Time for change

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Abstract

In order to get an angle on deep-time historical relationships between languages, beyond what can be fathomed by the Comparative Method, and in order to model and thereby understand the evolution of typological diversity, attention is increasingly being paid to the question of TIME-STABILITY of lexical as well as of grammatical traits. Proceeding by inference rather than through longitudinal study, crosslinguistic distributions have been interpreted as revealing how stable or unstable particular traits are. Despite all methodological sophistication, the conclusions that have been reached about time-stability in this indirect way are alarmingly contradictory. As a corrective, I suggest that this research programme be reoriented and that time-stability be studied directly, namely diachronically.

Within this general context, the particular issue addressed here is the TEMPO OF CHANGE: traits will appear relatively time-stable, not only if they are wholly resistant to change, but also if the tempo of changes affecting them is slow. When this matter is addressed at all, the literature again is remarkably contradictory: uniformitarians would assert that the tempo of change is uniform and diversitarians that it can randomly be rapid or slow.

A particular development, the grammaticalisation of a local adposition 'at' from a noun 'dwelling, home', will be examined in detail here with the aim of determining the length of time this kind of change takes and of comparing its tempo across several languages where it has occurred. Relevant instances are French *chez* 'at' from Late Latin *casa/chiés*; Swedish, Danish, Norwegian *hos* 'at' from Old Norse *hus*; Icelandic and Faroese *hjá* 'at, next to, by, with; of' from Old Norse *hión* 'family, household'; and late Pāli *gē* 'at; of' from Prakritic Indo-Aryan *geha* (with the postposition turned into a suffix in Sinhalese and Maldivian). All four occurrences have indeed taken about the same length of time to reach completion: approximately 400 years, or some 16 generations, 16 cycles of acquisition. I conclude that grammaticalisation of this kind is very slow, and ceteris paribus proceeds at a uniform tempo. I suggest that the most significant factors that can prolong change are that a change is a whole cascade of individual reanalyses rather than elementary and that it diffuses through speech communities slowly rather than rapidly.

1. Real times in historical linguistics

1.1. Signals from the past, 1: Absolute dating

Dates are not a strength of historical linguistics. We compare poorly with historical sciences which are able to date the origin of the Earth, born 4.54 ± 0.05 billion years ago, or indeed the origin of the (our) Universe, which happened as long as 13.798 ± 0.037 billion years ago. (Of which only the very first beginnings of the yet unstable Planck Epoch, lasting from 0 to approximately 10^{-43} seconds, seem somewhat in the dark.) These dating feats of geophysicists, astrophysicists, and physical cosmologists are possible (a) because appeal can be made to immutable laws of nature, (b) because plausible developmental models have been worked out (rarely uncontroversial, but hopefully testable) which can accommodate the dates calculated, and crucially (c) because there are SIGNALS that can be observed and measured long after the events to be dated – such as temperature fluctuations in cosmic microwave background radiation, light curves of supernovae, the decay products in radioactive isotope of meteorite material, and rock layering.

In comparison we have only the foggiest of ideas of the dates of past LINGUISTIC life events. For example, take events such as the origins of major language families that we are reasonably confident about, and allow for a certain inherent fuzziness in the delimitation of "events" of this kind. Even about unusually well documented and intensely studied families such as Indo-European vastly different dates continue to be proposed for the beginning and end of proto-language unity[1] – vast in our dimensions, that is, differing by three millennia or more, for events that only occurred some 5–10 millennia ago. Often we would be lost entirely unless OTHER disciplines lent us a hand which know how to date what falls in their own domains and these domains can plausibly be related to ours. Speech communities will hopefully have produced durable distinctive artefacts or other organic matter, which archaeologists can recover and date for us, courtesy of the decay rate of radiocarbon in dead organisms. (Unless they are older than some 50,000 years, in which case thermoluminescence dating will help.) Also, linguistically significant events in the history of populations – such as ethnogeneses, genocides, movements, splits, and mergers – may have been chronicled in their own contemporary annals or have been reported by historians of their neighbours. When, in our most wildly ambitious moments and heedless of contrary advice from august scholarly bodies, we attempt to date the first origin of grammatical language, we are entirely at the mercy of geneticists, palaeontologists, and physical anthropologists, who are telling us that this must have happened, one way or another, right at the advent of *Homo sapiens* some 100 to 200,000 years before present. Without their expertise, we could not even be as confident as many of us are that all languages ever spoken can indeed be traced back to one singular creation event. Supposing, for the sake of the argument, that linguistic polygenesis might yet turn out to be a possibility, we would not know either, when left to our own devices, how to date any of

[1] Its origin in a Sprachbund would raise the same question.

these several independent origins.[2] For all we could tell, lacking circumstantial information, the birth of a language not derivative of others could have happened 50 or 50,000 or 500,000 years ago. Actually, among those scholars reckoning with polygenesis (of transmitted languages, not necessarily of the capacity for Language) as partly responsible for modern diversity, some envisage the relevant independent origins as very ancient events, having occurred some 100–200,000 years ago and in fact constituting the multiple first linguistic origins (e.g., Nichols 2012), while others posit independent language origins among some groups of humans as late as 45,000 years ago or even later, on the assumption that existing and historically attested language isolates are real loners, not the solitary remnants of families that vanished without a trace and themselves of ultimately Proto-Human origin (e.g., Hombert 2010, Hombert & Lenclud 2014).

Am I exaggerating here? Suppose such a *de novo* language of independent origin would, at the time of observation, boast elaborate inflectional morphology of the flexive type, could it really have originated as recently as 50 years, or two generations, ago? Assuming that the cumulative exponents characteristic of this morphological type have originated through the fusion of forms originally separate[3] – à la French preposition-cum-definite article *au* [o] from *à le* [a.lə] or Norwegian definite noun plural *-ene* from *-er-ne* -PLURAL-DEFINITE.PLURAL (*gutt* 'boy', *gutt-en* boy-DEF.SG.M, *gutt-er* boy-PL, *gutt-ene* boy-PL.DEF; *jente* 'girl', *jent-a* girl-DEF.F, *jent-er* girl-PL, *jent-ene* girl-PL.DEF) – such large-scale fusing of entire inflectional systems would seem to need more time than that. But then, if the choice is an origin at either 500 or 5,000 years ago, we would be at sea again with our homemade linguistic dating efforts: 20 generations of language acquirers and speakers should comfortably manage to get quite some fusing done, while over 200 generations, what had once been fused may well have fallen victim to erosive phonology or imperfect learning and new morphology may have been created and fused once more, perhaps even several times over in repeated cycles of creating, compacting, losing, and re-creating morphology (like the crust of the earth: Plank 1992).

The very question of age is not unequivocal in the linguistic domain, even if we grant that the past events behind the entities which we are asking about are reasonably discretely bounded. What is at issue if a speaker of English is asked such questions as these: How old are your interdental fricatives? Your weak preterite tense? The lack of an inclusive-exclusive distinction in your 1st person pronouns? Your SVO basic clause order? Your nominative-accusative alignments? If not taken aback by the ostensible oddity of the questions, an adult speaker of today's English might, correctly, answer that s/he has used these inflections and syntactic patterns practically all her/his speaking life, with perhaps the interdental fricative, superseding a dental stop of early childhood,

[2] Unless of course we can actually observe such creation events, as in newly devised sign languages in rural deaf communities; cf. Meir, Sandler, Padden, & Aronoff 2014.
[3] Which would not be an entirely realistic assumption, because there are also other, and "faster", sources of cumulation: Plank, Mayer, & Poudel 2009.

as the latest acquisition over her/his lifespan. But the question of the age of linguistic units and patterns could also be asked about successions of GENERATIONS rather than about the lifespan of INDIVIDUAL SPEAKERS – and be calculated in terms of the cycles of acquisition over which they have been reproduced unaltered. In this latter sense the answer would be that nominative-accusative alignment and the dental preterite of today's English are older, being of Germanic or earlier origin, than its interdental fricative and its basic SVO order, both innovated much later, after English had split off from its West Germanic relatives. But again, are linguists able, on purely linguistic grounds and excluding the physical side of written records, to date the historical entry or exit of a phonological, morphological, or syntactic unit or pattern? Lacking circumstantial non-linguistic evidence, could they tell whether interdental fricatives or nominative-accusative alignments were innovated one or 1,000 generations ago?

