into agreement morphemes, bearing only uninterpretable features:



Recently Traugott & Trousdale (forthcoming) have reconceived grammaticalization as “constructionalization” within a Construction Grammar approach (see also Bergs & Diewald 2008, Trousdale 2012).

The loss and renewal of grammatical and semantic categories is a long-term cyclic process in language change (van Gelderen 2011), e.g. Latin *cantabo* > *cantare habeo* > French *chanterai* > *je vais chanter* ‘I’ll sing’. In pragmatically triggered “inflationary” change, overuse of affective or emphatic elements causes their semantic “bleaching”. Recently well-explored cases include the renewal of hypocoristics, polite forms of address, and negation (JESPERSEN’S CYCLE, e.g. Kiparsky & Condoravdi 2006, Schwenter 2006, Breitbarth 2009, Willis 2012, Romero 2012). There appears to be a similar cycle on the affirmative side, which deserves study. Early English had two versions of ‘yes’ and ‘no’, depending on whether the question was affirmative or negative:¹⁸

	Affirmative answer	Negative answer
Affirmative question	yea	nay
Negative question	yes	no

The more forceful *yes*, *no*, historically formed from *yea*, *nay* with a strengthener, replace the plain affirmation and negation.

To the extent that languages can be assigned to overall morphological types, they seem to undergo a cyclic development from isolating to agglutinating to fusional morphology, and from there back again to isolating morphology (an idea that goes back at least to Gabelentz). For example, English is close to isolating, its Germanic ancestor was fusional, Indo-European was agglutinative, and internal reconstruction from Proto-IE suggests an antecedent more isolating stage.

2.5 Syntactic change

The development of explicit models of syntax and historical corpora has made it possible to track the time course of change with new precision, bringing the actuation problem in syntax closer to a

¹⁷See also Longobardi 2001 for a formal analysis of the origin of the French preposition *chez* from the noun *casa* ‘house’.

¹⁸“*No* answereth the questyon framede by the affyrmatyue. . . yf a man sholde aske..is an heretyke mete to translate holy scrypture into englyshe..he muste answere *nay* and not *no*. But and yf the questyon be asked..Is not an heretyque mete to translate holy scripture into englysh. To this questyon..he muste answere *no* & not *nay*.” (Thomas More, 1532). The affirmative contrast is illustrated by this example from the OED: Thynk ȝe not he is worthy to day? ȝys! ȝys! ȝys! Alle we saye he is worthy to day, ȝa! ȝa! ȝa! (1450)

solution.¹⁹

It is widely observed that the spread of a linguistic change through a speech community follows an S-shaped curve: innovations spread slowly at first, then fast, then go slowly again to completion. But how does change spread in the grammar? The weakest hypothesis would be that innovations spread independently in each context. Many scholars have taken for granted the natural assumption that innovations originate in favoring contexts and spread more quickly in them, reaching the least favoring contexts last. But surprisingly, corpus studies of syntactic change show that “when one grammatical option replaces another with which it is in competition across a set of linguistic contexts, the rate of replacement [...] is the same in all of them” (Kroch 1989). This is known as the CONSTANT RATE EFFECT. The natural explanation for this generalization is that “contexts change together because they are merely surface manifestations of a single underlying change in grammar. Differences in frequency of use of a new form across contexts reflect functional and stylistic factors, which are constant across time and independent of grammar.” (p. 199).

That innovations spread at the same overall rate across all contexts does not in itself tell us which contexts are favored, still less what causes the changes. The actualization of change is probably determined by multiple factors, of which two important ones are SALIENCY and FUNCTION (for a review of other proposed candidates, notably markedness, see de Smet 2012).

The SALIENCY HYPOTHESIS states that actualization is most frequent in those innovating forms that are most like the old ones (Naro 1981, Scherre & Naro 2010). The actualization is “constrained by more or less superficial generalizations based on similarity to established usage” (de Smet 2012). Essentially the same effect has been long known to govern the course of morphological analogy; Hermann Paul observed that Verner’s Law consonant alternations are leveled first in those verbs where they are not accompanied by vowel alternations; e.g. OHG **sluoh* : *sluogun* is leveled to *sluog* : *sluogun* ‘hit’ (3Sg./3Pl.), whereas *zōh* : *zugun* ‘drew’ retains the alternation (Kiparsky 1992).

The FUNCTIONAL HYPOTHESIS states that actualization begins with those contexts where the innovation confers the maximum processing advantage, normally understood as parsing efficiency (minimization of ambiguity, reduction of the burden on short-term memory). Studies of the development of *do*-support in English show that it is most frequent in exactly those cases where it serves to keep the verb and its object together: in transitive questions and transitive negative declaratives, with NP objects more than with sentential objects, and more in adverbial Wh-questions and yes-no questions than in Wh-questions with fronted objects (Kroch 1989). All these asymmetries follow directly from the parsing hypothesis. For example, verb-object adjacency favors *Whither dost thou take it over Whither takest thou it*, but is not a factor in the choice between *Whither dost thou go* and *Whither goest thou*. Warner 2004 suggests that the parsing advantage drives the whole change, with the other contexts being carried along because *do*-support is a single grammatical process, which is plausible since the parsing-based preference actually hardened into a *grammatical* requirement in late Middle English.²⁰

The Constant Rate Effect has to do with the frequency distribution of competing forms across contexts. It should not be taken to exclude the possibility of syntactic change in an orderly sequence of discrete steps, each constituting a possible grammar. Large-scale change of this type is

¹⁹For historical syntax in general, see the chapters by Frajzyngier, van Gelderen, and Barðdal in this volume.

