Where's diachrony?

FRANS PLANK

1. Universals matter, never mind

No universals? If there WERE linguistic universals, what is it they would be doing? Universals would not be mere playthings for typologists – call them "crosslinguistic descriptive generalisations" for respectability, but never mind what they mean. To have or not to have universals is no petty matter: at issue is the human mind and its history. Universals would be enforcing certain options, categorically or preferentially, and ruling out or disfavouring others as linguistic know-how is transmitted across generations and perhaps overhauled or impaired over the life spans of individuals. Everywhere and at all times, they would thus be superintending the construction of mental lexicons-and-grammars on the basis of the linguistic experience of the members of speech communities, in accordance with what the human brain, and those other parts of human bodies involved in the expression and perception of thought for purposes of communication, can and cannot do (well). Without universals, mental lexicons-and-grammars and the speech acts performed accordingly, and minds insofar as they are tied up with language, would be more diverse than they are and conceivably could be.[1]

Let me illustrate, chiefly, with adpositions and the ordering inside adpositional phrases, one of the structures figuring in Dunn, Greenhill, Levinson & Gray 2011, where the existence of universals has recently been contested, causing a similar stir as the more expansive universals bashing of Evans & Levinson (2009). No statistics or hard words are to follow here; but let's try to be clear conceptually.

1. Among the many questions of universals that I cannot go into here is that what they are meant to be true of; arguably the answer is not necessarily languages (Lahiri & Plank 2009, among others) – hence the terminological preference for "linguistic universals" over "language universals".

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2. Are adpositions post, pre, circum, in, or ambi by chance or necessity?

When learners encounter, in the speech of their elders, a particular adposition BEFORE a noun phrase, or a particular adposition AFTER a noun phrase, will they perpetuate this order or reverse it in their own speech? Is it a matter of historical contingency which way a learner will go, or is there some universal law dictating that a new generation of speakers must follow or deviate from the model of the previous generation in the ordering of particular adpositions? This is an empirical question, but I would be surprised if the ordering of any particular adposition (identified semantically) turned out to be prescribed by law, applicable to any and all languages at any and all times.

In the presumed absence of SUCH a timeless law, any particular adposition will have a potential for a change of its ordering across generations, and also over the life span of individuals. The question then would be why and how particular re-orderings do come about – if any do. When they have no evidence of positional variation for a particular adposition, learners would universally seem more prone to be conformists than innovators, at least in such matters as the internal order of adpositional phrases, and there is therefore always and everywhere a certain likelihood that from the way one generation orders a particular adposition it is predictable how the next generation will order that same adposition.[2] For example, in the English of the kids of my friends in Leicester, *ago* is a postposition rather than a preposition because my friends themselves and all others of their generation always order *ago* after the noun phrases it heads (nobody would ever be heard by a learner to say *Granny has died ago two years*): and this would seem a satisfactory explanation of this small part of the grammar of present-day Leicester English.[3] (The many other languages, mostly Indo-Aryan, which kids have an opportunity to hear in Leicester, an English city where English is a minority language, only confirm that postpositions rather than prepositions are the done thing.) You need not go far to convince yourself that there can be no question of a universal timeless law governing the placement of that particular adposition: sometimes the relation at issue is expressed through a postposition (English *ago*, Italian *fa*, Georgian *c'in-*, Turkish *önce*), sometimes through a preposition (German *vor*, Portuguese *há*, French *il y a*, Arabic *ʾabil*), rarely through a circumposition (Amharic *kə … bəfit*).[4]

2. Which is not to exclude the possibility that, without re-ordering, an adposition is re-analysed as being positioned relative to constituents other than noun phrases – say, verbs, with postpositions of noun phrases preceding verbs thus turning into "preverbs", or vice versa.
3. I am discarding here a more abstract analysis of *ago* as a PREposition, permitting preposed specifiers (*long ago*, *two days ago*), but requiring its following complement, referring to a deictic centre (with now as default) to be silent.
4. Never through an inposition, apparently – if you insist on a universal. But this one is perhaps TOO trivial, apart from missing out on the generalisation that very few other adpositions have

The law that could, nonetheless, be at work here would be diachronic and very simple: Conform! With regard to the placement of adpositions, always do as your elders are doing (provided you can detect a consistent pattern)!

