

## Why can't we talk to each other? ☆

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Received 14 September 1998

### 1. The functionalist Chomskyan

Frederick Newmeyer is perhaps the only Chomskyan linguist who is at the same time a functionalist. This has allowed him to accomplish the remarkable feat of writing *Language form and language function*, a book that examines the functionalist orientation in linguistics from a fairly orthodox Chomskyan<sup>1</sup> perspective. This unprecedented event opens up new possibilities for a dialog across the gulf of mutual incomprehension that separates the two orientations, in several ways: First, Newmeyer takes great pains to explain the fundamental differences between the two orientations, thus helping to understand why functionalists and Chomskyan take such different approaches and can hardly talk to each other at the level of their everyday research. Second, he adopts a moderate position that accepts at least some of the results of functionalist research as valid and shows by writing the book in the first place that he does not dismiss the functionalist approach out of hand, that he deems it at least worthy of criticism. Third, he criticizes many individual theoretical proposals and research strategies adopted by functionalists from a Chomskyan perspective and thereby gives functionalists the chance of sharpening their claims and focusing their own critique of the Chomskyan approach. (Chomskyan, of course, often get the benefit of outsiders' critical attention.) And fourth, the book contains high-quality summaries of functionalist views, thereby providing an excellent intro-

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\* Review of: Frederick J. Newmeyer, 1998. *Language form and language function*. Cambridge, MA: MIT Press, xii, 428 pp.

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<sup>1</sup> Newmeyer's terminology contrasts the functionalist orientation to the *generative* orientation (p. 9). I will mostly use *Chomskyan* for the latter because linguists working within this tradition seem to be held together primarily by the overwhelming influence of Chomsky's ideas, whereas the term *generative* is opaque and puzzling to outsiders.

duction to functionalist thinking for readers who are reluctant to go to the original literature. Of course, since Newmeyer's overall evaluation of functionalist positions is more negative than positive, I would not (being a functionalist myself) recommend this as a beginner's introduction, but in general the descriptive part is amazingly accurate, and the whole book is totally free of polemic and a model of rhetorical fairness.

Thus, Newmeyer has done a big service to the field of linguistics, which certainly does not profit from its current schism, so difficult to explain to non-linguists. The book shows that it is indeed possible to be a Chomskyan and a functionalist (of sorts) simultaneously. But it thereby leaves open the question in the title of this review article: Why can't we talk to each other? If the Chomskyan approach is fundamentally compatible with the functionalist approach, why are there so few practicing linguists who combine them in their work? It may be that ultimately we need a book called *The sociopsychology of linguistics* to answer that question fully, but I think that even at the level of the philosophy of science we can say more than Newmeyer does in his book. My main claim in this review article is that Chomskians and functionalists differ not so much in their basic *assumptions* (as Newmeyer makes it appear), but in their basic *goals*: *Chomskians primarily want to characterize the innate faculty underlying grammatical competence, whereas functionalists want to explain why language is the way it is.* Newmeyer completely fails to see this difference in underlying goals, and as a result his book leaves important questions unanswered.

After the introductory chapter (pp. 1–21), Newmeyer devotes two chapters to the most general issues. In Ch. 2 (pp. 23–94), he raises the question of autonomy and argues that contrary to functionalist claims, language is indeed autonomous in three different senses. In Ch. 3 (pp. 95–164), Newmeyer discusses the nature of linguistic explanation and argues that both internal (i.e. 'formal') and external (i.e. 'functional') explanations must be recognized as valid. The three remaining chapters are devoted to three topics in the study of grammar on which functionalists and Chomskians have taken very different perspectives: the nature of word classes ('syntactic categories') (Ch. 4, pp. 165–223), which functionalists have claimed to have fuzzy boundaries and a prototype structure; grammaticalization (Ch. 5, pp. 225–295), a type of language change that functionalists have claimed defies treatment by autonomous linguistics; and world-wide typology (Ch. 6, pp. 297–364), which has often been important in functionalist argumentation. The book ends with a short conclusion (pp. 365–369).

