Legitimate counterexamples to unidirectionality
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0. Introduction

I am often asked what are the hottest topics in grammaticalization these days. I think they are whether unidirectionality is of theoretical significance, how to think about specific changes known as grammaticalization in contact situations, and how research in grammaticalization, which largely grew out of functionalist thinking, can best be reconciled with formalist linguistics. I will talk today about the first of these: unidirectionality, most especially about what is or is not a legitimate counterexample.

Unidirectionality (or what Vincent, seeking a more neutral term, has called "directional asymmetry" (2001: 20)), has been associated with grammaticalization from very early times. One line of thinking involves bleaching--in 1891 von der Gabelentz discussed "Verblassung" in the process of morphophonological weakening. Another is lexical to grammatical change, which Meillet (1912) suggested was the only way to introduce real (structural) innovation into a language (but not the only way in which grammaticalization can proceed, cf. his mention of word order change).

Various potential counterexamples to unidirectionality began to be discussed in the early 1990's--several papers in Traugott and Heine (1991) raised questions whether grammaticalization is always unidirectional. Hopper and Traugott (1993) summarized the then current issues (in a couple of pages). In the last few years unidirectionality has become a matter of very heated debate, fueled largely by a chapter called "Deconstructing grammaticalization" in Fritz Newmeyer's *Language Form and Language Function* in which he claimed that "there is no such thing as grammaticalization, at least in so far as it might be regarded as a distinct grammatical phenomenon requiring a distinct set of principles for explanation" (1998: 226), by David Lightfoot's claim that the search for unidirectionality is a holdover from the nineteenth century (1999) (i.e. anti-structuralist, which is of course true), and by a number of papers by Richard Janda (e.g. Janda 1995, 2001) and Brian Joseph (e.g. Joseph 2001). A whole issue of *Language Sciences*, (2001, edited by Lyle Campbell) was devoted almost exclusively to denying the theoretical importance of unidirectionality and to providing empirical evidence against it, and many articles supporting this view have been published in the last three or four years. On the other hand, a whole issue of *Linguistics* (1999, edited by Ans van Kemenade) was devoted to understanding how functional categories arise out of lexical ones, and to providing bridges between primarily functionalist and primarily formalist approaches to grammaticalization; this volume includes a paper by Martin Haspelmath entitled "Why is grammaticalization irreversible", though it also includes one by Frank Beths (1999), which I will discuss, arguing that the development of *dare* is a counterexample.

I do not want to claim that grammaticalization is a distinct phenomenon--it isn't. Broadly speaking, and in the most neutral terms possible, it's a subset of cross-linguistically recurring changes, that involve correlations across time between semantic, morphosyntactic (and sometimes also) phonological changes. I also do not intend to argue that grammaticalization is irreversible--that is far too strong. My intention is to show that grammaticalization is a hypothesis about a robust tendency. My current formulation of it is:

(1)   Grammaticalization is the change whereby lexical items and constructions come in certain linguistic contexts to serve grammatical functions or grammatical items develop new grammatical functions.

Testable hypotheses must be approached with proper tests, methodologies, and assumptions (see also Vincent 2001), and it is this question of what is a legitimate test that

I want to focus on. I take the search for counterexamples and the published criticism very seriously, and have been skeptical of Lehmann's and Haspelmath's claims that many examples are spurious, but preparing this paper has convinced me that indeed many do not hold up under scrutiny.

In presenting (1) I recognize that ultimately the question of legitimacy cannot be answered without extensive discussion of what "lexical item", "construction", "grammatical function", etc. in (1) themselves mean. Given the limited amount of time, though, I hope it can suffice for our purposes to think of:

a.　　"lexical items" as language-specific units that are typically stems and affixes (often, but not always independent units, open-class words, in their earliest stages).

b.　　"constructions" as the basic language-specific units out of which clauses are constructed, including both open-class lexical items and closed-class grammatical forms like prepositions and auxiliary verbs.

　　c.　"grammatical functions" as a relatively closed class of language-specific items such as SUBJ, OBJ, ADJUNCT, C, T, M, whereby we understand who did what to whom when, what the discourse act is (question, imperative, etc.), and what information status (definite, indefinite, etc.) is involved.

Importantly, I consider various markers of discourse, including discourse markers to be part of grammar. If (1) is right, GR can be modeled as in Figure I.
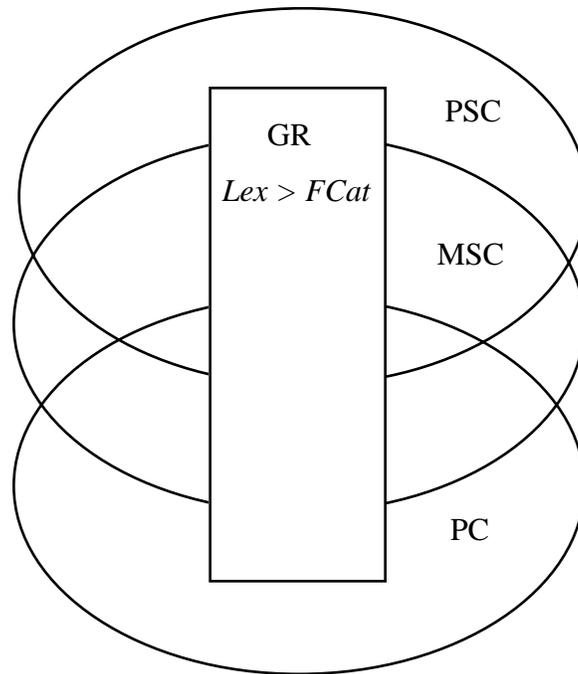
Note (1) does not incorprate unidirectionality; this is to avoid any possible circularity engendered by incorporating unidirectionality into the definition (see cautions concerning this in Newmeyer 1998, Vincent 2001). However, it resembles several current definitions, such as (2), which Newmeyer (1998: 227) terms the "standard definition", where "more" appears instead of "new grammatical function":

(2)　　grammaticalization ... where a lexical unit or structure assumes a grammatical function, or where a grammatical unit assumes a more grammatical function
　　　　　　　　　　　　　　(Heine, Claudi and Hünnemeyer 1991: 2).