Ultimately, the difference between us and our colleagues elsewhere who are ever more more successful at dating could seem to be trivial, being a question of SIGNAL PERSISTENCE. An event such as the Big Bang was of such a magnitude that, nothwithstanding its enormous distance in time, it left measurable traces which ingenious physicists can translate into a time frame. Ditto for geological or other events in the material world. We, on the other hand, find ourselves in the unfortunate position of speech sound being so elusive (and equally the movement of body parts in the case of signing) that the most sensitive tools and the most ingenious measurings will not pick up signals that would put dates to past events so ephemeral as utterances. When a speech (or signing) act is over, nothing remains of it – unless recorded on the spot. But the recording of sound waves produced by speech only became possible in the mid-19th century, which is the blink of an eye ago even on a human time scale. The recording of speech through phonographic writing has a longer history, but the oldest signals of this kind, reaching back a respectable five millennia, only bear witness of a very few languages in one small area (Mesopotamia, Egypt, the Mediterranean).

However, it would be throwing in the towel for the wrong reason if we were to give up on distant dating because (unrecorded, unwritten) speech sound lacks persistence. Historical linguistics is not really about the history of evanescent sound and gesture AS SUCH. Its remit is the history of the human MIND, that part of it which is concerned with mental lexicons and grammars – and here the impermanence of speech sound is not an insurmountable obstacle for dating: the question is what is the signal and how to read it. For one thing, to the extent that lexicon and grammar are transmitted genetically, which is probably small but important, there is continuity across generations, and GENETIC dating methods should be feasible, leading us back to very first human or even primate origins for genetically programmed linguistic universals or to the times of relevant mutations. To the huge extent that lexicon and grammar are transmitted SOCIALLY, there is DIScontinuity between minds, but this discontinuity is mediated by speech acts on the basis of which lexicons and grammars are (abductively) arrived at by successive generations. Over cycles of acquisition, the mental lexicon and grammar of every speaker of every generation is linked to antecedent mental grammars and lexicons. There is no principled impossibility of tracing back the histories of

lexical and grammatical units and systems over generations: the difficulty, however daunting, is merely practical. THE SIGNALS FROM THE PAST ARE IN TODAY'S LEXICONS AND GRAMMARS. But one needs to be able to READ them, in order to devise measures of their age. They will need to be read in terms of how primary linguistic data are processed by learners and how the ensuing lexicons and grammars are represented in their minds/brains, enabling speakers to express thought and to participate in communication with those whose speech acts have informed their own lexicon-and-grammar constructing.[4]

1.2. Signals from the past, 2: Relative dating

The poor dating record of historical linguistics sits ironically with spectacular successes in working out RELATIVE chronologies: practitioners of the Comparative Method can tell, often plausibly if not always undisputed, whether one change occurred before or after another, however long ago, given that the relevant changes have left traces.

For example, although historical linguists on their own are unable to oblige when asked to put an absolute date to the ethnogenesis of the Germanic people and the beginning of Proto-Germanic (although non-negligible lexical and grammatical diversity within the family will incline them not to suggest a VERY recent date, on a population-historical time scale), they are able to determine – on purely linguistic grounds: through comparative reconstruction combined with plausible scenarios of change – the relative chronology of several of the changes which gave unity to the Germanic proto-language and distinguished it from other varieties of Indo-European:

- the set of changes referred to as Grimm's Law – first changing voiceless stops to continuants (p, t, k, k^w > f, θ, x, x^w), later voiced stops to their voiceless counterparts (b, d, g, g^w > p, t, k, k^w), last aspirates to unaspirate (b^h, d^h, g^h, g^{wh} > b, d, g, g^w, via continuants) – occurred BEFORE the change referred to as Verner's Law, with continuants getting voiced when word stress fell not on the preceding but on a following syllable (f, θ, x, s > v, ð, ɣ, z);

- the Verner's Law change in turn happened BEFORE word stress was morphologised, which meant prosodic prominence invariably fell on stem syllables instead of varying between stem and ending, as elsewhere in phonologically determined word stress in Indo-European at the time.

[4] Phoneme inventory size as such, for example, if not read in such terms, will be useless as a signal from the past, however near or distant. At any rate, it has not been used for actually DATING the origin of language, but only for LOCATING this event (unconvincingly: see Atkinson 2011 and discussion in *Linguistic Typology* 15(2) by Bybee et al. 2011).

Obviously, differences in the interpretation of the Indo-European and early Germanic consonant system are not immaterial here, and on the basis of different interpretations the Verner's Law change has also been argued to have occurred BEFORE Grimm's Law. The point remains that relative sequences of change events can be determined on purely linguistic evidence, even if that evidence sometimes permits alternative interpretations and diachronic scenarios. In this sense, since nothing else has happened to them afterwards over numerous cycles of acquisition and over the lifespans of individuals, /f/ in English *father* can be said to be older than /θ/ in English *thorp*, which in turn is older than /b/ in English *to bear*, with /z/ in English *chosen* the youngest of the lot (on the assumption that Verner's Law came after Grimm's Law; and there were subsequent further changes changing /z/ to /r/ (rhotacism) and back to /z/ (paradigmatic levelling)).

Even without reconstructing the diachrony of particular languages and their "sound laws", relative chronologies can also be figured out on UNIVERSAL grounds. Naturally, these grounds are sometimes insecure, since universals are not established easily. Here are a few random examples where the supporting universals – as it happens, constraints on transitions rather than on states, and in this sense diachronic – are robust:

- OUTER affixes tend to be younger affixes than coexisting INNER affixes (e.g., Swedish *upptäck-te-s* discover-PAST-PASSIVE); the reason is that, when affixes are created from independent words through univerbation (such as, in the Swedish example, the weak past suffix from the verb 'do' in Proto-Germanic times and the passive suffix from the reflexive pronoun much later), they will not be added inside words close to the stem, but at their margins, where they were when they were still parts of syntactic rather than morphological constructions. (Subsequently, outer affixes may get internalised in order to fulfil semantic scope or prosodic requirements, thereby complicating the determination of relative morphological ages from relative positions alone.)

- DECLENSION classes (relevant only unto themselves) tend to be older than co-existing semantically more transparent GENDER classes (= agreement classes); the reason: declension classes are the relics of, and can only (?) result from, earlier gender classes as these are losing semantic motivation and cease to be involved in agreement.

- VELAR nasals tend to be younger than co-existing ALVEOLAR nasals; the reason: alveolar nasal plus velar consonant are their historical sources, if not the only ones, but by far the most common.

Thus, signals from the past are not necessarily destined never to reach us in the domain of lexicon and grammar, either; but, as interpreted above, they reveal RELATIVE chronologies, not ABSOLUTE dates.

1.3. Why date at all?

But then, isn't relative dating the worthier achievement anyhow? Absolute dating is likelier to grab headlines in the popular press; but does it matter? Why would one want to know when the Universe burst on the scene in a Big Bang, or when Earth materialised, or when life began on that accretion from the solar nebula, or when Proto-Human and Proto-Indo-European were first and last spoken, or when nominative-accusative alignments or the interdental fricative first became part of the mental lexicon and grammar of English or of an ancestral language?

