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NOTES AND DISCUSSION

More myths

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It is not a novel or radical position to argue that possibilities for syntactic reconstruction are very limited; this was the view of Allen (1953), Collinge (1960), Hoenigswald (1960: 137), King (1969: 140), Dressler (1971), Anttila (1972: 355ff) and many others. I revisited the matter of these limitations in Lightfoot (2002a) with a view to examining claims of recent work that there were new tools which permit more extensive reconstructions, notably Harris & Campbell (1995) and work based on grammaticalization theory. In my view, the new work does not significantly change the possibility for reliable reconstruction.

Now Campbell & Harris (C&H, this volume) have misinterpreted some of Lightfoot (2002a) and I need to disentangle the 'Lightfoot' that they have mythologized. I will leave much of this for the reader, who can compare what I wrote, what C&H attribute to me, and what we have each written elsewhere. Let us just look at their first paragraph and then we will move to the issues.

First, they say of Lightfoot (2002a) that 'the intent appears to be to deny that syntax can be reconstructed' (p. 599). Later they become more extravagant: by p. 603 'Lightfoot attacks both reconstruction generally and syntactic reconstruction specifically', by p. 611 I am 'dismissing the entire enterprise' and 'denying reconstruction of syntax generally'. Compare what I wrote: 'reconstruction of aspects of Proto-Indo-European (PIE) was one of the great intellectual achievements of the nineteenth century' (114). Far from showing reconstruction in general to be impossible, my stated intent was to distinguish ABSTRACT, formulaic reconstructions, 'artefacts which express historical relationships precisely and abstractly and involve various idealizations', from REALIST reconstructions whereby one can establish

> some prior reality, learn about how people spoke prehistorically, before our records begin, and even learn something about the nature of change … [in the domain of syntax] the possibilities for reliable reconstruction of this [latter] type are quite limited, because getting beyond

> the limitations requires a theory of change which seems to be unattainable. (Lightfoot 2002a: 114)

So I sought to demonstrate the relationship between realist reconstructions and certain kinds of theories of change, and concluded that 'realist reconstructions are very limited and unlikely to tell us anything new about the nature of change because change is "chaotic"' (114) but they are not impossible.

Second, they say that 'Lightfoot promotes grammatical theory ... as the only significant explanation of grammatical change and denies the legitimacy of any alternative view' (p. 599). I wrote that 'explanations for structural changes may be GROUNDED in grammatical theory' (113), but not that grammatical theory itself constitutes the explanation, which would be inconceivable. Rather, a grammatical change is explained when one points to new primary linguistic data and the relevant parts of grammatical theory which TOGETHER guarantee that a particular grammar emerges in the innovating child. This assumes the usual explanatory schema (1) ((3) in Lightfoot 2002a), whereby children, endowed with a particular set of genetic properties (elements of UG), acquire a particular grammar (part of their phenotype) on exposure to a particular triggering experience (a certain set of Primary Linguistic Data, or PLD):

(1) (a) triggering experience (linguistic genotype → phenotype)
    (b) Primary Linguistic Data (UG → grammar)

This makes grammar change a particular instance of grammar acquisition, where the new grammar is a function of new PLD. This does not '[deny] the legitimacy of any alternative view' (p. 599), but I did criticize certain deterministic approaches to change which have been proposed in the literature and which have been used as a basis for postulating reconstructed forms, including the approach of Harris & Campbell (1995) based on notions of cognate sentence patterns and directionality.