²⁰Other functional advantages might include production efficiency (minimization of speaker effort) and computational efficiency, in Minimalism syntax expressed as a preference for the shortest derivation, minimization of long-distance dependencies by the shortest move, MERGE >> MOVE etc.

called DRIFT. An instructive miniature-scale example is the reanalysis of the nouns *key* and *fun* as adjectives (de Smet 2012: 624). The new adjective *key* was at first categorically restricted to attributive position (*a key first step*) and began to be used predicatively several decades later (*this step was (absolutely) key*). The new adjective *fun*, on the other hand, was at first categorically restricted to *predicative* position, and became available for attributive use some six decades later.²¹ De Smet’s explanation is that the reanalyzed adjectives remain favored in the positions where the reanalysis took place, “mimicking the syntactic behavior of [their] source item” (p. 628). He posits a single global (though not necessarily abrupt) noun-to-adjective reanalysis, followed by gradual actualization of the new adjective, beginning with the attributive environment for *key* and the predicative environment for *fun*. The idea that the change is actualized faster in environments that resemble earlier usage contexts is inconsistent with the Constant Rate Effect, and raises the further question how, if *key* and *fun* are just adjectives, they preserve the diachronic “memory” of their locus of origin. But de Smet’s insight can be reconciled with the Constant Rate Effect by assuming that the nouns became adjectives in two discrete stages. *Key* first became an obligatorily prenominal adjective, joining the many semantically related members of that class such as *main*, *prime*, *premier*, *principal*, *chief*, *cardinal*, *top*, *ace*, *crack*. In the second step, it became a regular adjective available for predicative use (as *prime* had done in earlier British usage, see the OED’s sense 1c). *Fun*, on the other hand, first became an obligatorily predicative adjective, such as *alive*, *alone*, *unable*. In the second step, it was generalized to a regular adjective. On this view, neither word underwent a single global noun to adjective reanalysis followed by actualization in small-scale analogical increments. Both changes took place in two smaller steps (in fact the minimal discrete steps that could be encoded in the grammar without introducing new types of adjectives into it), with across-the-board actualization at each step, consistent with the Constant Rate Effect. At the completion of the change, they quickly acquire a frequency profile typical of adjectives.²²

These data suggest that syntactic reanalysis looks forward to an available attractor structure — an existing category of the language in cases of ordinary analogy, a potential category provided by UG in cases of grammaticalization. Actualization, on the other hand, is conservative and utilitarian. It begins timidly with minimally salient and maximally useful instances of the new structure and leaves the most radical ones for last. As *key* and *fun* show on a small scale, the Constant Rate Effect makes available a new kind of probe into syntactic structure: different actualization profiles diagnose different grammatical analyses.

²¹A search of the *Corpus of Historical American English* and the *Corpus of English Novels* netted as the earliest unambiguously adjectival example *Them air devils threw up their hats ’n’ stomped ’n’ hollered powerful, es ef ’t were mighty fun to see a man cut t’ pieces* (in a fictional dialect narrative attributed to a “pure-bred Yankee” in Irving Batcheller’s novel *D’ri and I*, 1901), followed by *rather fun* (1907) and *quite fun* (1908). Adjectival prenominal *fun* is first attested in 1959 (*a fun evening*).

²²There is considerable variance among adjectives, and *fun* falls well into the range. In the COCA Corpus, 29.1% of a total 134 occurrences of *very fun* are preadjectival. For a sample of other adjectives after *very*, the corresponding figures are: *entertaining* 15.0% (107), *amusing* 23.1% (69), *funny* 24.4% (first 1000 hits), *cool* 24.6% (552), *enjoyable* 28.0% (57), *sweet* 29.0% (548), *loud* 40.0% (392), *intense* 49.6% (440), *rough* 64.0% (269). Figures for *rather* and *quite* are similar. Note that examples like *John is fun (to talk to)* are *not* unambiguously adjectival, cf. *John is a nuisance (to talk to)*.

3 Causes and mechanisms of change

3.1 Stability and change

Textbooks of historical linguistics begin with the causes of linguistic change, but they don't address the prior question of the causes of linguistic stability. Yet the fidelity of 'normal' language transmission is more surprising than the failures of transmission that result in 'change'. Keenan's (2009) dictum that "things stay as they are unless acted upon by an outside force or DECAY" (INERTIA) expresses a two-part mystery. The first part of the mystery is how a language *can* be acquired. It first posed in its full generality by Chomsky, and remains the central motif of formal linguistic theory. The second part is why learners *bother* to reproduce their ambient social and regional dialects so accurately. Saussure was perhaps the first to raise this question, and he looked to his cardinal principle of the arbitrariness of the sign for an answer. But he was not really able to explain why arbitrariness requires such accurate reproduction. Anyway, Saussurean arbitrariness is only relative: there *is* a good reason to prefer **goed* over *went*, and yet exceptions and seemingly useless complications like *went* are ubiquitous and can persist in a language for millennia. An alternative explanation for inertia is that communicative efficiency requires some degree of complexity and/or homogeneity, which together inhibit change. The problem with that is that the complexity of languages, and the linguistic homogeneity of a speech community, exceed any communicative needs: speakers of different regional and social dialects often understand each other perfectly. Perhaps the most plausible explanation, due to Klein & Perdue 1997, appeals to the function of language as a marker of social identity. Exceptions and idiosyncrasies make the best social indicators, so they are needed in languages (except in contact languages and artificial languages, where perfect regularity is acceptable and even desirable), and they must be shared by all members of the community.

