

# Rescuing traditional (historical) linguistics from grammaticalization theory\*

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## 1. Introduction

There is a rich history, dating back, according to Heine (2003), at least to observations in the 18th century by Condillac (1746) and Horne Tooke (1857) (first published 1786), if not earlier, associated with the study of the phenomenon of grammaticalization, i.e. how grammatical elements arise out of less (or non-) grammatical elements.<sup>1</sup> Still, only in the past twenty-five or so years has it attracted considerable attention among professional linguists. Indeed, what began as an interesting observation – one might think of Givon's (1971) dictum that “today's morphology is yesterday's syntax” as the starting point for the ‘modern’ era of grammaticalization studies, though Meillet (1912) and Kurylowicz (1964) are also possible benchmark studies in this regard – has turned into a ‘cottage industry’ of sorts, and is now to the point where it can be considered a ‘movement’.

Among the evidence that points towards such a ‘movement’ are the following indicators. First, there is a huge amount of relevant literature now, with textbooks and surveys (e.g. Diewald 1997; Heine, Claudi, & Hünemeyer 1991; Hopper & Traugott 1993; Lehmann 1982/1995), two dictionaries or similar compendia (Lessau 1994; Heine & Kuteva 2002), singly-authored studies (e.g. Bowden 1992; Heine 1993, 1997; Kuteva 2001; Ziegeler 2000, to name a few), and numerous edited volumes (such as Pagliuca 1994; Ramat & Hopper 1998b; Traugott & Heine 1991b; Wischer & Diewald 2002, among many others) all dedicated to different aspects of the study of grammaticalization phenomena. Second, there are now many conferences devoted to aspects of grammaticalization.<sup>2</sup> Third, ‘grammaticalization’ is mentioned specifically as a separate category in that part of a recent annual report of the editor of *Language* enumerating the different sub-areas for submitted papers (see Aronoff 2002) and it is a subheading in Library of Congress subject

headings for classifying the content of books.<sup>3</sup> Finally, a less traditional but no less indicative fact of relevance here is that searches of the World-Wide Web using a search engine such as ‘Google’ (www.google.com) yield the results noted in (1):<sup>4</sup>

- (1) ‘Grammaticalization’ searches on google.com  
 using search word *grammaticalization*: 6460 hits  
 using search word *grammaticalisation*: 1770 hits  
 using search word *grammaticization*: 783 hits  
 using search word *grammaticisation*: 104 hits

These numbers are fairly robust, to be sure, though admittedly other frameworks for the study of grammar yielded higher numbers; for instance, HPSG (Head-Driven Phrase Structure Grammar) yielded some 17,000 hits, Optimality Theory 88,000 hits, and Minimalist Syntax 19,300. Nonetheless, in absolute terms, the results in (1) suggest a high level of scholarly activity.

All these indicators suggest that ‘grammaticalization studies’, generally referred to now as ‘grammaticalization theory’ (cf., e.g., Heine et al. 1991; Heine & Kuteva 2002:2),<sup>5</sup> have ‘made it’, so to speak, and command the attention of serious linguists. These various pointers are telling, as is the fact that some of the more exuberant and ardent practitioners of grammaticalization have put forth very strong claims about the relationship of grammaticalization studies to other areas within linguistics and to the very field itself.

For instance, Hopper (1991:33) suggested that the study of grammaticalization is coterminous with the study of language change itself, saying that “since grammaticization is always a question of degree, not an absolute, the criteria which control this gradation are not restricted to grammaticization, but are simply general criteria of change”, and this viewpoint was picked up by Ramat and Hopper (1998a:3–4) in their statements that “an extreme formulation ... is that ultimately all grammaticalization is not separately definable from the concept of change in general “ and that “we may still speculate on whether there are cases of language change that are not part of the phenomenon of grammaticalization”. Moreover, it is echoed as well in the assertion by Katz (1998:95) that “the process of grammaticalization is a constant force that drives language onward, and it can account for all change that moves from iconicity to formalism”.<sup>6</sup> To be fair, there is disagreement expressed in the grammaticalization literature on this very point: for Traugott and Heine (1991a:3), for instance, grammaticalization is merely “a kind of language change”, and so does not encompass all change.

Furthermore, some have taken an expanded view of grammaticalization theory in which it essentially points the way to a new theory of language itself. This view is implicit, for example, in Hopper’s ‘emergent grammar’ model (Hopper 1987) in which “there is ... no ‘grammar’, but only grammaticalization – movements

towards structure” (148). And, as Newmeyer (2001:188) describes this particular stance, for “Heine et al. (1991:1), ‘grammaticalization theory’ challenges what they see as the predominant conception of theoretical linguists since Saussure. Indeed, they feel that it calls for a ‘new theoretical paradigm’”.

Given such evidence of acceptance for grammaticalization theory and such viewpoints as those quoted here,<sup>7</sup> a question that naturally arises is what the relation is between grammaticalization and the more traditional areas of study within linguistics, and in particular, given the importance of diachrony in grammaticalization studies, for historical linguistics, the subfield upon which the modern scientific study of language was built in the 19th and into the 20th centuries.

Accordingly, in this paper, I explore various facets of this question. For instance, many linguists trained in traditional historical linguistics seem somewhat reluctant to jump on this grammaticalization bandwagon and embrace the new theory;<sup>8</sup> is it because their methods and results are so at odds with those of grammaticalization? In view of grammaticalization being a relatively new paradigm for analyzing language change,<sup>9</sup> is it the case, as is often so with newly emerging paradigms and theories, that historical linguists have been doing things so wrong in the past that everything needs to be redone?<sup>10</sup> I suggest that this reluctance is not because those with traditional training are hidebound dinosaurs unwilling to give up on putatively obsolete methods and retool for a new era of historical linguistics – rather, I would say, it is the result of there not being any distinct advantage to this new ‘paradigm’.

In particular, grammaticalization theory, it seems to me, perhaps inadvertently, often takes stances that are quite at odds with constructs and notions about language and language change that have long been held and upheld within traditional historical linguistic frameworks; for those schooled traditionally, therefore, grammaticalization comes across as just flat out wrong. For instance, just to give a taste of what is to come, certain ways in which phonetic reduction is invoked in discussions of grammaticalization fly in the face of what is known about the regularity of sound change and the sorts of conditioning that can hold on sound changes. Similarly, well-understood processes of analogy and reanalysis are often abandoned in favor of claims about grammaticalization as a process of change. I argue here, moreover, that although purporting to be a theory of language change, grammaticalization theory – at least as it is practiced by many – is often ahistorical, not giving due consideration to the full range of information about the steps in a particular development and attempting to work out the history of various phenomena from synchrony alone. Also, far from being a theory of language in general, it is often asynchronous as well, with only vague synchronic analyses given, even though understanding the historical development generally depends crucially on understanding the synchronic status of a given element at various stages. These properties make grammaticalization as a framework problematic if it were to serve as a replacement

for – or even simply as a complement to – traditional historical linguistics, that is if its status as a new paradigm for analyzing language change is taken seriously. My remarks here, therefore, are intended to stand as somewhat of a corrective to the onrush of enthusiasm shown in recent years for grammaticalization.