This begs the question what exactly "more grammatical" means. For Kurylowicz over forty-five years ago the answer was fairly straight forward: inflexion is more grammatical than derivation (as Kurylowicz 1976[1965] claims), but since then the issues have become more complex. Is a relativizer more or less grammatical than a complementizer introducing an argument (see Givón's 1991 analysis of the development of *'asher* from relativizer to complementizer in Hebrew)?

(1) also contrasts with definitions such as (3):

(3)   the process whereby lexical items and constructions come in certain linguistic
        contexts to serve grammatical functions, and, once grammaticalized, continue to
        develop new grammatical functions. (Hopper and Traugott 1993: xv)

There are two main differences between (1) and in (3).

The first difference is that (3) includes the word "process", (1) does not. "Process" is clearly not a good term if it invites the idea that grammaticalization is distinct; as I indicated, it isn't. -- The original point was to highlight the fact that when we study grammaticalization we are looking at all the minor changes that can lead to major catastrophic changes, as well as at the latter, and we are looking at dynamic change (what is in the arrow >), not at comparisons between synchronic layers. By contrast, Lightfoot (1979) had (and still does) cast the spotlight on catastrophic changes that are the result of a long chain of minor adjustments, and on comparisons between synchronic states. The original view is still in my view a valid one, and the term "process" should not be abandoned too easily. But how dangerous this term is can be seen from how much it has been misunderstood, as evidenced by comments like Newmeyer's that grammaticalization "fails to evince the most important distinguishing feature of a distinct process--the unfolding of its components parts in a determinate sequence in which one step of the sequence inevitably engenders the following one" (Newmeyer 1998: 251). Of course, Lass (1980) pointed out two decades ago that at some level we can never "explain" change, if by "explain" we require a deterministic program that can predict precisely what will happen, so Newmeyer isn't saying anything specific to grammaticalization. Language is a human and a social capacity, and there simply is no way in which we can usefully think of it in the same terms as a scientific theory that will get our rocket to Mars. Changes do not have to occur. They also do not have to go to completion, in other words they do not have to move all the way along a cline, or even continue down it once they start out on it.

The second difference between (1) and (3) is that (3) implies that continued development of grammatical functions (what Givón 1991 called "secondary grammaticalization"), presupposes prior grammaticalization. This may be too strong. I'll come back to this point.

Let me say from the outset that what I am going to discuss today makes sense only on the assumption that it is legitimate to talk about tendencies. Grammaticalization grew out of functionalist thinking, and has always been associated with tendencies, not absolutes (despite the title of Haspelmath's paper, "Why is grammaticalization irreversible?"). If one is of the opinion that a single counterexample is enough to refute a linguistic universal, and that only linguistic universals can explain anything, and therefore only they are worthy of study, then there is at some level nothing to talk about. Newmeyer does at one point take the 100% position: "I take any example of upgrading as sufficient to refute unidirectionality" (Newmeyer 1998: 263).

But nevertheless he obviously does take grammaticalization seriously. And so do many others who share his views about absolute universals. Why? One reason may well be that, as Dryer (1997) has pointed out, statistical universals may in fact be better indicators than absolute ones of what constitutes language. That is, empirically supportable strong tendencies can be better than inviolable ones, precisely because they are harder to support, and inviolable universals are virtually impossible to find. The recent interest in Optimality Theory, especially stochastic optimality theory (e.g. Boersma and

Hayes 2001, and, from a diachronic perspective, B. Clark In Progress), tries to address precisely the problem of violability while at the same time maintaining the importance of universals, so even within the ranks of formal generative grammar the 100% claim is becoming suspect. Another reason why Newmeyer and others spend so much time on grammaticalization presumably is that it is widely attested. If we are ever to understand what language is, how it is transmitted, how it stays the same and how it changes, we have to address the challenging question:

(4)   What empirical evidence is there for or against the claim that change is "a random 'walk' through the space defined by the set of possible parameter values"? (Roberts 1993: 252)

Empirical support for (4) would be evidence that there are many examples of (5)

(5)   zig-zag from lexis to grammar and back again and movement in the direction of greater grammatical function and then retracing of those steps
                        (based on Vincent 2001: 20)

"Many examples" would presumably be a sufficient number of examples to count as evidence for randomness, i.e. about 50%. But this is simply not the case. Even Newmeyer says

(6)   "unidirectionality is *almost* true" (Newmeyer 1998: 275; italics original).

So, whatever view of the task of linguistics that we have, we need to know what is a good counterexample to unidirectionality, or, to put it the other way round, good evidence for non-directionality. The number of putative counterexamples that have been adduced are a measure of the liveliness of the field and the scientific enthusiasm that the hypothesis of grammaticalization still engenders, but we do need to be able to evaluate them (rather than simply list the references--Janda 2001: 291-292 gives us an invaluable bibliography, but without any assessment of which are more or less legitimate than others and on what grounds).