After this start, C&H proceed to muddy the distinction between the abstractions that the comparative method generates and the nature of the reconstructed proto-language, calling the former a hypothesis about the latter and claiming that everybody is a realist. This is to beg all the questions which fascinated Meillet about discrepancies between the results of the comparative method and the nature of the parent language in cases where we have attestation. His favorite laboratory involved seeing how much of Latin one could and could not reconstruct simply by examining the modern daughter languages. If the word for 'five' generally has an initial fricative in the daughter languages (Portuguese *cinco*, Spanish *cinco*, French *cinq*, Italian *cinque*, etc.), it is not possible to reconstruct the initial stop of Latin *quinque* by the comparative method. Discrepancies of this kind led him to point out the limitations to reconstruction and to draw the distinctions in the passage

I quoted. C&H's reading of this is that 'both the "abstract" and the "realist" camps are actually relativized realists, seeking to recover a real past but acknowledging limitations' (p. 600). If C&H prefer to see the 'abstract' reconstructions as hypotheses about the parent language, hypotheses subject to severe limitations, then there is little difference between our positions. The point is that we know that the 'hypotheses' achieved by the comparative method often differ sharply from the parent language, where we can test them against evidence, and sometimes it is not possible even to formulate relevant hypotheses by the comparative method. That is why syntacticians have sought other methods for their reconstructions.

Analogous to the problem of reconstructing Latin *quinque*, analyzing the syntax of the modern Romance languages by the comparative method is unlikely to yield the case endings or the characteristic object-verb word order of Latin. Similarly, some of the modern Indo-European languages allow null subjects, others do not, but nobody has any reliable idea of whether the speakers of the parent language, perhaps akin to some form of reconstructed Proto-Indo-European, allowed null subjects or not.

Whatever the motives of comparativists, from Meillet on they have known that there is likely to be much about a parent language that they cannot reconstruct. That doesn't bother them if they are seeking to express historical relationships among languages. They are working with myths, created by following precise methods and expressing historical relationships, hence useful myths. Far from 'disparaging of the work of practicing historical linguists', undermining the work of linguists who 'have been at some pains to argue to the public at large that linguistics is a "scientific" discipline', and being 'deleterious to the field' (p. 602), I wrote that 'It is a myth, an appropriate one, and one which involves other idealizations. I do not use the term pejoratively; myths may have functions and this one certainly does' (115). The point was to distinguish this abstract view of reconstruction from 'the view that one can reconstruct a prior reality ... and that realist view is based on notions of language change which seem to be false, as we shall see – myths in the pejorative sense' (115).

C&H seek to read the passage from Meillet very differently and they object to my translation: 'note that Meillet's *correspondances* is "correspondences" in the technical sense of comparative linguistics and not merely "similarity", as Lightfoot translates it' (pp. 600–601). I invite readers to consult my translation, where they will see 'correspondence' used five times and 'similarity' not once. This is part of a bigger deal for them: they attribute to me the

> misconception that reconstruction is based on similarities [whereas] it is based on much more – on strict correspondences. A correspondence is a relationship between specific sounds, morphological patterns or sentence patterns that are established inductively from numerous examples; a

> similarity, on the other hand, is a vague resemblance, and may be due to borrowing, chance and other non-inherited factors. (p. 603)

Now compare this to what I wrote:

> A crucial preliminary step in applying the comparative method is that one needs to have a notion of a corresponding form. We at least have intuitive notions about what these might be in the area of the lexicon, and these are words which are cognate, having the same historical source: French *père* corresponds to Spanish *padre* and to Latin *pater*. However, it is hard to know what a corresponding form could be in syntax, hard to know how one could define a sentence of French which CORRESPONDS to some sentence of English, and therefore hard to see how the comparative method could have anything to work with ... It certainly cannot be keyed to cognateness, since there is no sense in which two particular sentences are historically cognate, coming from the same historical source. (Lightfoot 2002a: 119)

So, yes, correspondence is crucial if one uses the traditional comparative method, and Campbell, Harris and I agree on this point.

