

TIME-STABILITY IN HISTORICAL LINGUISTICS

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1. A historical question

A *why?* question, as obvious as profound, that a mature science of linguistics could be expected to be able to answer is this: Why is a language, **any** language, the way it is, in each and every particular?

The most consistent approach to this question is historical: A language is the way it **is** because this is what it has **become** in the course of time. What this is saying – in substance rather than through the ever-popular imagery of languages as changeable “things” or behavioural patterns – is that the mental lexicons and grammars of individual speakers, as to some extent shared within speech communities, are the way they are because this is what was acquired by these individuals on the basis of their particular linguistic experiences – what they heard others, from the same or different speech communities, say or not say and how they analysed what they heard or didn’t hear. And so on, for generation after generation.

This sort of reasoning could seem unsatisfactory insofar as all appears to be a matter of historical contingency. But this is not an inherent flaw in the argument: rather, here lies the real challenge for a genuinely historical linguistics – namely, to determine what is **chance** and what is **necessity** about the being and becoming of languages. The task at hand is to sort out in which particulars mental lexicons and grammars will always be exact replicas across successive generations, as dictated by general laws of language (or of even more general constraints on human brains and bodies), and in which particulars they can be different. As to the general laws, there is the question of their temporal nature: Are they universal constraints on **states** or on **transitions**? As to permissible difference, the salient questions are: Will changes be brought about with ease or with difficulty, and what is it that makes them easy or difficult? What is it that makes some linguistic structures and processes so transient and others so pertinacious, susceptible or resistant to change – so resistant perhaps that they give the appearance of immutability? (Ease of comprehension or production? Easy to learn and hard to unlearn?) Will changes occur often or rarely, at whim or subject to particular linguistic experiences? Will they take a short or a long time to effectuate, few or many cycles of acquisition and few or many life-spans of language usage?

To a greater extent than becomes a mature science, sorting out what is chance and what is necessity is unresolved business in linguistics. Our knowledge about the time-course of linguistic transitions and possible constraints on them remains remarkably slight. Consequently, for many particulars we cannot be very confident and convincing in our reasoning that languages are (or are not) the way they are (or are not) because this is (or is not) the way they **could** become.

Associated with the *why?* question is a *what?* or *how?* question: What is a language, any language, like, in each and every particular? How does a language, any language, function? Although this could seem an antecedent question, the *what?/how?* and *why?* questions are really complementary: the one cannot meaningfully be answered without the other. It would be equally ill-advised to take the *why?* question for an optional extra when the focus is on states and to skirt the *what?/how?* question when it is on transitions.

The only way of answering that *why?* question really is historical, then: Languages can only be what they could become. The objects under historical study must not be mistaken for things or just behaviour: the objects under study are lexicons and grammars, and these are **mental**. Special methods are required for this purpose, which are not the same as those appropriate for the historical study of things or behaviour.

It is fair to conclude that a widespread reluctance to take on, or even appreciate, the “big” *why?* question about languages and its ultimately historical nature has been a seriously retarding moment in our understanding of Language and languages, of a human capacity (capital L) and its manifestations in space and time (little l's). It is time for change.

2. *Erkenntnisinteresse*

Our subject matter, in the most general terms, is the **temporal dynamics** of language. How “language” occurs in time and how linguistic structures and processes are shaped by their temporality is a vast complex of issues. Our particular concern here is the distinction between **transience** and **pertinacity** in linguistically relevant time.

Our business in this respect is both discovery and insight, finding truths yet unknown and making sense of what has been found and may yet be to discover; we will have failed if not producing both. The vision is to re-orient the field of historical linguistics, in its relation to universals research and typology, through innovatively combining theoretical linguistic, neuroscientific, and computational approaches.

The focal question is this: Are kinds of forms and constructions, categories and relations, rules and constraints, structural principles and processes – in short, anything pragmatic, semantic, syntactic, morphological, phonological, phonetic, or lexical – **transient** or **pertinacious**, or in other, plainer words, **unstable** in time (changing fast) or **stable** in time (changing slowly or resisting change altogether, thus appearing static)? And, moving from description to explanation, from the ascertaining of temporal scenarios to conditions and causes of developments, the question is: **Why** is something linguistic transient and something else pertinacious?

This are the same kind of questions as can be (and has been) asked, in the respective scholarly disciplines, about anything else 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. While questions of life expectancy, longevity, permanence, persistence, immutability and such have been prominent on the research agendas of historical (or more appropriately: temporal) sciences, the question of time-stability has only been touched on, or has only received inferential treatment, in historical linguistics. Historical linguistics has a long and illustrious tradition, with the perfection of the comparative method and the resultant genealogical trees as the proudest achievements still. Nonetheless, such basic issues as the temporal stability and instability of linguistic structures and processes have remained remarkably underinvestigated and ill-understood, owing to a certain preoccupation with identifying, cataloguing, and taxonomizing attested changes, and to a disinclination to investigate changes **directly** and instead to merely infer from results of changes what changes themselves may have been like. A selective sketch of previous work where questions of pertinacity and transience in linguistic development **have** been addressed, if sometimes indirectly, is given below, Section 4.