From curiosity. If there IS something to be known, our species is curious to know it. And "when?" is the cardinal question to ask, for whatever occurs in time. And what doesn't? (Other than perhaps the pre-Universe, when time wasn't there yet.) There is probably a cline of inherent interestingness that guides curiosity, and on most people's cline, lay and professional, the question of the origin of the universe – the origin of something, matter, where there was nothing – probably ranks higher than those about the origin of Indo-European or of English interdental fricatives or even of grammatical language as such.

Can knowledge about dates and durations, in addition to quelling general curiosity, also be USEFUL? Well, regardless of more mundane and ancillary uses (knowing dates in one domain may be useful in others), it is useful in at least one inherent vital sense: knowing the periods of time that were available for past states of affairs to have obtained and for past change-of-state events to have occurred affords us a perspective on the historical profiles of developments. For example, given an age of the Earth of between 20 and 400 million years, as calculated by Lord Kelvin in the mid-1800's on the basis of the time it takes for a molten mass to cool down to the current temperature of the Earth's surface, would there have been enough time for life on Earth to originate and diversify? Evolution by Darwinian natural selection immediately militated against such a limited geological time frame. Then the Molecular Clock for measuring the rate of genetic divergence of species and other taxa (suggested by Emile Zuckerkandl and Linus Pauling in the 1960's) would date the ancestor of all living organisms to no more recently than 3.5 to 3.8 billion years ago. While Lord Kelvin's Earth thus proved far too young to accommodate the evolution of life,[5] this would prove to be consistent with an age of the Earth (4.54 ± 0.05 billion years) as later calculated on the evidence of radiometric age dating of meteorite material.

Analogues for uses of absolute dating in linguistics, if perhaps less spectacular, are not hard to imagine. When did a proto-language end, and how much time did the daughter languages accordingly have to diversify to the extent that can be observed? How much time did a linguistic family or a geographical area need to spawn a given

[5] Not to mention Archbishop James Ussher's Earth, whose creation only began on the eve of Sunday, 23 October, 4004 BC – a date arrived at through exegesis of ancient texts, not contemporary observation and measurement.

amount of language diversity? How much time did a language need to produce the alternations that internal reconstruction thrives on? Suppose the first origin of language can be dated to 100 to 200,000 years before present, does this provide the right time frame – or too little time or too much – for the development of crosslinguistic diversity as we know it? Or is what we know of crosslinguistic diversity only a random glimpse of what would have been humanly possible over the time given, had not events, natural or man-made, at some particular time(s) since, decimated whatever diversity then existed, robbing future generations of language acquirers of models and thereby curtailing future diversity? (The Toba supereruption, occurring 73,000 ± 4,000 years BP in Sumatra, and its regional and global aftermath would have been such a catastrophic event, drastically reducing the population of humans to possibly less than 10,000 individuals who could breed and genetically and socially transmit their lexicons and grammars.)

For those seeking to understand the present in light of the past and vice versa, it would not seem entirely pointless, then, to have evidence and measures for calculating linguistic dates and durations.

1.4. Life expectancy of lexicon and grammar

But then, linguists seem comparatively incurious about the temporal profiles of linguistic states and transitions. In disciplines that deal with just about anything existing in time, living or non-living – be it atoms and molecules, cells and bodies, individuals and species, people and their beliefs and artefacts, peoples and their customs and institutions, planets, galaxies, and this universe and perhaps others – questions of life expectancy, longevity, permanence, persistence, immutability and such have long been prominent on the research agendas. What is on OURS that would be comparable? In linguistics, what do we know about the life expectancy – over the lifespan of individuals and across generations – of forms and meanings, constructions, categories, paradigmatic systems, rules and constraints, processes, anything really about mental lexicons and grammars?

Focusing on generational time – that is, diachrony – there are two sets of issues here, about non-change and change, and neither is especially well studied:

- Do some forms, meanings, constructions, rules etc. last longer than others? How long precisely? Are some so time-stable as to be eternal, hence perforce universal across all languages that have sprung from the same source?

- When forms, meanings, their matchings, constructions, rules etc. do change, are some changes faster than others? How long precisely do changes take?

1.4.1. Time-stability

As to the first complex, the lexicon – or rather parts of core vocabulary, as enshrined in Swadesh lists of some 100 or 200 culturally neutral items – was once assumed to be susceptible to serious life-expectancy study. The aim of GLOTTOCHRONOLOGY was to absolutely date splits among relatives and thus to put dates to nodes in linguistic family trees. This was allegedly made possible because lexical turnover – the replacement of lexical items by others to express the same meaning – was proceeding at a constant rate, with around 14% replaced and 86% retained after 1,000 years, or 40 generations. (Just as speciation was proceeding at a constant rate in the domain of organisms, according to the Molecular Clock hypothesis of Zuckerkandl and Pauling, or as constantly as carbon-14 was decaying, enabling the radiometric determination of the age of organic materials.[6]) By counting the ratios of cognates to non-cognates one could tell the absolute date at which the languages concerned had separated. However, that proved a vain hope. The glottochronological constant has been so decisively discredited, and the identification of cognates has proved so formidable or indeed impossible a task without an in-depth expertise in the histories of the languages concerned, that one can only marvel at the recent surge of neo-glottochronological enthusiasm and its gullible reception in high-profile science journals and the general press.

The focus has nowadays shifted from vocabulary retention/replacement rates onto determining the time-stability of INDIVIDUAL lexical items; but the methodological problems about the identification and matching of cognates remain and realistic models and longitudinal studies of lexical change continue to be neglected. Nonetheless, there is some crosslinguistic evidence that some lexical items are time-stabler than others: 'louse/nit' is the current methusalem frontrunner, resisting replacement (or effacement) even longer than low numerals, basic body-part terms, or personal pronouns (according to the Automated Similarity Judgment Program consortium, Brown et al. 2008 etc.). There are lexical fields where turnover is rather brisker, and for a good reason, to do with meaning rather than form: for intensifying adverbs, for example, colour and novelty is at a communicative premium, hence words such as English *very* (< 'truly') or German *sehr* (< 'painfully'), in constant competition with flashier (quasi-)synonyms, are past their prime fast.

Outside the lexicon, time-stability has become a hot issue recently, although in typology ("dynamicised typology", if you will, in Greenberg's terminology) and theoretical syntax more than in historical linguistics. The reasoning is that it helps to account for typological distributions to know how time-stable particular values of variables are: the stabler, the more invariant across related languages. Also, for deep-time aficionados (often amateur linguists with a command of phylogenetic tools acquired in evolutionary biology) time-stable variable-values are more useful than

[6] In parentheses, the Molecular Clock as such would only compare the length of time periods and did not actually enable absolute dating, either: for absolute dating, it needed to be calibrated against dates established on independent evidence, such as the fossil record.

rapidly changeable ones as signals of relationships among languages not accessible by the more conventional Comparative Method, which is as limited as tree-ring dating and does not reconstruct further back than some 8,000 years, when the signal of cognacy fades.

Just about all of the research along these lines – which cannot be surveyed here,[7] but see Plank 2010 for critical discussion – has been INFERENTIAL rather than longitudinal. Distributions of variable-values over the members of language families or genera or also areas at a given time have been taken as the basis to INFER, through various statistical techniques, what is pertinacious and what is transient. There is some agreement among the results of different inferencing methods (e.g., SVO is the stablest of the basic word orders; definite and indefinite articles as well as inclusive/exclusive contrasts are very unstable); which is perhaps to do with the ever more popular use of one and the same database from which the inferences have been made, the *World Atlas of Language Structures* (Haspelmath et al. 2008).[8] But still, there is even more DISagreement over what is supposed to be time-stable and what isn't. For example, ergative-absolutive alignment, basic VSO, a perfective/imperfective aspect contrast, basic lexical valence orientation have variously been inferred to be stable or unstable; syntax is sometimes believed to be inert relative to other modules of grammar, sometimes it evidently couldn't be erter, while morphological change is lagging behind. Despite some basic plausibility, the inferential approach holds little promise of revealing actual DURATIONS of given grammatical patterns even when there is agreement about their stability/instability as inferred from distributions within families, genera, or phyla: we would need to be able to determine the time depths of the families, genera, or phyla concerned to gauge just how much time there was for the pattern to have remained constant or to have changed – but then the nodes in our trees only reflect RELATIVE chronologies of splits in the family/genus/phylum. Also, given some sizable time depth of a family/genus/phylum, identity of a grammatical pattern across its members does not perforce prove diachronic stability: if the relevant changes are relatively fast – and the tempo of change is itself a wide-open question (to be addressed presently) – an impression of stability may well be due to repeated (and isochronous) returns to the same states.