But then, if learners have both the capacity and the motivation to acquire an exact replica of the language around them, how is change even possible? This has been called the logical problem of language change. It is particularly acute for theories which attribute language change to imperfect learning. A prominent family of such theories posit that change arises by covert REANALYSIS of ambiguous structures followed by overt extension of the new structures to unambiguous cases. In the covert phase, learners acquire a new grammar (an I-language) that generates an output (an E-language) which is indistinguishable from the old one. The covert phase is followed by an actualization phase in which the new grammar becomes detectable in recognizably novel outputs (Langacker 1977, Harris & Campbell 1995, Roberts 2007). The question then arises how, if the old grammar is indistinguishable from the innovating grammar at the covert stage, the speech community comes to converge first on one, then on the other.

One possible answer is that change does not occur by reanalysis, but in the use of language by fluent speakers, triggered by pragmatic factors, intrinsic variation, and speech errors (Beckner and Bybee, this volume). These are undoubtedly real sources of change. But there are a number of good reasons to believe that acquisition plays a role too: (1) Changes involving structural discontinuities are unlikely to be initiated by mature speakers, (2) children's evolving grammars show features which are identical with actual or possible historical innovations of the target language,²³ and (3)

²³For example, some languages allow pronominals with coargument antecedents *except when they are quantified* (bound anaphors). This system emerges spontaneously in English child language. Grimshaw and Rosen (1990) report that some children accept sentences like *Is mama bear washing her?* but reject *Is every bear washing her?* Kroch 2001 notes the spontaneous emergence in German child language of Yiddish-style syntax, with embedded clauses with V2 and Topicalization. On the relationship between phonological acquisition and change see Cook 2006.

change is accelerated in social situations where the capacity or motivation for accurate reproduction is weakened.²⁴

Many proponents of reanalysis theories solve the logical problem of language acquisition by assuming that changes are caused by prior changes in learners' triggering experience. Some locate the trigger in E-language (in line with Saussure's view that change originates in *parole*), and assume that random fluctuations in usage may cause the frequency of a piece of primary linguistic data to drop below a hypothetical learnability threshold, after which it fails to be acquired and disappears, or causes a grammatical parameter to be reset (Lightfoot 1991:67-68, 1999, Clark & Roberts 1993, Hale 1996: 127). The appeal to hypothetical E-language precursors of I-language change is however inconsistent with the discovery that variation (or "grammar competition") is systematic, and is governed by the same constraints that underlie categorical regularities of I-language (Anttila 1997 *et seq.*). Others seek the triggers in prior changes in I-language, which could themselves ultimately be triggered by language contact or perhaps other factors.

Reanalysis theories have been criticized recently, especially in work on syntactic change. Apart from the triggering problem, their weakness is that on a modern understanding of syntax no general syntactic reanalysis (as opposed to minor changes such as those discussed above for *key* and *fun*) can have a covert stage. The new grammar is bound to be detectable in data that is available to learners early in the course of normal acquisition. Besides, a number of standard instances of reanalysis have been questioned on empirical grounds: nominative experiencer subjects did not arise by reanalysis of the surface dative object as a nominative subject in sentences like *the woman liked those words* (Allen 1995, *contra* Stockwell 1976, Fischer 1987, Harris & Campbell 1995: 63, 83), *for*-infinitivals did not arise by reanalysis of prepositional phrases (Garrett 2012, *contra* Harris & Campbell 1995: 62).²⁵ Whitman (2012) reviews the material and concludes that there are no credible cases of "rebracketing" reanalysis, which leaves "relabeling", essentially grammaticalization, as the only surviving type. But these are precisely the cases where the existence of a covert stage is least plausible. For example, the reanalysis of a relative pronoun as a complementizer or of a verb as an auxiliary can hardly take place without detectable effects in the language.

This suggests giving up the postulate that change passes through a covert reanalysis stage, and allowing the innovative grammar to be distinct from the old one from the beginning, if only in non-salient respects. This is done by BIAS-DRIVEN models, which attribute change to learners' creativity rather than to simple learning failure. Change is steered by a set of prior learners' biases which can be strong enough to override evidence to which the learner has been exposed (Garrett & Johnson 2011, Garrett, this volume, Culbertson, Smolensky & Legendre 2012). Each individual seeks to construct an optimal internalized grammar guided by certain biases. These biases may cause learners to converge on "wrong" innovative grammars, which may spread when they have a sufficient advantage over the old grammar. In effect, this replaces Saussurean change-in-*parole* with the Jakobsonian conception of change as an evolutionary process governed by the same principles that constrain language itself.

The basic arguments for bias-driven models of change over purely error-driven reanalysis models are the following: (1) Change involves not just loss of old ones features and categories, but the

²⁴For example, when population movements or other situations reduce the amount of exposure and/or the indexical value, as in pidgins and immigrant dialects, which are known to undergo rapid simplification.