## 2. Grammaticalization as anti-neogrammarian (or: a-historical linguistics as well as a-historical)

I regret to say that it seems to me that much of what passes for historical linguistics in a grammaticalization framework is ill-informed and quite simply seems not to really know or understand traditional historical linguistics; as a result, claims that the proponents of grammaticalization make about how it relates to traditional historical linguistics really carry no weight at all. This fact in turn would explain why traditional historical linguists generally see no benefit to working within this new paradigm.

The ‘School’ most associated with traditional historical linguistics is that of the Neogrammarians, so I consider here how grammaticalization as a guiding principle for investigating language change measures up against results and methods associated with Neogrammarian practices.<sup>11</sup>

One telling remark made within the grammaticalization literature is the following on the Comparative Method, taken from the Cambridge University Press advertising blurb for Heine and Kuteva (2002): “While the comparative method is concerned with regularities in phonological change, grammaticalization theory deals with regularities of grammatical change”. Admittedly, these may not be Heine and Kuteva’s own words, but they are consistent with the statement found in Heine (2003:596) that the Comparative Method looks to “regularities ... for example in sound correspondences”. They seem to be indicative of a claim concerning the Comparative Method that is hard to justify, however.

Statements of this sort display a misunderstanding of the Comparative Method on the part of grammaticalization adherents. The implication of such a statement is that the Comparative Method deals *only* with phonology, in contrast to grammaticalization theory, but in fact it has never been the case that the comparative method is concerned just with phonology. The essence of the comparative method is the determination of comparable features across related languages, with the goal being to use that comparison as a basis for understanding what the starting point for the related languages was like, i.e. for reconstructing aspects of the proto-language for the languages in question regardless of the level of structure for the feature at issue. Therefore, it is unclear why, for purposes of contrasting it with grammaticalization, one would focus just on the Comparative Method as it pertains to phonological de-

velopments. Indeed, the Comparative Method has been applied in domains other than phonology, with considerable success, where success is measured by the degree of acceptance the results have enjoyed among specialists.<sup>12</sup>

In contrast to the view that the Comparative Method is limited to phonology, it can be noted that Franz Bopp's famous first application of the comparative method in 1816 was his study *On the Conjugation System of Sanskrit in Comparison with that of the Greek, Latin, Persian, and Germanic Languages* (emphasis added); that is, his goal was morphological reconstruction. Similarly, the comparative method has been used successfully to reconstruct many aspects of linguistic (sub)systems other than the phonological – for example, for Proto-Indo-European (PIE) alone the following striking (and generally agreed upon) results can be cited as evidence of what the Comparative Method has yielded (with similar results well-known for other languages and language families):

- (2) *Non-phonological successes from traditional application of Comparative Method in IE*
- a. **forms**, besides roots and derivational affixes such as \*-ti- for abstract nouns, also inflectional elements such as person/number endings, e.g. \*-mi for 1SG present, or case endings, e.g. \*-s for nominative singular, \*-m for accusative singular
  - b. **categories**, such as present tense, aoristic aspect, nominal cases (nominative, accusative, locative), etc.
  - c. **morphological processes**, e.g. *e/o/∅* ablaut, both for derivation and for inflection
  - d. **morphophonemic rules**, e.g. the shared degemination of /\*-ss-/ to [-s-] as in Sanskrit 2SG *asi* “you are” from /as-si/ paralleled by Greek *ei* “you are” from /es-si/ (admittedly, one reaches these results in each language by internal reconstruction, but then in putting the results of the internal reconstructions in each language in juxtaposition with one another, one is using the comparative method to project back to the common ancestor of Greek and Sanskrit, that is, PIE)
  - e. **syntax**, e.g. the agreement pattern by which neuter plural nouns trigger singular agreement, found in Hittite, Vedic Sanskrit, Avestan, and Ancient Greek; thus presumably a PIE pattern
  - f. **semantics**, e.g. Sanskrit *vadhris* “castrated”, Greek *étrhis* “castrated ram”, so that if from a root \*wedh- “strike” (thus, \*wedh-ri-), then presumably the derivative had the specialized semantics referring to castration already in PIE
  - g. **poetic formulas**, e.g. Adalbert Kuhn's (1853) demonstration that Sanskrit *svravah*  $\geq$  *aks*  $\geq$  *itam* and Homeric Greek *kléos áphthiton*, both

meaning “imperishable fame”, are to be equated and reflect a PIE poetic formula; themes of poetic diction (cf. Watkins 1995 on IE dragon-slaying myths)

h. **legal practice** as well as **legal diction** (cf. Watkins 1987).

This listing in (2) is a fairly impressive catalogue of successful results – successful in that, as noted above, they are generally accepted by most practicing Indo-Europeanists – from the comparative method, so that it is fair to ask the question of whether any of those results could have been achieved with ‘grammaticalization’ alone as the guiding methodology, even concerned as it is with ‘regularities of grammatical change’. That is, is something more really needed to allow reconstruction beyond what the comparative method can achieve?

Similar sorts of questions can be raised with regard to statements made about Internal Reconstruction and grammaticalization; for instance, Heine (2003:596–597) has said that grammaticalization “may as well concern language-internal analysis. In this respect grammaticalization resembles internal reconstruction . . . . Compared to the latter, however, which concentrates on unproductive/irregular alternations, grammaticalization studies are not restricted in such a way.” This alleged ‘restriction’ of language-internal analysis, however, is not really substantiated by the facts of what internal reconstruction is and how it has been used: in essence, internal reconstruction is just a method of generating hypotheses, by *any* means available, about otherwise unreachable earlier stages. While there are a number of textbook examples of internal reconstruction that do work from irregularities (e.g. the classic analysis and reconstruction of PIE laryngeals given by de Saussure (1878), based on irregularities in vowel-alternation (‘ablaut’) patterns) they need not, and studies have not been limited to these types of phenomena. For instance, the regular but generally morphologically conditioned palatalizations that are quite widespread in Slavic, as in Bulgarian 1SG present *plača* “I cry” vs. 1SG aorist *plakax* “I cried, or Russian *vostok* “east” vs. *vostočnyj* “eastern”, coupled with what is known generally about possible phonological triggers for palatalization, lead to a reasonable, internally arrived at reconstruction of some [+front] segment after the velar in cases like these, after root in present, e.g. \*plak-j- or \*vostok-i.<sup>13</sup> Of course, one could say that the Slavic case *does* involve an irregularity (in the form of an unmotivated [c] in the present stem or in the adjective), but the basic idea is *always* that of drawing inferences from gaps in patterns and from what is known about language change in general. Viewed in this way, internal reconstruction is not supplanted or complemented even by grammaticalization; rather, in this sense, grammaticalization is rather just another instantiation of the basic methodology of internal reconstruction. This last point is made clear by many of the cases reported on in Heine and Kuteva (2002), where, for example, the co-existence of a noun with a given meaning and of an adverbial element with a similar form and a related

meaning is taken to justify a claim of historical relationship leading (generally) to a hypothesis of the historical derivation of the adverbial from the noun. Such cases can involve synchronic polysemy, as with Colonial Quiché *chi* “lip” and *chi* “in, into, out of” reported by Dürr (1988: 52; see Heine & Kuteva 2002: 195) or synchronic alternations, as with Kono *kùn* “head” alternating with *kù-* in *kùma* “over”, where *-ma* is a synchronic postposition meaning “on” so that *kùma* is quite plausibly derived synchronically (and thus historically, by inference) from */kùn-ma/*, as reported in Heine and Kuteva (2002: 69–70, drawing on information provided to them by Lessau). The historical inferences here are quite reasonable and may well be right, but the important methodological point is that the basis in such cases is entirely synchronic in nature; internal reconstruction is an inherently ahistorical method, working as it does from synchronic facts and patterns and drawing inferences therefrom.<sup>14</sup>