Time-stability has rarely been studied DIRECTLY rather than through inference, that is, by longitudinally investigating the lifespans of individual speakers and sequences of generations of language acquirers – across sufficiently wide ranges of speakers and speech communities to support generalisations – in order to determine how long the values of variables have remained the same, and if they have changed,

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[7] Only one characteristic recent paper shall be mentioned, owing to its exceptional misproportion between rhetorical flourish and phylogenetic sophistication on the one hand and historical linguistic substance on the other: Greenhill, Atkinson, Meade, & Gray 2010.

[8] For some discussion of *WALS*'s problems, besetting any diachronic inferencing based on this typological database, see issue 13(1) 2009 of *Linguistic Typology*, including Plank 2009.

how long it took to change them. This research programme has once been set out by Johanna Nichols (2003: 290) in admirable clarity, and bears full quotation:

> Since stability is never absolute, it can be thought of as the mortality rate or life expectancy of a feature of an ancestral language. It can be modeled as the inheritance rate for ancestor-to-daughter transmission, or (more accurately) as the timespan through which the feature can be expected to perdure in a language family. Life-expectancy distributions are modeled with what is known as *survival analysis* [...]. Survival analysis applied to linguistic transmission would compute, for each element and under each transmission scenario, a probability of loss over a given timespan and the influence of various conditions on this rate of loss. Working out such survival probabilities for linguistic stability even in the broadest terms will be a very large task, for it requires tracing numerous elements of grammar and lexicon through numerous transmission scenarios, each in enough different languages (genetically, structurally, and areally independent) that the proportion of changed and unchanged, inherited and acquired, etc. in each set can be taken with some confidence to represent actual probabilities. This in turn will require thorough comparative and historical work in many different languages of many different families. [...] For instance, a survival analysis of ergativity would gather data from as many ergative languages as possible and determine or reconstruct whether the ancestor was ergative; control for family age to the extent possible; examine clause alignment in every descendant of every ergative ancestor and thereby determine the percentage of daughters that inherit ergativity; determine the effect on this heritability of such factors as having mostly ergative neighbors, having no ergative neighbors, split versus unsplit ergativity, ergativity in different parts of speech, etc.; examine cases where ergative languages have descended from non-ergative languages and determine the percentage of languages that acquire ergativity in the various ways; and other relevant factors. Then we would have a basic understanding of the stability of ergativity.

Ten years later, this programme has largely remained programmatic. Even Nichols herself in subsequent work has preferred to infer time-stability from crosslinguistic distributions rather than, the other way round, to investigate change and non-change over time, with typological distributions then seen to follow from developmental dynamics.[9]

What we meanwhile have, first, is a substantial number of studies of the life cycles or natural histories of kinds of linguistic forms/meanings, constructions, and processes – among others of diphthongs (Stampe 1972), consonantal assimilation and dissimilation (Hutcheson 1973, Johnson 1979), negation (Jespersen 1917 any many

[9] As was early urged by Greenberg 1993: "the basic fallacy [...] is the notion that we can use statistics concerning the relative frequencies of typological features in different areas to reconstruct remote prehistory [...] it is rather the distribution of such typological features [...] that itself requires historical explanation".

others since, culminating in Horn 1989, but not ending there), definite articles and deictics (Greenberg 1978a, Hansen 2004, Clark 1978), infixation (Yu 2007), reduplication (Hyman 2009), or of morphology in general (Hodge 1970, Dahl 2004, Hurford 2009). Also, the time course of sound change (Neogrammarian or other/diffusional, abrupt or gradual) and the life cycle of phonological rules have continued to be on the historical linguistic agenda. But although much light has been shed on developmental dynamics by work such as this, the recognition that certain developments are cyclical or follow some other predetermined course does not answer the questions of what is stable and unstable and why and how fast or slow cycles cycle.

Second, the "pertinacity" of selected phonological and morphosyntactic rules and overt patterns has been investigated diachronically rather than inferentially in a series of studies of which my own work continues to form part (including Lahiri & Fikkert 1999, Lahiri & Kraehenmann 2004, Dresher & Lahiri 2005, Martin 2007, Fikkert, Dresher, & Lahiri 2009, Plank & Lahiri 2009, Breu 2011, Plank 2011, 2012, Butt & Lahiri 2013, Lahiri 2014).[10] However, the focus here was usually language-particular (Germanic) rather than comparative, and the temporal profiles, other than distinguishing what is or is not pertinacious, were not especially detailed on dates and durations.

1.4.2. Tempo of change

Which leads on to the second complex of issues, to do with the TEMPO of change: How long does change take? There are many preliminary problems here, such as that of distinguishing kinds of change which are likely to have different temporal profiles: abrupt vs. gradual, depending on whether there are discrete boundaries or continua for the units concerned; Neogrammarian vs. lexical or constructional diffusion. One major problem is that of individuating changes, distinguishing ELEMENTARY changes and CLUSTERS of interrelated elementary changes. But then, we are at a preliminary stage of inquiry.

The theoretical MINIMUM for a(n elementary) change to be initiated and reach completion has been suggested to be three generations:

- first generation: (group of) individuals innovate
- next generation: variation in the speech community,
  with some following the innovators and others continuing in the old way
- third generation: whole speech community following the innovators

[10] Impulse to this line of research was given by a workshop devoted to pertinacity, convened by Aditi Lahiri at Schloss Freudental near Konstanz in July 2002.

There are actual examples of changes that appear to have taken just three generations to complete: one is the loss of the dual number, (a) in Attic Greek, where dual forms were simply discontinued and plural forms were used instead for reference with cardinality 2, thereby eliminating the morphological category, and (b) in Icelandic, where dual 1st/2nd personal pronoun forms have been retained, being re-employed as informal plurals, with the erstwhile plurals now serving as formal/honorific plurals (Guðmundsson 1972: 90). But then, there are other languages where the demise of that same marked number was drawn out much longer: over 600 years, or 25+ generations, were lying between early signs of precariousness of the dual in earliest Old English (indicated by the use of plural forms where the reference was to pairs) and its definitive discontinuation as an inflectional category of 1st/2nd person pronouns in early Middle English. One dual historian (Cuny 1930: 52) concluded, not without reason, that dual loss, while in the long run always inevitable, can be "*lente ou rapide*", with no measure for tempo given.[11]

What is the MAXIMUM duration of a change? Grammaticalisations – spanning the full developmental gamut of lexical word > grammatical word > clitic > affix – are perhaps the best candidates for slow pace and long-drawn out progression, and they will be our testing ground presently (Section 2). But then, a grammaticalisation event is not an elementary change, but a whole cluster of connected semantic, syntactic, morphological, and phonological changes. Even more composite, because it involves all of a language's inflection, is the entire cycle of synthesis (inflectional morphology) > analysis (function words) > synthesis. Such comprehensive cycling does take time: when it occurred in the history of Old Egyptian > Late Egyptian > Demotic/Coptic, unparalleled as to historical documentation, it took 3,000 years overall (120 generations; Hodge 1970). Episodes of intense contact and especially of pidginisation and creolisation in the histories of erstwhile inflecting languages would no doubt speed up such wholesale exchanges of grammatical technique.