²⁵I conjecture that it is connected to the late Middle English innovation by which IP becomes an obligatory syntactic projection (Kiparsky 1997). At that point, *to* joins the modals as a nonfinite Infl head (van Gelderen 1993). Being nonfinite, an IP whose Infl is *to* must form a subordinate clause. But a subordinate clause needs a complementizer. This is the function of *for*. Thus the rise of *for-to* infinitives is part of — in fact, *caused by* — a larger syntactic shift.

creation of new ones that have no pre-existing model in the language, such as grammaticalization. (2) Since change is not instantaneous, a causal mechanism is required for its initiation, spread, and completion across environments over many generations — in the extreme case, drift, or long-term unidirectional change. Purely error-driven models would expect a kind of Brownian motion rather than directed drift. (3) Convergence, i.e. independent parallel change in related or unrelated languages, can be explained by acquisition biases but not by the error-driven view. It is really quite remarkable how constraints like the Final-Over-Final Constraint (FOFC, Biberauer, Holmberg, & Roberts 2013) or the Person Case Constraint (PCC, Bonet 1994, Haspelmath 2004, Rezac 2008) emerge and are maintained independently across the languages of the world.

Bias-driven models of change depend on some characterization of relative complexity (“markedness”). A promising new approach uses a generalization of Anttila’s theory of variation, introduced by Riggle (2010). The *r(anking)-volume* of a language *l* under *k* constraints is the number of rankings that generate *l*, divided by *k!*. The *r*-volume of a language turns out to be a good predictor of the direction of change (including long-term drift), consistent with a straightforward learning theory according to which the learner prefers the most *probable* language consistent with previously encountered data.

3.2 How phonology changes

Let us concretize these theoretical choices for phonology. SPEAKER-ORIENTED theories of sound change locate its origin in the inherent variability of speech. Speakers initiate new reduced variants for rapid and easy articulation and hyperspeech variants to achieve perceptual clarity. In a feedback loop, the target of articulation shifts as speakers accommodate to their own and others’ gradually changing outputs (see Wedel 2006 for discussion of possible mechanisms behind this). In this way physical constraints on speech shape sound change and sound systems directly. Abrupt changes such as metathesis are consigned to a different mechanism involving misperception. LEARNER/LISTENER-ORIENTED theories hold that sound change originates when speakers’ acoustic signals are misparsed, either by wrongly attributing phonetic effects to the phonological computation, or vice versa (Ohala 1981, 1993; Blevins 2004, 2006, Hale, Kisser, & Reiss 2006). On this view, *all* sound change is inherently abrupt, because of the discontinuity between speaker and hearer/learner, and the physical constraints on speech constrain sound change indirectly because hearers’ misparses reflect speakers’ implicit understanding of them.

Learner/listener oriented theories posit two types of sound change, HYPOCORRECTION and HYPERCORRECTION. Hypocorrection results from the failure to undo coarticulatory effects. Ohala illustrates hypocorrection with the schematic example of the rise of phonemic nasalized vowels in (8).

(8)		Time 1	Time 2
	production	/VN/ > [ṼN]	/VN/ > [Ṽ(N)]
	perception	[ṼN] > /VN/	[ṼN] > /Ṽ(N)/

The nasalization of a vowel before a nasal consonant is the result of physical constraints of the vocal tract. At time 1, a listener ‘normalizes’ a perceived [ṼN] as intended /VN/. At time 2, the nasal is “weakly implemented”, so that the listener may perceive only [Ṽ], and reconstructs /Ṽ/ as the intended pronunciation.²⁶

²⁶Ohala’s /.../ are not necessarily phonemes, but “intended pronunciations”.

Hypercorrection results from the converse misparsing, which occurs when intended features are perceived as coarticulatory, and the results of such misparses then become a new norm. This mechanism is responsible for dissimilation, among others, and explains why dissimilation is restricted to features which are manifested over long temporal intervals (labialization, aspiration, glottalization, retroflexion, pharyngealization, ‘glottalization’, place of articulation), and does not apply to manner features like ‘stop’, ‘affricate’.²⁷ Unlike what is the case in assimilation, in dissimilation, the conditioning environment can’t be lost at the same time (since the listener analyzes it as the trigger of the putative assimilation process that she undoes, i.e. imputing responsibility to it for the imagined perturbation). For similar reasons, dissimilation produces no new phonemes.

As in syntax, it is often assumed that reanalysis begins with a *covert* phase (Andersen 1973, 2001). A new abstract representation is acquired which initially converges on a pronunciation which is perceptually indistinguishable from the old one, and the covert distinction is later enhanced and becomes perceptible. The problem again is how learners can ever converge on the first stage of the innovation. If the two pronunciations sound the same, how can they tell them apart? And what makes learners diverge systematically from the language they actually hear?

The schema in (8) avoids this problem by interposing a prior *articulatory* change as a causal factor — the coda nasal is first weakened, and the weakening then triggers the perceptual change that results in distinctive vowel nasalization and complete loss of the nasal. On this variant of the story, the emergence of the phonemic nasal vowel is not simply “the result of an unintended failure of the perceptual process,” but a consequence of a prior change in pronunciation. This version of the listener-based account nevertheless differs from a conventional articulatorily driven account of phonologization, where allophones become distinctive when their contextual conditioning is eliminated by sound changes, in that it does not equate sound change with synchronic variation; sound change emerges from variation only when a listener fails to normalize or correct it.

A weakness of perceptual theories is that they do not account very well for neogrammarian sound change. They lead to the unwanted prediction that frequent words should resist sound change because they are heard often enough to prevent misperception (just as frequent words resist analogical remodeling in virtue of being heard often). Hale (2007: 141) addresses this problem by distinguishing between two kinds of misparsing, which we can call MISPERCEPTION and MISANALYSIS. Misperceptions are singular events that happen to specific items on particular occasions, and give rise to sporadic sound change. MISANALYSES are across-the-board reinterpretations which systematically attribute a feature to some other articulatory mechanism than that being used by the source, and give rise to regular sound change. Phonemes cannot split spontaneously by misperception, for learners do not posit new contrasts for the sake of individual misperceived items. So new phonemes must arise by misanalysis. But that actually aggravates the trigger problem: how do learners arrive at a *systematically erroneous* analysis of the language they are exposed to?