More generally, besides these traditionally recognized methods of studying change, there are the various well-recognized mechanisms of change to wonder about in regard to grammaticalization, e.g., sound change, analogy, metaphoric extension, etc. In particular, where do they fit in within a grammaticalization framework, and more to the point, if they are needed independently, is grammaticalization needed as something yet again beyond them?<sup>15</sup>

Among the argumentation that is relevant here is the discussion in Joseph (2001a) (see also Joseph 2003) regarding the Modern Greek future marked with *θα* from earlier *thélei na* “it-wants that” and the development of the Modern Greek weak nominative pronoun *tos* (cf. earlier Greek *a(u)tos*). I claimed that although these look like textbook cases of grammaticalization, in that, for instance, the emergence of *θα* involves semantic bleaching (‘want’ → FUTURE), phonetic reduction, and a shift of lexical status to affixal status, in fact the bleaching took place very early on, at a point at which the full verb was intact, and to account for the phonetic reduction and shift in status, all one needs to posit is regular sound change and analogy as the processes by which the changes leading to a future marker with the form *θα* were effected. Based on that, I suggested that grammaticalization is not a process in and of itself but rather just a label given to a particular type of outcome of independently needed mechanisms of change.<sup>16</sup>

I offer a further speculation here. This result is reached in a situation where there is access to a significant amount of detail about the historical development, as in fact all the relevant stages in the emergence of *θα* are attested generally with abundant documentation.<sup>17</sup> Perhaps, then, this sort of scenario, in which well-understood processes of change give results that one might interpret as showing a development towards a greater degree of embedding in grammatical structure for some element, might well be so for all cases of grammaticalization, especially those based on considerably less direct evidence and more a matter of internally arrived at inferential reconstruction. At the very least, I would argue, putative cases

of grammaticalization should be re-examined in light of the process vs. outcome interpretation of just what sort of phenomena are subsumed under grammaticalization.

To some extent, a basic problem here, as I see it, is that either by working from synchronic data and drawing even well-reasoned historical inferences (i.e., making a claim of grammaticalization based on internal reconstruction) or by comparing two stages somewhat distant in time and trying to infer what the pathways from one stage to the other were, one is falling into the trap discussed by Andersen (1989) of confounding a diachronic correspondence with an actual innovation or change. While this is not a problem that is restricted to grammaticalization studies, it is a real one, and, speaking just impressionistically here, it is one that proponents of grammaticalization, with its appeal to universally applicable pathways of change that in essence do the work of historical investigation for one, seem rather prone to fall victim to.

These musings suggest that a thorough re-examination of all putative cases of grammaticalization in this way would be revealing, but a task of that sort is well beyond the scope of this paper. Still, I mention here one instance that is consistent with the sort of approach that I advocate and thus relevant to the concerns I raise here. In particular, Rosén (2002) has provided an extremely detailed account of the emergence within historical Latin – thus involving stages of the language for which there is ample documentation – of the generalizing indefinite relative pronouns *quiuvis* and *quilibet*, both meaning “any one whatever (i.e. any one you wish) of three or more”. Their development shows some characteristics that are reminiscent of what happens in grammaticalization, in that clausally linked elements (e.g. relative pronoun *qui* and a form of *velle* “to want”<sup>18</sup> occurring in a full relative clause) come to be compacted into a single word, with the result that without the details of the history one might be inclined to simply refer this as a case of grammaticalization and then move on. However, Rosén concludes (2002: 104) that it “was a gradual process ... [and] no delexicalization, or semantic depletion, was involved, nor morphophonemic attrition, but eventually fossilization of verb-form and – in certain patterns – syntactic metanalysis of sentence constituents”. Moreover, she suggests, ultimately (111), that “the inevitable, but not unexpected conclusion is not to take for granted, when encountering one materialization, any other of the facets of grammaticalization”. Thus here too, an apparent instance of grammaticalization of a seemingly classical sort evaporates upon closer examination of the facts.

Another area in which assumptions and practices of grammaticalization are at odds with traditional wisdom about aspects of language change is sound change. The traditional, i.e. Neogrammarian, understanding of sound change together with what, to my thinking, is the best available evidence, points to sound change as being purely phonetically based; apparent cases of nonphonetic conditioning

usually represent results of reanalysis or reinterpretation of once phonetically conditioned phenomena (see Hock 1976, and Section 6 below). Such a view, among other things, allows one to *derive* regularity of sound change since phonetic factors can be thought of as the most generally applicable sorts of conditions, being defined entirely in terms of aspects of production of sound and thus not linked to particular lexical items or morphological categories (see Labov 1981, 1994; Joseph 1999; Janda & Joseph 2003).

Nonetheless, some of the discussion in the grammaticalization literature seems to take a view that is counter to this Neogrammarian stance on sound change, in that phonetic ‘erosion’ of grammatical forms (e.g. English *not* → *n’t*) is often talked about as if there were nonphonetic conditioning of sound change. Thus, Hopper and Traugott (1993: 150), while acknowledging the value of recognizing regular sound change, nonetheless talk about ‘special phonological changes’ in particular collocations of grammatical elements. Cases like this seem to be saying, contra the Neogrammarians, that the fact of being an (incipient) grammatical element alone conditions the phonetic reduction; relevant here also is the fact that frequency is often referred to in the grammaticalization literature as a conditioning factor (as if phonetic erosion were like geological erosion).<sup>19</sup> Both grammatical status and frequency would violate Neogrammarian principles, as they are nonphonetic conditioning factors, being in the first instance a matter of a higher level of analysis beyond phonetics alone and in the second a matter of usage.

Still, in keeping with the Neogrammarians, one could say that phonetic ‘erosion’ or ‘reduction’ would not *directly* be a result of grammatical status; rather it could reflect the effects of low prosodic prominence that function words often show. Under such a view, the change of *nòt* → *n’t* would be due to low salience in phrasal prosody or to stresslessness, not to grammatical status or frequency *per se*; indeed, Hopper and Traugott do recognize the relevance of prosody and low stress in this particular change. It can be noted here, in a somewhat similar case, that in the case of the development of the Greek future marker alluded to above, some of the relevant steps were simply ordinary regular sound changes and not any sort of special developments that affected only grammatical forms; e.g., the apocope of *-a* from the intermediate stage *thana* (from *thélei na*, as noted above) was regular before certain vowels, and in the resulting form *than*, the loss of *-n* was regular before (most) consonants, and so on. Thus there is no reason *a priori* to say that grammatical forms have to be treated differently by sound change from non-grammatical forms;<sup>20</sup> sound change can indeed operate in the mechanical way in which the Neogrammarians conceived of it, even with regard to grammaticalized elements.