I will start in section 2 by attempting to clear the brush of irrelevancies that cannot contribute usefully to the debate. I will then discuss a few putative sets of counterexamples that I think are clearly not legitimate (or not legitimate in the way they have been presented). In section 4 I will look at a couple of claims that do seem to be legitimate, and finally I will mention some issues that I think should be considered carefully as we continue testing the unidirectionality hypothesis.

2. Clearing the brush

First I want to clear the air of spurious argumentation by pointing out that using double standards get us nowhere. Haspelmath (2000) has argued this point very cogently in his review of Newmeyer's book.

• *Premature generalizations*. It won't do to complain as do Newmeyer (1998), and later Campbell, Joseph and Janda in *Language Sciences* (2001) that some generalizations are premature, or that multiple definitions of grammaticalization exist, when the only way in which linguistic theory has advanced is by putting forward premature generalizations (e.g. about specific "parameters"), and by modifying definitions (e.g. of "head", "valency").

• *Some hypotheses don't fall out from the theory*. Nor will it do to complain that grammaticalization is not a theory because it is a list of hypotheses, several of which do not emerge from an internal theory (Newmeyer 1998: 240), especially when the list is not accurate. Haspelmath seems to agree with Newmeyer. But I don't think this is right. How many theories exist that meet Newmeyer's criterion? In any event Newmeyer gives as his example a phenomenon that is not unique to grammaticalization. He says layering does not fall out from unidirectionality. Certainly it does not. Layering is a phenomenon associated with all change, phonological, morphosyntactic, or semantic (it is simply the

principle of variation; in semantics it is the principle of polysemy); therefore it is not specific to unidirectionality. I am aware of only one paper that claims evidence for lack of layering in a change, Allen (2001) on the development in Middle English of the recipient passive as in (7).

(7)   He was given a book.

She claims it never coexists in texts with an earlier dative-fronted passive constructions as in (8a) where the NP is bare, only with a prepositional phrase as in (8b):

(8)   a.   Him   wearð gesealdan snæd   flæsces
             He:DAT was   given   a piece:NOM flesh:GEN
                    (c. 1000 AELS (Basil) 158 [Allen 2001: 45])
        b.   And to them which be repentaunt...for thyr synnes .. is promysed forgyuenes
                    (after 1480 Bishop Fisher, 238 [Allen 2001: 63])

This was a change, as she says, in a very small corner of the grammar; passives are used rarely in any event, recipient passives even less so, and to what extent it is reasonable to deny that the bare and prepositional NPs were in variation rather than competition deserves further study.

        Hopper (1991) identified layering as a "principle" of grammaticalization, but only Heine, Claudi and Hünnemeyer (1991) have to my knowledge tried to claim that it is peculiar to grammaticalization. Why layering is important to grammaticalization is that it allows the researcher to a) allow for polysemy not homonymy in early stages of change (even if *be going to* is no longer polysemous, it appears to have been so for most of the Early Modern English period), b) to recognize that strings that have different functions in different contexts may be historically related.

        • *Metaphor*. Another example of double standards is to complain that metaphors like "path" are used (what about metaphors like "parameter", "tree", "node"?). It particularly won't do to reify them and to assume that "path" refers to some conduit in the brain! "Path" is a useful metaphor, because paths can branch--likewise a single root like *have* can be grammaticalized in several different ways (perfect as in *I have finished it* , vs. causative as in *I'll have it finished*); Craig (1991) called this kind of splitting "polygrammaticalization". We do not have to go down paths--likewise change does not have to happen. A single path usually not the only way to get to some location--likewise, there may be several paths of grammaticalization leading to one grammatical category, e.g. conditionals.

        Most importantly, "path" is a metaphor or a schema, a linguist's idealization against which individual changes are tested. As Andersen (In press) reminds us, while social valuation is everywhere around us, guiding local behaviors and leading to actual change, unidirectionality is a schema, an idealization, allowing the investigator to abstract away from noise and focus on the bigger picture; acquirers' access to token frequency across social groups, styles etc. gives them access to small-scale, local evidence for directionality, but not to long-term, large-scale "paths". It follows that much of the argument that "path" is misleading or wrong because children cannot have access to "paths" is off the point. For example, Lightfoot suggests that there cannot be (uni)directionality because, if there were, and a child were to be thought to access unidirectionality, one would have to falsely postulate a "racial memory of some kind" (1999: 209). "Racial memory" is obviously an incoherent idea. Unidirectionality as an idealized schema is not. Once we think of a path as a schema, a type, then its status as a testable, predictive hypothesis can be properly recognized.

        • *Unidirectionality violates the uniformitarian principle*. Whenever I talk about unidirectionality, the question inevitably arises whether I am not postulating a stage of language in which there were only lexical items, a stage which, as Lass (2000) points out, would violate the uniformitarian principle. This principle requires that we reconstruct only languages which are of a type empirically evident to us from extant or recorded languages.

