

On directionality in language change with particular reference to grammaticalization

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Introduction

The issue of the directionality of grammaticalization has attracted considerable attention in recent years, as illustrated by works such as as fRajzyngier (1997), Newmeyer (1998: ch. 5), Haspelmath (1999a) (and the ensuing debate: Geurts 2000a, Haspelmath 2000b, Geurts 2000b), the papers in the recent special issue of *Language Sciences* (Campbell (ed.) 2001), Traugott (2001), Kim (2001), van der Auwera (2002), Heine (2002), and quite a few others.

In this paper, I would like to put this issue in a somewhat broader perspective, discussing not only the unidirectionality of grammaticalization, but also unidirectionality in other areas of language change. But the main focus will be on grammaticalization, and after defending the unidirectionality claim for grammaticalization against several criticisms, I will examine a substantial number of alleged exceptions to the unidirectionality and show that only very few of them can be accepted as real exceptions.

The paper is divided into three main sections, in which I will make the following larger points:

- §1: If we want to **understand** language change, we need to identify **universals of language change**. **Directionality constraints** are among the strongest universals of language change.
- §2: The **unidirectionality of grammaticalization** is the most important constraint on morphosyntactic change.
- §3: Most cases of "**degrammaticalization**" that are cited in the literature do not show the **reversal of grammaticalization (or "antigrammaticalization")**, but something else.

1. Universals of change and directionality constraints

Most of the time, historical linguists are occupied with the business of **describing** language change, which is quite a challenging task in itself, given that change is so difficult to observe. But ultimately we would also like to **understand** language change to the extent possible, or in other words, we want to answer why-questions. My own personal interest happens to be understanding **why language structure changes in the way it does**. There are other why-questions about language change, such as the question why languages change at all, the question why the social propagation of an initial innovation can often be described by an S-curve, and so on. These will not be addressed here.

Linguists working on particular languages are also often interested in particular why-questions such as the question why the Romance languages lost the Latin case inflections. But unfortunately, particular why-questions of this kind are for most practical purposes unanswerable. The number of factors affecting language change is so enormous and we can control only so few of them that most change events must appear to us as historical accidents. Latin could have kept its cases, even with all the phonological erosion that made them difficult to distinguish, simply by applying morphological changes serving to preserve the case contrasts. Or Latin could even have developed more cases the way Hungarian and Finnish did. It so happened that it lost its cases, and trying to understand this unique historical event typically leads to frustration. In general, understanding requires that we identify non-accidental phenomena, and for understanding language change, this means that we have to find **universals of language change**.

To illustrate what I mean by this, a few random examples of proposed universals of language change (of different degrees of generality) are given in (1).

- (1) a. **Survival of the Frequent** ("Unmarked")
(e.g. Winter 1971, Wurzel 1994)
When a grammatical distinction is given up, it is the more frequent category that survives.
(E.g. plural forms survive when dual/plural distinction is lost.)
- b. **Sound Alternations Result from Sound Change**
(phonetics > phonology; *morphology > phonology)
- c. **From Space to Time** (e.g. Haspelmath 1997b)
(spatial > temporal marker; *temporal > spatial marker)
- d. **From Something to Nothing** (Haspelmath 1997a:230)
'something' > 'nothing' (*'nothing' > 'something')
- e. **From Esses to Aitches**: $s > h$ (* $h > s$) (Ferguson 1990)

These are all general laws which we can potentially explain, and when we have such an explanation, we can apply it to individual instances of these universals. For example, we might want to say that the universal "Survival of the Frequent" is explained with reference to the cognitive notion of frequency-induced entrenchment (cf. Bybee 1985:119): A frequent linguistic unit is remembered better because frequency of exposure leads to greater memory strength. When a distinction is given up, only the most entrenched category survives. Now let us take an individual instance of the Survival of the Frequent, say, the fact that when the Classical Greek dual/plural distinction was given up, only the plural forms survived. The plural was more frequent than the dual (cf. Greenberg 1966:31-37), so this change is in line with the universal, and if we want to know why the plural rather than the dual survived in Greek, we can appeal to the explanation that we just gave. So in this sense we can say that a particular change was explained after all; but of course the explanation of the particular change has nothing particular about it. We cannot explain why this changed happened in Greek but not, say, in Slovene (where the old dual survived), and we cannot explain why it happened two and a half millennia ago rather than a thousand years later or a thousand years earlier. So wherever we can understand structural change, it is really universals of structural change that we understand. But unless

we know whether a given instance of change is part of a larger trend, we do not know whether there is anything to explain.

Now when we look at reasonably robust universals of language change, we see that many of them take the form of **directionality constraints**. Of the five examples in (1) four have the form "A can change into Y, but Y cannot change into X". Especially in phonology, it is easy to find cases of this type, and I list a few more in (2).

- (2) a. [k] > [tʃ] (*[tʃ] > [k]) d. [z] > [r] (*[r] > [z])
 b. [p] > [f] (*[f] > [p]) e. [ts] > [s] (*[s] > [ts])
 c. [u] > [y] (*[y] > [u]) f. [l] > [w] (*[w] > [l])

So quite a few sound changes appear to be unidirectional, but there are of course also bidirectional sound changes, such as those in (3). Some of these changes are more likely in some positions than in others, and maybe a more fine-grained description of the type of change would reveal a directionality tendency in some of these cases as well.