But then, if there IS variation in the speech of one's elders, this might be a strong motive NOT to conform. If the particular adposition in question were an ambiposition randomly occurring sometimes before and sometimes after its noun phrase (e.g., German *wegen dieser Sache* vs. *dieser Sache wegen* 'because of this matter', *wegen ihr* vs. *ihretwegen* 'because of her', with the adposition fusing with the pronoun when postpositional), or if its position were to depend on the kind of noun phrase it occurs with (e.g., on whether the noun phrase refers to a person or rather a thing or circumstance: cf. German *mit ihm* 'with him/?it' vs. *damit* 'with it/*him', in the latter case fused again), learners might be tempted to settle on just one given option for simplicity. But this reasoning works the other way round, too: learners might be prizing variety and might therefore feel constrained by the uniform ordering they encounter and introduce variation. Either way, it should not be taken for granted that the placement of particular adpositions is subject to timeless or diachronic universals leaving no choice for a learner, however inertial. It is a matter for empirical investigation if, how, and why particular adpositions are or re-ordered over time – over the successions of generations and the life spans of individuals. Relevant laws, if such were to be found, would be to do with preferences of particular lexical items for uniformity or diversity and their advantages for syntactic constructing and processing.

Now, adpositions are not loners: languages that recognise this class of words (and most do) tend to have dozens of them (some central and simple, others more peripheral and complex) and put them to a variety of syntactic and semantic uses, in proportion to the (morphologically bound) cases also available for the same purposes of the relational marking of noun phrases.[5] When learners encounter one particular adposition after another, the question is how they will relate them to one another with respect to ordering in their emerging mental lexicon-and-grammar. True-blue conformists will do whatever their elders did, however consistent or inconsistent the ordering of ONE adposition is with that of the OTHERS. Inconsistency, however, could be an especially strong incentive for change, irrespective of possible other motives for doing things differently: the rare or unique postposition of one generation of speakers might

been found to be genuine inpositions (rather than second-position phrasal clitics). For yet another universal, and one less trivial, I would parenthetically hazard the hypothesis that of all adpositions, 'ago' is universally among the most likely candidates (perhaps THE most likely) for postposing even in languages where prepositions hugely predominate. Such deviant inclinations should be accounted for through history. For more on 'ago' adpositions see Kurzon 2008.

5. A recent comprehensive survey of adpositions is Hagège 2010.

be especially likely to be brought in line with the majority of prepositions by the next generation or also over the life spans of individual speakers. And vice versa for rare or unique prepositions (or also circumpositions or inpositions) in a sea of postpositions.

But is there a universal diachronic law ENFORCING harmonising change here? If so, after how many generations would such a law become effective, forcing individuals to reform ordering misfits and whole speech communities or parts of them to follow suit (thereby splitting the speech community in two)? How much longer do we need to wait until *two years ago* will be *ago two years* among the English speakers in Leicester and elsewhere, on the analogy of *in/after/during two years* and the vast majority of non-temporal prepositions, too?[6] Exactly as long as it will take Italians to re-arrange *due anni fa* as *fa due anni* for the same reason? If it is not perfect harmony that is prescribed by law, will there be a universal threshold of tolerable disorderliness? Will learners and accomplished speakers faithfully perpetuate any arbitrarily mixed grammar of adposition order? Or will there inevitably, in all languages and over any shortish stretches of generational time, be ameliorating change to the effect that the ordering of one adposition becomes predictable from the ordering of others? I am not aware of a language whose adpositions were, at some point of its history, about equally and randomly divided between dedicated pre- and postpositioning (which is different from favouring ambipositions, with no unmarked order for particular adpositions), while at some time later one or the other order clearly prevailed. If this is a valid historical generalisation, it would be hard to tell whether there is a universal DIACHRONIC tendency going in this direction. Synchronically, most languages would seem to have a clear preference for either prepositions or postpositions – and from this one might infer a TIMELESS universal preference for harmony, never ever allowing much disharmony in the first place, and with no need, therefore, for diachronic harmonising.