A second often noted problem for the perceptual approach is that hypercorrection and hypocorrection are formally symmetrical, so that there is no basis for the unidirectionality of sound changes. For example, there is no intrinsic explanation for the fact that consonants normally palatalize rather than depalatalize before front vowels. Ambiguity sets the stage for reanalysis, but something else must explain the directionality of the change. Assimilation and dissimilation are quite different in other ways as well: dissimilation (by hypothesis, hypercorrection) never gives rise to new phonemes, but assimilation (hypocorrection) does. Such intrinsic asymmetries are not predicted by the theory as it stands.

²⁷Ohala does note counterexamples in both directions: *l:r* dissimilation, and the absence of voicing dissimilation.

Garrett & Johnson (2013) go a long way towards a solution of both problems by integrating the speaker's and the listener/learner's role in sound change and locating intrinsic asymmetries in the speech chain itself. The core of their theory is that "the structure imposed on the phonetic input to sound change, via the directionality of phonetic variation, is a key source of the typological patterns of sound change." Articulatory and perceptual biases direct sound change via motor planning, aerodynamic constraints, gestural mechanics, and perceptual parsing. Each of these bias factors furnishes the basis of different types of sound change.

4 Reintegrating historical and theoretical linguistics

4.1 Explaining change

Serious work in historical linguistics must be built on some explicit and consistent grammatical framework, for there is no theoretically non-committal way of talking about language. Does the rise and loss of verb-second order in Germanic involve a rule? a constraint? a construction? a word order template? a functional head? Does a language become ergative by acquiring an ergative case marker? an alignment constraint? a parameter setting? all of these things? Such choices are dictated by the linguistic theory one adopts. They matter for understanding the history, and that puts historical linguists in a good position to contribute evidence bearing on these choices. In the past historical linguists tended to opt out of the debate and to regard linguistic theory with some suspicion. This is understandable in view of their long tradition, going back to a time when language was analyzed primarily as an inventory of *items* — categories and constructions — rather than as a system of interacting rules or constraints. On top of that lies some unexamined structuralist baggage, including the idea that categories are distributionally defined by segmenting and classifying speech, which underlies the classical phoneme and item-and-arrangement morphology. Recently the more complex dynamic view of language offered by morphophonology, generative grammar, and Optimality Theory has had major impact on historical linguistics, and forced some rethinking of change itself. The flourishing of historical syntax has gone hand in hand with the development of generative syntax. Generative phonology and OT have renewed historical phonology and morphophonology (Bermúdez-Otero & Hogg 2003, Bermúdez-Otero 2006, 2013; see Holt 2003).

Let us illustrate these points with examples from phonology. Any causal theory of sound change has to address the connection between change and structure:

- (9) a. *Secondary split*: how and why do new phonemes arise from allophones? Under what conditions does sound change lead to restructuring of the phonological system?
- b. *The embedding problem*: how, if at all, is sound change channeled by the phonological system?
- c. *Typology and universals*: does sound change explain typological tendencies and universals? And/or do linguistic universals explain sound change?

Bermúdez-Otero (2013) shows that Stratal OT (essentially Lexical Phonology implemented in Optimality Theory) is a good platform for formulating and answering these questions. He shows in particular that the architecture of the phonological component accounts naturally for the life-cycle

of phonological processes. A sound change is PHONOLOGIZED when an automatic coarticulation process come under the control of a phonetic implementation rule. It may then become STABILIZED as a categorical postlexical rule applying in across the board in a phrasal domain. The new rule can then rise from the phrase level to higher strata, first to the word level, then to the stem level, acquiring morphological conditioning and lexical exceptions on its way, and eventually exiting into the morphology and the lexicon.

The classical phoneme has turned out to be something of a straitjacket and has not been helpful for understanding the rise and merger of phonological contrasts. Stratal OT offers a perspicuous account of two types of phonologically relevant units which fall short of being full-fledged phonemes and play a crucial role in these processes: NEAR CONTRASTS (near mergers and incomplete neutralization), which are distinguished in production but in perception (Labov, Karen & Miller 1991, Yu 2007) and QUASI-PHONEMES (Korhonen 1969: 333-335, Harris 1990, Janda 2003, Ladd 2009), such as Russian /i/ and English /ɪ/, which are perceptually salient and specified by categorical feature values but not phonologically distinctive. Classical phonemics equated CONTRASTIVENESS (unpredictable distribution), a *structural* notion, with DISTINCTIVENESS, a *perceptual* notion. Synchronic and diachronic evidence shows that they must be separated, and Stratal OT provides the theoretical tools for doing so. Quasi-phonemes are non-contrastive but distinctive — they are predictable but perceptually salient. Near contrasts are the fourth case: contrastive but non-distinctive (Kiparsky 2013).

	contrastive	non-contrastive
distinctive	phonemes	quasi-phonemes
non-distinctive	near contrasts	allophones

The conjecture is that all phonemes arise as quasi-phonemes, and that all mergers pass through a near-merger stage.