- (3) a. [t] > [ʈ] and [ʈ] > [t]
 b. [o] > [a] and [a] > [o]
 c. [i] > [ɨ] and [ɨ] > [i]
 d. [au] > [o] and [o] > [au]
 e. [b] > [v] and [v] > [b]

Thus, it is an empirical question whether a type of sound change is unidirectional or not. I am not aware of any extensive discussion of this issue in the theoretical literature on phonological change, but as Ferguson (1990) observes, every linguist with some experience in diachronic phonology has the intuition that there are often directionality constraints at work:

"One of the most powerful tools in the armamentarium of linguists engaged in the study of diachronic phonology is the often implicit notion that some changes are phonetically more likely than others. Thus if a linguist finds a systematic correspondence between [g] and [dʒ] in two related language varieties, it will be reasonable to assume that the stop is the older variant and the affricate the younger one until strong counter evidence is found. The linguist makes such an assumption because experience with many languages has shown that the change of [g] to [dʒ] is fairly common and tends to occur under certain well-documented conditions whereas the reverse change is unusual and problematic."
 (Ferguson 1990:59-60)

Ferguson goes on to observe that this powerful tool of directionality constraints is not generally covered in textbooks or handbooks of phonology or historical linguistics. These typically include taxonomies of attested sound changes and introduce technical terms like lenition, assimilation, syncope and epenthesis, but they usually do not say what an impossible change is, or which changes are ubiquitous and which ones are exceedingly rare. For synchronic universals in phoneme systems, we have Maddieson's (1984) handbook with inventories of 317 languages. Diachronic phonology, whether theoretically oriented or primarily interested in reconstructing particular protolanguages, would profit enormously from having a handbook of attested sound changes in the world's languages.

Such a handbook would make it possible to identify constraints on possible sound changes, and many of the most interesting constraints will no doubt be directionality constraints. After all, that [u] presumably never changes to [a] in one step, or that [l] never changes to [b], is not surprising, whereas the unidirectionality of the [u] > [y] change and the [l] > [w] change is much harder to explain. There are also some clear tendencies of lexical semantic change (e.g. 'cup' can change to 'head' and 'head' can change to 'chief', but the opposite changes are extremely unlikely).

Once we are confident that we have a universal directionality constraint in some domain, the question arises as to how it should be explained. If the source structure and the target structure are similar enough so that one change into the other gradually and often imperceptibly, why can't they change in either direction? This issue is beginning to be addressed by researchers working in the area of grammaticalization (e.g. Lehmann 1993, Haspelmath 1999a), and this discussion could profit from analogous discussions in the other subfields of linguistics.

In this paper, I will not say anything about the correct explanation of unidirectionality in grammaticalization, because at present I have nothing to add to my earlier proposals. I will, however, address a number of criticisms and counterexamples that can be found in the recent literature.

2. Unidirectionality of grammaticalization

2.1. How important is unidirectionality?

Although it is very difficult to quantify language change, it seems to me that it is undeniable that the unidirectionality of grammaticalization is by far the most important constraint on morphosyntactic change, simply because grammaticalization changes are so ubiquitous. As far as I can see, the only serious competitor of unidirectionality is the diachronic universal "Survival of the Frequent" (see 1a). This universal seems to hold not only when categorial distinctions break down, but also in analogical leveling in inflectional morphology. For example, when a stem alternation such as *dream/dreamt* is leveled, it is the more frequent present-tense stem that survives (so that we get *dream/dreamed*, not **drem/dremmed*). This is a fairly important universal for morphological change, but it seems to be much less important for syntactic change.

Grammaticalization, by contrast, is of paramount importance both for syntactic change and for morphological change. A rough estimate is that two thirds of the papers on diachronic syntax published in recent volumes such as van Kemenade & Vincent (1997) and Pintzuk et al. (2000) deal with grammaticalization changes (even if they rarely mention the term "grammaticalization"). The relatively high number of non-grammaticalization papers in these volumes has to do with the fact that word order change is so salient in some European languages, especially of course word-order change having to do with verb-second phenomena. But we know that verb-second word order and the changes related to it are highly unusual phenomena that are

hardly found outside of Europe. My guess is that if we were able to study syntactic change on all continents, grammaticalization would play an even greater role in diachronic syntax. Of course, this is not more than an impressionistic statement, but I challenge anyone to come up with a long list of interesting syntactic changes that are unrelated to grammaticalization.

Like unidirectionality in sound change, unidirectionality in grammaticalization is very important in practical terms for the historical-comparative linguist. Suppose we have two related languages with no historical documentation, and one of them has a future-tense affix that looks similar to a future-tense auxiliary of the other language. If both directions of change were equally likely, we would not know what to reconstruct for the ancestor language. But because grammaticalization is overwhelmingly irreversible, the historical linguist can safely reconstruct the future auxiliary for the protolanguage in this case.

Moreover, unidirectionality helps us assess the likelihood of competing etymologies even if older stages are attested. For instance, historical linguists of Indo-Aryan have long debated the etymology of the Hindi/Urdu ergative-case clitic *=ne*. Quite a few linguists in the 20th century traced this element back to Sanskrit *-ina*, an instrumental suffix that would be a very plausible source from a semantic point of view. In a recent contribution to this debate, Butt (2001:114) has pointed out that such a change would constitute a counterexample to unidirectionality and is hence very unlikely (one would have to postulate phonological expansion from [na] to [ne:] and decliticization). This, among other reasons, leads Butt to reject this etymology and look for some other possible source of *=ne* in a full lexical item.¹

Now despite the theoretical importance of grammaticalization studies for understanding language change and their practical importance for historical linguistics, there have been a number of critical voices in recent years. In the remainder of this section I would like to address some of these points of criticism and show that while some are well-taken, others are quite unfounded.

2.2. Partially valid criticism

Three points made by grammaticalization critics that I regard as partially justified are listed in (4).