## 2. The pseudo-issue of autonomy

Functionalists have sometimes claimed that the difference between Chomskyan and functionalist linguistics is that the latter rejects the autonomy of language (or grammar, or syntax). One may wonder why they find it necessary to characterize their approach in negative terms, but it cannot be denied that a statement such as the following is not atypical:

“All functionalists subscribe to at least one fundamental assumption *sine qua non*, the non-autonomy postulate: that language (and grammar) can be neither described nor explained adequately as an autonomous system.” (Givón, 1995: xv)

But what exactly is autonomy? Croft (1995) has shown that there are different kinds and different degrees of autonomy, and that it is not always easy to tell immediately what kind of autonomy claim is made. Newmeyer spends all of Ch. 2 discussing three different autonomy hypotheses, which are to some extent independent of each other, and defends them against attacks by functionalists. (1a–c) are reproduced from his pp. 23–24.

(1) a. *The autonomy of syntax* (AUTOSYN):

Human cognition embodies a system whose primitive terms are nonsemantic and nondiscourse-derived syntactic elements and whose principles of combination make no reference to system-external factors.

b. *The autonomy of knowledge of language with respect to use of language* (AUTOKNOW):

Knowledge of language (‘competence’) can and should be characterized independently of language use (‘performance’) and the social, cognitive, and communicative factors contributing to use.

c. *The autonomy of grammar as a cognitive system* (AUTOGRAM):

Human cognition embodies a system whose primitive terms are structural elements particular to language and whose principles of combination make no reference to system-external factors.

Of these three autonomy theses, we should focus on the first, because according to Newmeyer, only a few extreme functionalists reject the latter two. In particular, he finds evidence in Givón’s, Langacker’s and Lakoff’s writings for AUTOGRAM, and the only functionalist approach cited as rejecting AUTOKNOW is Paul Hopper’s “emergent grammar”. But of course Hopper’s papers on emergent grammar are far too sketchy to be taken seriously as a theory that can be refuted. And it is not clear to me whether even Hopper’s extreme statements can be taken as rejecting AUTOKNOW:

“[Speaking is more a matter of] remembering procedures and things than ... following rules. It is a question of possessing a repertoire of strategies for building discourses and reaching into memory in order to improvise and assemble them.” (Hopper, 1987: 145; cited by Newmeyer, p. 59)

This passage can be interpreted as distinguishing clearly between knowledge of language (‘procedures’, ‘things’, ‘possessing a repertoire of strategies’, ‘memory’) and use of language (‘remembering’, ‘building discourses’, ‘reaching into (memory)’, ‘improvise’, ‘assemble’). Newmeyer notes that in order to refute AUTOKNOW, “one would have to show that grammatical structure is created ‘on the fly’, as it were, with whatever systematic properties grammars might possess emerging from the dynamics of the communicative interaction” (p. 72). But then there could be no differences in grammatical conventions, i.e. no cross-linguistic grammatical differ-

ences. So one wonders why Newmeyer devotes so much space to justifying AUTOKNOW.

So we are left with the autonomy of syntax as distinguishing Chomskyans from functionalists. Newmeyer adopts from Croft (1995) the distinction between three degrees of strength of AUTOSYN (p. 28):

- (2) a. At least some elements of syntax are arbitrary (ARBITRARINESS).
- b. The arbitrary elements participate in a system (SYSTEMATICITY).
- c. That system is self-contained (SELF-CONTAINEDNESS).

Newmeyer acknowledges that (2a) is too weak in that it is accepted by virtually all functionalists, and (2c) is too strong in that many generativists posit a model of grammar in which there are systematic links between syntax and semantics (for instance, thematic roles may play a role in syntactic analyses). Thus, Newmeyer settles on (2b) as distinguishing between Chomskyans and functionalists.

But what exactly is systematicity? Newmeyer makes no attempt to define this notion and limits himself to giving three examples of ‘structural systems’ in English grammar: inverted auxiliaries, wh-movement occurring in different constructions, and lexical government accounting for the ‘*that*-trace phenomenon’ (\**Who do you think that saw Fay?*). The latter counts as an argument only if the elaborate government-binding machinery of the 1980s is correct, and of course few generativists are confident that it will turn out to be correct in the end (in fact, most Chomskyans seem to have abandoned the theory that Newmeyer is discussing). Subject-auxiliary inversion and wh-constructions are better examples in that the significant generalizations are relatively straightforward. If they represent what Newmeyer means by systematicity, it is difficult indeed to deny systematicity.<sup>2</sup> And in fact, Croft (1995: 496) claims that “those functionalists who accept arbitrariness also accept systematicity”. So at the end of this discussion, we happily note that mainstream Chomskyans and mainstream functionalists agree in accepting AUTOGRAM, AUTOKNOW, and AUTOSYN in the sense of systematicity. Why, then, isn’t the next West Coast Conference on Formal Linguistics held at UC Santa Barbara?