One place where we disagree is that they believe that sentence PATTERNS, not sentences, may be historically 'cognate' (p. 607) and constitute points of correspondence; I will leave readers to decipher their claims both here and in Harris & Campbell (1995). I will also leave readers to decide whether their quotations from Meillet (1967), disregarding their misleading interpolations, support their claim that he thought that he was reconstructing prior reality; in my view, they do not. To establish the EXISTENCE of a parent language like Latin (attested) and proto-Germanic (not attested) is very different from establishing its PROPERTIES, just as there is a big difference between the insight of Sir William Jones that there must have been a parent language from which Greek, Latin and Sanskrit were derived and the detailed reconstructions of Karl Brugmann one hundred years later. In one of their quotations, Meillet writes: 'But it remains to understand what happened between the period of "unity" and the date when the languages begin to be attested' (Meillet 1967: 59). Similarly, their quotation from Bloomfield undermines their claim: 'We assume the existence, at some time in the past, of a *Primitive Germanic* parent language, but the speech-forms of this language are known to us only by inference' (Bloomfield 1933: 299).

In fact, notice the scare quotes around Meillet's 'unity'; French was derived from the Latin spoken in Gaul and Spanish, a different language, was derived from a somewhat different source, the Latin spoken in the Iberian Peninsula. The idea that there is a single, homogeneous parent language is an idealization inherent in the comparative method, an innocuous (indeed, necessary) idealization if one is constructing abstract representations of historical relationships but not if one is seeking to identify some prior linguistic reality.

'Lightfoot suggests', they tell us, that a language might be 'a member of Family$_i$ in its phonology but a member of Family$_j$ in its syntax' (p. 603). Nowhere did I write any such thing; I was discussing the limitations of historical claims based on the comparative method. Indeed C&H's paper is full of statements about what I 'suggest', 'imply' or 'appear to deny', when the words say something very different.

'Throughout his paper, Lightfoot seems to imply', here we go again, 'that the syntactic reconstructions discussed in Harris & Campbell (1995) are of the sort proposed by word order typologists in the 1970s' (p. 605). Having discussed the difficulties of using the traditional comparative method in syntax, I pointed out that new methods were needed and briefly considered again the work of the 1970s word order typologists, concluding that the 'typological approach was not able to provide useful tools' (p. 121). But maybe there were other, more recent approaches which could be more successful? Then, beginning a new section:

> In recent years there have been new attempts to reconstruct proto-syntaxes, new claims that it is indeed possible beyond the narrow limits just discussed, and things have appeared to be different. There are two lines of work, one represented by Harris & Campbell (1995), which I will discuss in this section, and the other by work based on grammaticalization theory (section 5). (Lightfoot 2002a: 121)

Harris & Campbell (1995) are treated very differently from the typologists; they are treated differently because they have different methods, introducing the notion of cognate sentence patterns.

C&H are concerned here, quite oversensitively in my view, to distinguish themselves from the word order typologists, but they get themselves into a tangle. They point out that they have criticized word order reconstructions and 'cautioned' against word order claims in reconstructed systems. But now they go full circle; they are so keen to distinguish themselves from the typologists that they claim precisely what I was arguing, that one can reconstruct certain kinds of things but not others, because of limitations in our methods. As they put it when they began to conclude their chapter 12, 'There are many obstacles to successful syntactic reconstruction and their impact should not be underestimated' (Harris & Campbell 1995: 374). I agree completely.

I am mystified by what they describe as my 'attack on the treatment of the *adder* example,' which 'is, in effect, a misunderstanding of the whole field of historical linguistics' (p. 609). I wrote that, in this case, Harris & Campbell's 'strategy is to identify relics and archaisms, on the one hand, as distinct from innovative forms, on the other ... This seems to be a perfectly appropriate strategy' (p. 124) and is an instance of successful reconstruction, it seems to me, even where the daughter languages do not show identity. Again, one can reconstruct in some cases, and the discussion of 'adder' was an endorsement of their methods in this particular case, not an attack.

Campbell, Harris and I agree that reconstruction of syntax is sometimes possible but that there are limits. We disagree about where the limits to reconstruction lie and their cognate sentence patterns do not seem to me to be useful.