### 3. Grammaticalization as ahistorical

Many practitioners of grammaticalization make claims about language change and historical linguistics, even if, as argued above, they do not follow the tenets and practices of traditional historical linguistics. That is perhaps reasonable enough, as it is conceivable that the grammaticalization perspective offers something new to the study of language change (even if the previous section casts doubt on such an assertion). Still, it can be argued further that despite a desire to contribute to an understanding of language change and language history, certain aspects of the way in which proponents of grammaticalization arrive at these contributions show instead that they take an ahistorical approach that often bypasses crucial considerations needed to make historical accounts work.

In particular, a key concern in any historical investigation is what may be called the issue of ‘direct lineal descent’. That is, given an element *X* in Stage *n* and an apparent *X'* in Stage *n + m*, is there a direct lineal connection between the two so that we can legitimately say that *X'* is an altered form of *X* at a later stage of the language? Oftentimes the connection is obvious and no one has any objections, as for the most part with the examination of languages with relatively long historical records, such as English or Greek or the Romance languages. Thus, no one seriously disputes that Old English *nu:* is the direct lineal ancestor of Modern English *now* or *hu:s* of *house*, and so on, and with good reason – there is the principle of regularity of sound change to make this connection work. But a similar connection between earlier English *pi:pen* “to make a high-pitched noise” and Modern English *peep*, with roughly the same meaning, is problematic because it runs afoul of regularity of sound change; rather, the Modern English verb may well have been created independently, most likely as an onomatopoe, and thus may not truly be in a direct lineal descent connection with earlier *pi:pen*.

This same concern arises with grammatical forms, even those generally talked about with regard to grammaticalization. Murtoff (1999) provides considerable detail on the development of the Spanish adverbial marker *-mente*, often cited<sup>21</sup> as a classic case of grammaticalization in that a Latin lexical item – the ablative singular of *ment-* “mind” – has come to serve a purely grammatical function. She shows that there really cannot be a straight line of descent from lexical item in a phrase, e.g. Latin *clara mente* “with a clear mind” to word-level marker, e.g. Spanish *claramente* “clearly”.<sup>22</sup> In particular, these formations show developments in Medieval Spanish, involving variants with *mientras* and *miente*, that suggest that the introduction of the specific form *mente* (which, importantly, does not show the regular sound change development of medial *-e-*) into later Spanish adverbials represents a learned borrowing and not a direct development out of a Latin antecedent. Borrowing therefore interrupted the ‘pathway’ of development and this additional information suggests that this case is not, in fact, as straightforward as

it first seems, and significantly is not a matter of direct lineal descent from Latin ADJ + *mente* to modern Spanish ADJ + *mente*. In this case, only a more detailed look at the relevant historical documentation brings to light the separation of the seeming Latin predecessor from the apparent later Spanish instantiation, meaning that caution is called for when a full historical record is not available.

The same kind of caveat holds of course when the connections are made between elements that are in the same synchronic stage that differ in some respect from one another, as, for instance, if someone were to connect the English future marker *will* with the noun *will* meaning ‘volitional force’ and draw conclusions from that linkage about the possible development of a future marker out of a volitional element, or, to take a case from the existing literature, when Heine and Reh (1984:120) argue for a development from adverb meaning ‘then’ to a future marker in Bari based on the homophony between the two forms (*dé* in each case). While all practicing historical linguists do exactly this sort of hypothesis-formation in their work, and oftentimes the connections and the inferences are perfectly reasonable (as in both examples just given), without the establishment of historical or synchronic connections but rather with just an assumption of them, all one really has is speculation.<sup>23</sup>

It is essential, therefore, to approach any such connections with a healthy dose of skepticism, especially when they are based on obvious-looking pairs of items, because looks can often be deceiving. This holds whether one is looking across different stages of a language or within a single stage, and anyone interested in the truth about historical developments must constantly keep that in mind. A few examples should make this clear where the most obvious pathway is most likely not the correct one. I present each of these, which to my knowledge have not been discussed previously in the relevant literature, not to counter some specific extant account of them, but to point out the perils of jumping to conclusions, as they offer the kind of situation in which one could easily be led to the wrong results by not exercising suitable caution.<sup>24</sup>

For instance, in contemporary American English there is a noun *ultimate* that refers to a game known also as *Ultimate Frisbee* (an extreme form of sport involving a flying disc called a Frisbee that is very popular in the US now). The word *ultimate* has been in use in English for some time as a noun meaning ‘final point, conclusion’, so it is fair to ask if the new usage meaning ‘Ultimate Frisbee’ is perhaps simply an extension of the already existing noun, the result of a semantic shift. Most likely, however, this is not the case, but rather *ultimate* in the new sense seems to result from a ‘clipping’ (a type of change sometimes also called ‘beheading’ or simply ‘ellipsis’) and reanalysis of the noun phrase *ultimate frisbee*, as schematized in (3):

- (3) [[ultimate]<sub>ADJ</sub> [frisbee]<sub>N</sub>]<sub>NP</sub> ⊗ [[ultimate]<sub>ADJ</sub> [∅]<sub>N</sub>]<sub>NP</sub> ⊗ [[ultimate]<sub>N</sub>]<sub>NP</sub>

Thus linking the two meanings of the noun *ultimate* directly involves actually falling into the trap of treating a diachronic correspondence (the result of various processes/mechanisms of change) as being a diachronic process/mechanism; the link between the two is indirect (non-lineal) at best, and what is really crucial is the mediation of the NP containing the adjective *ultimate*.

A similar case is the Contemporary American English slang use of *Mc-* as a prefix in words like *Mcpaper*, as a derogatory characterization of the newspaper *USA Today*, or *Mcjob*, for a low-paying job with no future, based on the corporate name *McDonald's* and accompanying connotations of less than satisfactory quality, and of products that are fast but not satisfying, ephemeral, with high turnover, etc. *Mc-* in *McDonald's* derives from Proto-Celtic \**mac-* “son” (cf. Old Irish *macc*), but one must ask whether it would make sense here to say that *Mc-* of *Mcpaper* is just a case of grammaticalization of a word (early Celtic *mac*) to an affix, a change of a generally somewhat familiar type? I would say not, since what should be at issue here is the determination of the proximate source of the *Mc-* prefix, not its ultimate source.<sup>25</sup>

More particularly, the creation of *Mcpaper* is rather like a case of truncation than any direct ‘grammaticalization’ *per se*; that is, it seems to represent an extreme reduction of an associative phrase ‘Newspaper that is to other (real) newspapers as McDonald’s is to (real) food’, as if from *Mc(Donald’s-esque) paper*. Such developments can give the appearance of direct lineal descent and thus also of ‘grammaticalization’, but there is no grammar-creating process at work here *per se*, only an ordinary and well-recognized type of change that in its outcome gives a form that looks as if it could be related directly to a piece of an existing word.