I conclude that autonomy is a pseudo-issue. The fundamental difference is in the respective goals of functionalist and Chomskyan linguistics. If one’s goal is to explain why language is the way it is, then considerations of language use have to be taken into account, because regularities of language use can lead to regularities of language structure through grammaticalization, as Newmeyer acknowledges (p. 58). Thus, functionalists can accept AUTOKNOW only as formulated in (1b), not in the stronger form (3).

- (3) Competence should be *studied* independently of performance.

<sup>2</sup> An even simpler demonstration of the correctness of AUTOSYN in the sense of systematicity comes from non-semantic morphological elements such as inflection classes, stems and thematic extensions (cf. Aronoff, 1994), whose existence has never been doubted by anyone (only sometimes ignored). However, Newmeyer does not discuss morphology at all.

If one's goal is system-internal description (or 'characterization'), then one should ignore performance and its influence on the system, but if one's goal is to understand the system, then one cannot ignore performance. Since functionalists have observed generativists practicing (3), their negative reaction could be interpreted as rejecting (1b), although they in fact accept it.

Similarly, although Newmeyer states AUTOSYN as in (1a), Chomskyans have generally practiced a much stronger version, roughly as in (4).

(4) As many facts as possible should be described as a system whose primitive terms are nonsemantic and nondiscourse-derived syntactic elements.

Again, functionalists have reacted negatively to (4) as observed in generative practice, not to AUTOSYN as formulated in Newmeyer's theory. The reason for adopting (4) as a practical research strategy is that Chomskyans understandably want to extend their inherently narrow methodology to as many facts as possible. Newmeyer in passing notes this tendency to overapply arbitrary syntax,<sup>3</sup> but he fails to draw the correct conclusion from it.

### 3. On explanation

#### 3.1. *Types of explanation and types of explananda*

It is in Ch. 3 on explanation in linguistics that Newmeyer finally reveals himself as a functionalist by accepting both system-internal and system-external (or 'functional') explanations as valid and interesting. This is the chapter that those linguists who want to build bridges across the functionalist-Chomskyan divide should read first of all. But again, by focusing on the philosophical common ground shared by the two approaches, Newmeyer misses the chance of explaining why there is so little practical common ground. He claims that it is an oversimplification to associate Chomskyan linguistics with internal and functionalist linguistics with external explanation, because functionalists also practice internal explanation and Chomskyans also practice external explanation (namely, attributing principles of grammar to innateness). Unfortunately, this has little relevance in practice: Functionalists find their own internal explanations trivial, and the Chomskyan appeal to innateness is not particularly interesting either, because nothing about the nature of basic grammatical principles seems to follow from their being innate (except their universality, but functional principles are also universal).

The most serious omission from Newmeyer's discussion is the possibility that the explananda of functionalist and Chomskyan linguistics differ fundamentally. He merely notes that Chomskyans are interested in explaining acceptability judgments,

<sup>3</sup> "There is a widespread belief among many of those who study grammar-discourse interactions that generative syntacticians are much too quick to ascribe to a *syntactic* principle the deviance of some sentence type, when a pragmatically oriented one would be more adequate." (p. 65)

whereas some functionalists focus exclusively on naturally occurring data. Now it seems to me that many of the differences between generative and functionalist linguistics can be explained easily once we realize that the basic explanandum for functionalists is the structure of language(s), whereas the basic explanandum for Chomskyans is the possibility of effortless language acquisition despite the poverty of the stimulus (this point is also made in Kirby, 1999: 14–15). The latter is grandly called “Plato’s Problem” in Chomsky (1986), suggesting that it is Chomsky’s central concern in the study of language. The main explanans that solves Plato’s problem for Chomsky is of course the innateness of the principles of Universal Grammar. This is stated quite clearly by Hoekstra and Kooij (1988: 45):