To give the relevant figures as they emerge from the Supplementary Information of Dunn et al. 2011: as they code them, not always beyond doubt, 190 languages have prepositions, 45 have postpositions, and for only 8 languages has no dominant order been found (5 Indo-European, out of 79; 2 Austronesian, out of 130; 1 Uto-Aztecan, out of 26; 0 Bantu, out of 48); "no information", however, for as many as 39 languages in the sample.[7] Which seems to

6. The other temporal candidates for re-ordering would be *hence* and *back*, if indeed they are (used as) postpositions rather than adverbs in *two years hence/back*. The emergent adposition *come*, as in *But come fight night you will see something different* (a popular manner of speaking among boxers at pre-fight interviews), is emerging in the right position.
7. The corresponding figures in *WALS* 2011 (Dryer & Haspelmath (eds.) 2011), reversing the two top ranks, are as follows: 577 languages (out of 1185) with postpositions, 512 languages with prepositions, 8 languages with inpositions, 58 languages with no dominant order, 30 languages with no adpositions.

me suggestive of a solid statistical – probably timeless rather than diachronic – universal of positional harmony AMONG adpositions. This is not something highlighted by Dunn et al.: perhaps they need reminding that there is no logical necessity for any particular two adpositions to harmonise in their ordering.

The same line of questions concerning the options, unlimited or limited, for acquirers must be pursued for other classes of constructions, including verbs and objects and head nouns and their noun-phrase dependents. The default could always be assumed to be conformism on the part of learners: i.e., they would be seeking to do as their elders are doing, regardless of rhyme or reason. Possible action again would be to increase either uniformity or diversity, depending on whether the speech of one's models is felt to be either too diverse or too uniform. Looking at it synchronically, it would seem that ordering variation WITHIN verb-object constructions and WITHIN noun-genitive constructions is substantially greater than WITHIN adposition-noun phrase constructions. The figures in Dunn et al.'s Supplementary Information confirm this educated guess, however dubious some of their codings are: no dominant order was found for verb-object in 17 languages (11 Indo-European, 5 Uto-Aztecan, 1 Austronesian, 0 Bantu) and for noun-genitive in 21 languages (14 Indo-European, 6 Austronesian, 2 Uto-Aztecan, 0 Bantu).[8] In a diachronic perspective, it would also seem that there are stronger incentives for change in the former two, often in the form of marked alternatives (usually to do with information structuring) being re-analysed as basic orders – a mechanism of change clearly not prohibited by universal law in any department of lexicon and grammar. My hypothetical diachronic universal, thus, is: Basic order changes more readily, hence more frequently in verb-object and noun-genitive constructions than in adpositional phrases, because of the greater displacement potential in the former.

Returning to adpositions, determining the ordering of those words which are adpositions in the speech of their elders and constructing their own adposition-ordering grammar is not the only relevant task for learners. Sometimes deci-

8. And while "object" is clear enough (well . . .), it would be good to know what exactly is meant by "genitive": any kind of adnominal construction, irrespective of morphosyntactic realisation? In this comprehensive sense a language such as German, simply coded NGen by Dunn et al., would have to be recognised as showing both GenN (in the cross-reference construction: *der Kirche ihr Turm* 'the church-DAT its-FEM.SG tower'; with possessive pronouns: *ihr Turm* 'her tower'; with the *s*-form of names and terms of address, definiteness-inducing and probably not a genuine case form: *Oma-s Turm* 'Granny's tower'; in compounds: *Kirch-turm* 'church tower'; with certain derivational adnominals: *der Hölderlin-sche Turm* 'the Hölderlinian tower') and NGen (in the genuine genitive construction, not definiteness-inducing: *Turm der Kirche* 'tower of the church'; with prepositional adnominals: *Turm von der Kirche* 'tower of the church'). *WALS* can be asked the same question (Plank 2009). At any rate, the corresponding figures in *WALS* 2011 are: 101 languages (out of 1519) with no dominant order of verb and object, 96 languages (out of 1248) with no dominant order of noun and genitive.