A similar concern – thus giving a reason not to simply accept ‘obvious’ connections at face value – arises with what we might call ‘false’ positives etymologically speaking, as with *ear* (of corn) and *ear* (the body part) in English. These two words are etymologically and thus historically distinct, reflecting Germanic \**ahuz* and \**awz-* (PIE \**ak'-u-* / \**ows-*) respectively. Nevertheless, they can be plausibly connected synchronically, inasmuch as both are types of appendages, and one might be tempted to draw historical inferences from such a connection, looking to metaphor or the like as the mechanism of semantic change. Given what is known about the etymology, however, caution about leaping to metaphor-driven conclusions here seems essential.

Such caution is also needed for grammatical material. The strong plural pronoun *them* in modern English has a corresponding weak form ‘*em*. An apparent synchronic connection can be made, as the weak form seemingly derives via reduction from the strong form, not unlike the relationship seen in the masculine singular ‘*im* versus the strong form *him*. It turns out, though, that such a connection and the accompanying inference of change are counterfactual: ‘*em* is the inherited form (cf. OE oblique *him*) while *them* shows effects of Norse contact

(borrowed, replacing OE *hie*). Thus the ‘reduction’ process, while plausible synchronically for relating *them/’em*, has no historical basis, and indeed the borrowing account allows one to avoid the embarrassing stipulation of getting rid via ‘reduction’ of a relatively robust consonant like the initial *th-* (whereas reducing *him* to *’em* involves only the loss of a relatively weakly articulated and acoustically depleted *h*).

Another such case involves two Modern Persian morphemes: *be* “to” marking indirect object and direction towards with nouns, and *be* marking subjunctive mood on verbs. These can be plausibly connected in much the same way that English *to* is polyfunctional (cf. *go to school*, *live to fight again*, etc.), especially if one takes subjunctive as an extension from subordination marking in general – in somewhat archaic usage, *be* is an infinitival marker too – and draws on claims in which infinitives are derived from purpose expressions, themselves deriving from directionality marking (as with English *to*). However, etymologically these two *be*’s are distinct, the nominal one deriving from an adposition as seen in Old Persian *patiy*/Avestan *paiti*, and the verbal one deriving from a verbal prefix as seen in Avestan *vi-* (cf. Joseph 1975 for discussion and references); among other things, these etymologies give the modern nominal vs verbal usage directly, without any further assumptions about cross-categorial generalizations or alterations of distribution.

Thus caution is certainly in order, but one can ask whether, more generally, it is the case that any forms that *can* be connected *should* be connected in order to see what historical inferences are possible. Admittedly, this is how etymological conjectures are made and oftentimes eventually proven (to the extent that any etymology can be considered proven) but it is also the case that ‘obvious’ connections often prove to be wrong, as the above cases show, whereas ‘true’ etymologies are often anything but obvious. For instance, within English, from the synchronic evidence of the modern language only, it would not be at all obvious that the *-s* of *yes* and the verbal form *is* are connected, yet in fact they are, the former being from the optative of \*(H)es- “be” and the latter also from \*(H)es-. Another such case is the reflexes of earlier English *ric-* “realm”, found today only in the *-ric* of *bishopric* “bishop’s realm” and in the *-ritch* of *eldritch* “other-worldly”, two pieces that are probably not connected synchronically by any speakers of the language even if they happen to know the rare word *eldritch*. Such caution is necessary of course when looking for connections across languages, as in the case of the set of Albanian *dirtsë*, Armenian *khirtn*, and Greek *hidros*, all cognate with (and translation equivalents of) English *sweat* (from Proto-Indo-European \*swid-ro-, under the most widely accepted formulation of PIE phonology) or the well-known example of Armenian *erku*, Sanskrit *dva:*, and Latin *duo*, cognate with (and translationally equivalent to) English *two* (from Proto-Indo-European \*dwo:)

In fact, based on these examples, which are quite illustrative of a general problem in working out language history, it would seem that a useful rule of thumb in making historical connections perhaps is that the *less* obvious the connection the *better*. In a sense, in the less obvious cases, one has to work harder to be sure the connection is valid, and that additional work is what pays off in a more secure etymology. Too often, though, it seems that in applying the tenets of grammaticalization, linguists just ‘eyeball’ forms and go with what looks obvious; a goodly number of the cases catalogued in Heine and Kuteva (2002:passim) are of that sort – all reasonable hypotheses to be sure but based often on little more than similarity of form and a possibility of a linkage.<sup>26</sup>

#### 4. Unidirectionality and traditional historical linguistics

A key tenet of grammaticalization theory is the principle of unidirectionality, the claim that movement involving grammatical elements is never in the direction of a given element developing a less grammatical status, e.g. never, on the assumption that affixes are always more grammatical in a sense than free words, from an (bound) affixal element to an independent word-level unit. This principle has guided much of the research within the framework of grammaticalization. It differentiates work in grammaticalization from earlier work in historical linguistics, and thus it bears on the general question of the relationship between grammaticalization and more traditional approaches.

Actually, though, the claim inherent in this principle can be taken in at least two related but slightly different ways, as sketched in (4):

- (4) *Two ways (at least) of interpreting notion of unidirectionality:*  
 UNIDIRECTIONALITY-A = there exists *only* movement towards *greater* grammatical status  
 UNIDIRECTIONALITY-B = there exists *no* movement towards *less* grammatical status

In each case, the claim would be that one never finds a more grammatical element becoming less grammatical in nature, but Unidirectionality-A makes the stronger claim that there can be no ‘lateral’ moves, ones in which the degree of grammatical status does not change even though the element changes, whereas Unidirectionality-B permits lateral moves. Unidirectionality-B, being the weaker claim, is subsumed under the A interpretation, and so it could be true even if A is not. Related to these claims is what Hopper (1994) has referred to as “phonogenesis”, a way in which elements gain phonological ‘bulk’ through the ‘erosion’ and ‘semantic bleaching’ of elements that at one time were full-fledged morphemes;

indeed, for Hopper, adding a “functionally empty [set of] phonological elements [is] an advanced stage of grammaticalization” (p. 31).<sup>27</sup>

The question of the proximate source of the *Mc-* in *McPaper* mentioned earlier is relevant here since if it is from the *Mc-* that occurs in *McDonald's*, as seems to be the case, it represents a case of an affix deriving from another affix (if the *Mc-* of *McDonald's* is synchronically analyzable as an affix) or from an arbitrary piece of a word (if *McDonald's* as a proper name is treated as being monomorphemic<sup>28</sup>). In the first case, it would be a lateral move, and thus a counter-example to Unidirectionality-A, and in the second case, it would be an instance of ‘counter-erosion’ in that an empty string of segments would have taken on grammatical status as an affix. In this latter case, an element would have added to its ‘bulk’ moreover but not via the devolution of a word to affixal status, contrary to Hopper’s claims.