- (4) a. Unidirectionality is not exceptionless
- b. "Grammaticalization theory" is not one theory
- c. "Pathways of morphemes" must be linked to speakers' actions

Unidirectionality was apparently first stated explicitly as an important universal property of grammaticalization in 1982 by Christian Lehmann (cf. Lehmann (1995[1982]:16-19). By a stroke of genius, he coined the term

¹ Interestingly, Butt observes that this conclusion had already been reached by 19th century linguists (such as Beames 1872-79), presumably because at that time all linguists were familiar with grammaticalization and (implicitly) unidirectionality. It must be remembered that it was only the structuralism of Saussure and Bloomfield that made linguists forget about grammaticalization, until it was rediscovered toward the end of the 20th century.

degrammaticalization for a phenomenon that he believed did not exist. But now the phenomenon had a name, and it seems that Lehmann's strong initial claim plus his nice neologism spurred linguists to look for actual examples of degrammaticalization. And indeed, a number of good exceptions were found (see Newmeyer 1998, Campbell 2001, Janda 2001, Norde 2001), and this has generally been acknowledged by grammaticalization researchers. The counterexamples did not pose a serious threat to the original generalization, but a presumed absolute universal had to be weakened to a statistical universal. What are the consequences of this for the theory of grammaticalization? One might say that now that we know that unidirectionality has exceptions, it has become somewhat less interesting, but I would put it in opposite terms: Because unidirectionality is so interesting, we know about the exceptions. If someone proposes an uninteresting universal, we may never discover the exceptions because nobody bothers to look for them.

So from my point of view, unidirectionality has not become less interesting just because there exist a few exceptions. The basic generalization stands unchallenged as long as nobody shows that degrammaticalization is as common as grammaticalization. If one is interested in generalizations rather than arbitrary facts, one must ignore the exceptions, because unless they can be subsumed under some further generalization, they cannot be explained. Harris & Campbell (1995:338) say in this context: "An adequate theory must account for infrequent phenomena, not merely for the most common patterns." This is of course right if by "theory" they mean "descriptive framework": We need terminology also for rare phenomena. But if by "theory" we mean understanding and explanation, this is not right, because exceptions cannot be understood by definition; they are the residue that resists explanation. Thus, although it is true that unidirectionality is not exceptionless, this does not make it any less intriguing and important for our understanding of language change.

The second point of criticism that I find partially justified is Newmeyer's (1998) claim that what linguists commonly call "grammaticalization theory" is not a theory in the sense of a well-defined system of interconnected falsifiable hypotheses. What unites researchers in the area of grammaticalization is not that they subscribe to a single monolithic theory, but that they see a large class of semantic and morphosyntactic changes as sharing similarities and as theoretically interesting. There are a fair amount of quite different theoretical ideas and hypotheses concerning grammaticalization changes, and some of them are not really compatible. So I would say that "grammaticalization theory" is more like "evolutionary theory", which is not one single monolithic system either, but describes a range of related approaches and basic issues in the area of historical biology. Of course it would be more accurate to say "theorizing about grammaticalization" (instead of "grammaticalization theory"), but of course the inflationary use of the prestigious term "theory" is not an invention of grammaticalizationists. Like grammaticalization, inflationary processes are generally irreversible, so it seems unlikely that the term "grammaticalization theory" will be abandoned. But personally, I do not like the term "theory" particularly: I prefer to talk about the goal of understanding, or explaining, or answering why-questions. These are terms from our everyday language that everyone understands, and our endeavors can be accurately characterized with them.

The third point of criticism is Janda's (2001) reminder that it is impossible to understand language change phenomena if we see them as divorced from the speakers.² If we talk about a morpheme traveling along a pathway, we should be aware that this is a very abstract metaphor that may invite all kinds of unwarranted inferences. We need to be careful with metaphors, and we should make more efforts to go down to the micro-level of individual speakers and derive the observed constraints on structural changes from known constraints on speakers' linguistic behavior. But at the same time it is clear that we cannot do linguistics without abstract metaphors, and so far at least concepts like 'grammaticalization path' have done far more good than damage. We would know far less about possible and impossible changes if we had not started drawing diagrams of grammaticalization paths and semantic maps. We can see them as analogous to tree diagrams in syntax: These are not literally in people's mental grammars either, but if syntacticians drew no tree diagrams, we would know much less about syntax.

2.3. Invalid criticism

Three points made by grammaticalization critics that I regard as unjustified are listed in (5).

- (5) a. Unidirectionality implies a fully isolating prehistoric state and thus contradicts uniformitarianism.
- b. Unidirectionality is built into the definition of grammaticalization and is hence not an empirical claim.
- c. There is nothing unique about the kinds of changes that are associated with grammaticalization (Newmeyer 1998, Campbell 2001, Janda 2001)

The first point, about the contradiction to uniformitarianism, was recently brought up by Roger Lass (see also Hoenigswald 1991:25):

"The claim that all grammatical material is ultimately lexical means that there was a time when all human languages were 'isolating' (in the days of *Homo erectus* or whatever everybody spoke Vietnamese)... [This] is counter-uniformitarian, and so methodologically inadmissible." (Lass 2000:216)

But first of all, "the claim that all grammatical material is ultimately lexical" does not follow from unidirectionality, because it may well be that some elements such as demonstratives or interrogative pronouns are never created by grammaticalization from full lexical items, and have simply always been demonstratives or interrogative pronouns. Moreover, at least since Meillet (1912) it has generally been recognized that analogy is another important source of grammatical items, besides grammaticalization. But even if one were to make the speculative claim that all grammatical material is ultimately lexical, there would

² This point was made by Osthoff & Brugmann (1878) in their Neogrammarian Manifesto: "...dass die sprache kein ding ist, das außer und über dem menschen steht und ein leben für sich führt, sondern nur im individuum ihre wahre existenz hat." [...that language is not a thing which stands outside or above people and leads a life of its own, but has its true existence only in the individual.] (Osthoff & Brugmann 1878, cited after Ahrens 1969:344)

be no methodological problem because the principle of uniformitarianism does not require the assumption that early hominids such as *Homo erectus* (if they already had some kind of language) had languages of the same type as modern humans. If we allow ourselves speculation about the distant past, we can easily imagine that the first modern humans inherited part of their lexicon from the cruder languages of earlier hominids and added more lexical differentiation and grammatical elaboration. But since language has been around for tens of thousands of years and we know next to nothing about its origin, we really do not have to worry about the consequences of diachronic universals for prehistory.³

The second point of criticism is that unidirectionality is built into the definition of grammaticalization and hence represents a tautology (Campbell 2001:124, Janda 2001:294). This is very easy to counter: Yes, it is true that unidirectionality is built into the definition of grammaticalization. My current definition of grammaticalization is given in (6).