“[T]he explanation of so-called language universals constitutes only a derivative goal of generative theory. The primary explanandum is the uniformity of acquisition of a rich and structured grammar on the basis of varied, degenerate, random and non-structured experience ... This situation contrasts sharply with the one found in [functionalist theories]. The explananda for these theories are the language universals themselves.” (1988: 45)

The primary task of generative linguistics is then that of ‘characterizing’, i.e. describing, the principles of Universal Grammar through in-depth studies of individual languages. To be sure, this activity can be called ‘explanation’ as well, inasmuch as every general statement that subsumes a more particular statement can be called an explanation. But the recognition that Plato’s Problem is the central explanandum of Chomskyan linguistics helps us understand why Chomskyans generally ignore functional explanations in their work. This would be puzzling if their central goal were that of explaining language structure, because it is evident that many properties of language structure can be explained only functionally (e.g. the fact that roots are generally longer than affixes, or that no language has the three-vowel system  $i - e - \text{æ}$ ). If, however, describing the innate principles of grammar is one’s primary goal, then ignoring all functional explanations makes some sense: Regularities that are functionally motivated are not due to UG, so they fall outside the scope of generative linguistics. Newmeyer seems to have missed this point in his book. In Ch. 6, he repeatedly talks about “facts that a theory of grammar has to explain” (e.g. p. 335). In most Chomskyans’ parlance, ‘theory of grammar’ is equated with ‘theory of innate UG’, and from this point of view there can be no facts that such a theory *has* to explain. Any facts that a theory of UG happens not to explain are thus automatically irrelevant to generative linguistics, and vice versa (but cf. Newmeyer, 1999, where he does make this point).

In a personal communication (29 April 1999), Newmeyer writes, commenting on an earlier version of this review article: “If you asked the vast majority of generativists what their goal is, they would answer the same way as your functionalist: to explain why language is the way it is”. He thinks that the search for an innate UG is of prime importance only to Chomsky and a few philosophically minded followers like Hoekstra and Kooij. He even claims in the book that “the generative research program would not have to budge one centimeter (p. 89)” if it turned out that the principles of generative grammar can be learned inductively and need not be innate. This seems to betray a rather simple-minded view of science, in which the ideologi-

cal context plays almost no role in the scientists' day-to-day work. Perhaps it is true that ordinary generativists are also interested in why language is the way it is, but apparently the social context requires them to look only for those kinds of generalizations that can reasonably be conceived of as being part of an innate UG. Thus, Chomsky's goals have an overwhelming influence on generative work, if perhaps only indirectly.

The view that functionalists and Chomskyans differ in their fundamental goals may strike the reader as overly pessimistic, but if it were generally recognized, it would help to avoid many misunderstandings. Ideally, of course, we would arrive at a division of labor: Some linguists specialize in studying the innate properties of grammars, and others study those properties of grammars that are due to functional factors. In practice, however, this is difficult, because the boundaries of the two research domains are not given in advance (and they anyway overlap). As a result, each of the two orientations practices a kind of 'imperialism', trying to extend their domain as far as possible, and almost certainly overextending it.

### 3.2. *Kinds of functional explanations*

As one might expect, the functionalist approach that Newmeyer describes in greatest detail and regards most favorably is Hawkins's (1994) parsing theory of word order universals. Since Hawkins adopts a lot of the terminology and notation of generative linguistics, his theory has not found many followers among mainstream functionalists, but Newmeyer correctly identifies it as thoroughly functionalist. He is more skeptical regarding functional explanations based on iconicity, information flow, text frequency, and economy. While his discussion of iconicity and information flow is fairly thorough (and here I share much of his skepticism), he devotes far too little attention to frequency and economy. As regards frequency, Newmeyer claims that the markedness asymmetries in morphology that were derived from frequency asymmetries by Greenberg (1966) are not directly connected to frequency, but to other causes such as ease of production and relative usefulness (pp. 134–135). Every functionalist will readily agree with Newmeyer that "[i]t is not frequency per se that makes singular morphemes shorter than plural morphemes. Rather it is some principle involving ease of production that makes frequent items shorter than infrequent ones" (p. 134). But one can go even further: The shortness of frequent items is not due to ease of production per se but to a diachronic shortening process caused by some speakers' deviating from earlier norms, based on their assessment that reduced articulation suffices because of the higher predictability of frequent items. Functionalists are well aware that 'frequency' is only a shorthand expression for a more complex set of events. Another way in which frequency is relevant to the explanation of language structure is in causing differential entrenchment: Highly frequent linguistic units are entrenched to a greater degree, with important consequences for their structure (cf. Bybee, 1985). For instance, more frequent items are resistant to loss not because they are more useful (as Newmeyer would have it, p. 135), but because they are more entrenched. If syntax is described as a network of constructions rather than as a set of rules, then constructions show-