sions need to be made as to whether particular words in particular constructions are adpositions-in-adpositional-phrases or something else in the first place. The differences here are sometimes less than categorical, and the reason often are historical transitions among words and phrases of different classes. Adpositions typically have been something else in their history (perhaps ALWAYS, going back far enough). They are not created *ex nihilo*, and they are comparatively rarely borrowed, as adpositions, from other speech communities: usually adpositions come about through the re-analysis ("grammaticalisation") of other kinds of words in other kinds of constructions in the speech of one's own elders. The commonest sources of adpositions-in-adpositional-phrases are verbs-in-verb-phrases (primarily transitive, typically in some non-finite construction), head-nouns-in-attributive-phrases (with body parts and other relational nouns as heads), and (local and temporal) adverbs gaining an obligatory complement; less commonly, adpositions derive from (transitive) adjectives and interjections through more complex re-analyses. Rarely, adpositions are case markers that have become detached (e.g., possessive *'s* in English and Danish). The other way round, sometimes adpositions can also be re-analysed ("degrammaticalised") as verbs or nouns or adverbs, equally blurring synchronic distinctions. Relevant instances are transitive verbs of possession deriving from locative or comitative adpositions ('have' $<$ 'at, with', in Maltese and elsewhere), German *ahnen* 'have a presentiment, feel' deriving from the locative preposition *an* 'at, on', English or German *utter/äußern* deriving from the ablative preposition *out/aus*, or the English noun *inn* deriving from the locative preposition *in*.

For present purposes, the only question about such grammaticalising or degrammaticalising re-analyses is what they are doing to order: Do, or indeed must, re-analysers maintain the ordering of constituents in the source constructions at the point when words and phrases are recategorised and are accordingly changed as to their semantics, morphosyntax, and phonology? Thus, whatever the ordering in the particular verb phrases (VO or OV) and in the particular adnominal phrases (NGen or GenN) re-analysed as adpositional phrases, will the new adpositions remain in the same position relative to their noun-phrase complements as their ancestral transitive verbs and relational nouns had been? And vice versa for the rarer reverse re-analyses. If so, this would then be another diachronic universal superintending the re-organisation of mental lexicons-and-grammars: Order must be preserved in grammaticalising and degrammaticalising re-analyses – at the point of re-analysis, that is, regardless of what is to happen subsequently.[9] Assuming such a law of order preservation, *ago* in English had no choice but to be a postposition, right at its origin, because the source

9. With respect to adpositions, Bybee 1988 is among those having made this general point before.

construction – an absolute participial clause to be incorporated into the main clause as an adpositional phrase: *Granny died, two years a-go-ne* – had the verb (stative-resultative prefix *a-*, strong suffix *-n* for resultative participle) after the noun phrase that was to become the adposition's complement. (Ditto for *fa* in Italian: *due anni fatti* 'two years done'.) Analogously, if French is to turn *il y a* 'there is' (lit., 'it there has') into a full-fledged adposition corresponding to English *ago*, it would be bound to be a preposition, because this ancestral existential clause has the verb before the noun-phrase-to-become-complement. Upon detachment, possessive *'s* in English and Danish was bound to be a postposition, because its source had been a case suffix (or one of its sources, the other being an enclitic possessive). On the same reasoning, causal *wegen* had no choice but to be an ambiposition in German, because in its source construction the head-noun-to-become-adposition (itself originally governed by a preposition, *von*, that was dropped in the process) could be before as well as after its genitive at the time of its grammaticalisation: *von dieser Sache Wegen – von Wegen dieser Sache* 'from the ways/sides of this matter';[10] order preservation here means preservation of variable order.

If order must be preserved in such re-analyses, this might have the net effect of introducing inconsistency. Suppose adpositions are grammaticalised from different sources with different orders, they will reflect this variety in their own ordering. Given an unmarked ordering OV for verb phrases and NGen for attributive phrases, then if these sources are tapped for new adpositions, some of these newcomers will be postpositions and others prepositions. Ironically, abiding by the diachronic universal of order preservation may thus mean violating a timeless universal of intra-categorial ordering harmony.

But again it is an empirical question whether order preservation is a valid diachronic law, and although random re-orderings are unlikely, I do not think it is a foregone conclusion that the answer will be a resounding yes. All kinds of things are known to be able to change concomitantly in grammaticalisation, including case government when nouns or verbs become adpositions: so why not the internal ordering of a re-analysed construction, too? English *ago* is an instructive case. In early instances of constructions to be re-analysed as (or already being, sort of) adpositional phrases, the participle-to-become-postposition could be after as well as before the noun phrase: cf. the earliest attestations in the OED: *For it was ago fif yer That he was last ther* (ca. 1314) – *I speke of mony a hundred yere a-go* (ca. 1386). In Romance, participle-derived adpositions 'ago' can be postpositional (Italian *fa*) or prepositional (Portuguese *há* < 'have', perhaps Spanish *hace* < 'make', if this is the right source), which

10. An alternative historical story sees this ambiposition as derived from an Indo-European verbal root, 'to wish, want'; but this would sit uneasily with *wegen*'s late acquisition of adpositional status (not before the fourteenth century, originating in Low German).

also suggests a certain positional independence from ancestral clausal syntax in the course of such re-analyses.