(6) A grammaticalization is a diachronic change by which the parts of a constructional schema come to have stronger internal dependencies.

This describes a unidirectional process, so saying that "grammaticalization is unidirectional" is strictly speaking tautologous. The point is, of course, that the easily imaginable reverse of this process does not occur (apart from a few exceptional instances). So this is not a substantive point at all, and one wonders why one hears it repeated so often.

The third point of criticism is that there is nothing special or unique about grammaticalization changes. Campbell (2001) expresses it as follows:

"Grammaticalization has no independent status of its own; it merely involves other kinds of changes and mechanisms of change which are well understood and are not limited to cases involving grammaticalization: sound change, semantic change, and reanalysis." (Campbell 2001:117)

And Janda (2001:266) maintains that grammaticalization "is actually an epiphenomenon which results from the intersection and interaction of other, independently motivated domains" (see also Newmeyer 1998:237ff.).

Somehow these authors seem to think that grammaticalization is wrongly regarded as a primitive concept, although I know of no claim to this effect. On the contrary, studies of grammaticalization such as Lehmann (1995[1982]), Heine & Reh (1984), Hopper & Traugott (1993) are quite explicit in listing the various low-level changes that are associated with grammaticalization, such as phonological erosion, desemanticization, reanalysis, decategorialization, and so on. The claim that these authors and other have made is that grammaticalization is a particularly interesting concept, because it is largely irreversible and because

³ It is worth remembering that two hundred years ago, the situation was very different. At that time, it was perfectly reasonable to speculate that our reconstructions of protolanguages brought us close to the first human languages. Until well into the 19th century, it was widely believed that life on earth was no more than six or seven thousand years old, and it was only the great discoveries of historical geology and evolutionary biology that made it clear that the biblical creation stories were way off the mark.

we observe strong correlations between phonological, syntactic and semantic-pragmatic changes. It is a macro-level phenomenon which cannot be reduced to the properties of the corresponding micro-level phenomena. Campbell's, Janda's and Newmeyer's criticism is similar to an objection against sociological studies of social classes on the grounds that social class is not a primitive concept, but an epiphenomenon which results from the interaction of human individuals.

Campbell says in the above quotation that sound change, semantic change and reanalysis are "well understood", but unless he refers to the terminology and really means "well defined", I find this far too optimistic. Diachronic phonologists and diachronic semanticists have not even begun collecting the systematic cross-linguistic data that would allow us to arrive at empirically well-founded universals of sound change and universals of lexical semantic change. Whereas for grammaticalization we now have Heine & Kuteva's (2002) *World Lexicon of Grammaticalization*, we are still waiting for a *World Lexicon of Sound Change* and a *World Lexicon of Lexical-Semantic Change*. In diachronic syntax, all we have is the handbook by Harris & Campbell (1995) with an exhaustive classification of syntactic changes and ample cross-linguistic exemplification, but few constraints and thus little explanation. In all these areas we are far from really understanding language change.

Newmeyer (1998:259) urges linguists not to "invite the conclusion that some dynamic is at work in grammaticalization that cannot be understood as a product of [certain independently occurring semantic and phonetic changes]." But this is exactly what is claimed by grammaticalizationists. Even if there is no single universally accepted explanatory architecture for grammaticalization yet, I believe that we have made a lot of progress in understanding the dynamic of grammaticalization. At the very least we have thorough cross-linguistic documentation, and a strong generalization, unidirectionality

3. Antigrammaticalization and "degrammaticalization"

My third main point is that most cases of "degrammaticalization" that are cited in the literature do not show the reversal of grammaticalization, but something else. I will discuss a fairly large number of changes that have been mentioned in the literature, and I will classify them into various types. There is no space here to describe the changes in any detail, so I must refer the reader to the earlier literature. The purpose of this section is twofold. On the one hand, I want to show that exceptions to the unidirectionality universal are not "rampant" (as Newmeyer 1998:263 claims), but are quite rare. Although probably around a hundred cases of degrammaticalization have been mentioned in the literature, the number of real exceptions is much lower. On the other hand, since the phenomena called "degrammaticalization" are so heterogeneous, it seems useful to identify various subclasses of "degrammaticalization". I do not think that these cases have anything in common, so that we do not really need a term like "degrammaticalization" for them, and I only use this term in scare quotes.

3.1. Antigrammaticalization: the reversal of grammaticalization

One important new term that I want to introduce here is *antigrammaticalization*. By this I mean a change that leads from the endpoint to the starting point of a potential grammaticalization and also shows the same intermediate stages. For instance, a change from a case suffix to a free postposition with the intermediate stage of a postpositional clitic would be an antigrammaticalization. This implies that the change occurs in a construction which can be seen as preserving its identity before and after the change, as in grammaticalization, where we also have a gradual change of the properties of a construction, but we do not get a new construction. On this point grammaticalization and antigrammaticalization differ crucially from analogy and reanalysis, where the change results from the fact that an expression or a construction is subsumed under a different construction.