The move from item-and-arrangement morphology to morphophonology is proving helpful in languages with complex word structure, such as Uralic (Nikolaeva 2000) and some of the Indo-European languages. In the latter, the study of word accentuation had been dominated by a PARADIGM-CENTERED approach which reifies various accent types (acrostatic, hysterokinetic, amphikinetic, proterokinetic, etc., see Meier-Brügger 2002/2003 for a summary). The COMPOSITIONAL approach derives the accentuation of words from the lexically specified accentual features of their constituent morphemes, together with a set of general phonological rules or constraints. It is typologically well supported (Dybo 2011, Mamet 2011), and allows a unified analysis not only of athematic primary inflection, but of words of arbitrary complexity including derivation and compounding (Garde 1976, Kiparsky & Halle 1977, Dybo 1981, Kiparsky 2010). As Garde 2011 points out, accent is fundamentally a relational property, not a “thing” that can be segmented and classified like a speech sound. The locus of morphophonological variation and change are not the word accents themselves but the system which assigns them, comprising the lexically specified accentual properties of morphemes and the rules by which the accent is computed from them in the lexical phonology.

Against the structuralist/generative view that the quantitative regularities revealed by variationist work should be dealt with outside the theory of grammar, e.g. phonetics, pragmatics, or “performance” (e.g. Newmeyer 2003), OT work has shown that an important class of them belong in grammar (e.g. Anttila 2003). The two key discoveries that motivate bringing variation into grammar are: (1) it is governed by the same factors that govern categorical regularities, and (2)

the frequency of a variant is proportional to the number of fully ranked constraint systems that generate it. A sound change goes to completion when the relevant ranking becomes obligatory.

If we adopt OT, sound change must be constraint promotion, and the effects of the promoted constraint then necessarily depend on the rest of the phonological system. Shockingly, this calls into question the time-honored formulation of sound changes as context-sensitive replacement processes of the form “A changes to B in the context C___D”. There are in fact many kinds of cases where that schema is unilluminating. For example, if a tone-bearing vowel is syncope, the tone is displaced to the right or to the left, *as dictated by the existing constraints of the language*. Sound changes can also be *blocked* from creating surface exceptions to an existing synchronic constraint of the language. For example, Old English syncope fails to take effect just in those cases where it would create a stress lapse or clash, or a prohibited syllable structure. Technically, such conditions on sound changes can be specified as conditioning factors (C___D in the rule schema), but only at the cost of a loss of the generalization that the conditioning factors are manifestations of active phonological constraints of the language.²⁸

OT also accounts for diachronic CONSPIRACIES, the joint satisfaction of a single constraint, typically prosodic/metrical, by diverse segmental and suprasegmental means. The classic example is the enforcement of the CV syllable canon in early Slavic by coda deletion, metathesis, degemination, prothesis of consonants, and coalescence of C+y clusters and V+nasal rhymes. The categorical elimination of contrastive vowel length in much of West Germanic was implemented by multiple changes in each language, most importantly by open syllable lengthening and gemination (Lahiri & Dresher 1999, Page 2007). In North Germanic, 3-mora syllables were eliminated by deletion of *-j-*, by vowel shortening, and by the insertion of anaptyctic vowels, and prevented from arising by restrictions on processes such as syncope that could have created them.

Modern linguistics also calls into question the construal of the comparative method as an algorithm by which phoneme correspondences are sorted into sets by complementary distribution, each set is associated with a proto-phoneme, and the reconstruction proceeds bottom-up from phonology to morphology and finally to syntax. A corollary of the item-centered operational approach inherited from structuralism (particularly the American variety), it may still have a place in textbooks for pedagogical reasons, but it is as foreign to the practice of historical linguistics as its synchronic counterparts are to grammatical analysis. In real life the data sets required for phonological reconstruction do not drop from the sky onto the linguist’s desk, but must be discovered and augmented throughout the entire process as the analysis grows. New hypotheses emerge at all levels simultaneously. For example, the prefixal cognates between Ket and Na Dene and the phonological correspondences they support could hardly have been established independently of the morphological correspondence between the prefix patterns in (1)-(2). The tables of sound correspondences that students are asked to compile, modeled on synchronic phonemic analysis, can actually be misleading since they do not represent the chronology of sound changes and their interaction with analogical changes, which is often crucial to their formulation. A reconstruction is a theory, not a product of data-processing operations; it stands and falls with the historical explanations it provides for the individual languages.

²⁸On the Stratal OT model (Bermúdez-Otero 1999, Kiparsky 2000b) constraints must interact this way if and only if they are visible at the same level (e.g. the word-level phonology, or the postlexical phonology). Rule-based generative theories of sound change could accommodate such interactions descriptively as “rule insertion” (King 1973), but had no explanation for when they occurred and why.

4.2 Historical explanation

In a reversal of the project of grounding the way language changes in its structural properties as outlined in the preceding section, one could view structure as emerging from change. There are broadly two versions of this historicist program. EVOLUTIONARY LINGUISTICS posits a sparse faculty of language and seeks to derive typological generalizations or universals as emergent outcomes of recurrent historical processes. Another approach starts from a rich faculty of language and limits historical explanation to such properties as overgeneration and frequency (Myers 2002), or to matters of phonetic “substance” (Hale 2007).²⁹ These are both nontrivial research programs which, if successful, will shed light on the language faculty.