In the case of grammaticalisations, if constructions prior to re-analysis show freer order than after, with ordering at clause or sentence level characteristically freer than at the phrase or word level, then the question would be which of the alternative ancestral orders adpositions will be tied down to. Is there a universal diachronic law to the effect that the predominant order of EXISTING adpositions – and it seems that there virtually always and anywhere IS such a predominant order – will dictate the ordering fate of newcomers to this category? This would seem plausible, and the anti-universalist expectation that newly grammaticalised adpositions can adopt any arbitrary order independently of anything else in the respective grammars, past or present, would seem too negativistic. Still, the cases of English *ago* and Italian *fa*, made into postpositions amidst almost exclusively prepositions, urge caution. The question of whether or not there is such a diachronic universal prescribing intra-categorial uniformity for results of grammaticalisation needs to be investigated empirically. For reverse developments of de-grammaticalisation I feel confident that newly re-analysed verbs or nouns will always and everywhere follow the ordering rules for old verbs or nouns in the language concerned, regardless of whether their sources were pre-, post-, circum-, in-, or ambipositions. This would be a diachronic universal – not a timeless one, because at any particular time different subsets of verbs or nouns MAY be ordered differently relative to their arguments or dependents.

Instead of, or in addition to, the links between adpositional phrases on the one hand and verbal and attributive constructions on the other that are owed to historical ancestry, as inputs and results of re-analyses, others are conceivable that are timeless. Over and above their categorial differences, constructions may still be similar in a more abstract sense: their constituents, though of different kinds, may instantiate the same abstract relationship of head and dependent; or they may show other relevant structural similarities such as being non-branching (internally simple) and branching (complex, amenable to elaboration). Such higher-level similarities have a long record of inspiring universals of order:[11] all instances of head-dependent constructions or of constructions of non-branching and branching constituents, regardless of concrete categories, would accordingly have to be ordered analogously in every and all mental grammars at every and all times, with all kinds of heads/non-branching constituents before all kinds of dependents/branching constituents or vice versa. However formulated precisely (as a one-way or two-way implication, with or without further restrictions), this would be a timeless law (or laws), subserving

11. As further bases for analogising, there may be prosodic differences that come with such general differences of syntactic status (e.g., heads = prosodically weak, dependents = strong).

the ease of syntacting constructing and processing – and of learning, insofar as from order in ONE instantiation order in ALL OTHER instantiations of the same general family of constructions would be predictable for the learner.

3. Ye shall know them by their fruits.[12] **Do men gather grapes of thorns, or figs of thistles? [. . .] A good tree cannot bring forth evil fruit, neither can a corrupt tree bring forth good fruit. [. . .] (Matthew 7, 16, 18)**

Having set out – without undue universalist zeal – where I see questions of universality in the domain of adpositions, which ones have been answered in the negative by Dunn et al. 2011? Which ones have and have not been asked in the first place?

Their contention is that universals of cross-categorial ordering harmony, conceived of as timeless constraints, are too constrictive: they maintain that the ordering of adpositions relative to their complements is NOT implicationally related to the ordering of verbs relative to their objects and of head nouns relative to their "genitives"; while some large families show such dependencies, others don't. Well, ONE out of four they have investigated doesn't: Uto-Aztecan; Austronesian, Indo-European, and Bantu do. (Bantu does so convincingly as to overwhelm their statistics.) I do not in fact read Dunn et al.'s own figures for Uto-Aztecan so negatively as they themselves do in Figure 2 (reflecting the surprisingly low Bayes Factors of 3.82 and 3.21 they calculate for ADP-OBV and ADP-GEN): there seem to me pretty robust, in fact categorical rather than statistical implications in this family to the effect that (i) if OV, then postpositions, and not vice versa, and (ii) if GenN, then postpositions, and vice versa. But then, one reason for this debate in *LT* is that the findings and interpretations of Dunn et al. are not as uncontroversial as one might expect from such a high-impact publication.