It should be noted that my definition of *antigrammaticalization* is of course intended to cover types of changes, not tokens. Janda (2001:295) and Norde (2001:236) seem to interpret the term "reversal" as "token reversal", so that irreversibility would only mean that once a structure A has changed into a structure B, it does not change back to A. This claim, that token reversal does not occur (or is very unlikely), is of course not particularly interesting. My term *antigrammaticalization* is intended to cover any type of change that goes against the general direction of grammaticalization (i.e. discourse > syntax > morphology).

Armed with this new term, we can now say that only antigrammaticalizations are exceptions to unidirectionality (cf. Lehmann 1995b:1256), whereas other kinds of "degrammaticalization" are not necessarily expected to be rare or exceptional. This is not a weakening of the unidirectionality claim, because at least the way it was originally formulated (in Lehmann 1995[1982]), it is clear that only antigrammaticalizations were supposed to be ruled out, not any kind of change from grammar to lexicon. Janda (2001) has made a similar terminological distinction between reversibility of grammaticalization and counterability... (however, I am not aware that anyone ever claimed that grammaticalization should be not only irreversible, but also "non-counterable")

Let us now look at some antigrammaticalizations in this sense. Eight cases are listed in (7).

- (7) attested antigrammaticalizations
 - a. English and Mainland Scandinavian genitive suffix *-s* > clitic *=s* (Janda 1980; Norde 1997; Newmeyer 1998:266, 256; etc.)
 - b. Irish 1st person plural subject suffix *-muid* > independent pronoun *muid* (Bybee et al. 1994:13-4; Roma 1999)
 - c. Japanese adverbial subordinator *-ga* 'although' > free linker *ga* 'but' (Matsumoto 1988)
 - d. Saame abessive suffix **-ptaken* > clitic *=taga* > free postposition *taga* (Nevis 1986a)
 - e. Estonian question marker *-s* > *=es* > free particle *es* (Nevis 1986b, Campbell 1991:290-2)
 - f. English infinitive prefix *to-* > proclitic *to=* (Fischer 2000, Fitzmaurice 2000; but cf. Heine 2002:§2.7)

- g. Modern Greek prefix *ksana-* 'again' > free adverb *ksana* 'again' (Méndez Dosuna 1997)
- h. Latin rigid prefix *re-* 'again' > Italian flexible prefix *ri-* (e.g. *ridevo fare* 'I must do again')

For me, these cases are real exceptions, which means that they do not fall under any other generalization, and I cannot say more about them. This does not mean that more could not be said about them in the future. For instance, Bybee et al. (1994:13-4) say about the case of the Irish personal pronoun *muid* that there was "strong paradigmatic pressure" that facilitated the change. It could be that we eventually will be able identify further factors such as "paradigmatic pressure" that make antigrammaticalization possible, but until we have a solid generalization, any attempt at explaining these cases away seems premature.

All other cases of "degrammaticalization" that I have found in the literature are not antigrammaticalizations, as I will now show.

3.2. Delocutive word-formation from function words and affixes

A first type of change that has been called "degrammaticalization" is delocutive word-formation from function words and affixes. A delocutive lexeme is one that was derived by some regular word-formation process from another lexeme whose use in speech somehow determines the meaning of the derived lexeme. For instance, Latin *negare* 'deny' is said to derive from the negative marker *nec*, so it literally means 'say not', and French *tutoyer* is derived from the pronoun form *tu* and means 'use *tu* as address form'. An example of a delocutive noun would be *a hello* in English (meaning 'an act of saying hello'), as in *I heard many hellos and few good-byes*, or the noun *yes* in *I never know whether her yes is really a no*. Now I would say that expressions such as *ifs and buts* are delocutive nouns of the same type, and *iffy* is a delocutive adjective. A few further cases are listed in (8).

- (9) some delocutive formations
 - a. Latin *negare* 'deny, say no' <: *nec* 'and not; neither' (Benveniste 1966[1958]:279)
 - b. French *tutoyer* 'use *tu* as address form' <: *tu* 'you.familiar' (Norde 2001, Ramat 2001:396)
 - c. *ifs and buts* (van der Auwera 2002:22), *iffy* (Newmeyer 1998:274), *must* (from auxiliary to noun; van der Auwera & Plungian 1998:117)
 - d. Dutch *Is het een hij of een zij?* 'Is it a he (male) or a she (female)?' (Norde 2001)
 - e. French *le pour et le contre*, German *das Für und Wider* (Hagège 2001:1622)
 - f. Chinese *sa@tóng* 'the three withs', *sì huà* 'the four -izations' (Hagège 1993:210)
 - g. *ism* 'doctrine ending in -ism', *itis* 'disease ending in -itis' (Ramat 1992)

The original sense of *ifs* must have been 'situations in which one uses the word *if*', and the relevant sense of Dutch *hij* is 'person for which one uses the pronoun *hij*'. Delocutive word-formation presupposes some kind of reflection about linguistic expressions; it is a metalinguistic act that is probably very conscious. It is

therefore not surprising that function words can be the basis of delocutive word-formation processes.⁴ It is probably not an accident that such changes have primarily been reported from languages used by highly literate societies. If speakers know their language also in a written form, even affixes can become sufficiently salient to serve as the basis for a delocutive conversion process, as illustrated in 8f-g).

When a word such as *ism* is coined, this can be regarded as a change which has a grammatical item (the suffix *-ism*) as its input and a lexical item (the noun *ism*) as its output, and in this sense it is "degrammaticalization", defined as a change "from grammar to lexicon" (van der Auwera 2002). But clearly this is not antigrammaticalization as defined above, because this is not the reverse of a grammaticalization change. There is no intermediate stage at which *ism* is a clitic, and there is no sense in which we would say that the two items occur in the same construction.