ing different frequencies will be entrenched to different degrees, again with consequences for their structural properties (cf. Bybee and Thompson, 1997). Newmeyer finds it difficult to conceive of syntax in terms of frequency-sensitive constructions: “Each time [a] sentence is uttered, do the speaker and hearer really tick off in their mental note pads one more use of each [of the constructions it instantiates]?” (p. 135). The answer is yes, and the difficulty in conceiving of syntax in this way seems to be due exclusively to the long habit of thinking of syntax in a very different way. But it is not only Newmeyer’s fault that he has not properly addressed the role of frequency and economy in functional explanations, because functionalists have tended to focus more on less tangible notions such as iconicity and metaphor, leaving economy accounts insufficiently explicated.

### *3.3. Functional explanation is not description*

Although Newmeyer makes a strong effort to distinguish properly between external and internal explanation, he himself seems to confuse the two occasionally. In generative linguistics, all interesting generalizations must be somehow incorporated into the descriptive apparatus because all grammatical explanation is system-internal. But functionalist linguistics is different: Grammatical description can be redundant and unrevealing, because the interesting generalizations are primarily found outside of the system, in performance, diachrony, etc. Newmeyer does not seem to realize this: He argues that functionalist competing-motivations analyses are problematic because the fact that external forces are incompatible with each other makes it hopeless to think of grammatical descriptions as incorporating external functional forces.<sup>4</sup> He sees an “implicit assumption in much functionalist work that an optimal grammatical description specifies direct linkages between the formal properties of language and the external forces that are responsible for them” (pp. 140–141). However, I see no reason why functionalists should claim that the ‘external forces’ play a role in grammatical description, and I am not aware of functionalists who have made such a claim explicitly. It is only on the Chomskyan view that one is forced to incorporate revealing generalizations directly into the grammatical description.

## **4. The nature of word classes**

Chapters 4 and 5 are entirely devoted to arguing against particular claims in the functionalist literature, so Chomskyan readers need not fear any challenges from Newmeyer here. But functionalists are given the benefit of detailed criticism of their positions, part of which is justified in my view and should be addressed by them. At the very least, Newmeyer forces functionalists to be more precise in the future.

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<sup>4</sup> One wonders whether this objection would also apply to Optimality Theory, which Newmeyer does not mention in this connection. In OT, competing constraints are used for language-particular description by ranking the constraints differently for different languages, but in contrast to functionalists, OT practitioners assume that the constraints are directly innate.

Ch. 4 deals with word classes (or ‘syntactic categories’), which functionalists have argued possess a prototype structure and/or are separated only by fuzzy boundaries. Newmeyer defends the classical view of word classes, arguing that they have discrete boundaries, are not organized around central ‘best cases’, and are not definable notionally (against Langacker’s (1987) well-known proposal). Since normally Chomskyans simply assume the discreteness and non-prototypicality of word classes, this discussion is very welcome. Unfortunately, however, Newmeyer makes no attempt to explain why views on the nature of word classes should correlate closely with the functionalist/Chomskyan division, so that the chapter is not connected well to the earlier chapters. As far as I can see, views on the nature of word classes are conceptually independent of views on the autonomy of syntax and the relative weight of internal and external explanation. In principle, fuzzy categories could be incorporated into Chomsky’s Minimalist Program, and there is no a priori reason why competing functional motivations should not regulate the behavior of purely syntactic, discrete categories. But there is undeniably a strong correlation between views on the two sets of questions, and an understanding of the reasons for this correlation helps us understand the nature of the Chomskyan/functionalist split.