The definition in (3) certainly invites this interpretation, with its "once grammaticalized continue to develop new grammatical functions", but in my experience ANY discussion of unidirectionality raises similar questions.   To date we still need evidence that grammatical items arise full-fledged, that is, can be innovated without a prior lexical history in a remote (or less remote) past. Some grammatical items show enormous longevity, and we cannot look back into their pre-history. As has often been pointed out, among the highly stable grammatical items with no known lexical origin is the Indo-European demonstrative *to-*. Given the unidirectionality hypothesis, proponents of a strong unidirectionality hypothesis like (3) would have to argue that *to-* originated in some currently unknown lexical item or construction. We do not at this stage of our knowledge know what that item or construction was. But neither do we know that there was none, or indeed that there might theoretically have been none. We must leave for future empirical study the question whether grammatical items can arise fully formed, and if so under what circumstances.

        One thing is clear: claiming that grammatical items originate in lexical ones does NOT entail hypothesizing a language state in which everything is lexical. This would be true only on a creationist view of language development, the Chomskyan position that the language capacity emerged suddenly, full-fledged. I submit that the creationist view is in error, not unidirectionality (at least in regard to this point!). Presumably, in the history of evolution, changes occurred including those characterized as grammaticalization, and by the time language as we know it was used by Homo Sapiens, it included markers of the grammatical glue as well as lexical items that had already had long histories.

3. Some putative counterexamples.

        I have time to give only a few examples of putative counterexamples which I believe are not legitimate under the assumptions that I have alluded to as correct. Many are frequently cited without much attention to what assumptions their authors have made.

CASE A. The *'s* genitive

        The data for some alleged counterexamples are simply not well enough understood to cite as clear cases of counterexamples. For example, one often cited example is that of the cliticization of genitive *-s* in English (see Janda 1980, updated in Janda 2001). But there are so many different analyses and counter-analyses that I think we can only say the jury is still out on this one (see also Allen 1997, Tabor and Traugott 1998). A particularly important contribution to the discussion is Norde's analysis of the (partial) replacement by a clitic of inflexional genitive in English, Swedish, Danish and the variety of Norwegian known as Bokmål (an upper-class variety of Norwegian influenced by Danish) (Norde 2001). For example, Old English inflectional genitive as in (9) shows concord within the possessive NP (Ecgfrith the king):

(9) a. ðæs   cyning:es sweoster Ecgfrið:es

        the:GEN king:GEN sister:NOM Ecgfrið:GEN
                'the sister of Ecgfrith the king' (c. 1000 Aelfric Hom 11, 10 87, 215)
but three hundred and fifty years later we find a clitic:
(9) b.   the god of slepes heyr
            'the god of sleep's heir' (c. 1368 Chaucer, Book of Duchess 168)
The use of the clitic spread gradually to increasingly more varied contexts, and is a case of generalization across types of NP, including pronouns (cf. *anyone else's cat*). In this it is like grammaticalization. As Norde (2001: 260) points out, the examples discussed are strictly speaking not examples of the REVERSAL of grammaticalization; we do not find a form that has become an inflection in its recorded history revert to an earlier attested stage. Instead, the development of English *-s* genitive, realignments of Swedish case and other examples of inflection to clitic crucially involve NEW (but not MORE) grammatical

function. Crucially, these changes are part of similar major shifts in both language groups: the loss of case inflections. I will come back to this kind of point later.

CASE B. Instrumental case in Russian
       One putative kind of counterexample to the proposal that grammaticalization involves changes in the degree of grammaticality of an item was proposed by Nichols and Timberlake (1991). They point out that in the history of Russian there have been changes in the uses to which the instrumental case has been put that are akin to grammaticalization in so far as they involve the coding of grammatical relationships, but they are unlike grammaticalization in its prototypical directional sense, in so far as they simply demonstrate a shift in the way relatively stable grammatical networks operate, not a shift to more grammatical status. Thus in Old Russian, the instrumental was allowed only with nouns expressing status or role that could change over time (e.g., 'tsar,' 'secular leader,' 'nun'), and only in contexts of entering that status (inception), or continuing in it for a period of time. Later Russian, however, virtually requires the instrumental with such nouns referring to status or role; also quasi-status nouns (agentive nouns such as 'bribe-giver,' 'bribe-taker') can now allow the instrumental in contexts of durative aspect. This example shows that grammatical morphemes can remain stable over a very long period of time. There is no case of "more > less grammatical", though there is some evidence of "new grammatical function" here. As the authors themselves say: "the overall effect has been to fix usage in one domain and develop variation in another" (Nichols and Timberlake 1991: 142)--for reasons that are not entirely clear they call this "retextualization". In other words, the history of the Russian instrumental is an example of prototypical rule generalization and spread over a lengthy period of time (about 1,500 years). It also illustrates the potential longevity of certain types of grammatical organization. It is a counterexample to the unidirectionality of grammaticalization only on the requirement made explicit by Newmeyer: that directionality be deterministic.
       As I said, change does not have to occur. Rules do not have to go to completion. A particular grammaticalization process may be, and often is, arrested before it is fully "implemented," and the "outcome" of grammaticalization is quite often a ragged and incomplete subsystem that is not evidently moving in some identifiable direction. There is nothing in the hypothesis in (1) that requires a change to go to completion, nor do I know of any definitions that require it. Even Givón's famous
(10)   Discourse > Syntax > Morphology > Morphophonemics > Zero
                   (Givón 1979: 209)
only implies it, but does not require it. The notion of going to completion is a widely held but misguided view of language change, based in part on assumptions that may be a hold-over from Neogrammarian thinking about regularity in phonological change. It may also be based in part on unfortunate teleological ideologies. Most probably it is based on a kind of "conditional perfection" with which we pragmatically enrich statements of the kind A > B (interpreted as 'A necessarily to B', not 'A may be to B').