Especially when the affix that has been turned into a noun is a prefix and when this happens in English, a good alternative explanation is that we are dealing with a clipping (ad-hoc shortening), because English uses this device so freely. In cases like *ex* 'ex-partner', *pro* 'in favor', *anti* 'against' (cf. Crowley 1997:148, Lazzeroni 1998:277), I find an explanation in terms of clipping more plausible than an explanation in terms of delocutive word-formation.

3.3. Back-formation of bound compound members

Next let us look at back-formation, which is a kind of reanalysis or analogical change (cf. Becker 1993). Cases like English *burger*, which was evidently backformed from compounds like *ham-burger*, *cheese-burger*, etc. (Crowley 1997:148), can be explained in this way. I would also regard numerals such as English *seventeen*, German *siebzig* 'seventy' and Italian *settanta* as compounds, so that free forms like *teens*, German *zig* and Italian *anta* (Ramat 1992:550; Norde 1997:3) can be explained as back-formations. Again this could not be antigrammaticalization because we do not have the same construction after the change, and there are no intermediate clitic stages.

3.4. Adverb-to-verb/noun conversion

Another set of examples that can be described by ordinary word-formation is adverb-to-verb conversion as in English *up_v* <: *up_{adv}*, *down_v* <: *down_{adv}*, *off_v* <: *off_{adv}*, and so on (Hopper & Traugott 1993:127; Newmeyer 1998:273), or the analogous Spanish examples *sobrar_v* 'be extra' <: *sobre_{adv}* 'above'; *dentrar_v* 'insert' <: *dentro_{adv}* 'inside' (Harris & Campbell 1995:432, n. 23). We also find an example of an abstract noun derived from an adverb (Finnish *pääll-ys_n* 'upper part' <: *päällä_{adv}* 'above'; Hagège 1993:209). Sometimes it has been claimed in the literature that these verbs and adverbs were formed from prepositions and not from adverbs, so that they would be potential cases of "degrammaticalization". But even if they were derived from prepositions, i.e. grammatical items, they would not

⁴ In Haspelmath (1999a) I argue that function words do not in general replace content words in unconscious changes, because function words are produced more automatically than content words, and that this in part explains the irreversibility of grammaticalization.

constitute antigrammaticalizations. It should be uncontroversial that these verbs and nouns were created by regular word-formation, not by the gradual modification of a construction.

3.5. Phonogenesis

So far we have seen instances of word-formation which represent instantaneous changes, and for this reason alone they are evidently not antigrammaticalizations. But there are also several kinds of gradual "degrammaticalization" changes that are not exceptions to unidirectionality. One is what Hopper (1994) calls phonogenesis, i.e. the loss of structure in a polymorphemic lexeme which thereby becomes monomorphemic. Hopper (1990: 155) mentions examples like modern German *bleiben*, whose initial consonant is a former prefix (older German *be-lîben*). Ramat (1992:551) cites English *drench* (from the Proto-Germanic causative **drank-jan*), and Hopper & Traugott (1993:127) discuss English *tomorrow*, which is no longer analyzed as *to* + *morrow* (cf. also Traugott 1994:1485). Van der Auwera (2002:21) gives English *twit* (from Old English *æt-wi@n* 'at + blame'). In all these cases one can say that former grammatical constituents became purely phonological constituents, so we have phonogenesis. This change is also known as *demotivation*, and its reverse is called *remotivation* or *folk etymology*. This is "degrammaticalization" in the sense that grammatical elements lose their grammatical status, but it is of course not the reverse of grammaticalization.

3.6. Loss of an inflectional category with traces

In demotivation we are mostly talking about derivational morphology being lost, but entire inflectional categories may disappear in languages as well, and linguists have sometimes called this "degrammaticalization". The older Indo-European dual no longer exists in Latin, and there are individual lexical items like *ambo* 'both' which have an ending that goes back to the old dual ending but synchronically no longer has grammatical status (Ramat 1992). Similarly, Wichmann (1996) calls the loss of the inflectional category of agentivity in Tlapanec "degrammaticalization". These cases are quite similar to derivational demotivation.

But it may also happen that a disappearing inflectional category leaves so many traces in surviving lexical items that the morphological pattern remains productive, but only as a derivational pattern. For instance, the Latin present participle has disappeared in many Romance languages as an inflectional category of the verb, but it survives as a derivational pattern in Spanish and Italian (*-ante/-ente*; cf. Luraghi 1998, Newmeyer 1998:264). A fairly similar case is the Swedish property-bearer suffix *-er* cited by Norde (1997:230) (e.g. *dummer* 'a stupid person' <: *dum* 'stupid'), which goes back to the Old Norse inflectional nominative-case suffix. So this is a change from inflection to derivation, and Kuryłowicz (1965) had said that changes from derivation to inflection show the reversal of grammaticalization. However, I do not think that such changes, which are clearly attested, should be lumped together with grammaticalization. Inflectional patterns do not show stronger internal dependencies than derivational patterns. While changes from discourse to syntax and from syntax to

inflection do form a natural class, changes between inflection and derivation should not put in this class.

Sometimes we also observe changes from a semantically empty stem extender to a meaning-bearing morphological affix or vice versa. Thus, the Old High German stem-extender *-ir* became the Modern German plural suffix *-er* (as in *Kalb/Kälb-er* 'calf/calves'; Harris & Campbell 1995:338), and the Latin derivational inchoative suffix *-escere* somehow became the Romance stem-extender *-isc(o)* (as in Italian *finisco* 'I finish'; Ramat 1992:552, Allen 1995). Again, I would say that these are changes internal to the morphology which are unrelated to grammaticalization and cannot be regarded as counterexamples to unidirectionality.