In parentheses, the negative news of Dunn et al.'s are perhaps not entirely new. Far from being the "robust" universals that Dunn et al. introduce them as, the specific Greenbergian universals Nos. 2, 3, and 4 – in languages with prepositions/postpositions, the genitive almost always follows/precedes the governing noun; languages with dominant VSO order are always prepositional; with overwhelmingly greater than chance frequency, languages with normal SOV order are postpositional (Greenberg 1963) – have long been known not to be unproblematic: see discussion in the *Universals Archive* (Plank et al. 2011), Nos. 1, 2, 55, 489. On the other hand, Greenberg's results have the unusual stamp of quality of having been replicated through areally and genealogically informed re-sampling (Haspelmath & Siegmund 2006).

12. Jesus was metaphorically referring to false prophets here; but let's construe the reference of *them* as literally being to trees.

But the point I would like to make in the present debate is that there are more questions about universality than those addressed by Dunn et al. For one thing, intra-categorial ordering harmony would seem to be confirmed for adpositions by Dunn et al., if only tacitly. More importantly, however, questions of diachronic universals have not been addressed at all. This is a baffling omission, for two reasons. First, universals have often been conceived of as laws of development, and of co-development in the case of implications. Most prominently Joseph Greenberg, main target of Dunn et al., has long advocated the dynamicising of typology (e.g., Greenberg 1978, 1979, 1990; Croft et al. (eds.) 1990). The universals of cross-categorial harmony, questioned by Dunn et al., themselves have plausible diachronic interpretations – although the grammaticalisation link was admittedly not highlighted in Greenberg 1963. Second, what Dunn et al. themselves are setting against timelessly constraining universals, as "the primary factor determining linguistic structure", is "cultural evolution". In their own words, "the current state of a linguistic system [is] shaping and constraining future systems" – which is precisely what the laws of conformity, consistency-increase (or, as the case may be, variety-increase), intra-/cross-categorial harmonisation, and order preservation would be seeking to safeguard, universally. But such possible laws were not on their agenda.

For those putative laws they did deal with, concerning cross-categorial harmony, Dunn et al. did take history into account: indeed this is what distinguishes their study from pure crosslinguistic sampling studies. When your task is to induce crosslinguistic descriptive generalisations, you take a sample of languages, and although you do not see yourself as a historical linguist, you make sure your sample is genealogically (and areally) balanced. Since particular language families should not be over- or underrepresented in the sample, elaborate measures have been devised in sampling methodology to avoid such biases (e.g., the "diversity value" of Rijkhoff & Bakker 1998). Being of comparable time depth is an important prerequisite for families in the draw for equal representation: the idea is that the languages that will eventually make it into a typological sample have all had about the same time to develop independently and realise their potential for diversity. An alternative way of controlling for family membership influences on crosslinguistic generalisations and of eliminating family biases is to judiciously select several samples and to see whether the results are replicable through repeated re-sampling (e.g., Haspelmath & Siegmund 2006).

Now, rather than keeping them separate, Dunn et al. integrate the inductive inference of crosslinguistic generalisations with genealogical grouping. The languages in their sample come with their genealogies, and the focus of their methodology is on the teasing apart of those individual and correlated similarities among languages which are owed to common origins from those which

are not. There is no need to worry over sampling here: any selection will do, really. Controlling for shared inheritance, which can be done for any group of languages, provided its genealogical structure is known, is here at the core of the business of comparing.

The methodology championed by Dunn et al., inspired by computational phylogenetic methods used in evolutionary biology, has been amply discussed elsewhere in this issue. I will only point out a few peculiarities of Dunn et al. 2011 as a contribution to historical-comparative linguistics, which I think throw doubt on their typological conclusions.

Dunn et al. consider the family trees for their four families which they crucially use for controlling for shared inheritance "well-established". This is debatable, and on rather fundamental grounds. The nodes in classical family trees in historical linguistics are based on shared INNOVATIONS, and for good and oft-discussed reasons. Dunn et al. base their trees on shared RETENTIONS, limiting themselves to the lexicon. They compare basic vocabulary across languages for cognates,[13] and construct optimal ("maximum clade credibility") trees consistent with the sharing and not-sharing of cognates among the languages concerned. Sharing cognates means the respective languages have retained the same lexical items; not sharing cognates means lexical replacement has occurred somewhere.[14] Given such different bases, it is instructive to see how the non-standard shared-retention-based trees of Dunn et al. compare to standard shared-innovation-based trees. For example, for higher-level subgrouping in Indo-European, Dunn et al. have Hittite vs. rest as the highest branching and Tocharian A/B vs. rest as the next branching – which is not *communis opinio*, but not wildly idiosyncratic, either. Further down, the subgrouping of Armenian-Albanian-Greek vs. all the remaining rest and the two-way subgrouping of that rest into Italic-Germanic vs. Celtic-BaltoSlavic-IndoIranian are more unusual. It would, further, be instructive to see how playing around with different trees (like Rijkhoff & Bakker 1998 did in diversity-value sampling) would influence the results of the controlling for shared retentions in the typological part of the enterprise.