CASE C. Grammatical > lexical item
       Probably the most often cited putative counterexamples are those involving the use of grammatical items, including derivational morphemes, categorially as nouns or verbs, e.g., *to up the ante*, *that was a downer*, *his uppers need dental work, I dislike her use of isms* (see e.g. Ramat 1992). Similarly, in German and French the second person singular familiar pronouns *du* and *tu* are lexicalized as the verbs *duzen* and *tutoyer*, respectively, both meaning 'to use the familiar address form'. Changes of the type Prep. *up* > V *up,* and most especially of the type derivational morpheme *-ism* > noun *ism*, typically involve a quotation or mention of some kind. These kinds of changes are instantaneous--one can take any element of language, including the letter with which it is graphically represented or

to which it is iconic, and use it lexically, e.g. *N-word, T-square, two's*, and use it like a noun; one can do the same to phrases like *forget-me-not* and acronyms like *laser* ('light amplification by the stimulated emission of radiation') (Norde 2001: 236). Likewise, given certain semantic constraints (see e.g. E. Clark and H. Clark 1979), one can take any noun and instantaneously convert it into a verb (e.g. *to calendar, to typo*). Innovations of this type may or may not spread to other speakers, just like other changes.

Because they are not limited to minor > major category (N > V, and V > N are shifts across major categories), and because they are instantaneous, changes exemplified by *-ism* are quite unlike change associated with grammaticalization, and are not counterexamples to it (see also Haspelmath 2000: 249, ft. 10, Vincent 2001: 22). Rather, they are instances of recruitment of linguistic material to enrich the lexicon; they have to do with word formation, part of a process typically called "conversion". They are therefore not counterexamples to grammaticalization, which crucially involves enrichment of the grammatical component of language.

CASE D. Changes leading to new paradigms

Most early studies of grammaticalization took the development of new paradigms or reorganization of extant ones as prime examples of grammaticalization. The search for counterexamples has, however, led to some odd interpretations of criteria suggested especially by Christian Lehmann. One such odd interpretation is to be found in Giacalone Ramat's (2001) paper when she says that "if we take for granted what Lehmann (... 1995: 132) claims, namely that morphological degeneration is an attribute of grammaticalization" then the rise of inflections does not conform to this attribute (Giacalone Ramat 1998: 116). The example she gives is the cliticization of subject pronouns in Alemannic dialects to the verb, as in:

(11)   a.   hätter gseit 'has-he said'
        b.   hätsch gseit   'has-she-said'
        c.   häts gseit   'has-it-said' (Giacalone Ramat 1998: 117)

Certainly there is new complexity for the verb. However, there seems to be less complexity for the pronouns, at least in these contexts. The point here is that we need to remember that claims about unidirectionality in grammaticalization are claims about the history of forms, not of whole systems and paradigms.

CASE E. The development of *dare (to)*

I now come to the to me most interesting case, *dare to*, as studied by Beths (1999). Vincent (2001) seems convinced by Beths' claims regarding increased use since the sixteenth century of main verb forms of *dare*. I myself, when I started writing this paper, found myself partially agreeing with Beth's (1999) analysis. However, on doing some more research, I found myself moving this example from the category of "possible counterexample", to the category of definitely not a counterexample. It demonstrates how important data from various sources including contemporary language are.

According to Beths this verb is attested in Old English (OE) as a main verb with a directional PP, and very occasionally with *to* (a phenomenon that he considers evidence for main verb status in OE). However, most of its uses in OE and Middle English (ME) have what are usually considered pre-modal (and hence pre-auxiliary) properties. One syntactic criterion for pre-modal use in OE is lack of an infinitive form (infinitive **durran* is not attested; likewise **magan* and **motan* are not attested). Another is coordination; Warner (1993) claims that only pre-modals can "share" an infinitive in OE:

(12)   nan   nære   ðætte   þætfærelt ...   anginnan *dorste*
        none   NEG-were who   thatjourney ...   begin   dared

        oþþe mehte

or   could
'there would not be anyone who dared or could begin that journey'
            (c. 880 Orosius 8.100.12-17 [Beths 1999: 1079

Also Beths shows that in ME, *dare*, like other premodals, acquired increased deontic meanings:

(13)   Forrþi darr   mann   hatenn wel All þatt tatt æfre   iss sinne.
            'Therefore one dare/must   hate well everything which is always a sin'
            (c.1200 Orm 5066 [Beths 1999: 1091])

In Early Modern English it continued on a path typical of modals: for example, it did not occur with *do*-support, and, as with *must* and *ought* past tense forms could be used in present tense contexts:
(14)   He looks as if he *durst* not approach.
            (1693 Congreve, Old Bachelor IV [Beths 1999: 1097])
All of these changes suggest increased grammaticalization.

However, new main verb-like uses began to appear and then to predominate; indeed, uses such as (13) and (14) became, Beths says, obsolescent in the Modern English period. Instead we find increase of uses with *to* (15a, c), other modals (15a), an NP object (15b), and *do*-support (15c):
(15)   a. That none of youre officers roialle, ... shalle *darre ...to* take no bribe.
            (1451 Bk. Noblesse 72 [Ibid.: 1094])
        b. An English man ... [cannot] suffer ... to *be dared* by any.
            (1580 Euphues 316 [Ibid.: 1096])
        c. as if they ... *did not dare to* venture on the lines of a face.
            (1668 Dryden, Essay D. P. 306 [Ibid.: 1099])
He also claims that there are no auxiliary uses of *dare* in PDE.