We disagree more radically, however, in how we view the directionality of change and acquisition. C&H try to distinguish DIRECTIONALITY (the notion that change follows a direction) from UNIDIRECTIONALITY (presumably, the notion that change proceeds in one direction and not the reverse), a distinction without a difference that I can see. They say, correctly, that directionality is key to their reconstructions but then add that 'the claim of unidirectionality is untrue' (p. 614), accusing me of confusing the notions. In fact, as I noted, Harris & Campbell (1995: 362) invoke unidirectionality: they claim that partitives may emerge from genitive or locative cases and not vice versa: 'It is assumed that this development is unidirectional, from genitive or locative to partitive use', they say, and they conclude their discussion:

> The known universal directionality (ABL > PART) of the change provides part of the means by which we determine that ablative was the original syntactic function of this case form. Thus, this example shows how directionality of change can provide a basis for reconstruction. (Harris & Campbell 1995: 363)

They note correctly that I am skeptical of notions of (uni)directionality. They say that certain lexical verbs have become modals in a change which 'took place independently over and over in language after language' (p. 613), but they cite no examples. The claim of certain lexical verbs becoming auxiliary modals has been made for the history of English (Lightfoot 1979, 1999), but no comparable argument has been made for any other European language, as far as I am aware, and not 'over and over in language after language'. However, even if C&H were right and if the reverse change of a modal becoming a lexical verb is less common, then, they say, we have a 'statistical universal ... and this fact still requires an explanation' (p. 614). It has never been clear to me what is to be explained if some phenomenon occurs 70% of the time, whatever that might mean in actuality. And that brings us to the radical disagreement.

C&H complain that I allow only local causes for grammatical shifts, namely, new primary linguistic data which trigger a new grammar in some children. They play fast and loose, alternating between 'grammar change', a property of individual brains, and 'language change', a group phenomenon. Grammars, as I and others construe them, are biological entities, while languages are not coherent units and least of all biological notions; the sentences of English are manifestly not a recursively enumerable set. I have sought to explain GRAMMATICAL change in terms of the triplet of (1) above. Apart from pathological cases, a person will grow a different grammar if and only if she is exposed to different initial experience, different primary linguistic data. It is conceivable that there might be some chance factors, but it is hard to imagine how that might be shown and even harder to see how that could drive change across a population, certainly at this stage of our understanding. Now, grammar change is only part of language change and there is more to language than grammar; a body of language (i.e. expressions actually used) will be influenced by how a speaker uses her grammar (the stuff of discourse analysis). And a body of language seen as a group phenomenon (i.e. expressions used in some community) may change as a result of changes in the USE of grammars, perhaps socially induced. For discussion, see Lightfoot (1999) and the introduction to Lightfoot (2002b). So the triplet of (1) is a mechanism to explain grammatical change but that is only part of language change, which must be explained (if possible) in ways which go beyond grammar change (see Lass 1997 for soundly skeptical discussion).

These are simple category distinctions, which need to be kept straight. The focus of my work has been on grammars and grammar change, and Lightfoot (2002a) dealt with the reconstruction of grammars. It seems to me to be entirely appropriate to say that a child, endowed with certain genetic properties, grows a certain grammar on exposure to certain experiences. Those experiences, the primary linguistic data, are the sole source of grammatical variation and therefore of grammatical change. Hence 'local causes', although that is not enough for C&H. In particular, a child's grammar is not influenced by factors outside those initial experiences and certainly not by properties of an ancestor's grammar or language nor by the fact that the language has a certain history. This is what historicist notions of GRAMMATICAL directionality entail for the acquisition of grammars and I have criticized those notions extensively (e.g. Lightfoot 1999). They involve not just myths about grammatical change, but downright mysticism.

My article tried to explore where reconstruction of grammars might be possible, where it was undermined by demonstrably false notions about change, and where it seems not to be possible. If somebody thinks that they can reconstruct grammars more successfully and in more widespread fashion, let them tell us their methods and show us their results. Then we'll eat the pudding.

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