3.7. Retraction

Next I will consider a change type that I would like to call *retraction*. This is in some sense the opposite of expansion in grammaticalization, but it is not antigrammaticalization. Expansion is the development of new constructions or meanings that exhibit a greater degree of grammaticalization. Figure 1 shows a prototypical case of grammaticalization. As an item expands to the right and forms a grammaticalization chain, some of its earlier manifestations on the left typically disappear, so that the chain loses on the left what it gains on the right. Now of course we know that the older members of the chain do not have to be lost (this is often described as *layering*; Hopper 1991, Bybee et al. 1994:21), so that, for instance, A_1 does not have to be lost at stage 3 in Figure 1 (cf. stage 4, where A_2 is still there despite the further expansion to A_4). We also know that further expansion need not occur: an item may get lost from the language before it expands further (Hopper & Traugott 1993:95). Now one additional possibility is a grammaticalization chain in which a right-hand member becomes obsolete. Everything in language can become obsolete, independently of its degree of grammaticalization, so there is no surprise here. Schematically such a situation is depicted in Figure 2. Until stage 4, the element B expands rightward, but then the members B_4 and B_3 are lost. Here I say that B has *retracted* to B_2 .

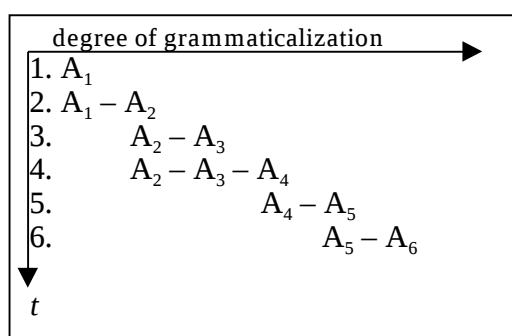


Fig. 1: Rightward expansion
(= Grammaticalization)

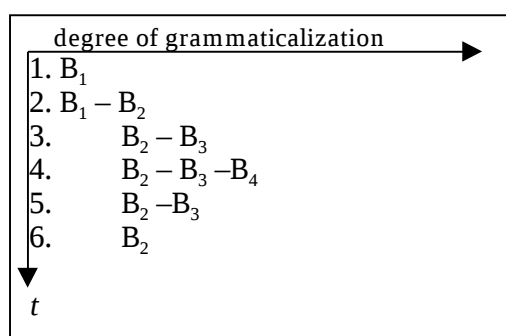


Fig. 2: Retraction

But crucially, it has not expanded leftward. Such an example is seen in Figure 3, where the element C first grammaticalizes and then (at stage 5 and 6)

antigrammaticalizes. The crucial difference between retraction and antigrammaticalization should be clear.

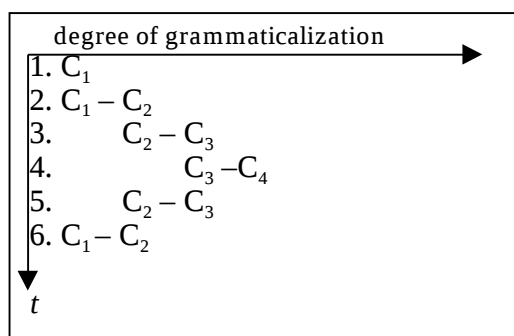


Fig. 3. Leftward expansion
(= Antigrammaticalization)

However, retraction has sometimes been cited as counterevidence to unidirectionality. I will mention three cases. First, Newmeyer (1998:273) discusses the English word *man* and notes:

"...the history of the word *man* also presents challenges for any sweeping claims about unidirectionality. In Old English, its predominant use was as an indefinite pronoun (cf. German *man*). Subsequently it seems to have swung back and forth from pronoun to full lexical noun and back again. In any event, it is the *less* grammaticalized use that has survived into Modern English."

But of course the non-grammaticalized word *man* always existed, so this is a case of retraction rather than leftward expansion. Second, Newmeyer (1998:270-1) (citing Kroch et al. 1982:287-91) mentions the case of English postverbal subject clitics in Early Modern English (e.g. *Where dwellyth=she?*). The pronouns allegedly underwent decliticization after 1550, so that in Modern English no subject clitics are found. But of course the independent subject pronouns always existed side by side with the subject clitics, so again this is not antigrammaticalization but simply retraction.

The third example is the development of the English verb *dare*. According to Beths (1999), this goes against unidirectionality because *dare* was a semi-lexical verb in Old English, then became an auxiliary in Middle English and has reverted to a lexical verb in Modern English. But again, this is a case of retraction, not of leftward expansion. Traugott (2001:9) observes:

"...this is not a conclusive counterexample to unidirectionality, because main verb *dare to* uses were always attested in the data. The best we can say is that the earlier main verb use was marginalized in the early periods and then the grammaticalized one was marginalized in turn and then lost in the later periods."

Thus, these changes do not provide counterevidence to the unidirectionality claim.⁵

⁵ A somewhat similar case is the Swedish verb *må* (cognate with English *may*, German *mag*), which originally meant only 'may', but now has acquired the meaning 'feel' as well (van der Auwera & Plungian 1998:105). When it means 'feel', it follows a different inflection class. Van der

4. Conclusion

I have tried to make three major points in this article: That directionality constraints and other constraints on language change are an important prerequisite for understanding language change; that the unidirectionality of grammaticalization is one of the most important constraints on morphosyntactic change, despite various general criticisms; and that many of the alleged counterexamples to unidirectionality are not antigrammaticalizations and hence do not provide evidence against it. I will end the paper by making a few more general remarks.