Decidedly idiosyncratic is the presence of older stages of several languages as sisters or aunts of the modern stages of the same languages in genealogical trees. For example, Ancient Greek and Modern Greek appear as sisters

13. And let's grant that cognate identification is not a huge problem. At any rate, where it is problematic it is a problem for anybody, not just Dunn et al.

14. And let's here ignore other mechanisms for loss or gain of recognisable cognacy status, such as phonological and semantic change, and lexical specialisation and marginalisation.

and Old English appears as a sister of all of Modern West Germanic including Modern English in the Indo-European tree of Dunn et al. In a sense this brings in genuine diachrony, but to no systematic avail.

Best practice in traditional historical-comparative linguistics requires that trees are accompanied by diachronic stories. First, systematic form correspondences are identified between languages: what is now SYSTEMATICALLY different is conjectured to have once been the same. The diachronic stories around systematic correspondences are about the sequence of states involved in the change: What was the initial state and what the resultant state? And the concomitant question is what the change of state consisted in. For example, when a historical linguist observes that English words systematically have plosives /p, t/ where High German words have affricates /pf, ts/ (*pound – Pfund*, *apple – Apfel*, *town – Zaun*, *sit – sitz-en*, etc.), s/he will want to know whether the common ancestor of these languages had plosives, affricates, or perhaps something else; the most plausible story here is (and in this case it happens to be documented, too) that the initial state was /p, t/ and the resultant state /pf, ts/ rather than the other way round, because of the possible changes that would make the connection, affrications are phonologically more natural than de-affrications. A further part of diachronic stories is, or ought to be, about the time course of change: How many generations did it take for the change to be effectuated in a speech community, three or (many) more? Answers will depend greatly on the kind of change (Neogrammarian or diffusional in the case of sound change) and on the size and homogeneity of the speech communities through which change is diffusing.

Dunn et al. have none of this: no diachronic stories, only syntactic variables with three values (for two ordering alternatives and no dominant order) for each language, related to other languages in tree structures. There is therefore no way of telling which orders are initial states of changes: those of the languages at the ends of branches must be resultant states, but no BRANCHING NODES, representing proto-/common-languages, bear values for the variables. For example: Afghan (Northern Pashto?) has prepositions, its sister Waziri (Central Pashto dialect) has no dominant adpositional order, both have OV; but what had Common Afghan-Waziri been like and what had happened since? Both Persian (Western Farsi?) and its sister Tadzhik have prepositions (and OV), but on their sister branch are no-dominant-order Baluchi and the sisters (?) Ossetic with postpositions and Wakhi with no dominant order: Did Common Indo-Iranian have prepositions, postpositions, or no dominant order, and how had the original adpositional order, whatever it was, been re-analysed? As to rates of change, with a substantial time depth of the trees (at 8,700 years that for Indo-European would be considered somewhat exaggerated by most experts), lots of change events may have occurred as the trees were branching. Further, no provisions are made for different mechanisms of ordering changes, or of

changes implicating order, to be inherently faster or slower: they are all treated alike, switching the value of a variable.[15]

I defer to others for assessing how well Dunn et al. can distinguish independent and correlated changes of variables: their own contention is that there are correlated changes, but different ones in different families. Hence no universals.

What I find difficult to follow from the paper and the Supplementary Information is how "transition probabilities" enter the picture, which Dunn et al. introduce when speculating about what may be behind the lineage-specific correlated changes. They claim that in Austronesian changes are primarily leading to the correlated states adposition-NP (i.e., prepositions) & VO, while in Indo-European two separate correlated states are major "attractors", adposition-NP & VO on the one hand and NP-adposition (i.e., postpositions) & OV on the other. Universalists might in fact welcome such findings, because the resultant state-pairs, though different for the two families, show harmonic orders in both: disharmonic orders are changed for the better. But then, where do DIRECTED transitions come from to begin with, with diachronic stories that would identify initial and resultant states missing? Or am I missing something?