There is no denying that different languages, or the same languages in different periods of their history, can change slower or faster in the sense of undergoing only a few or quite a lot of separate changes. Contact between speech communities and social instability and upheaval within communities are usually considered crucial catalysts and accelerators of wholesale change, basically by virtue of increasing the sheer amount of linguistic diversity facing a speaker/learner and thereby pointing up more options for the future.

But this is a different issue from a rather more fundamental question: Do the SAME kinds of changes always take the SAME amount of time? Even disregarding differences between language change in isolation and in contact that might also make a

[11] An aside: Do INNOVATIONS of categories such as the dual take longer than their LOSSES? They are more frequently observed across languages, within the time window that we have; and if the former took longer this would simply give us better chances of observing such longer-lasting events.

difference here, diametrically opposite opinions continue to be held about that seemingly simple question.

Uniformitarianism suggests an affirmative answer; but here philosophy of science would sometimes seem to be up against historical facts. We have seen above that dual losses can be rapid, getting done with within three generations (Attic Greek, Icelandic), or they can keep 25+ generations of losers occupied (English). Contemporary historical linguists who recognise the problem have remained divided. For example, Kroch (1989: 200) favours uniformitarianism, if we grant that his "constant rate hypothesis" that sees change proceed at the same rate in all contexts can be generalised across languages. The admission that the Comparative Method does not reach back further than some 8,000 years tacitly endorses tempo-uniformitarianism too, insofar as sound change is assumed to comprehensively obliterate the recognisability of cognacy for all cognates for all languages within a period of this same extension. (Even for the noun for 'louse'.) On the other hand, Traugott & Trousdale (2010: 26, 38), with special reference to grammaticalisation, advise that anything should be reckoned with:

> Changes may occur at different times and at different rates [...]
> Sometimes such successive occurrences [of micro-reanalyses] may take place over many centuries, sometimes they may be fairly rapid.

On this diversitarian view, a completed grammaticalisation would offer the historian no hope of firmly dating its beginning. Phonological change has sometimes been seen as equally fickle: "sound change proceeds at very different rates in different languages" (Blust 2007: 40) – although it is not always clear whether the idea is that particular changes can proceed slowly or rapidly or that different changes occur en masse or one by one and once in a while (the latter seems closer to Blust's concept of "hot spots" and "cool spots" of change). Demonstrations of the invalidity of glottochronology by finding different rather than constant lexical replacement rates in the histories of different languages could also be pointing in a diversitarian direction, concerning the vocabulary as a whole. However, little is known about the actual elementary changes, namely the replacement or rather gradual marginalisation of one lexical item by another, gradually gaining supremacy: these might still proceed at a uniform tempo everywhere, regardless of how few or many other words are replaced over a given period.

Who is right about tempo, then, uniformitarians or diversitarians? However unpredictably the rate of overall change may vary across languages and across periods, answerable only to social circumstances (isolation – contact, social stability – instability; cf. Trudgill 2011 for a recent synopsis), some support for uniformitarianism, with hints as to actual durations of change, comes from the comparison of the SAME developments across the histories of DIFFERENT languages. The example chosen here for demonstration is a particular case of grammaticalisation, namely the reanalysis of a noun meaning 'dwelling, home' as a local adposition (and eventually, if a postposition, as a local case). The best-known instance is French *chez* 'at', but there are parallels elsewhere – parallels not only as to initial and end states, but also as to the mechanisms

effectuating the transitions from 'dwelling, home' words and their constructions to 'at' words and their constructions.[12] How long do these developments take? Do the mills of grammaticalisation, when such lexical words are reanalysed as grammatical words in tighter syntactic constructions, grind quickly or slowly, and equally quick/slow in each case in each language undergoing such reanalyses?

2. The tempo of grammaticalisation: From noun to adposition, within 16 generations

2.1. French

The story of French *chez* 'at' has often been told (richest in detail in Lagerqvist 1993), and on one recent re-telling it took over 1,000 years, from ca. 500–1500/1600 CE, for the preposition to complete its metamorphosis from an ancestral noun, Late Latin/Old French *casa/chiés* 'house'. But this long duration is probably an exaggeration of Longobardi's 2001 analysis, which has the chain of change events begin with the lexical replacement of *casa* by *mansione* in early Old French, allegedly freeing *casa* for a "construct state" construction, which itself allegedly persisted over centuries, before the final steps in reanalysis of *chez* as a proper preposition. More straightforwardly, omitting the construct-state arabesque, only SOME 400 YEARS, from the 8th–12th/13th century, were required by SOME 16 GENERATIONS to reanalyse a lexical noun as a local preposition (Lagerqvist 1993, Harrison & Ashby 2003).

To just sketch the whole cluster of changes that had to be executed (following these sources and standard handbooks):

(i) repeated lexical replacements of basic term for 'house, home, place of residence':
  (a) *domus* > *casa* (originally 'cottage, hut'),
  (b) *casa* > *mansione(m)* ('lodging place') / *hospitale(m)* ('guest-chambers, hostel')
  • when? 3rd–5th and 10th–11th century;

(ii) change of word-class category of *casa/chiés*, involving a range of individual inflectional and distributional properties, as well as of phrase-class category of construction headed by *casa/chiés*:
  (a) count noun, with all properties associated with this word class
    > no number and case marking, indeed inflectionally inert;
  (b) loss of the ability to take ADJ modifiers as well as determiners;

[12] This last parallel is important. In the evolutionary modelling of crosslinguistic diversity, different alignment types, also highlighted in Nichols's programme of survival analysis, have variously been inferred to be historically stable or unstable. What may have been contributing to unclarity here is a point that has tended to be neglected: the actual transitions between nominative-accusative and ergative-absolutive (and other) alignments can be of very different kinds (e.g., Plank 1995), and these are of different complexity, hence might be expected to take less or more time to bring about.

(c) loss of lexical gender (feminine), in the absence of modifiers and determiners that could agree in gender;
(d) an NP-complement becoming obligatory;
(e) NP-complement no longer in possessive (genitive, preposition *de*), but in oblique form, with personal pronouns the last to productively appear in this construction (thus, examples like *chez le boulanger* 'at the baker's' and *chez Paul* 'at Paul's' with common nouns and proper names are earlier than *chez lui* 'at his [place]' etc. with pronouns), probably signalling completion of the N > PREP reanalysis on the formal side;
(f) omissibility and increasingly strict rejection of a local/directional preposition such as *à*, *en*, or *de* for the whole construction (as formerly in *en chies son hoste* 'at his host's home' or *je vais à chez les Dupont*, *je viens de chez les Dupont* 'I go to/come from the Duponts' [place]', with orthographic fusion as an intermediate stage: *enchiés(e)*, *achiés(e)*, and with ablative *de* holding out longest;[13]
(g) rigid ordering with the complement after the head (modelled on the unmarked ordering in ancestral N–NP construction in Vulgar Latin and early Romance, as well as on established adpositions, which were prepositional);

– with changes (a)–(g) thus cumulatively severing all morphosyntactic ties *casa*/*chiés*/*chez* used to have with nouns (originally its near-synonyms) heading noun phrases and realigning it with prepositions heading prepositional phrases:

à/*en ma maison, à*/*en mon ostel* ---------- *chez moi*
à/*en la maison*/*l'ostel de NP* ---------- *chez NP*

• when? spread out over 8–12th/13th century, with text frequencies of full-fledged prepositional *chiés*/*chez* uses not rising significantly before the 13th century (Lagerqvist 1993: 18-19, 215);

(iii) discontinuation of the use of *chiése* as a noun (other than in the fixed expression *chiése deu* 'house of God');
• when? by 10th and 11th century;

(iv) irregular phonological change of *casam* > *chiés*, rather than expected **chèse*;
• when? before the 12th century; but not the first change to occur: rather, apocope was licensed by *casa* being standardly atonic, which itself suggests a reanalysis of N as PREP was already incipient;

[13] When the 'home' noun was turned into an ADVERB in Germanic languages (cf. English *home*, German *heim*), the "internalisation" of relational senses was similar, with allative and locative senses prevailing, based on accusative and locative uses of the ancestral noun. (With the locative distinguished through an extra deictic adverb in German: *daheim* 'at home'.) But no NP complement, the hallmark of adpositions vis-à-vis adverbs, was ever acquired by *home*/*heim*: English and German wouldn't replace the prepositions they had, or had adapted, for this purpose (*at*/*to* + NP(*'s*), *bei*).