Evolutionary linguistics is most clearly articulated by Blevins 2004 for phonology. She allows UG to provide only features and categories, while Barnes 2002 assumes that there are no restrictions at all on possible phonological systems; their structure is wholly explained by the changes that shaped it. Greenberg attributes typological preferences to the frequencies of different change types: “In general one may expect that certain phenomena are widespread in language because the ways they can arise are frequent and their stability, once they occur, is high. A rare or non-existent phenomenon arises only by infrequently occurring changes and is unstable once it comes into existence.” (Greenberg 1978: 75). As Harris (2008) points out, “. . . this only sets explanation forward one step. Then we must ask why these changes are infrequent, and why this construction is unstable.” She suggests that phenomena are typologically rare if they can only arise by a relatively large number of changes, or only under certain conditions (see also Harris 2010, and cf. de Vogelaer & van der Auwera 2010 for a similar idea). Even if all changes and all conditions were equally common, the more of them have to come together, the rarer the result.

Actually parametric and OT approaches already make an analogous prediction on the *synchronic* level: the frequency of a “phenomenon” (“property”, “trait”) is proportional to the number of parameter settings or constraint rankings that must be fixed for it to be manifested in the grammar (see above on ranking volume). Structural complexity (defined in either of these ways) is probably a *better* predictor of rarity than the complexity or rarity of the originating change. The reason is that complex and rare changes can produce typologically unremarkable and simple structures (for example, some of the pathways to ergative case reviewed in Kiparsky 2008), and conversely, simple and common types of change can produce typologically rare and complex structures. For example, consider the English *rarissima* in the Konstanz database (Plank 2006): non-zero exponent for 3rd person agreement but zero for all other persons (no. 34), neutralization of both case and number restricted to second person (no. 70), V2 in declarative main clauses only if the first constituent is an adverbial with strong negative force (no. 81), relative pronoun as the only target for agreement in animacy (no. 84), and a definite article formally distinct from any pronoun (no. 122). As far as I can tell none of these extraordinary features of English have particularly complex historical origins. In general, *every* language has thousands of “properties”, some of which will inevitably be rare or even unique just by statistical necessity.³⁰

The strong evolutionary program of deriving actual linguistic universals from change requires showing that no sequence of possible changes can subvert them, i.e. that there can be no historical path to any grammar that violates them. To my knowledge this has never been achieved for any

²⁹For a range of view on historical explanation in phonology see the articles in *Linguistic Typology* 2006 and Hansson 2008.

³⁰The rare “properties” of pidgins and artificial languages might be the absence of exceptions and the lack of certain expressive resources.

universal, and it is hard to see how it could be. For example, it is easy to imagine banal analogical changes and grammaticalizations that would create exceptions to the inviolable generalizations about pronoun systems presented in Cysouw 2003. Some intrinsic constraints must be preventing them from taking place. In contrast, Greenberg’s and Harris’ project of finding diachronic causes for typological tendencies is quite promising (Kiparsky 2006).

A more dubious kind of historicism appeals to the past to explain apparent anomalies in the present. Heine & Kuteva (2008) claim that the Bulgarian enclitic article (*masa-ta* ‘table-the’) contradicts “grammatical theories expecting demonstratives and determiners to appear in similar (or even the same) position in a syntactic structure” (prenominal *tazi masa* ‘this table’), and they explain the article historically as “frozen” in the former position of the demonstrative. Actually the article is placed after the first available prosodic host on its left, as is normal for a clitic. It would be surprising if the non-clitic demonstrative ever had such a prosodically governed distribution, and H&K give no evidence that it ever did. There is no credible historical explanation here, and the real explanatory work, historically as well as synchronically, is done by the principles of clitic placement (Dost & Griбанова 2006, Harizanov 2012). H&K also explain the Swedish double determiner system as the result of contact with Western European languages with preposed articles. The Swedish system may well have arisen through contact, but it is neither “peculiar”, nor violates any putative linguistic universals (Hankamer & Mikkelsen 2005).

Heine & Kuteva (2008) also propose to explain the supposed absence of a 1PI “we” in Chinese Pidgin English (CPE) by the social context in which CPE developed, in which communication was typically dyadic (Mühlhäusler and Harré 1990: 259). In a social context where “it’s always you against me”, “why bother about a group *we* — at least as a first person plural inclusive category?” According to Hall (1944), CPE had no plural at all. But this is like classical Chinese and classical Javanese (Corbett 2000: 50-51), languages which were hardly limited to dyadic communication.³¹ 19th century CPE texts (Li, Matthews, & Smith 2005) present a more variable and probably more realistic picture, with occasional examples of ‘we’ (e.g. *we tomorrow makee move* ‘we move tomorrow’), ‘they’, and English-type plural nouns.³² Here again the first person pronoun patterns with the other pronouns and the nouns with respect to number, a quite normal type of language.³³ Either way, the explanation does not go through, and the first person in any case has no special properties.

Mithun’s argument concerns the arrangement of Navajo verb prefixes in a sequence of outer (‘disjunct’) prefixes followed by a sequence of inner (‘conjunct’) prefixes; we saw the core of the latter in (2). She proposes that the reason conjunct prefixes are closer to the stem is that they are older, and claims that this historical explanation has the advantage of also accounting for the phonological and semantic differences between the two prefix layers: the disjunct prefixes consist of whole syllables, drawn from the full phonological inventory of Navajo, with relatively concrete and specific meanings, whereas the inner prefixes are restricted to coronals, glides, and laryngeals,

³¹Also reportedly Mura Pirahã. The obsolete conjecture by Forchheimer (1953) that every language has a separate form for 1PI “we” has long been known to be false.