Let us assume for a moment that Beths is right, and bona fide auxiliary uses of *dare* are no longer used, as least in Standard English. Then what we have here is an example of what must be assumed to be an original main verb use giving rise to auxiliary-like uses (typical grammaticalization); after a considerable period of coexistence of the two uses, the main verb use won out.

Like Newmeyer, Beths seems to think that grammaticalization theory requires or ought to require a deterministic outcome. In his view unidirectionality is a "natural outcome of grammaticalization, not a necessary one" (Beths 2000: 1074). It is a natural outcome because, from his perspective, syntactic change typically involves Kayne (1994)'s leftward movement constraint; this constraint leads historically to shifts from lexical to functional status, and semantic bleaching (p. 1105). Auxiliary *dare* is an example of this shift. But since auxiliary *dare* does not win out, although it was in competition with main verb *dare to*, Beths regards it as a counterexample to grammaticalization. From my perspective, however, assuming Beth's data are correct, and auxiliary *dare* did die out, 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. We could just think of a "short-lived innovation".

BUT, and this is where the British National Corpus and Manfred Krug's work comes to be very important. Beths (and I should add many other scholars whom Krug 2000 mentions) is wrong in saying that auxiliary form died out. Krug's summary of evidence from the BNC data is as follows (Krug 2000: 200-201):
• in the vast majority of cases *dare* takes the bare, not the *to* infinitive
• the predominant negative strategy is *n't*
• *dares* (third person singular) as opposed to *dare* is never used, and *do*-negation is used for present contexts only four times.

Krug regards *dare* as the most marginal of the "emerging auxiliaries" *going to, got to, want to, have to, need to, ought to* and *dare* (note no *to*) (Krug 2000: 236), but it clearly does not behave exclusively like a main verb. So the conclusion must be that main verb and emerging auxiliary uses have coexisted for over a thousand years, with one type predominating over the other at different periods and in different styles. We have another example of stability, like Case B.

By way of summary the following are not legitimate counterexamples:
Case A, *'s* genitive: this is not well enough understood yet
Case B, Russian instrumental: this is an instance of stability; change is not deterministic
Case C, *-ism,* etc.: these are instances of conversion; they concern word formation, and have nothing to do with grammatical function
Case D, Alemannic verb agreement: there's a misunderstanding about claims about unidirectionality
Case E, *dare*: inadequate data were studied; again we have a case of stability; change is not deterministic

I turn now to some probably legitimate counterexamples.

4. Some probably legitimate counterexamples
CASE F. The development of infinitive *to*

According to Los (1999), and Fischer (2000), the infinitive marker *to* originated in a purposive *to*-PP construction. *To* was on a grammaticalization trajectory during the Middle English period, but by the end of the period it degrammaticalized.

Evidence for grammaticalization is that *to* underwent a variety of changes including:
(16)   a.   strengthening by *for*
        b.   phonetic reduction
        c.   loss of semantic integrity (direction, purposiveness)
                (Fischer 2000: 155)
But toward the end of the Middle English period there was a significant change: the development of the split infinitive, as in
(17)   He wolde shewen the new *to not discorden* fro the old testament.
        'He wanted to show the New Testament to not be in discord with the Old
        Testament' (c. 1380 Wyclif, Prol. Rom 4, 299 [van Gelderen 1993: 60])
It certainly appears that *to* became more independent than it had been. The usual analysis is that it acquired a new function; in generative syntax terms, what had been a complementizer (a non-finite alternative for *that* in subjunctive clauses) became a non-finite tense marker in complementary distribution with modals (Los 1999, refining van Gelderen 1993). As Los points out, there is no coherent way to think of the abstract category T as MORE grammatical than the abstract category C. What we have is a new function. This conforms to (1)--"or grammatical items develop new grammatical functions"--and we might be tempted to stop there, but the particular form that the functional category T takes IS in this case "less grammatical" in some ways than the earlier expression of C because it no longer has to be cliticized to the following V--it is less bonded. I will come back to this in a moment.

First I want to mention that Fischer tries to adduce more than the appearance of the split infinitive structure as evidence for degrammaticalization. She also cites
(18)   a.   absence of "reduction of scope"
        b.   no loss of semantic integrity
                (Fischer 2000: 158)
With respect to (18a), Fischer points out that in Dutch, scope has been reduced in the sense that *te* cannot have syntactic scope over coordinated verbs. Thus the equivalent of

English "to wash and clean your hair" is ungrammatical in Dutch, which only allows "to wash and to clean":

(19)   a.   Eng. You can use this shampoo *to wash and clean* your hair.

          b.   Du. *te wassen en je kleren schoonmachen.

               (Fischer 2000: 159)

Here she invokes Lehmann's parameters of grammaticalization (1995[1982]). But there are two problems. One is that scope-reduction, which Lehmann claims is one of the parameters, has been shown not to hold (for example, he predicts that verbal gerunds such as (20a) would by scope reduction give rise to nominal gerunds such as (20b):

(20)   a.   John's constantly reading magazines

          b.   The constant reading of magazines (Lehmann 1995[1982]: 62)

This is false. Gerunds of the type (20b) historically precede those of the type (20a): (Tabor and Traugott 1998: 240-44), so structural scope-reduction is not a good criterion for grammaticalization. Indeed, structural scope reduction as a parameter would preclude the development of complementizers out of a variety of well-known sources like demonstrative pronouns and verbs. The second problem with Fischer's approach to scope is that the failure of a construction to undergo a change in one language or language variety compared to another is no evidence for degrammaticalization, only of less grammaticalization than in the other variety.