Such lateral moves have long been recognized within historical linguistics, so it does not take the recent case of *Mc-* to disprove Unidirectionality-A; for instance, Latin offers the example of the denominal adjectival suffix *-nus* becoming *-ánus* via the resegmentation (or reanalysis) of forms such as *Romá-nus* “pertaining to Rome” as *Rom-ánus*, and numerous others can be found.<sup>29</sup> Moreover, it has long been recognized that affixes can be stable over thousands of years; Fortescue (2003), for instance, states that evidentiality affixes in present-day Eskimo languages are to be reconstructed as suffixes in Proto-Eskimo, over a span of some three thousand years, and the 1SG *-m* of many modern Slavic languages (e.g. Macedonian) has been an affix since Proto-Indo-European (cf. Janda 1996 on this affix). If affixes can show long-term stability, and can move laterally, as it were, how can we be confident that a given affix must have been a lexical item in some stage prior to the period of stability or prior to a lateral move? Why not treat the default position here as being that the element in question has been an affix for as long as one can tell?

Thus, traditional historical linguistics is really silent on the issue of unidirectionality and in essence has let the chips fall where they may; if an affix is the most reasonable source of another affix, so be it. If an affix is created out of originally non-affixal material, so be it. If an affix is the starting point for the creation of a word,<sup>30</sup> so be it. While the lack of any constraints here might seem to some to be problematic, especially as opposed to the constrained system that grammaticalization offers (only movement from less grammatical to more grammatical), one answer would be that the traditional approach, while less constrained, is more realistic, in that there *are* cases of counter-directionality, lateral movement, and movement from empty phonological strings to meaningful affixal status. Under this view, then, it is not clear that grammaticalization offers much in the way of an advance over traditional viewpoints.

### 5. Grammaticalization as asynchronous

I have argued here that in a sense, grammaticalization proponents, while purporting to be interested in the history of particular forms and constructions, actually in practice are often ahistorical, not paying enough attention to the full range of details of the development and unfolding of a form through time and not paying careful enough attention to matters of etymology. At the same time, though, there are some indications that advocates of grammaticalization are somewhat ‘asynchronous’ as well, in that they do not pay as much attention to details of synchronic analysis as might be desirable too and in a sense eschew definitive accounts of the synchronic accounts of given elements.

For instance, much has been said about the way in which the form *to*, universally assumed to have been nothing more than a preposition in earlier stages of English, e.g. Old English, came to serve a grammatical function in marking an infinitive in Modern English, as in *I live to do linguistics* or *I yearn to study historical linguistics*. And, in terms of what it means to be a marker of the infinitive, many studies (see, e.g., Fischer 2000) simply (but quite reasonably) follow the characterization given by Jespersen (1927) that *to* in this use is “a mere empty” marker. But, being able to chart the development of a grammatical morpheme presupposes that we have a clear idea of just what the element was to start with and what it has ended up as. Thus, it is fair to ask what being “a mere empty” marker actually means in terms of a formal characterization of infinitival *to* in an explicit grammatical account. For instance, Pullum (1982:181) argued that infinitival *to* is “not an affix, particle, complementizer, preposition, ... or tense morpheme ... [but rather is a member of] the category V ... [and specifically] is an auxiliary verb and head of a verb phrase”; Pullum has since given up that specific claim (see Huddleston & Pullum 2001:1185–1186) in favor of treating it as a subordinator, but if the verbal analysis were to be maintained,<sup>31</sup> then the view of the historical development of *to* would look quite different, and would involve a drastic reanalysis of the element from a non-verb to a verb. Admittedly one could just focus on the function and say that whatever infinitival *to* is in Modern English from a formal structural standpoint, its function is as a mere marker of infinitival status. However, since at least one thread within grammaticalization studies has always been on such issues as categorial status (especially, e.g., word vs. affix), it seems that it is indeed important to ask about the categorial status of *to* and make an explicit statement of that a part of the diachronic account. To fail to do so leads to claims and accounts that I would call ‘asynchronous’.<sup>32</sup>

My plea here, therefore, would be that before claims are made about diachronic developments, it behooves us to pay serious attention to what the best synchronic analysis is at the various stages being examined. There is no doubt in my mind that early English *to* was a preposition and that it ultimately gave rise to the infiniti-

val marker *to* (whatever its formal characterization is to be) in Modern English. However, if the modern *to* is a verb, then is this an instance of a ‘universal pathway’ of grammaticalization or simply a reanalysis – admittedly perhaps an unusual one but a reanalysis nonetheless – brought on by the role that the element plays in the overall system; that is, in comparison with other combinations of verbal elements such as *must/may/can* + VERB, as Pullum originally suggested.

#### 6. Conclusion – being fair to grammaticalization but at the same time (and overridingly) being fair to language history

I have been somewhat critical of grammaticalization in my remarks so far, but so as to be fair, I would like in this conclusion to be absolutely clear about my position here. In particular, I do not for a moment deny that there is a ‘phenomenon’ of grammaticalization; as I have argued elsewhere (Joseph 2001a, 2003), however, I see it as a result, not a process, an epiphenomenon perhaps. I would say that we could just do with grammaticalization as a result, a product, and reserve the designation ‘process’ or ‘mechanism’ for the traditionally recognized sound change, analogy, reanalysis, and metaphorical extension; moreover, once social dimensions are taken into consideration (e.g. in contact situations, whether between dialects or languages), then borrowing (and all this last entails, like calquing) and hypercorrection also need to be added in, the latter being a powerful process/mechanism in change due to dialect contact. In this regard, grammaticalization theory adds little to the insights of traditional historical linguistics despite purporting to offer a new way of looking at data concerning grammatical forms.

Still, one thing that grammaticalization definitely has gotten right in recent years is the emphasis on constructions and on forms in actual use, and not in the abstract. That is, it has been realized that it is not enough to simply say, for instance, that a body part has become a preposition (e.g. HEAD → ON-TOP-OF) but rather one must recognize that it is HEAD in a particular collocation, e.g. *at-the-HEAD-of* that has yielded a preposition, or that HAVE turning into EXIST is not necessarily just a random semantic shift but rather is one that happens in the context of adverbials such as in Late Latin *habet ibi* → Spanish *hay* (Fr. *Il y a*) or that the shift of *since* from temporal to causal meaning came from in contexts in which causality can be safely inferred from temporal priority, and so on. This is a big step forward, since it takes semantic change especially out of the realm of the purely lexical and places it into the pragmatic domain, deriving changes from inferencing and the like that are possible for words in constructions with other words and in actual, contextually keyed usage.

What is particularly interesting about this view, in the light of the concern herein for the relation between traditional historical linguistics and grammaticalization, is that it accords with an insight that the Neogrammarians had concerning sound change. Even though sound change is often talked about as happening to individual sounds, in fact, the sounds that undergo changes are always embedded in morphemes and words and even intonational phrases and utterances. In order to understand sound developments oftentimes one must look beyond the immediately adjacent sounds in the same morpheme or word and look instead to the broader surrounding context, including constructional aspects such as prosody or adjacent phrasal elements. For instance, the seemingly unusual voicing of voiceless stops that occurred in word-final position between PIE and Latin, as e.g. in PIE \*Hs-ye:-t “it would be” → (Old) Latin *sied*, was understood by the Neogrammarians not in terms of word-final position, i.e. triggered by a word-boundary following the \*-t, but rather in terms of the possibility of voiced sounds following it; that is, it actually involved an original phrasal *sandhi* (combinatory) process of -t # [+voice] → -d # [+voice] (and, by the same token, -t # [-voice] staying as such) with the ultimate generalization of one of the variants.<sup>33</sup> This view of sound change thus involves essentially a construction-based perspective over what can affect individual sounds, and in that way is rather similar to what grammaticalizationists have been doing with regard to the emergence of grammatical elements. In this way, grammaticalization theory and practice converge with Neogrammarian practice in recognizing that the key to understanding language change is not to look at elements atomistically but to see them in their connections with other elements in actual use.