4.1. Broader agendas

The first point concerns the usefulness of directionality constraints in the study of language change in general and in grammaticalization in particular. As I made clear in §1, I find them very useful, because only when we have a universal generalization do we have anything to explain. Thus, identifying and refining generalizations about directionality is high on my agenda. But is there also an opposite agenda? In other words, is there a theoretical perspective on language change that would want to ignore or deny directionality constraints because they do not fit into its general goals and assumptions? The answer is yes: If one thinks of language change as occurring exclusively in language acquisition, and if one thinks of cross-linguistic variation in terms of different settings of innate parameters, then one expects language change to be "essentially a random "walk" through the space of possible parameter settings" (Battye & Roberts 1995:11). So it is in particular linguists with a Chomskyan perspective on language and language change that should see unidirectionality as a challenge, and they should try to discredit it. And indeed, at least David Lightfoot has argued against the general notion of directionality constraints (Lightfoot 1999:34ff.) and against unidirectionality of grammaticalization in particular (Lightfoot 2002: 125-7).⁶ Also, Frederick Newmeyer has included a very critical chapter on grammaticalization in his (1998) book *Language form and language function*, in which he discusses many alleged counterexamples to unidirectionality. However, Newmeyer does not seem to be interested in defending a Lightfoot-style or Roberts-style approach to language change; his main concern seems to be to show that the evidence from grammaticalization is not incompatible with generative grammar. Moreover, he recognizes himself that there is a strong quantitative asymmetry favoring grammaticalization over its reverse, and he even proposes his own explanation of this asymmetry.⁷

Auwera & Plugian (1998:116-7) regard it as a counterexample to unidirectionality, but the morphological change in itself does not make the word less grammaticalized, and the semantic change does not go against any well-defined semantic grammaticalization path. So it is certainly not a good exception to unidirectionality (see also Burridge 1995 for a somewhat similar case in Pennsylvania German).

⁶ See Haspelmath (1999a) for a critical review of Lightfoot (1999). Unfortunately, Lightfoot (2002) does not respond to my criticisms but mostly confines himself to simply repeating some of the points from his (1999) book.

⁷ See Haspelmath (2000a:247-50) for further discussion of Newmeyer's views on grammaticalization.

But counterexamples to unidirectionality have also been highlighted by non-Chomskyans who do not seem to have a broader agenda (Paolo Ramat, Richard Janda, Brian Joseph, Lyle Campbell, Alice Harris, Muriel Norde, Johan van der Auwera, Roger Lass). In the writings of some of these linguists, one senses a frustration with theoreticians who make broad sweeping claims but do not back them up with solid and careful historical linguistic work. Clearly, once one starts asking larger questions, there is the danger that one pays less attention to the data and more attention to the ideas, but there is also the opposite danger of missing the generalizations and the big picture because one sees too many details. Progress in linguistics will depend on finding a proper balance between these two ways of viewing the world of language change.

4.2. Terminology

It seems to me that the history of the term *degrammaticalization* shows how important it is to pay attention to terminological ambiguities. As I said earlier, I think coining this term was useful because it made people look for actual instances of the phenomenon. But it is also easy to take the term too literally, for instance in the sense of 'loss of grammar', or 'lexeme creation on the basis of a grammatical item'. The potential for misunderstanding is particularly well illustrated by the term *demorphologization*, which has been used in two totally different senses: In Joseph & Janda (1988), it refers to a change from morphology to phonology, while in Hopper (1990), it refers to a change from a morphologically complex word to a simple lexical item (cf. also Ramat 2001:394).⁸ These two change types are clearly unrelated, and similarly I have made the point that the different cases of "degrammaticalization" have rather little to do with each other.⁹ So when encountering the term "degrammaticalization", one should first make sure to understand what exactly the author means by it before drawing conclusions from it. My own practice is to avoid the term entirely, and to use it only in scare quotes when talking about others' terminological usage.

4.3. Broad agreement

As I made clear in §3.1, I accept the existence of exceptions to unidirectionality, and in this respect I find myself in agreement not only with grammaticalization critics, but also with other grammaticalization enthusiasts (Traugott 2001, Heine 2002). Thus, where is the disagreement? Do the detractors of grammaticalization studies claim that grammaticalization and antigrammaticalization are equally common? I am not aware of such a claim in the literature, and some of those linguists who have emphasized "degrammaticalization" have simultaneously made it clear that grammaticalization occurs much more often (e.g. Ramat 1982:549, Harris & Campbell 1995:338, Janda 2001:270). Newmeyer (1998) even

⁸ Another term that is used in multiple senses is "lexicalization" (see Himmelmann (to appear) for some recent lucid discussion). Here, too, the terminological polysemy seems to derive from the fact that linguists have not necessarily followed other linguists' usage, but have used *lexicalization* for diverse phenomena that can be interpreted as 'putting in the lexicon' or 'making lexical'.

⁹ The term *degrammaticalization* has even been used in a totally different sense, to refer to the tendency in informal e-mail communication to violate normative grammatical rules (Pansegrau 1997).

offers an estimate of the relative proportions of both types of changes: "a rough impression is that downgradings have occurred at least ten times as often as upgradings" (Newmeyer 1998:275-6). I have said elsewhere that my impression is that they are rather a hundred times as frequent (Haspelmath 2000a:249), and it would be interesting to try to assess these impressionistic estimates in a systematic empirical way. Unfortunately, such an enterprise encounters some serious difficulties, not only the problem of defining a reasonable sample of languages and language changes, but also the problem of defining discrete events of grammaticalization (cf. Lass 2000:214-5). When a free adposition first becomes a clitic and then an affix, is this one change of grammaticalization or two? Eventually these difficulties could be overcome, but I still wonder whether such a quantitative study would be worth the trouble, in view of the fact that there is really broad agreement: Grammaticalization is far more common than anti-grammaticalization, and this is a surprising fact. Anyone who is interested in understanding language change should be interested in why this is the case. I have proposed an explanation in Haspelmath (1999a), and I look forward to further contributions to this discussion.

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