      With respect to (18b), absence of loss of semantic integrity, Fischer proposes that "*to* went back to its original meaning, again strongly expressing goal or direction" (Fischer 2000: 158). But then she points out that it is not really the original meaning: in Early Modern English it serves primarily tense shift, not subjunctive functions as in OE and ME. There does not seem to be any need to think of reversal to an older directional meaning if Los is right that the reanalysis of *to* took place only in the context of certain types of verbs. Rather, what we may be finding is not a backward development, but an extension of what till then was a dormant use. This is actually suggested by Fischer and Rosenbach (2000: 32-33, ft. 9, referring to Stein Forthcoming) in their introduction to the volume in which Fischer's paper appears.

      Let us now return to the bonding issue. Increased bonding (or erasure of morphological boundaries, "univerbation") is said to be one of the hallmarks of grammaticalization not only in Lehmann's but most especially in Joan Bybee and her colleagues' research on cross-linguistic universals of grammaticalization (e.g. Bybee, Perkins and Pagliuca 1994). Decrease in bonding is often claimed to be indisputable evidence for counterexamples to grammaticalization. I have suggested elsewhere (e.g. Traugott 1995, Tabor and Traugott 1998) that bonding is actually a weak criterion in grammaticalization, since it may be overridden by other factors, such as preemption of forms to do grammatical work which is associated with increased syntactic scope, such as the development of complementizers or discourse markers. However, in this case we do not seem to have that. T is hardly wider in scope than C, so we probably do need to regard this as a clear case of decliticization, as Los does.

      Having said that, I do think that if we take a look at the larger context of the change, there is a motivation for the decliticization, not one involving scope, but one involving changes ongoing throughout the syntax of Middle English. Los contextualizes the change in the larger shifts that Middle English syntax was undergoing: loss of OV order, loss of V2 order, most especially loss of subjunctive morphology and replacement of the subjunctive functions by independent forms (the modals). Furthermore, she distinguishes *to* in various constructions, so that the story is not about *to* alone, but about finely articulated shifts in the context of different classes of verbs, as well as of major ongoing changes in the grammatical system. I will return to this shortly.

The examples I have adduced all involve the schematic idealizations that we find in claims of the type:

(21)   a.   lexical item in specific context > grammatical item
         b.   clitic > inflection

None actually involves reversal of the history of the individual morphemes ("grams") in question, though as I pointed out, Fischer suggested that to reverted *to* its original meaning. I turn now to one example which HAS been alleged to reverse its functional status (Case G).

CASE G. Pennsylvania German *wotte*

One proposed counterexample, Case G, is from the domain of modals--the development in Pennsylvania German of the rounded form *wotte* of the preterite subjunctive *welle* 'would < wanted' into a main verb 'wish, desire' (Burridge 1998). This case does appear to be a genuine counterexample to grammaticalization. At the same time it appears to be an instance of preemption for the ideological purposes of the community: Burridge proposes that it is a kind of euphemism or avoidance of expressing a wish too bluntly, in other words a case of hypercorrection (overuse of an item considered to be socially or stylistically salient) is of this kind, as is discussed at length in Janda (2001).

CASE I. Exaptation

There is a class of changes that deserves special attention. They have been called instances of "exaptation" by Lass (1990) and "regrammaticalization" by Greenberg (1991). Lass's concept of exaptation involves the reuse of what has become totally marginal ("junk") in a new grammatical context (but others, e.g. Vincent 1982, Giacalone Ramat 1998, and Norde 2001) have pointed out that in many cases the reused material is not truly marginal. Greenberg's idea of "regrammaticalization" does not require "junk". His examples include the desemanticization of numeral classifiers and articles and fossilization or absorption into other morphemes, followed by reuse in a different productive paradigm, in Chibchan, Salish and other languages.

Greenberg also mentions the Indo-European *-sk-* which was twice reused. Originally a suffix for forming present tense, sometimes with iterative value, it was first reused as an inchoative in Latin (see *pallesco* 'grow pale') and then in French as an affix that "allows to fix the stress for the whole paradigm in a position after the stem, which remains unstressed" (see *je finis/nous finissons*) (Giacalone Ramat 1998: 110-111).

In these kinds of examples of exaptation we certainly have recruitment of old material to new grammatical function. What these cases have in common and is specific to them is that individual morphemes have become relatively unanalyzable, or have lost connectedness with other members of their class and have opportunistically and idiosyncratically been reused.

Interestingly, Norde has suggested that we might think of forms that a) are residues of earlier morphology, b) change their status and get new functions in the context of wider morphosyntactic changes, also as cases of exaptation. In particular, she suggests that changes in the *-s* genitive (Case A) are examples of exaptation in the context of loss of inflexion and word order change. From this perspective we might also think of the development of the *to*-infinitive as a case of exaptation in the same contexts.

I think this is a very fruitful line of thinking to pursue.

In summary, the best counterexamples would be
• those in which a form has "reverted to (a reflex of) its starting point" (Janda 2001: 287), as *wotte* appears to have done. Janda (1995) has suggested a partially similar kind of development for Regional Spanish. In this language 1st plural verb inflectional suffix -*mos* was reanalyzed as a first person subject clitic *-nos*. But we don't have nearly as

detailed textual analysis of these cases as we do for *dare*, and the analyses may have to be modified later..
• those in which an item which has undergone grammaticalization is used for a new grammatical function, but becomes freer than it was, while not serving a function that is normally associated with wide scope (syntactic and semantic). *To* is a good example.
It is probable that some of the most important counterexamples will turn out to be instances of exaptation.