Notions such as universal grammar, markedness, structural (a-)symmetry, complexity, or structural integration and centrality have sometimes been invoked to account for transience

and pertinacity. The danger here is that of circularity, when transience and pertinacity themselves are relied on in determining what is supposed to be marked and unmarked, complex and simple, structurally marginal and central. Whatever the explanatory concepts that will account for diachronic transience and pertinacity, our working assumption is that they need to be grounded in language comprehension and production and in language acquisition (both L1 and L2) and attrition.

Taking into account how noisy, variable, and indeterminate the input tends to be for language acquirers (even for statistically astute ones), it must seem paradoxical how conservative mental lexicons and grammars are: overall, change should be expected to be more drastic and rapid. It is the imperative of conformism that would seem to guide lexicon and grammar acquirers more than the imperative of extravagance. (And even extravagance is not haphazard: it needs the foil of conformism.)

It is sometimes assumed that it is only **language contact** that is seriously destabilising – as if lexicons and grammars were destined to remain eternally immutable as long as not interfered with by “foreign” linguistic experiences. But it really needs more systematic research to distinguish transience and pertinacity as due to inherent factors or due to the historical contingencies of substrate, superstrate, or adstrate influences.

A better understanding specifically of pertinacity and transience is not only a major desideratum for **historical linguistics**, as grounded in the temporality of comprehension and production and of acquisition and attrition. It also has important implications beyond: most importantly, it promises to throw light on central questions concerning **linguistic diversity and its patterns**, insofar as what is pertinacious will, *ceteris paribus* (such as population-historical contingencies), therefore show less diversity across dialects, languages, families, and areas than what is transient.

3. Time

Change happens (or doesn't) in time, in any of the five salient senses that time has in the realm of language: (i) the time course of the planning and execution and of the processing of **speech events**; (ii) the **life span** available for individuals to acquire and reorganize a lexicon-and-grammar and to participate in speech events, and also for linguistic abilities to be impaired or undergo attrition with age; (iii) the time it takes for linguistic innovations of individuals or subgroups to diffuse through larger sections of generationally layered speech communities (**social diffusion**); (iv) the **history of generations** of members of speech communities having the same or a different lexicon-and-grammar as the preceding generation whose speech events they have been witnessing or as the members of other speech communities they have come to be in contact with; (v) the **evolution** of homo sapiens sapiens (= loquens).

Linguistic structures and processes, or rather their mental representations, can be transient/unstable or pertinacious/stable in time taken in any of these five senses. Our focus is on generational history, which is the very arena where linguistic change (or, no less significant, non-change) happens. The first two senses of time will also be on our agenda: our position as to the relationship between them is that in order to understand generational (= diachronic) dynamics it is crucial to understand speech-event dynamics and life-span dynamics, each with a dialectic of pertinacity and transience of its own.

What changed or did not change at the evolutionary transition from pre-language to language will not be our concern here. For a change to take effect, an innovation needs to spread through the social space of speech communities, a set of minds/brains with organs for

speaking and hearing – and, depending on many conditions, linguistic or social, this may happen fast or slow or not at all. With much work in socio-historical linguistics devoted to the social dynamics of change, this will not be a major concern in the present enterprise, either.

[...]

4. State of the art: Time-stability in historical linguistics

4.1 The life of languages and the life cycles of their pieces

The life cycles of languages would not seem to be a subject matter of great interest for theoretical linguistics. The life events of languages – their coming into existence through splits or mergers of speech communities and their coming to an end when they are no longer passed on to new generations (language death) or when what is passed on, as a result of splits or contacts/mergers, has come to be different from what used to be passed on – are something for population and social historians to study. However, the time course that languages run presents a formidable challenge to linguistic theory – **any** kind of linguistic theorizing – once the blank and blanket notion of "language" is given linguistic content. First, a "language" is not a spatiotemporally circumscribed set of speech acts or a collection of texts; a "language" means a **mental** grammar-and-lexicon – the socially shared individual know-how which enables the members of a speech community to perform speech acts and thereby to express and communicate their ideas. Second, a mental grammar-and-lexicon is not all of a piece, but has **very many pieces** – units and constructions at various levels of complexity (from segments to sentences and beyond), categories and relations, rules and constraints, structural principles and processes – intricately dependent on or independent of one another in the complex business of relating meanings and forms and in acquiring this know-how.

To be meaningful, then, the question of time courses must be asked about these particular pieces of mental grammars-and-lexicons, as passed on from generation to generation and as acquired by generation after generation (or, as the case may be, not passed on and not acquired), and perhaps as modified during the life spans of members of speech communities. When the temporal dynamics of mental grammars-and-lexicons is on the research agenda, relevant questions are these: Do **all** pieces of mental grammars-and-lexicons have the **same** temporal dynamics, or are **some** pieces more time-stable than **others**? If the temporal dynamics are different for different pieces, what does this imply for **crosslinguistic diversity and unity**? More unity with respect to stable pieces, more diversity with respect to unstable pieces?

By now there is a substantial number of studies of the life cycles or natural histories of certain kinds of linguistic forms/meanings and processes – among others of diphthongs (Stampe 1972), consonantal assimilation and dissimilation (Hutcheson 1973, Johnson 1979), negation markers (Jespersen 1917 any many others since, culminating in, but not ending with, Horn 1989), definite articles and deictics (Greenberg 1978a, Hansen 2004, Clark 1978), aspectual markers (Deo 2006), infixation (Yu 2007), reduplication (Hyman 2009), or of morphology in general (Hodge 1970, Dahl 2004, Hurford 2009, and see Plank 1992 for a review of the early history of cyclic conceptions). And there is the age-old discussion of the time course of sound changes (Neogrammarian or other/diffusional, abrupt or gradual) and of the life cycle of phonological rules. Much light has certainly been shed on developmental patterns by work such as this; however, the recognition that certain developments are cyclical

or follow some other predetermined course as such does not answer the questions of what is stable and unstable and why, and how fast or slow cycles cycle.

