From a diachronic perspective

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As a historical linguist I welcome Martin Haspelmath's proposal that "[w]e need to consider diachronic change if we want to understand why OT constraints are the way they are".[1] Haspelmath argues that grammars come to be adapted to speakers' needs in ways that do not suffer from some of the worst aspects of the Teleological Fallacy. This is an argument that deserves to be seriously pursued. But if this pursuit is ultimately to be successful, a number of empirical questions will need to be answered.

(1) What motivates innovation and change? First and foremost is the question „How is it that variants are created from which speakers may choose?" It is a given of OT that differences among dialects and stages of a language can be accounted for in terms of different rankings. But what leads to the creation of the variants in the first place? And what motivates the different rankings? What is it in a particular cognitive and communicative situation that leads to speakers choosing "those variants that suit them best"? In other words, how do speakers' choices interact with the long-recognized mechanisms such as reanalysis, analogy/extension and borrowing (e.g. Harris and Campbell 1995), and the motivations of speed and clarity (e.g. Langacker 1977)? Most especially, what is it that leads to innovation by the individual (which may not lead to change) and spread to the community (which does lead to change)?[2]

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1 I am grateful to Brady Clark for stimulating discussion of the issues, and for comments on an earlier version of these remarks.

2 The importance of the distinction between innovation/actuation and spread was highlighted in Weinreich, Labov and Herzog (1968), and has been reemphasized by Milroy and Milroy (1985), Croft (In press).

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(2) Who originates change and who passes it on? Haspelmath does not specify what age the innovators and leaders of change are. One may infer that they are not small children, since Haspelmath appears to conceptualize them as fully able to adapt language to their needs; they may be preadolescents and even adults, on the assumption that the grammars of adults are subject to modification. One may also infer that they are conceptualized as producers/speakers rather than as interpreters/hearers, since they are active users of language (talkers engaged in Keller's maxims of action), not perceivers of it. These perspectives are widely held by functionalist historical linguists (e.g. Hopper and Traugott 1993, Bybee, Perkins and Pagliuca 1994, Croft in press), and also by sociolinguists (e.g. Labov 1982) and psycholinguists (e.g. Slobin 1994). They are, however, diametrically opposed to claims made by some formal generative linguists such as Lightfoot (e.g. 1999), who build on a tradition going back to Halle (1964) and far beyond, that only young children can innovate, and that they do so because they do not necessarily infer (or "abduce", see Andersen 1973) the same system as that of the input. Presumably an adaptive theory focuses not on innovation in the individual young child (who is acquiring a ranking), but on change, and spread through the community of older children and adults.

(3) If constraints are adaptive how should we think about non-change? In some ways the most interesting question for a historical linguist is precisely why change does not happen. One particular phenomenon is that a great deal of language gets sedimented as residue or "junk", only some of which gets to be reused in later changes in a process called "exaptation" (Lass 1990). Sedimentation is a problem for synchronic linguists because it tends to be non-systematic (e.g. morphophonemic alternations like those found in *house-houses* or *foot-feet*). It is a problem for diachronic, especially functional, linguistics, because such "junk" typically does not undergo change: Lass (1997) compares it to the non-functional persistence of male nipples in human biology. It is most particularly a problem for any theory of change that privileges simplification, whether construed as "therapeutic" change (see Lightfoot 1979) or optimization by rule extension/analogy (see Kiparsky 1968). From an adaptive OT perspective such inertia presents a challenge that must be addressed.

(4) Are any changes not adaptive? In Ft. 10 Haspelmath suggests a positive answer: "I am not claiming that all of language change is adaptive and motivated by user optimality". Which changes are to be excluded, and why would it be that they are? When in this footnote Haspelmath

> proposes that grammaticalization is a change that is not adaptive and at the same time acknowledges that it "probably account[s] for the great majority of morphosyntactic changes" he potentially undermines his whole enterprise: how effectively can a theory account for "why OT constraints are the way they are" if it does not address the "majority" of some set of changes.

It may be useful to consider under what set of assumptions grammaticalization might be considered not to be adaptive. One possible assumption may be that selection of morphosyntactic categories such as AUX or of rules such as V to I raising is not subject to the constraints discussed in the paper. Another possibility is that despite the concept of competing motivations discussed in Section 1, in fact the assumption is that "less is more", and therefore any increase in choice of members of a category (e.g. increase in the number of modals available), or in structure (e.g. the development of the category COMP) is "inflationary" and therefore not adaptive from an OT perspective. But surely morphosyntactic changes are as much subject to speakers choosing variants that suit them best as any other changes if one takes seriously the hypotheses that (a) some version of the competing motivations "be quick" (for the sake of speaker production) and "be clear" (for the sake of communication with the addressee) is correct, and that (b) internalizing a grammar does not involve only learning a lexicon with salient semantic and phonological components, but also morphosyntax, which provides the "glue" and the hierarchies that signal linguistic as opposed to dictionary meaning.

Consider, for example, the development of the CP in later Germanic and of the DP in Romance as outlined in Kiparsky (1995) and Vincent (1997) respectively. Let us suppose Kiparsky's analysis is correct that Indo-European had two left-peripheral operator positions, the inner hosting focal elements like *wh*-phrases and demonstratives, and an outer position binding a resumptive pronoun in the argument position; in later Germanic as adjunct clauses were reanalyzed as arguments/embedded clauses, indeclinable complementizers were innovated that, though derived from nominal elements such as demonstratives (e.g. *þæt* 'that') and quantifiers lost all their nominal properties. Let us also suppose that Vincent's analysis is correct that in Classical Latin there were no definite articles, only deictic or emphatic demonstratives; that in Romance there was a division of labor between the second person deictic *ipse* and the third person distal deictic *ille*, such that the former came to express focus/contrast and the latter topic/given information; that the weak pronoun *ille* became a clitic; and that finally a new category, DP arose. In both cases, important functional categories were introduced. In both cases there was grammaticalization from a fuller, more lexical element to indeclinable operator function (most instances of grammaticalization involve not just decategorialization of a full lexical item in a construction, but rather the further decategorialization of an already grammaticalized element). In both cases the data demonstrate uncontroversially that the new structure did not arise out of nothing, or as replacement of some putatively "lost" category marker, but out of the discourse uses of semantically susceptible elements.[3] Under what understanding of adaptation is it reasonable to exclude this sort of change from the kinds of phenomena that should be accounted for?

Grammatical operators may not be phonologically salient (hence MAX LEX), but they appear to be informationally and communicatively highly so in languages with certain kinds of word order and morphology. The innovation of grammatical, i.e. functional categories, is an issue that a theory of adaptive OT constraints must come to grips with. Otherwise we will not be able to understand why OT constraints are the way they are or implement a program of research that gives priority to understanding "synchronically adaptive structures ... in terms of a diachronic process of variation and selection".

3 For arguments against recursive cycles in grammaticalization in which a form (e.g. a case marker) becomes zero and is "replaced" by a fuller one (e.g. a preposition) see Lehmann (1985).