5. SOME ISSUES TO BE MINDFUL OF
The evidence is overwhelming that a vast number of known instances of the development of grammatical structures involved the development of a lexical item or construction through discourse use into a grammatical item, which could then be developed into a new grammatical item (new in function or in form). Developments need always to be considered in terms of the range of uses that a particular item can have, so that we are sure to trace local changes in relevant contexts (e.g. we need to think of the development of *to* in terms of a variety of contexts, as Los does with respect to infinitive *to*). Ideally we need a three-dimensional model of change.
In all cases we need to mindful of several things.
• *Regular vs. idiosyncratic changes*. One is that the strength of examples and counterexamples lies not only with the theory but also with careful and thorough analysis of the data, and where possible of the social contexts in which it arose, though these are often hard to determine for earlier periods.
We also need to remember Andersen's point that schemas are ways of organizing data in idealized ways. Most of the cases of well agreed upon grammaticalization in the sense I have adopted show a regularly replicated correlation of a formal schema, e.g. lexical item or construction > functional category, clitic > affix, etc., with a semantic one: SPACE > TIME, CONDITIONAL > CONCESSIVE.
In contrast, counterexamples appear not only to violate the formal directional schema, but also to be idiosyncratic with respect to semantics.
• *The role of contact.* Another thing we need to be mindful of is that our analyses have been conducted largely in the context of putative homogeneous developments. When we look at contact situations (as Middle English surely is), complications arise. This is particularly clear in the creole literature. In the early days of creole studies it used to be thought that grammaticalization could be identified in allegedly internal changes undergone by new form-function pairs that arose in the creole situation (another instance of creationist thinking; see DeGraff 2000 for attempts to show that creolization is not unique, and shows nothing special about the bioprogram hypothesis as suggested by e.g. Bickerton 1984). In many cases such putative new developments may actually be evidenced in the prior histories of donor languages (via layering).
For example Sankoff and Laberge (1973) suggested that Tok Pisin adverbial *baimbai* > future *bai* originated in English *by-and-by*, but Keesing (1991) showed that there was already a model in Melanesian Pidgin and purely internal development was unlikely. Bruyn (1996) discusses several instances of development in Sranan Creole, including the use of *gi* 'give' as a benefactive, which appear to have originated in the donor languages, and therefore must be regarded not as instances of grammaticalization within the creole, but of transfer from earlier stages.
• *Reasons for unidirectionality*. Hasplemath rightly asks the very interesting question WHY grammaticalization is (in his terms) irreversible. In answer he invokes a version of Keller (1994)'s maxims (which in turn are based on Grice 1989[1975]), together with invisible-hand processes leading to the adoption and spread of the new meanings. The maxims are:

(22)   1. Hypermaxim: talk in such a way that you are socially successful, at the lowest
                           possible cost.
        2. Clarity: talk in such a way that you are understood.
        3. Economy: talk like others talk.
        4. Extravagance: talk in such a way that you are noticed.
                           (Haspelmath 1999: 1055)
Like Grice's Maxims, most especially his two Quantity Maxims, Haspelmath's maxims are
clearly in competition, most especially Economy and Extravagance. Haspelmath admits
that "extravagance" must be understood "only in a generalized sense", since
grammaticalization typically arises out of lexical items with very general meanings, like
HAVE, BE, THING, or very familiar meanings, like body part terms (BELLY, BACK).
The maxims in (22) are useful for semantic change in general. There does not seem to be
anything about them that would help us understand why grammaticalization involves
recruitment to functional categories, i.e. to the overt marking of the architecture of
language.
        "Extravagance" surely invokes the wrong notions. One thinks of hyperbole, of
punning, e.g. ads like
(23)   a. "Shooting Stars" (heading of an article on Linda McCartney's' photos of the
           rock groups of the 60's, New Yorker Sept. 2001)
or of metaphorical purple prose like:
(23)   b.   "his career is seen as a particularly alarming example of how what began as
         house-grown grievances have metastasized into a fury of global dimensions"
                           (NY Times, Sept. 24th 2001).
I am the first to agree that simply invoking "competing motivations", "expressiveness"
(Lehmann 1995[1982]) or even pragmatic enrichment (Hopper and Traugott 1993:
Chapter 4), while necessary, is not sufficient, at least for early stages of grammaticalization.
But (22) does not seem to get us much further. We need in addition some attention to
another very general type of semantic change: the tendency to recruit meanings to serve
metatextual purposes. This is not unique to grammaticalization, of course, nor it is
sufficient, but it is an essential part of the mix. It is not unique because development of
metatextual meaning occurs in the lexicon too, cf. *pay tribute to*, or the development of
speech act verbs, e.g. use of *recognize, find* as speech act and even performative verbs.
Nevertheless I would suggest that it is an essential ingredient without which
grammaticalization cannot occur.
• *Lexicalization.* Finally, it is clear that we need to understand far more about the
relationships between grammaticalization and lexicalization (see e.g. Luraghi 1998,
Brinton In Press).
        In any event, I hope I have encouraged you to search for more counterexamples to
the unidirectionality hypothesis, and at the same to be careful to make explicit and to
evaluate the assumptions that are brought to bear on the analysis.

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