4.2 A brief history, in 13 chapters, of assumptions, inferences, and speculation about stability

To prepare the ground for our own research agenda and to put it in perspective, we need to be aware of what is on record about time-stability as such: What were the questions asked, or not asked, and what were the answers given, or not given – in relation to ours?¹

A. A natural starting point for this review is Sapir 1921. While Sapir's notion of "drift" attracted much critical attention, the associated imagery of its flow being slower or faster was largely ignored:

The general drift of language has its depths. At the surface the current is relatively fast. In certain features dialects drift apart rapidly. By that very fact these features betray themselves as less fundamental to the genius of the language than the more slowly modifiable features in which the dialects keep together long after they have grown to be mutually alien forms of speech. (Sapir 1921: 172)

Nor did Sapir himself go into details as to **which** particular features are supposed to be the stabler and **which** the fast-changing ones. Was he implying that stabler is what is fundamental to the "genius of the language". But how can we know what is fundamental to genius? By observing what is time-stable? In which case time-stability would not be explained, but would rather be used to explain something else, otherwise somewhat mysterious: the genius of a language.

Among those who recognized that time-stability **is** an issue, this descriptive and explanatory uncertainty was to continue long after Sapir.

B. When "geniology" had become a matter of implicational relations among values of crosslinguistic variables (that is, typology), for Greenberg **time-stability** was **primary** and **crosslinguistic distributions** were **secondary**, following from the diachronic facts of time-stability:

If a particular phenomenon can arise very frequently and is highly stable once it occurs, it should be universal or near universal [...] If it tends to come into existence often and in various ways, but its stability is low, it should be found fairly often but distributed relatively evenly among genetic linguistic stocks [...] If a particular property rarely arises but is highly stable when it occurs, it should be fairly frequent on a global basis but be largely confined to a few linguistic stocks [...] If it occurs only rarely and is unstable when it occurs, it should be

¹ As part of the preparation of this research proposal, we have compiled a bibliography of relevant publications, with PDFs of the texts available in a password-protected AIGAION database, "Pertinacity & Transience":

<http://typo.uni-konstanz.de/cald/index.php/topics/single/3>

highly infrequent or non-existent and sporadic in its geographical and genetic distribution [...] (Greenberg 1978b: 76)

Despite Greenberg's own concern with the "dynamicization" of typology, differential time stabilities as the rationale of crosslinguistic distributions have not become a priority issue for typologists – until 25 years later a programmatic call was issued by Johanna Nichols to investigate the time stabilities themselves which determine crosslinguistic distributions:

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. (Nichols 2003: 290)

In between Greenberg 1978b and Nichols 2003, and also continuing after Nichols 2003, some work was done which reversed this research agenda aimed at establishing links between time-stability (primary) and crosslinguistic distributions (derivative).

C. It is reading Greenberg (and Nichols 2003) backwards to define the research objective, as far as the relationship between diachrony and typology is concerned, as follows: time-stability is to be **inferred** from observed distributions of variable values over members of single families, across families, over members of single areas, across areas – at a given time, i.e., now (that is, **without** a time dimension, not by comparing diachronies).

The idea in following this reverse agenda is to have a window into the dark past, from where you can look beyond where the comparative method throws light (8,000 years B.P., as far as form-meaning matchings are concerned): If, from current crosslinguistic distributions we can tell what is particularly stable, then we can (perhaps) tell how languages have

developed (split up, mixed, moved) before their first attestations and their origins as reconstructable by the Comparative Method.

The basic question asked in Nichols 1992 (esp. Ch. 5: Diachronic stability: Genetic and areal; see also Nichols 1995) is this: In which kind of grouping – **genetic** (=genealogical) or **areal** – does a grammatical feature show more consistency/more diversity? (Which is not directly a diachronic question, and not the question posed in Nichols 2003, as quoted above.)

These are some of the results in Nichols 1992:

MOST GENETIC/LEAST AREAL
(time-stable)

MOST AREAL/LEAST GENETIC
(time-unstable, in the sense of: borrows easily)

relational alignment > morphological complexity >> head/dependent marking >> word order

That is to say, word order is the most consistent feature in areas, head/dependent type and relational alignment intermediate, and (morphological) complexity least stable. Relational alignment (nom – acc, abs – erg, etc., as encoded morphologically) is genetically stable and not greatly susceptible to areal spread; while word order is not particularly stable in families and is highly susceptible to areal spread. To summarise:

- | | |
|--|--|
| <ul style="list-style-type: none"> • Dominant relational alignment • Morphological complexity • Head/dependent marking • Word order • Voice | <p>high genetic stability;</p> <p>both genetic and areal, but more genetic than areal;</p> <p>both genetic and areal, but more genetic than areal;</p> <p>little genetic stability, highly areal;</p> <p>directionality of valency-changing processes has some genetic stability; argument affected is determined by universals;</p> |
| <ul style="list-style-type: none"> • Inclusive/exclusive | <p>high genetic and moderate areal stability (but contingent on geography);</p> |
| <ul style="list-style-type: none"> • Plurality neutralization • Inalienable possession | <p>neither kind of stability high [=transient];</p> <p>low genetic and moderate areal stability (but contingent on geography);</p> |
| <ul style="list-style-type: none"> • Noun classes | <p>high genetic and moderate areal stability (but contingent on geography).</p> |

The research agenda as set out and pioneered in Nichols 1992 was to prove immensely influential in typological circles and among those with a professional interest in deep history. After Nichols 1992, whose typological basis was essentially home-made (and would later mature into AUTOTYP), much was made of another crosslinguistic database, the *World atlas of language structures* (WALS, Haspelmath, Dryer, Gil, & Comrie 2005/2008), as the factual basis of assumptions about crosslinguistic distributions – the (non-diachronic) givens from which stability was to be inferred rather than being investigated directly.

The methodology of the inferencing has been developed in various papers by Wichmann and Holman and their associates (Wichmann & Holman 2009, Wichmann & Kamholz 2008, Holman et al. 2008, etc.). The basic idea is that the more languages **within** a genus share a feature, relative to the extent that this same feature is (not) shared **across** genera, the more stable this feature is. (For otherwise the frequency of the feature would have to be due to numerous independent innovations.) Looking at up to 150 features (but often far fewer) for 2,560 languages (that is, the 57,916 data points contained in WALS), they arrive at stability measures such as these (from Wichmann & Holman 2009) – distinguishing "very

stable" (with a stability value, calculated according to their preferred metric, of 51.8–100.0), "stable" (32.8–31.7), "unstable" (19.2–32.7), and "very unstable" (-62.8–18.9):

Feature	Feature description	Value	Description of value	Stability (%)
17	Rhythm Types	1	Trochaic: left-hand syllable in the foot is strong	22.6
		2	Iambic: right-hand syllable in the foot is strong	31.9
		3	Dual: system has both trochaic and iambic feet	(-9.8)
		4	Undetermined: no clear foot type	6.1
		5	Absent: no rhythmic stress	29.2
37	Definite Articles	1	Definite word distinct from demonstrative	19.7
		2	Demonstrative word used as marker of definiteness	-1.7
		3	Definite affix on noun	21.0
		4	No definite article but indefinite article	13.1
		5	Neither definite nor indefinite article	16.8
38	Indefinite Articles	1	Indefinite word distinct from numeral for 'one'	11.9
		2	Numeral for 'one' is used as indefinite article	20.1
		3	Indefinite affix on noun	(5.7)
		4	No indefinite article but definite article	14.6
		5	Neither indefinite nor definite	22.5
81	Order of Subject, Object and Verb	1	Subject-object-verb (SOV)	69.5
		2	Subject-verb-object (SVO)	59.2
		3	Verb-subject-object (VSO)	44.5
		4	Verb-object-subject (VOS)	(13.6)
		5	Object-verb-subject (OVS)	(8.0)
		6	Object-subject-verb (OSV)	(7.7)
		7	Lacking a dominant word order	24.5
98	Alignment of Case Marking of Full Noun Phrases	1	Neutral	52.5
		2	Nominative - accusative (standard)	15.8
		3	Nominative - accusative (marked nominative)	(100.0)
		4	Ergative - absolutive	65.5
		5	Tripartite	(100.0)
		6	Active - inactive	(-15.2)

Overall, the Wichmann, Holman et al. results agree with some previous assumptions (e.g., that SVO is very stable or that definite articles are very unstable) and disagree with others (e.g., that ergative alignment of case marking is unstable, as widely assumed). Sometimes the results are hard to make diachronic sense of.

Belyaev 2008 measures the stability of the same features by essentially the same stability metric on the basis of a different crosslinguistic dataset, that of *Jazyki Mira* (comprising 371 languages of Eurasia, with overall 3872 binary features). Sometimes the results are roughly similar: e.g., SOV and SVO are both "very stable" for Wichmann &

Holman, while for Belyaev SVO is likewise "very stable", but SOV is only "stable"; ergative alignment (especially for pronouns) is "very stable" for both Wichmann & Holman and Belyaev. Sometimes the results are quite at odds: e.g., VSO is (still) "stable" for Wichmann & Holman, but "very unstable" for Belyaev; adjective-before-noun is "very stable" for Wichmann & Holman, but "very unstable" for Belyaev; having a perfective/imperfective aspect contrast is "stable" for Wichmann & Holman, but "unstable" for Belyaev.