(v) change, or rather extension (with old senses retained), of the lexical semantics of the word morphosyntactically reanalysed, although an inherently possessive relational meaning was a continuing common denominator (a home is always someone's home):
'house/home (of)'
> '(at, also: to) the location of (somebody's business, typically pursued at home)'
> '(at) somebody('s place)'
> 'people living in the house' (a crucial metonymy)
> 'entire family or lineage' (an ASSOCIATIVE! *Chez Dupont sont venus* 'The Duponts have come' (Spitzer 1942))
> 'entire country of origin'
... > 'at abstract location' (such as literary works: *chez Plutarque* '(in) Plutarch('s writings)');
• when? as long drawn out as the several morphosyntactic reanalyses of N as PREP (abstract location only since 16th century, probably signalling that the 'house' > 'at' reanalysis had been completed on the semantic side);[14]

(vi) protracted competition and negotiation of respective semantic domains of *chez* with Latin *apud* 'at, near, among, in the presence of', which would drop out of most Romance vernaculars, and after continuing as *od* 'with' in Old French was supplanted by *avec* 'with' (< Late Latin **abhoc* < *apud hoc=que* 'at/with this=and').[15]

There were similar, if not all equally successful developments of other local prepositions from locality nouns in French or forms of French, but we cannot here go into the durations of these reanalyses; their time frames were probably comparable, except the last two, which were probably faster:

Latin *ad latus*, *in latus* 'side' > Old French *lez* 'next to';
Latin *per medio* 'through the middle' > French *parmi* among';
Latin *in medio* > Old French *enmi*, ditto;
Latin *de costa*, *in costa* 'from/at the side' > Old French *decoste*, *encoste* 'beside';

[14] For early attestations of these senses see *Le trésor de la langue française (informatisé)*, s.v. CHEZ. Further semantic extensions were necessary to license uses of *chez* in intensifying constructions such as *C'est de la merde de chez merde!* (not in *Trésor,* Harrison & Ashby 2003: 393). Also outside our ambit, *chez* + variable pronoun constructions were subsequently, in contemporary informal French, reanalysed as nouns reviving the old meaning of *casa* 'one's home': *C'est mon chez-moi*; *objets et souvenirs pour recréer un chez-soi dans un chez-nous* (Harrison & Ashby 2003: 395-396).

[15] It seems hard to determine whether the the demise of *apud* contributed to pulling *case*/*chiés* into the system of local prepositions, or the rise of *chez* contributed to pushing *apud* out. At any rate, for the synonymy situation as *chez* was carving out its niche see de Gorog 1972.

Latin *ad/in mansione(m)* > Picard, Wallon *(a/è)mon* 'chez';
French *côté* 'side' > Louisiana Creole, Cajun *kote* 'chez'.
Haitian Creole French *kay* 'house' > *ka* 'at (the house of)' (Hall 1953: 30-31)

2.2. North Germanic: East and West

There are also instances of 'dwelling, home' > 'at' grammaticalisations in other languages that are comparable as to the overall change and its component parts: the question for us is whether they are also comparable as to duration.

2.2.1. Continental Scandinavian

The preposition, *hos* 'at' is found in the Continental Scandinavian Germanic languages Swedish, Danish, and Norwegian (Bokmål and Nynorsk), and its source is the Old Eastern Norse noun *hus* 'house', an *a*-stem neuter. The grammatical and lexical circumstances, hence the semantic, syntactic, morphological, and phonological reanalyses involved, were not very different from the French story as outlined above. But there are also a few differences worth noting:

- *hŭs* > *hŏs* (found in a variety of spellings: *hwoss*, *(h)oss*, *host*, *hots*, *hås*, *hoos* etc.) was a REGULAR phonological change in atonic position, nothing that would specifically distinguish an incipient preposition from (except that prepositions would be even less likely than head nouns to receive phrasal stress);

- the source noun *hus*, which itself was not the original basic term for 'shelter, dwelling, house' in Germanic (**razn* was), but was to assume this status (like Late Latin *casa*), was RETAINED, with the tonic form of the stem vowel and with the core senses of 'home' and 'house', alongside the preposition *hos*, though increasingly alienated from it, and felt to be an unrelated lexical item by contemporary naive speakers;

- the strictly prepositional ordering of *hos* was not strongly motivated by that of *hus* relative to its genitival dependents, which more commonly came before than after their heads; thus, the Genitive–Noun construction (with possessive pronouns either before or after their noun) came to differ sharply from the PREP–NP construction – to illustrate from Norwegian:

kjøpmann-en=s hus	*hos kjøpmann-en*
grocer-DEF.SG.MASC=GEN house	at grocer-DEF.SG.MASC
'the grocer's house'	'at the grocer's'
m-itt hus / *hus=et m-itt*	*hos meg*
my-SG.NEUT house /	at me.OBJ

house=DEF.SG.NEUT my-SG.NEUT ‘at my place’
‘my house’

- the most pertinacious trait of the erstwhile noun *hus* seems to have been the possibility of genitival marking of its complement, when a bare noun and referring to persons, through genitival *-s* (or rather *=s*, with the inflectional affix becoming an enclitic), surviving relatively speaking longer than its French counterpart:
  hos Bergström(=s) ‘at Bergström’s’
  hos prest=en=s ‘at the priest’s’
  analogously, pronouns too are long able to appear in the possessive rather than the oblique form:
  hos mitt, alongside *hos mig*
  (examples in the *Svenska Akademiens ordbok* and the *Ordbog over det danske sprog*, s.v. *hos*);

- presumably through ellipsis of a contextually understood complement, *hos* could at some stage also be used as an adverb meaning ‘near, nearby’ (e.g., *han satte sig hos* ‘he sat himself nearby’).

To go by information as to be gleaned from historical dictionaries and handbooks, the time frame for this cluster of changes in Continental North Germanic was remarkably similar to that of its French counterpart. Beginning in the 10–13th centuries CE, after Old Norse had diversified into a Western and an Eastern group (with Westerly Norwegian acquiring/retaining many an Easterly trait), and completed at the early stages of Old Swedish, Old Danish, Old Norwegian in the 14th–15th century, they took around 400 years, or some 16 generations.

2.2.2. Insular Scandinavian

What happened to *hus* ‘house’ > *hos* ‘at’ in Eastern North Germanic had a close analogue in the West (cf. Noreen 1892: 178-179, Heusler 1932: 144, Magnússon 1989: 331). The insular Scandinavian languages, Icelandic and Faroese (and perhaps Norn, but that is extinct), have a preposition *hjá*, [çauː] and [tʃɔa] respectively, whose senses are locative (‘at (someone’s place)’, ‘beside’, ‘by’), comitative/instrumental (‘with’), and possessive or genitive/dative (‘of’, as in Faroese *húsini hjá mær/Turið* ‘my/Turið’s house’). The source of this preposition is a noun of Proto-Germanic (and earlier) provenance, **hīwą* ‘homestead, household, member of a family’ (cf. Latin *civis* ‘citizen’; possibly originating from a Proto-Indo-European root **key* ‘lie, recline’; *home*/*Heim* is etymologically related, too). It appeared in Old Icelandic as the neuter *n*-stem *hiá*, although this noun usually only occurred in analogically formed plurals *hión(a)/hiún(a)* ‘household, family, man and wife’. The singular form only survived in the prepositional phrase *í hiá* ‘in the household (of)’, which would then shed its locative

preposition *í*, with *hiá* itself deprived of all credentials of nounhood and turned into a dative-governing local preposition, 'at, beside'.