Further, though, as noted in Section 4, grammaticalizationists part company with traditional historical linguistics with regard to ‘unidirectionality’. My own position here is that while at best any controversy concerning unidirectionality may reflect nothing more than a definitional matter (see Janda 2001) and at worst the notion might be just completely wrong, I do not and cannot deny that movement from less grammatical items such as words to more grammatical items such as affixes is highly common and widespread; it is just I do not see this movement as an absolute and therefore feel that a more realistic approach has to allow for movement in different directions other than the direction of greater grammatical status. That is, for me, ‘counter-directionality’ (the inverse of ‘unidirectionality’) is not only allowed but actually occurs. A question that needs to be asked of grammaticalization adherents is the following: if unidirectionality is the bedrock principle of grammaticalization theory that many proponents make it out to be, then how can it permit even any exceptions without vitiating the theory?<sup>34</sup>

In this regard, it is important to note how ‘unidirectionality’ fits in with other models of change and especially what is recognized in traditional approaches. In particular, there are documentable cases where over a long stretch there is ‘mono-

tonicity', in the sense that there is a steady slope to the movement overall, but not in the short run, in that occasional 'blips' get in the way of there being movement only in one direction.<sup>35</sup> A case in point is the Greek loss of the infinitive, as described in Joseph (1978/1990, 1983). While the abundance of infinitival usage and forms in Ancient Greek is not lost altogether until after Medieval Greek of c. 1500AD, there is clear reduction in use and in forms from century to century, beginning with the period of the Hellenistic *Koine*, with the category eventually disappearing altogether. Importantly, though, the loss was not a steady-state (monotonic) decline, for there were a few renaissances of infinitives functionally and formally even as the infinitive was being replaced all over, i.e. at a stage when the category was rapidly sinking into moribundity. These counter-directional developments include the creation of a new infinitive for the verb 'be' (*eísthai*, as opposed to Ancient Greek *eínai*) that arose in the 12th century, and three new uses for remaining infinitives that emerged in the 12th–15th centuries: (i) Circumstantial Infinitive (the infinitive as an absolute clausal modifier setting the circumstances for the action of the main verb) (ii) a future formation with *thélo*: "want", and (iii) a perfect formation with *ékho*: "have".

Rather than being troublesome, it seems that accepting multi-directionality/counter-directionality in change in general<sup>36</sup> is a far more realistic view, as this seems simply to be a key characteristic of change in language. In analogy, for instance, it is quite common to find levelling in different directions (cf., Tiersma 1978) as is the case in Modern English where the irregular plural *wharves* (to singular *wharf*) is being replaced for many speakers by the regular *wharfs* while at the same time the (now) regular plural<sup>37</sup> *dwarfs* (to singular *dwarf*) is being replaced by the irregular *dwarves*. In sound change, too, one finds cases in which perception is the determining factor and cases in which production governs the outcome – both forces/directions must therefore be taken into account since each can govern how a change ends up. Thus a reasonable question, it would seem, is why grammaticalization should be any different.

Thus, as a final plea, I suggest that the following is what we should aim for in doing historical linguistics, within *any* framework:

- © (5) a. the best analysis of the starting point of developments in question and the best analysis of the end point of developments of interest to us; only then can we really know what changes have occurred
- b. the best account of the transitions between the points we (arbitrarily) select for our analysis, keeping in mind the axiom that as linguists trying to understand change as something that speakers do (not something that happens to a language), we should not take a perspective on language change which a speaker cannot take (thus no trans-generational 'diachronic processes' – cf. Janda 2001)

- c. the best coverage of the relevant historical facts, so as not to fall into the false-positive and non-direct-lineal descent trap.

Only with these aims and only when they are achieved, I would argue, can we ever hope to have a truly explanatory ‘theory’ of language change.<sup>38</sup> And, to a large extent, these are exactly the goals and methods that traditional historical linguistics have long offered the discipline.

### Notes

\* I would like to thank Hope Dawson, Olga Fischer, Bernd Heine, and Rich Janda for their insightful and helpful comments on an earlier version of this paper. Needless to say, they are not to be held responsible for the views expressed herein.

1. See Campbell and Janda (2001) for a compendium of various definitions of grammaticalization to be found in the literature; they conclude their survey by saying that “we are left with a notion of grammaticalization which minimally includes, at its core, *some linguistic element > some more grammatical element*” (107).
2. Many of the edited volumes noted herein stemmed from organized conferences, and indeed, this paper has its origins in a presentation given at a conference, held in April 2002 at the Universiteit van Amsterdam.
3. For instance, Bowden 1992 is listed under “Oceanic Languages – Grammaticalization”.
4. These searches were done on January 11, 2003.
5. I mention this without taking sides on whether the label of ‘theory’ is justified or not; see Newmeyer (1998: Ch. 5; 2001) for some argumentation against the use of this label and Heine and Kuteva 2002 for some counter-discussion.
6. Note also Ramat (1998: 123), who states that “grammaticalization theory [without a principle of] ‘unidirectionality’ would be left with a too vague definition of its field, including almost every instance of change”; as discussed below, unidirectionality probably should be given up in at least some form, thus leaving grammaticalization in the position Ramat cautions against.
7. In fairness, I point out that Heine (2003: 575) has explicitly stated that “grammaticalization theory is neither a theory of language nor of language change”; thus not all practitioners take an expanded view of what grammaticalization theory is and what it can do. The variety of viewpoints expressed by those who work within a grammaticalization framework admittedly makes it hard to know which to take as representative. By giving some actual statements that have been expressed in print, I am trying to at least document the viewpoints I discuss, though it should be clear that not all viewpoints are held by all.
8. Without naming names, I note that I count myself among this group, and would include as well the historical linguists represented in Campbell (2001a), among many others.