Parkvall 2008 again bases himself on *WALS*, but wants to differentiate stability as due to inheritance or borrowing: family-stable traits are ones whose incidence is higher within families (any families) than across families, and area-stable traits (traits easy to borrow) are ones whose incidence is higher within areas (any areas) than across areas. Ignoring here all mathematical detail, Parkvall measures the diversity and inversely homogeneity within families and within areas with regard to the features mapped in *WALS*, always comparing the family or area values to the average values for the entire set of *WALS* languages for which information for the respective features are provided in *WALS*. He then divides the homogeneity value for families by homogeneity values for areas for a given feature, and concludes that a high ratio indicates that the feature is family-stable while a low ratio indicates that the feature is borrowing-stable. Among the most family-stable and correspondingly least area-stable (least-easy-to-borrow) traits identified through Parkvall's method are clicks, noun plural expressed through suffix, both noun-before-genitive and genitive-before-noun basic orders, a single basic colour term for BLACK and BLUE. Among the least family-stable and correspondingly most area-stable traits are demonstratives with a distance contrast, overall dependent-marking as well as overall dependent-marking, a single basic colour term for GREEN and BLUE. Vowel nasalisation, found to be "stable" or "very stable" by Belyaev and Wichmann & Holman, comes out as only modestly family-stable for Parkvall (with its value increasing, however, when controversial large-scale families are excluded); a perfective/imperfective aspect contrast is not family-unstable for Parkvall (like for Belyaev and unlike for Wichmann & Holman); all basic orders at clause level are quite family-stable and correspondingly area-unstable for Parkvall (otherwise often claimed to easily yield to borrowing); ergative alignments for all purposes (case marking, agreement, of nouns as well as pronouns) get higher family-stability values than accusative alignments.

Also basing themselves on *WALS*, but not really getting much out of it, Greenhill et al. 2010 conjecture that syllable structure, a velar nasal, case syncretism, coding of plurality, tense/aspect suppletion, an optative, and especially negative values for such variables (no optative, no suppletion, no syncretism, no velar nasal) might be among the stabler features. Despite their grand title, 'The shape and tempo of language evolution', they are forced to admit that we/they know very little, really, about the shape and tempo of language change as far as non-lexical traits are concerned. (They feel they can be more positive about the shape and rate of lexical replacements; but let us not go into this question here.)

As an apposite commentary on this line of research, here is what Greenberg once concluded in his not very sympathetic review of Nichols 1992:

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. It can be inherited within small or large families, the result of areal contact, or a quite recent independent innovation. Thus, from the historical point of view, typological distributions are *explananda*, not explanatory principles. (Greenberg 1993: 505)

D. One result of Wichmann & Holman which has been found intriguing is that the *WALS* features, or at any rate the more stable ones among them on their own calculations, have a retention rate which is roughly the same – 0.86 over 1,000 years – as the (supposed) retention rate of core vocabulary according to glottochronology. Which introduces the research line where time-stability has been most prominent in historical linguistics: glottochronology.

The hailed and denigrated chief tenet of glottochronology is that 86 core lexical items – from the 100 relatively culturally neutral basic terms on the Swadesh list (short version: the long version has some 200 items) – are supposed to be retained and 14 replaced (by non-loans) over every 1,000 years in the life of each and every language – which gives the **glottochronological constant** of 0.86 or thereabouts ("lexical half-life", as precise as the radiocarbon dating of organic materials, hopefully). Thus, the ratio of cognates (retentions) to non-cognates (replacements) on the Swadesh list would allow the historian to calculate how long two languages have been separated: essentially, if there are 86 cognates and 14 non-cognates, the languages must have split 1,000 years ago.

Glottochronology is about the turnover rate of core vocabulary **as a whole**: no claims are being made about **particular** items on the 100-list being more stable and others less stable – **any** item might be among the 14 going to be replaced over 1,000 years, as long as it is one of 14. However, several attempts have been made recently to allow for differential retention rates for different lexical subsets. The Automated Similarity Judgment Program (Brown et al. 2008, Holman et al. 2008) works with a list of 40 lexical items, assumed to be the most stable ones, as identified on the basis of comparisons across 245 languages (by now above 1,400). To give an idea of what comes out of this line of research, here is the top (most stable), middle and bottom (least stable) of the Holman et al. 2008 stability ranking of the Swadesh-100 list – with conspicuous differences between the opposite extremes, but with otherwise an unpunctuated continuum.

Rank	# In list	Meaning	Stability
1	22	*louse	42.8
2	12	*two	39.8
3	75	*water	37.4
...			
49	36	feather	23.1
50	90	white	22.7
51	89	yellow	22.5
...			
98	93	hot	11.6
99	67	lie	11.2
100	15	small	6.3

Dolgopolsky 1986 has an even shorter list of 23 "most stable" lexical items, arrived at less systematically, on impressionistic Eurasian evidence:

'I/me', 'two/pair', 'thou/thee', 'who/what', 'tongue', 'name', 'eye', 'heart', 'tooth', 'no/not', 'fingernail/toenail', 'louse/nit', 'tear(drop)', 'water', 'dead', 'hand', 'night', 'blood', 'horn', 'full', 'sun', 'ear', 'salt'.

There are two adjectives, but not a single verb on this list. A scarcity of adjectives and verbs is conspicuous in the top region of the Holman et al. 2008 ranking, too. This suggests the question: Is **word class** a relevant factor in lexical time-stability?

Among the 15 adjectives on the Swadesh-100 list, 5 are COLOUR terms and 4 are SIZE/DIMENSION terms; although none of them made it onto the 40-list of Holman et al. 2008, the question here is whether **semantic field** is a relevant factor in lexical pertinacity.

This line of neo-glottochronological research raises several questions, all unanswered or indeed unasked, among them these:

(i) **Why** are some words (or word classes), or their meanings and/or forms, more stable than others? (Because they are culturally neutral, culturally salient, hence very frequent, early learnt, never forgotten, never abandoned in favour of more exciting alternatives? Still, why should ‘louse’ be so stable and ‘small’ be so comparatively unstable?)