For my own favourite family, Indo-European, the transition probabilities posited in Figure 3 see me at a loss. One of the thick arrows leads from the state pair NP-adposition & VO to NP-adposition & OV. When and where in the several thousand years of a dozen or so Indo-European subfamilies is VO supposed to have changed to OV, with postpositions remaining postposed? It is primarily the Indo-Aryan branch where postpositions predominantly pair up with verb-final order: but are there grounds to assume that verb-final order is here – or anywhere else in Indo-European – an innovation? The second thick arrow in Indo-European leads from adposition-NP & OV to adposition-NP & VO. This is an attested change, but the arrow's thickness is somewhat exaggerated owing to misanalysis or miscategorisation: for some of Germanic and perhaps Romance and Slavonic Sorbian, basic order remained OV, and what was added was a rule of finite-second (which is obviously not the same as SVO or no-dominant-order).[16] There are always dissenting voices with syntactic reconstructions, but being harmonically postpositional, verb-final, and head-noun-

15. Greenhill et al. 2010 add little of real value here. Acknowledging how little they know about the shape and tempo of (non-lexical) language change, based on *WALS*, they conjecture that syllable structure, velar nasals, case syncretism, coding of plurality, tense/aspect suppletion, an optative, and especially their absences, might be among the stabler features.

16. Kashmiri too innovated verb-second, but this Indo-Aryan language had and kept postpositions.

final would seem by far the safest bet for proto-/common-Indo-European – and Dunn et al. do not have this as a designated point of departure. The frequent preposing of postpositions – not captured in the transition probabilities of Figure 3, either, which have an equally thick (or thin) arrow for the postposing of prepositions – would seem to be somewhat of a mystery for Indo-European, as long as verbs and head nouns remained final; but they frequently did not remain final, either. In Figure 3 there is no arrow directly leading from NP-adposition & OV to adposition-NP & VO, and no other direct link involving simultaneous changes of two variables, either; but there is at least one Indo-European language with such a history behind it: Romany. When Indo-Aryan Romany left India, it went from one harmonic state to another, switching both variables together. Which suggests that changing orders can be done fast and without much re-analytic ado. Which suggests further that language contact across families (and within, too) can be a stabilising influence (witness South-Asia, the most conservative area as to head-postposing) as well as an accelerator (with Europe an area favouring prepositions and perhaps other head-preposing) – and certainly should be taken into account in modelling linguistic development.

What is my moral? Let me end by quoting two eminent typologists who could not have been clearer that typology needs diachrony, but that diachrony does not come for free.

Joseph Greenberg once criticised an attempt (Nichols 1992) to infer linguistic development, and in particular historical stability, from crosslinguistic distributions (Greenberg 1993: 505):

> 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.

As Greenberg (1978: 76) saw it, change or non-change (time stability) was primary and crosslinguistic distributions were secondary, following from diachrony:

> 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 highly

> infrequent or non-existent and sporadic in its geographical and genetic distribution [...].

Johanna Nichols concurred, issuing a programmatic call to investigate the developments themselves which determine crosslinguistic distributions (Nichols 2003: 290):

> 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.

Despite their evolutionary rhetorics, I doubt whether Dunn et al. 2011 can be construed as concurring, too. They have added genealogies to crosslinguistic distributions, rather than using them to control sampling. But to meaningfully model developmental dynamics and to get an angle on what changes individually and what changes together, it needs full diachronic stories, however hard these are to research. (Evidently, the Linguistic Birth & Survival programme of Nichols's isn't child's play, or by now it would have progressed much further.) Only then can such modelling hope to shed light on how – by chance or from necessity – mental lexicons-and-grammars are the way they have become.

But then, as far as such analogies go, linguistics does not really differ from biology here. Take Gregor Mendel: he could not have found his two laws of inheritance, that of Segregation and of Independent Assortment, without making DIACHRONIC studies, carefully studying (hybrid) generation after generation, not of minds, but of peas.

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Universität Konstanz

Correspondence address: Sprachwissenschaft, Universität Konstanz, 78457 Konstanz, Germany; e-mail: frans.plank@uni-konstanz.de

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