9. As noted at the outset, there are pre-contemporary-era antecedents to grammaticalization studies, but the recognition and formulation of governing principles, as in Lehmann (1982/1995) or Hopper (1991), *are* only fairly recent phenomena.
10. No one working in the grammaticalization paradigm has actually said anything to this effect, but paradigm shifts are often accompanied by revisionist views of earlier work, so this question is a reasonable one to ponder.
11. Just by way of comparison, a Google search for 'Neogrammarian' yielded 573 hits.
12. Admittedly, the Comparative Method is illustrated in most textbooks with phonological examples and application of the method in domains other than phonology generally gets short shrift, but that is still no reason to act as if the scope of the method must be restricted just to phonology.
13. Or conceivably \*plak-i- or the like; the reconstruction with \*-j- for the verb is most assuredly the historically correct one (see Scatton 1993: 194) but the point is that one brings to bear whatever tools one can muster in order to reach reasonable internally arrived at reconstructions.
14. This point has been made repeatedly by Raimo Anttila; see, for instance, Anttila (1972/1989: Ch. 10, 12, 13, *passim*) and note his statement (1989: 264) that "internal reconstruction is already known to the reader, as it is exactly the same as [synchronic] morphophonemic analysis".
15. As Bernd Heine has pointed out to me, proponents of grammaticalizationist have not ignored these mechanisms of change, and in fact have incorporated recognition of them into the framework of grammaticalization theory. Still, I would say that the real question that needs to be addressed here is if a notion of grammaticalization – and especially a separate *process* of change so labelled – is needed on top of these other processes. This was a focal point for papers such as Campbell (2001b), Joseph (2001a), and Newmeyer (2001).
16. Heine (2003: 585) raises an objection to this point, saying that such a view misses the fact that the Greek development involves "the same lexical verb ('want') and the same structural characteristics as observed in Swahili and many other languages" and fails to answer the questions of why the development "necessarily [led] from lexical verb to tense prefix; that is, why ... it [is] unlikely that the process could have proceeded in the opposite direction from verbal prefix to lexical verb". My response here is that since 'want' is not the only verb to do this, even though it is a common one, it is not clear that *any* theory of internally driven change can explain why 'want' is selected in a given language and not some other verb. And, as to the latter objection, the matter of 'likelihood' of directionality seems to be assuming a status for unidirectionality that is not warranted (see discussion in Section 4); in a sense, anyway, it is irrelevant as far as any single example is concerned as any one change can only go in one direction at a time (see Note 36).
17. See Pappas and Joseph (2002) for details and discussion.
18. Note that *uis* in *quiuus* is the second person singular present indicative of 'want', and *libet* is the third person singular present indicative of 'please' (thus the forms literally are 'who you want', 'who (it) pleases (you)').
19. See, for instance, the articles and discussion in Bybee and Hopper (2001).

20. Bernd Heine (p.c.) has raised a question here as to why grammatical forms typically are shorter than lexical forms – this observation, I would say, is easily derivable from the typically low prosodic prominence found with grammatical items and the effects, such as assimilations, syncope, and various reductions, that are often associated with that prosodic characteristic.

21. Indeed, I have cited this example myself by way of illustrating the phenomenon, e.g. in Joseph (2001b).

22. It may well be that in Spanish, *-mente* forms are best treated as compounds and not simple stem-plus-affix word-level units, due to facts of conjunction reduction (as in *rapidamente* ‘rapidly and clearly’) for instance, but the finer details of the analysis in modern Spanish, while interesting and important in their own right, are beside the point here.

23. Part of what is at work here in such speculation is an assumption that the principle of unidirectionality is correct and that given a relation between ‘then’ and ‘future’ the pathway could *only* be that of ‘then’ developing into a future marker and not the reverse. However, if the principle of unidirectionality is too strong (see discussion below), then (among other things) its value in determining the historical sequencing in such cases is severely diminished.

24. My impression, moreover, is that throughout much of the grammaticalization literature, one can find far too many instances of just these types presented as if they were robustly established, though I readily acknowledge that quantifying this impression for the purposes of an explicit comparison with work done in other frameworks would be tricky at best.

25. It can be noted as a sidebar that the proximate source of the *Mc-* affix in *Mcpaper/Mcjob* is an element that is at best another affix (if *McDonald*, *McPherson* etc. allow for identification in English of a prefix *Mc-*) but could just as well be a meaningless piece of a word (since it is part of a proper name and thus not really analyzable in the usual sense that lexical items can be). This would thus run counter to the claim made in Hopper and Traugott (1993: 128–129) that all grammatical elements, e.g. affixes, have a “prior lexical history”; admittedly, if one goes back far enough, *Mc-* does have a lexical source but its most immediate source is not a lexical item at all but either an affix or just a string of segments in a lexical item.

26. It is instructive to note the somewhat similar problems associated with an ‘eyeballing-the-obvious’ methodology in comparative linguistics, as exemplified by the multilateral comparison strategy espoused by Greenberg (1987) and claims of relatedness derived therefrom; see Campbell (1988), Greenberg (1989), Matisoff (1990) for some discussion of the issues. I reiterate here that the problems I note are not necessarily found only in the grammaticalization literature, and in many cases, practitioners are simply trying to use *any* available evidence to see what historical inferences are possible; however, that does not mean that putative connections must be taken to be established, hence my plea for caution here.

27. Though see Joseph (2003: 477, 490, n. 21) for some critical discussion concerning the notion of ‘phonogenesis’.

28. As pointed out in Note 25, the status of *Mc-* in names like *McDonald* or *McPherson* in modern English is tricky and I offer no definitive judgment on it here, though either analysis would be problematic for some version of unidirectionality.

29. For instance, the prehistory of the *-ness* suffix in English, coming from the resegmentation of the piling up of several affixes, in Indo-European terms, *\*-n-ot-tu-* (with *\*-t-t-* giving Germanic *-ss-*); cf. Gothic *ibnassu-* ‘evenness’, where the Germanic suffixal complex *\*-assu-* was added to an *n*-stem form.
30. See Janda (2001) for a catalogue of literally dozens and dozens of cases of this type in the literature, despite the frequent assertions in the grammaticalization literature that such a development is either impossible or so rare as to be not worthy of notice.
31. And note that Huddleston and Pullum (2001:1185) do consider it defensible, saying that “it would not be impossible to maintain . . . [though] the case for *to* being a VP subordinator is stronger”.
32. It may well be that Hopper’s (1987) notion of “emergent grammar”, which, as mentioned in the Introduction, has been embraced by many practitioners of grammaticalization, leads some to the position that an explicit synchronic analysis is not necessary, inasmuch as language, in this view, involves only “movements towards structure” not the development of structure *per se*.
33. This view is in keeping with the position regarding the nature of sound change expressed in Hock (1976) and Joseph (1999), as noted in Section 2.
34. See Campbell (2001b:134ff., and especially 140–141) on this point.
35. Indeed, ‘monotonicity’ might be a better term than ‘unidirectionality’ for the insight that proponents of grammaticalization are trying to capture.
36. Individual changes, of course, can *only* go in one direction; a given change is thus *not* like Canadian humorist Stephen Leacock’s Lord Ronald, who (in “Nonsense Novels”, 1911) “flung himself upon his horse and rode madly off in all directions”. I am indebted to my mother Harriet Joseph for bringing this quote to my attention.
37. I say “now regular” as the *-v-* plural, which seems to be the innovative form here, is a replacement for the *-f-* plural, which itself seems to be an innovation. That is, the OE plural *dweorgas* developed regularly into later *dwerwes* or *dwerows*, so that the plural in *-fs* is a replacement, based on the singular ‘dwarf’ (from OE *dweorg*) for the plural in *-wes* or *-ows*. Neither of those would have yielded a form in *-v-*, so the *-v-* plural must be based on the *-f/-v-* alternation of forms such as ‘wharf/wharves’, the point which is most relevant to the discussion here.
38. The rub here, of course, is that it is not always obvious what the ‘best’ is in a given case or who is to decide; we must strive to determine it but must always as well be prepared to reevaluate in the light of new data and new perspectives.

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