(ii) Stability is seen as a matter of resistance to **replacement** (by a lexical item from the same language, or also by loans). But what about stability in terms of resistance to (a) **semantic change**, (b) **phonological change**, (c) **lexical marginalization**? (For it may also be due to semantic and/or phonological change that cognates are no longer recognizable as such. Or also that items which are not cognates come to sound and mean like they are. And lexical items are rarely altogether replaced: more commonly, primary words for particular concepts are getting marginalised and others are getting centralised, with the result that cognates remain, but are no longer equally central in the languages concerned.)

In short, should such results as reported above prove robust on a wider crosslinguistic basis and on a more sophisticated understanding of lexical change, essential linguistic questions about lexical stability remain to be tackled.

E. What is at issue in work like that just summarized – approaching this question indirectly rather than directly, by looking at crosslinguistic distributions rather than at diachronies – is the differential time-stability of **individual** grammatical or lexical variables. Claims have also been made, however, about **entire components** or **modules** differing from one another in time-stability.

In a completely different research tradition, and with next to no cross-fertilization with the Greenberg-Nichols tradition, the idea was advanced that in language nothing ever changes, unless for the worse or when interfered with by a strong enough force.

The Principle of Inertia

Things stay as they are unless acted on by an outside force or decay.
(Keenan 2002: 2; also Keenan 2009)

Given an antagonistic social Principle of Extravagance: Be (conspicuously) different!, the empirical question here would be to see which force prevails under which circumstances, Conformity or Extravagance, Inertia or Ertia, and whether Conformity/Inertia – Acquire the same mental grammar-and-lexicon as the preceding generation, so far as you can abduce it from the speech acts you experience, and don't change it for better or worse! – is inherently stronger.

In an elaboration of the theme of inertia, different parts of grammar are claimed to differ radically in time-stability; in particular, **syntax** has been assumed to be the most stable component or indeed to be completely immutable:

syntactic change should not arise, unless it can be shown to be *caused* [...].

[...] *syntax*, by itself, is diachronically completely inert.

(Longobardi 2001: 278; also Crisma & Longobardi 2009, Longobardi & Guardiano

2009, etc.)

However, even syntax, on this theory, **can** change, if on the sole condition that a syntactic reanalysis is forced on the acquirer by phonological or semantic change. (And even then, it may really only be the phonology or semantics which are changing, not syntax in a narrow sense itself.)

It is perhaps no coincidence that it was a minimalist approach to syntax which has inspired the idea that syntax is the least changeable part of grammar, at least in comparison with sound/phonology and meaning/semantics: If there isn't much to begin with, there isn't much to be changed.

On the other hand, there is work on syntactic change in relation to inflectional change which suggests that syntax changes earlier and faster than inflectional morphology. What Cole et al. 1980 found and Haspelmath 2010 confirmed, looking across a range of languages at how non-subjects acquire subject properties in reanalyses of impersonal, subjectless constructions as personal, subjectful, was that the **behavioural** properties (e.g., raising, equi) of subjects were invariably acquired before the **coding** properties (case marking, agreement): noun phrases which were non-subjects insofar as they did not bear nominative case or did not control verb agreement would already behave like subjects insofar as they could undergo raising or equi-deletion (which would seem to be syntax, whatever one's preferred framework of syntactic description) just like bona-fide subjects of long standing. Syntax in such instances was not inert, and was not waiting to be forced into action by fast-changing morphology: it was not diachronically re-active, but active.

Also, as to the idea that syntax is the most inert part of grammar, it should be noted that it has often been observed that inflectional morphology, even when inherently complex, tends to be diachronically quite stable when left alone: that is, when L1-acquired. For its destabilization it seems to need intensive language contact: it is L2 acquisition which is especially prone to wreak havoc with inflectional systems. (Only compare Icelandic and English.)

Examining a different kind of reanalysis – categorial rather than relational: with common nouns and adjectives reanalysed as proper names (family names such as *König* 'King' or *Klein* 'Little') – Plank 2011 arrives at the opposite conclusion: while the semantics-pragmatics and morphology of newly created names was changing drastically (rigid designators from descriptive epithets; loss of case, number inflection; loss of gender and inflection class; acquisition of associative inflection; changing derivational potential), their syntax and phonology remained essentially unaltered, as if governed by the Principle of Inertia.

F. From an entirely different angle, it has been hypothesized that **grammar as a whole** – syntax in concert with morphology and phonology – is more time-stable than the sound-meaning matchings of the **lexicon**.

Dunn et al. 2005, 2008 suggest that a supposedly superstable overall grammatical profile still reveals very old family-tree genealogical relationships among the "Papuan" languages of Island Melanesia, reaching back beyond the time depth of ca. 8,000 years to a period long before the arrival of the Austronesians (ca. 3,200 BP) and after the first settlement of the area during the late Pleistocene dispersals (ca. 35,000 BP), with virtually no lexical cognates surviving at all over such a long time. The grammatical profile used for this study was defined through 125 (Dunn et al. 2005) or 115 (Dunn et al. 2008) structural variables with binary values, very much in a *WALS* vein: for example, phonemic voicing contrast

among stops present/absent; consonant clusters present/absent; phonemic stress present/absent; definite or specific article present/absent; article before noun or order of Art and N not fixed; inclusive-exclusive distinction present (in any pronoun paradigm)/absent; suppletive nouns present/absent; dual can/cannot be marked on nouns; noun classes or genders present/absent; case marking for core NPs present/absent; transitivity or intransitivity verb morphology present/absent; serial verb constructions present/absent; verb compounds present/absent; V-medial *is/is* not a pragmatically unmarked constituent order for transitive clauses.