Given that this was a separate development of Western descendants of Old Norse not shared with their Eastern relatives, which opted for grammaticalising a different, though semantically related noun, it cannot have begun before they began to separate in the 10th century. Whereas in the mid-12th century, *hiá* in the expression *í hiá* retains many of its old nominal properties with the exception of its original 'household' meaning in examples such as (i), by late Old Icelandic times, in the 14th–15th century, we see examples such as (ii) where *hiá* on its own is a full-fledged preposition on all semantic, syntactic, and morphological counts:

(i) *En þeir Gizorr fóro, unz þeir kuǫmo í staþ þann í hiá Ölfossvatne*
'But Gizurr and his men travelled on until they came to a place by the side of/beside Ǫlfossvatn' (*Íslendigabók*)
(ii) *á stall hjá konungs hestum*
'to the stable near/by the king's horses' (*Hrólfs saga kraka*)

Which once more adds up to a time frame of some 400 years, or 16 generations, for such grammaticalisations.

2.3. Pāli, Sinhalese and Maldivian

In a further reanalysis of a noun 'dwelling, home' as an adposition, where the elementary reanalyses are more difficult to trace (for me, at any rate), the resultant grammatical meaning was possessive/genitive rather than locative, and the adposition was a postposition rather than a preposition; also, grammaticalisation continued further, with the postposition becoming an enclitic and eventually a suffix. (Which is not a step reanalysers are keen to take with PREpositions, hence the dearth of case PREfixes.) The relevant languages are Indo-Aryan, namely Pāli and its continuation in Sinhalese and Maldivian. To only mention the starting and end points and crucial steps in between (Geiger 1938: 110, Wijayaratne 1956: 142-144, de Silva 1970: 147, Fritz 2002: 55-56):

(i) Pāli *geha* 'house', *gehi* LOC.SG, *geyi* in Sinhalese
(ii) *ge-yi* house-LOC.SG > *gē* house.LOC.SG > *=gē* > *-gē* LOC.SG > GEN.SG 'connected with', with this genitive marker, continuingly added to oblique stems, limited to personal names in early medieval Sinhalese, but extended to animate nouns in later Sinhalese;
analogously, *-ge* in South Maldivian limited to human nouns;
e.g., *goviyā-gē daruvō* farmer.OBL-GEN.SG children 'the children in the farmer's [house]' > 'the children of the farmer's'
(iii) North Maldivian *-ge* GEN of all nouns, completely supplanting the original genitive

The postposition stage had been reached by the 9th century, about a millennium after the first inscriptional attestations of Pāli in Sri Lanka (Prakrit group of Middle Indo-Aryan); it was only in Modern Sinhalese and Maldivian, not before the 19th century, that *-gē*/*-ge* got suffixed to nouns. The noun > adposition part of this grammaticalisation, thus, cannot have been executed brisker than in the French and North Germanic parallels; the adposition > affix sequel added no less than another 1,000 or so years – or in terms of generations: some 40.

2.4. Elsewhere

Four cases, however independent, are not an especially solid basis to generalise from about the tempo of such changes. There are in fact several more such grammaticalisations of a noun 'dwelling, home' as an adposition 'at' on record (cf. Heine & Kuteva 2002: 174-175, 176-177); but their known histories are insufficiently deep or too sparsely documented for purposes of the comparative-diachronic enterprise as envisaged here.[16]

- Akkadian (extinct Semitic, in close contact with isolate Sumerian; 2,900 BCE, in decline since 8th century BCE, last cuneiform texts 1st century CE): *bītu* 'house' > preposition *bīt* 'at' (Stolz 1991: 18)

- Susu (Mande, Niger-Congo): *khönyi* (*khön* + *yi* nominal marker) 'home, residence' > postposition *khön(ma)* (= *khön* + *-ma* multipurpose particle) 'to, toward' (Friedländer 1974)

- Acholi (Luo, Western Nilotic, Nilo-Saharan): *paàco* 'homestead' > preposition *pà* 'at' (Claudi & Heine 1989: 5-7)

- Ngiti (Central Sudanic, Nilo-Saharan): adverb *i̵ bha* 'at home' > postposition *bhà* 'at, with' (Kutsch Lojenga 1994: 154)

- Cagaba (aka Kog(u)i, = Chibchan, Northern Colombia, Arwako branch of Arwako-Chimila, within Kuna-Colombian, within Chibchan B): *hu* 'hut', *hú-vala* 'in front of the hut' > postposition *húvala* 'in front of' (Stolz 1991: 18)

- Chinese (Sinitic, Sino-Tibetan):

[16] Doing historical linguistics in a typological spirit, "typologising" diachrony, is as it were the complement to the "dynamicising" of typology, as advocated by Joseph Greenberg.

suǒ ‘house, place’ (Old Chinese, c. 1100–250 BCE)
> (i) classifier of buildings
> (ii) locative and instrumental preposition (Middle Chinese, 6th–10th century CE, continued into Modern Chinese, but discontinued in Contemporary Chinese, 20th century onwards; *suǒ* continued as N ‘house, place’ alongside classifier and prepositional usages; N itself also marginal in Contemporary Chinese) (Tianhua Luo, personal communication)

Also militating against easy equation, the structural circumstances have been partly different here from the Romance, Germanic, and Indo-Aryan cases, where the nouns to be grammaticalised were richly inflected – and the more numerous the inflections to be gotten rid of, the more complex (and probably time-consuming) the reanalyses as adpositions.

2.5. What takes so much time?

While there is some encouraging congruity among the instances that were examined here, it would be premature to definitively conclude that grammaticalisations of locative or similar adpositions from nouns for locations will ALWAYS take around 400 years or just over 15 generations. Nor do wish to imply that ALL adpositions will always take around 400 years to produce: they are not all grammaticalised from nouns, but also from verbs, adverbs, or interjections, and the kinds of reanalyses they have undergone are diverse, too – and such differences may contribute to the tempo of grammaticalisations being faster or slower.

Rather, in a more general vein, let us reflect what it is about change that CAN take more time or less time. I would like to single out seven factors:

(i) Ways of “actuation”: Are the conditions permitting or triggering an innovation simple or complex?
(ii) Is a change abrupt or gradual, without/with intermediate stages?
(iii) Is a change Neogrammarian (instantaneous across-the-board) or does it diffuse across the lexicon or across other structural domains (such as paradigms, word classes, clause or other construction types)?
(iv) Is a change elementary or composite, consisting of a cluster of elementary changes?
(v) Salience: Is the difference a change makes perceptually conspicuous or subliminal?
(vi) Intra-individual consolidation: How long do individual innovating speakers show variation before they categorically decide on one or the other variant, in all speech styles? During adolescence only or over their whole lifespans?
(vii) Social diffusion: Is the speech community small, homogeneous or large, heterogeneous? Are social networks in the speech community strong or weak?

Change should be rapid, reaching completion within the minimum span of three generations, IF ALL IS EASY: simple actuation; abrupt transition; Neogrammarian mode of implementation; elementary change; discernible, high-profile difference; decisive individuals; small, homogeneous, well-connected community. Everything else prolongs change. Change is prolonged MOST substantially if it is not elementary but COMPOSITE (iv) and if it needs to DIFFUSE through a large and fragmented speech community (vii). It is arguably ONLY these two factors which are responsible for dragging out grammaticalisations such as those of local adpositions for 'at' from nouns for 'dwelling, home' – such as French *chez*, Continental Scandinavian *hos*, Insular Scandinavian *hjá*, Sinhalese and Maldivian *ge* – to the rather remarkable length of around 400 years or 15 generations.[17]

2.5.1. Social diffusion

As to the social factor, the idea here is that the long duration of such grammaticalisations is to a significant part due to the time it takes for the relevant innovations to spread through speech communities. This is trivial,[18] except that we are claiming, more specifically, that the same kinds of change take the same amount of time to diffuse through populations of the same size and network structure. Consequently, the same kinds of change should be completed faster in smaller, more homogeneous, more closely interconnected speech communities, or even slower in the opposite social circumstances.