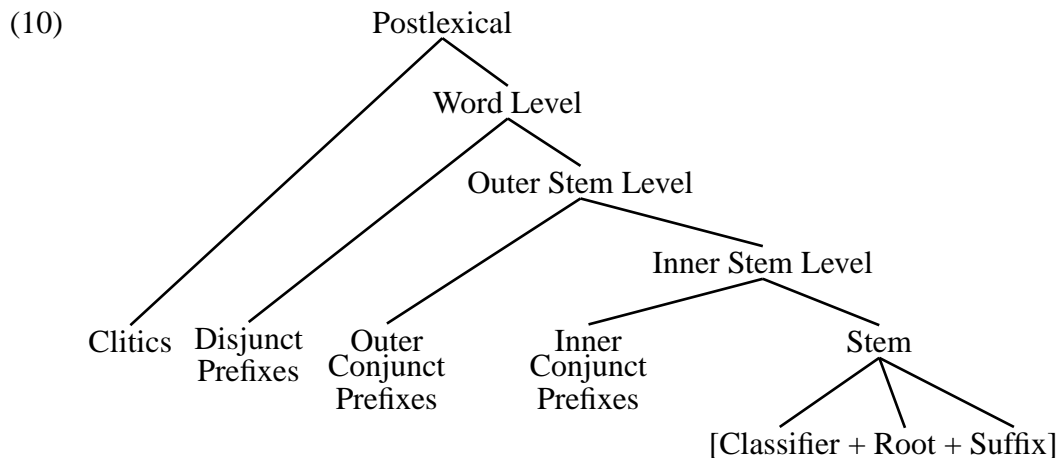
³²Perhaps significantly, all the inflected plurals that I found are irregular, except for *dishu* ‘dishes’. *My* is also used indiscriminately for singular and plural. A third strategy found in the text is periphrasis of the form numeral + *piecee*, and finally inclusive ‘we’ is expressed as a comitative *my (a)long you*. Hall actually acknowledges plural ‘we’ and ‘they’, but dismisses them as ‘Anglicisms’, though that seems to imply the dubious notion of a “pure” pidgin.

³³Note in particular that CPE, on either description, conforms to Corbett’s (2000: 56) generalization that a language’s number marking must affect a top segment of the Animacy Hierarchy, which implies that a language that has any plurals at all must have ‘we’. Since this generalization has the same explanation as other manifestations of the animacy hierarchy such as split case and split agreement, it is a good candidate for a true implicational universal (Kiparsky 2008).

and have abstract and diffuse meanings. This, she says, is because the conjunct prefixes have had more time to undergo phonological reduction and semantic change.

If this leaves you unsatisfied, it is probably because you want an explanation of the chronology itself. *Why* were the conjunct prefixes added first? Would the reverse development have been possible, in which case the disjunct prefixes would come inside in Athabascan (and to be the only ones of Eyak)? *Why* are the morphemes *within* the two prefix complexes ordered as they are? For reasonable answers to these questions we must turn after all to morphology, morphosyntax, and semantics. Operations that determine the grammatical functions (subject, object, etc.) of a verbal predicate's thematic roles must naturally precede operations that depend on those functions, such as agreement. In a lexicalist theory in which words are interpreted cyclically from the innermost affix outward, or in a theory where morpheme order reflects syntactic depth, valency-determining prefixes ('classifiers') are expected to be closer to the verb stem than valency-sensitive prefixes. Further, the reason subject agreement prefixes sit closer to the stem than object agreement prefixes, and were historically added earlier, is that languages normally have object agreement only if they also have subject agreement. This typological generalization is grounded on the principle that agreement elements (like anaphors) are oriented towards the highest (most prominent) available controller — the subject, unless agreement with it is already discharged. The diachrony is explained by the very same principles and preferences that advocates of historical explanation want it to replace.

As for the phonology of the prefixes, the morphological organization of the verb offers a far more illuminating explanation of them than their mere age. Jaker's (2012) Stratal OT analysis of Dogrib, building on the earlier Athabascan work of Hargus 1988 and Rice 1989, shows that the disjunct prefixes are attached at the word level, whereas the conjunct prefixes are added at the stem level (within which Jaker further distinguishes an outer and an inner layer, the latter corresponding to the portion reproduced above in (2)). He derives the phonological difference between the levels from a reversal in prosodic parsing: when the word level is reached, left-to-right iambic feet are replaced by right-to-left trochaic feet.



In general, for a diachronic fact to count as a nontrivial explanation, it should itself follow from some principle. And this principle should be independent of the synchronic fact to be explained. Otherwise historical explanations run the risk of begging the question by presupposing what they set out to eliminate.

5 Conclusion

Research on language change is hard because it requires expertise from many areas within and outside linguistics. More than any other part of linguistics, it needs to connect with neighboring fields such as human genetics, archeology, cultural and physical anthropology, history, and the philologies. Within linguistics, it is situated at a crossroads where almost all branches of the field meet. A historical study might draw on processing and pragmatics, morphology and corpus linguistics, sociolinguistics and syntax, phonetics and formal language theory. Such connections raise some of the deepest foundational issues in the field, and at the same time make the results exceptionally interesting and accessible to the public. The Saussurian firewall between synchrony and diachrony has been effectively breached in research practice, and it may be time to recognize that fact in the academic structure of the field as well. This would involve incorporating the historical dimension into regular syntax and phonology courses, and ultimately breaking down the conventional segregation of historical linguistics into a separate discipline within linguistics.

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