As reported in Evans & Levinson 2009: 477, Dunn et al. (in preparation) "have found that taken individually, a structural feature within a single large language-family like Austronesian changes on average just once about every 50,000 years". The accompanying footnote explains: "Lest this finding invite incredulity, given that the language family is assumed to be less than 6,000 years old, this figure is worked out by summing independent path-lengths in many branches of the family tree and looking for the total numbers of changes from an ancestral language. The number should be taken with a pinch of salt but is probably in the right general ballpark." If this is supposed to mean that the values of any individual variable – of the kind defining grammatical profiles in the Dunn et al. 2005, 2008 study – are to be expected to change once every 50,000 years, then incredulity continues unabated. Translated into actual diachronic scenarios this would mean that it would on average take 2,000 cycles of transmission from one generation to the next (assuming 25 years as the time span of one generation) before a variable in the mental grammar of speakers is given a different value and this re-valuation has been propagated through the speech community concerned. In the case of some of the simpler variables from the Dunn et al. 2005, 2008 list that have been diachronically studied, this seems a vastly exaggerated expectation: the loss of dual number marking, for example, has been observed to take not more than three generations in a speech community of the size of post-medieval Iceland or (Attic-speaking) Classical Greece – the first innovating (beginning to use plural forms alongside dual forms when reference to two is intended), the second living with variation, and the third embracing the innovation as the only option (with old dual forms perhaps lingering on and used for different purposes, such as politeness distinctions). On the other hand, a different (and slightly less outrageous) interpretation of the temporal claim at issue is that the SAME change EVENT is only to be expected to take place every 50,000 years within one (largish) family, with nothing said about the actual duration of this change event (three generations or more). Whatever the interpretation, the problem here is that diachronic value changes, and the particular kinds of reanalysis effectuating them, have not been extensively studied for many structural variables, and it is, therefore, hard to know whether rates and durations of change are uniform: the available evidence suggests they are not.

Within the time frame available to speaking and grammar-changing man (100 to 200,000 years, or 4,000-8,000 generations), diachronic superstability as envisaged by Dunn et al. (in preparation) would virtually amount to diachronic and crosslinguistic invariance. (Which is an intriguing convergence with inertia theory, as espoused by Longobardi 2001, if only for the hard core of syntax.)

After the superstable rhetoric of Evans & Levinson (2009: 477), Dunn et al. (2008: 737-739) themselves differentiate degrees of time-stability for some of their own variables, determined in terms of diversity among current languages relative to the Proto-Oceanic reconstruction (as in the Nichols and Nichols-inspired approaches above). Their most stable variables include these, and all are from morphology: inclusive-exclusive distinction; nouns not dual-marked; possessor-marking on nouns; absence of a verbal object-prefix or proclitic. Their least stable variables include these, and many are syntactic: pragmatically unmarked verb-initial order (last stable by a wide margin); definite article; indefinite article (whose

presence or absence is slightly less unstable); clause-initial negation; article-noun order; decimal counting.

Diachronic stability and instability are tacitly assumed to be a matter of being dominant or recessive in language contact – as if only in situations of competition a variable would show its true mettle, and as if the values of variables would perform remain invariable when not subjected to contact pressure.

G. Another issue revolving around the **lexicon** is whether basic lexical categorisations of verbs as transitive or intransitive, and corresponding derivational processes of intransitivization (decausativization) and transitivity (causativization), respectively, are diachronically stable or unstable. Those first advocating the view that such basic lexical categorizations are not lexeme-specific but characteristic of entire languages, Nichols et al. 2006, suggested on grounds of crosslinguistic distributions within families that this lexical preference is historically unstable. Looking at the diachronic profiles of two families, Semitic and Germanic, Comrie 2006 and Plank & Lahiri 2009 came to the opposite conclusion of great historical stability of basic lexical valence orientation.

H. It was particular kinds of developments which inspired the idea of syntactic inertia: grammaticalizations – of SELF-intensives to reflexives (Keenan), of relational nouns like Latin *casa* to prepositions like French *chez* (Longobardi). In yet another research tradition, namely that whose very focus is on **grammaticalization**, the question of what is diachronically most stable and slower-changing was raised too, because the gist of grammaticalization is that ultimately **everything** will come out changed: form (> less), meaning (> less), construction (> tighter). The idea advocated here, ironically, is that it is not form, but meaning and perhaps and semantically-based syntax which are relatively most time-stable:

When a form undergoes grammaticization from a lexical to a grammatical function, so long as it is grammatically viable some traces of its original lexical meanings tend to adhere to it, and details of its lexical history may be reflected in constraints on its grammatical distribution. (Hopper 1991: 22)

It is fair to say that not a great deal of crosslinguistic evidence was ever adduced to support the idea of supposedly special **semantic** inertia, nor that of supposedly special **syntactic** inertia. In fact, the exact opposite claim has also been made for grammaticalization developments:

conceptual/semantic shift precedes morphosyntactic and phonological shift
(Heine et al. 1991: 213)

I. Certain findings in historical linguistics have likewise suggested a contrary position, namely that it is **form** which is most time-stable – or can be most time-stable, on condition that some kind of new use can be found for it.