According to demographic estimates, the population who went along with the reanalysis of the noun *casa/chiés* as the preposition *chez* – the Old French speech community – had less than 6 million members at its onset (8th–9th century) and around 18 million at its conclusion (12th–13th century, prior to a sharp population drop in Europe in the 14th/15th century). The Eastern Old Norse speech community where the noun *hus* was population-wide reanalysed as the preposition *hos*, in between the 10th

[17] A relatively long production time for adpositions is also evident in the layered structure of adpositional inventories. Often "primary" adpositions can be distinguished from "secondary" adpositions; thus, in English the former include *at*, *by*, *for*, *from*, *in*, *of*, *on*, *to*, *up, with*, while a few of the latter are *into*, *upon*, *within*, *without*, *across*, *along*, *atop*, *before*, *behind*, *between*, *because of*, *by means of*, *on behalf of*, *in order to*, *toward*, *near*, *during*, *concerning*, *ago* (a rare postposition, from participle *a-gone*). There tends to be a considerable time lag between such primary, old adpositions and secondary, younger adpositions; thus, in English the old adpositions date from Proto-Germanic or even Proto-Indo-European times, while the next generation has only been created – often, but not exclusively, through the grammaticalisation of nouns and verbs – relatively recently, a thousand years or more after the senior members.

[18] Among recent work on social diffusion that is closest to our concerns see Nettle 1999a, 1999b and Trudgill 2011.

and 14th/15th centuries, was much smaller, peaking at around 2 million. The Insular Scandinavians of Iceland, originating from Norway and branching out to the Faroe Islands (and further to Shetland and Orkney), who instead concurred in reanalysing the noun *hiá* as a preposition over about the same period of time formed an even smaller population of around 50,000 speakers. The Indo-Aryan-speaking communities in Sri Lanka and the Maldives who took part in the reanalysis of *gehi* 'in the house' as a locative-genitive postposition would probably not have numbered more than 200,000 over the relevant stages of Pāli and early Sinhalese and Maldivian; the subsequent reanalysis from postposition to suffix was to occupy millions.

Thus, none of the speech communities effectuating these grammaticalisations within roughly the same time frames were either huge or tiny (ignoring the further possible cases mentioned in Section 3.4); still, their middling sizes were different from one another. For our claim of a constant diffusion tempo to stand, the communication channels would have to be faster in the larger populations, propelling the innovations to be replicated across the speech community. During the Middle Ages, population density was always substantially higher in France (as linguistically circumscribed at the time) than anywhere in Scandinavia, and higher in Continental than in Insular Scandinavia: it seems plausible, therefore, that over the same time span innovations should have reached higher or lower numbers of speakers in proportion to how densely or sparsely populated the areas were that the speech communities occupied. The less intense the interaction, the flatter and temporally more extended the S-curve along which innovations typically spread across speech communities: commencing slowly; picking up speed and progressing fast or not quite so fast depending on network density; reaching saturation slowly or petering out at the edges.

Other than population density there are presumably further differences in network structure that can interfere with how fast or slow the social diffusion of an innovation proceeds. But ceteris paribus we would maintain that, at the social diffusion rate that we must assume for the cases on record, a new local adposition 'at' will have stopped being a full-fledged noun 'dwelling, home' approximately 400 years or 15 generations ago – whenever such a noun is its source and grammaticalisation is the kind of reanalysis it underwent. In fact, in relation to differential median speech community sizes as calculated by Nettle (1999a, 1999b) – ca. 17,000 for the Old World, less than 400 for the New World, 1,800 for Australia and the Pacific, with perhaps somewhat lower numbers in the past – all our grammaticalising populations are above average; the average grammaticalisation duration should therefore be assumed to be shorter, insofar as it is social diffusion that determines the tempo of change.

2.5.2. Composite change

Change will be faster when the reanalyses to be effectuated are elementary than when they are composite. Since grammaticalisations are the prototype of composite change, comprising semantic, syntactic, morphological, and phonological "micro-changes", it

should not come as a surprise that they take relatively long to complete. On the other hand, if an entire cluster of interrelated reanalyses were executed simultaneously, all after all subserving the same master-plan, even highly composite changes could go as quickly as elementary changes. The interaction of the individual reanalyses involved in grammaticalisations is indeed a major and controversial empirical issue.[19] For present purposes the chief question only is whether they occur simultaneously or consecutively.

For the kind of grammaticalisation at hand, our examples suggest that the component micro-reanalyses cannot be wholly consecutive nor wholly simultaneous. As sketched above apropos of French *casa/chiés > chez* (Section 2.1), we count close to 20 elementary lexical-grammatical parameters whose values have to be altered when an inflected noun 'dwelling, home' is to become a local adposition. Assuming the minimal duration of three generations per elementary reanalysis, consecutive changing would keep some 60 generations busy – but 1,500 years is decidedly too long a gestation period even for adpositions. Simultaneous changing would be far too rapid, even allowing for ponderous diffusion across populous and poorly connected speech communities. Thus, to be consistent with the rather spacious time frame as established here, the individual steps in composite changes will partly have to be taken simultaneously and to some considerable part consecutively.

While this mode would be consistent with a gradualist as well as a catastrophist overall scenario, perhaps the most apposite image is that of a CASCADE of elementary changes, beginning with a trigger change and culminating in a categorial threshold change.[20] Some of these cascading reanalyses will perforce be simultaneous: thus, when number and case are cumulated, the inflection for these two categories cannot but be discontinued together when a noun is remodelled as an adposition. With some reanalyses others will not be long in coming. For example, there is a connection, dictated by universal grammar, between inflecting like a noun and being able to occur as the complement of an adposition, but it is asymmetric: if a lexeme inflects for number and case, it can be the complement of an adposition, but not vice versa; thus, in accordance with this implicational universal, it should be possible for a noun to lose its inflection and continue to occur as the complement of adpositions (as in Old French *en chies son hoste* 'at his host's home', see above) – but unless the process of

[19] One controversy is about whether semantic change always comes first and triggers others; another is about the inertness of syntax, with syntactic change allegedly only ever reactive to previous morphological or phonological change (discussion in Plank 2010). I have argued elsewhere (Plank 2011) that, while in RELATIONAL reanalyses (of subjects as objects and vice versa) syntax changes before inflection, in CATEGORIAL reanalyses (e.g., of nouns or adjectives as proper names) inflectional change takes the lead. Since noun > adposition is a categorial reanalysis, we would also expect inflection to change before syntax here.

[20] For my own take on gradualism vs. catastrophism in grammaticalisation see Plank 1984, concerning auxiliaries. While originally leaning towards catastrophism, generative historical linguists are now also favouring the cascade imagery (e.g., Biberauer & Roberts 2008).

adpositionalisation is halted, this distributional behaviour will sooner or later be realigned too. Other than excessive prosodic weight of grammatical formatives being discouraged, phonological modifications of nouns when adapted for adpositional service would seem independent of morphological and syntactic reanalyses, and could be expected at any time in a cascade of grammaticalisation.

Whenever they have come about through cascades of micro-reanalyses, new local adpositions 'at' can thus be dated to have been some 400 years, or roughly 15 generations, in the making, including the time the cascading elementary metamorphoses of nouns for 'dwelling, home' have taken to diffuse through speech communities arduously crafting an adposition out of this kind of material.

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