This is the idea of "exaptation", as introduced into historical linguistics by Lass (1990, 1997, etc.): form, in particular (obsolescent) morphology, tends to persist, perhaps for a while as linguistic "junk" (or bricolage), eventually to be made good use of (to be recycled) for purposes other than the original ones. Exaptation in biology, where this idea is borrowed from (originating with Steven J. Gould), means the co-optation during evolution of structures

originally developed for other purposes (e.g., feathers, originally thermo-regulatory devices of reptiles, exapted for flight in the evolution of birds). Here is a familiar linguistic example:

Proto-IE **-sk* PRESENT stem formative in a class of verbs
 > Italic *-esc* INCHOATIVE derivational suffix (inspired by *cr-esc-* ‘grow’)
 > verb-inflectional morpheme for 1/2/3SG and 3PL in PRESENT, SUBJUNCTIVE, IMPERATIVE in a class of verbs

The idea that morphology is so stable because it lends itself to novel uses is one that has been entertained in many circles and under various names, such as "regrammaticalization", "functional renewal", or "hyponanalysis".

J. The magnum opus of a leading historical linguist (though perhaps of a somewhat sociolinguistic bend), William Labov's *Principles of linguistic change*, has STABILITY in the index. Most relevantly here, the section 'The stability of individual phonological systems over time' (vol. 1, 98-112) concludes, among other things, that "GENERATIONAL change [where the speech of individuals is stable over their lifetimes, but there are incremental differences over generations] rather than COMMUNAL change [where all members of the community alter their frequencies together, or acquire new forms simultaneously] is the basic model for sound change [and morphological change]". Lexical and syntactic change is supposed to be communal. (Also see Chapter 2, 'Stable sociolinguistic variables' in vol. 2.) It will be interesting to see this conception of different modes of the actuation and propagation/diffusion of change for different parts of mental grammars-and-lexicons elaborated on in the volume 3, *Cognitive factors*, forthcoming.

K. What cannot here be surveyed individually is work of a more general kind where transience and pertinacity come up in passing, and are accounted for through notions such as **universal grammar**, **markedness**, **structural (a-)symmetry**, **complexity**, or **structural integration** and **centrality**. A typical danger here is that of circularity, when transience and pertinacity themselves are relied on in determining what is supposed to be marked and unmarked, complex and simple, structurally marginal and central. Whatever the explanatory concepts that will account for transience and pertinacity, as confirmed through thorough empirical study, they should be grounded in language comprehension and production and in language acquisition (both L1 and L2) and attrition.

Also, it is sometimes assumed that it is only **language contact** that is seriously destabilizing – as if lexicons and grammars were destined to remain eternally immutable as long as not interfered with by “foreign” linguistic experiences. But it really needs more systematic research to distinguish transience and pertinacity as due to inherent factors or due to the historical contingencies of substrate, superstrate, or adstrate influences.

L. Attention should also be drawn here to a growing interest in computational simulations and the modelling of language change (e.g., see the overview in Baker 2008²). A guiding motive here is in finding out about processes of language change that go beyond the written

² And also see the working bibliography compiled by the Konstanz research initiative ‘Computational Analysis of Linguistic Development’ (CALD): <http://typo.uni-konstanz.de/cald/index.php/topics/single/4>

documentation of 8,000 years, but working with actual historical data is being eschewed in favour of great simplification in the workable models. Within the simulation and modeling industry, typically just one or two abstract features are considered and these tend to have binary values (which is rather reminiscent of checklist-style typology as often figuring in stability scenarios, as surveyed above, although here the binary features tend to be surfacy). It will be interesting to see how simplified evolutionary simulations of the survival and death of linguistic features square with corpus-based empirical diachronic research for such languages where the historical record is sufficiently long and accessible to computational analysis – which is one of our own objectives.

M. At Konstanz, the time-stability of linguistic structures and processes has become a collective interest at least since a workshop at Schloss Freudental, convened by Aditi Lahiri on 10-14 July 2002 and entitled "Pertinacity".

The current research proposal is a further continuation of these efforts, with the thematic focus sharpened and with the envisaged interaction among disciplines somewhat restructured, giving greater scope to computational modelling and visualization of linguistic development. In a nutshell, the distinctive features of what might be called the Konstanz approach, vis-à-vis most of the approaches surveyed under A-J above, are:

- (i) a direct investigation of historical developments, rather than merely drawing inferences about them from crosslinguistic distributions;
- (ii) an in-depth, theoretically-informed analysis of a wide and diverse range of complex grammatical structures and processes undergoing or resisting change (with the lexicon least well represented), rather than an at-a-glance checklist treatment of simple (typically binary-valued) parameters for variation and change;
- (iii) the localization of change in grammars as mentally represented, rather than in social behaviour;
- (iv) an attempt to seek explanations of diachronic dynamics from speech-event dynamics (planning, constructing, and processing) and life-span dynamics (acquisition and attrition), rather than leaving differences in time-stability unaccounted for;
- (v) the use of computational tools and in particular of visual analytics to shed light on, and bring to light, complex developmental patterns, above and beyond the modest visualizations current in historical linguistics.

These programmatic statements of distinctive intent remain to be fleshed out.

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