The explanation for this correlation that I favor is that the different views on word-class discreteness derive from the relative weight that is accorded to language-particular description in both orientations. Since formal descriptions are not considered the primary locus of explanation by functionalists, they can afford to hold views on word classes that are difficult to apply in practice. Newmeyer suggests that J.R. Ross’s proposal for formalizing “squishes”, the assignment of a rating between 0 and 1, “struck many linguists as arbitrary and unmotivated” (p. 171). But of course this is a simple and straightforward formalization of the basic idea, which Ross had shown to be well motivated. The real problem with this formalization is that it is relatively unwieldy and greatly complicates the practical task of describing grammatical structures (and teaching all this to students). But descriptions (or internal explanations) are prestigious in Chomskyan linguistics, so Chomskyans preferred to ignore the evidence for fuzzy categories.

The insight that the nature of word classes is much more complex than traditionally assumed implies a whole new research agenda, and linguists have hardly begun to explore the limits of fuzziness and prototypicality. Too often one can get away with the impression that these notions are invoked simply to avoid precision in the argumentation. So functionalists should take up Newmeyer’s challenge and investigate more comprehensively the nondiscrete nature of word classes. Clearly Newmeyer is right in observing (p. 193) that it is insufficient to simply invoke prototypicality in cases where highly complex factors are at work, as in the case of the English preposed possessor, which Taylor (1989) had claimed is possible mainly with prototypical nouns (*the teacher’s house*, *?the table’s surface*, *??the sky’s color*). But his extensive discussion of Croft’s (1991) theory of word class prototypes is somewhat beside the point, because this is a typological theory of the markedness properties of word classes that makes predictions about the cross-linguistic behavior of nouns, verbs and adjectives in different functional contexts. Newmeyer accepts Croft’s functional explanation (“We should not be surprised that

the grammar-lexicon interface is organized to facilitate [speakers' doing what they find useful]", p. 177), but he objects to the claim that word classes are mentally represented as prototypes (p. 178), although Croft is quite noncommittal about the mental representation in particular languages.

While claims of prototypicality need to be made more precise before they can convince the skeptics, I find the evidence for fuzziness of word classes much more cogent. This is particularly the case for functional categories that have only recently become grammaticalized from lexical categories, e.g. (quasi-)prepositions like *because of*, *in spite of*, *on top of*, *on account of*, *according to*, *concerning*, *considering*, or (quasi-)conjunctions like *while*, *albeit*, *suppose*, *provided that*, *granted that*, *on condition that*. In such cases it is impossible to draw a clear-cut line between nouns/verbs and prepositions/conjunctions. Descriptive grammars have long recognized this by introducing terms such as 'prepositional locution', or 'secondary preposition', and these are no easier to delimit than the major word classes. The most straightforward theoretical solution is to assume a noun-preposition (or verb-proposition, etc.) continuum, although this does not immediately solve the descriptive grammarian's practical problems. Newmeyer discusses three cases in which categorial continua have been posited, J.R. Ross's clause-NP continuum ('the nouniness squish'), Ross's claim that *near* is intermediate between an adjective and a preposition, and Greville Corbett's adjective-noun continuum for Russian numerals. Only for the first does Newmeyer have a reasonably convincing alternative story. For *near*, he admits that its pronominal use (*the near shore*) is somewhat archaic and that the comparative inflection of the preposition (*The gas station is nearer the supermarket than the bank*) is unexplained, but he still insists that *near* is both a (somewhat idiosyncratic) adjective and a (somewhat idiosyncratic) preposition. It is difficult to avoid the impression that this conclusion is based more on an unrecognized prejudice against fuzzy categories than on the facts. In the case of Russian numerals, Newmeyer appeals to diachronic reanalysis (although this is of course irrelevant for the synchronic description) and to a not very elegant account by Leonard Babby that seems to get the facts right, but is no serious competitor for Corbett's beautiful description in terms of a continuum. When Newmeyer has trouble explaining away the examples of categorial continua that he has chosen himself, one wonders how he would deal with the countless others that can be found in the literature (though he must be given credit for choosing examples that are not very easy for him).

That Newmeyer's rejection of categorial continua is not much more than a prejudice with a time-honored tradition is also clear in his discussion of grammaticalization and reanalysis in ch. 5. He argues against my claim that reanalysis and grammaticalization are two separate phenomena (Haspelmath, 1998) and defends the traditional view that one of the aspects of a grammaticalization change is a reanalysis (p. 244–248). For instance, when a demonstrative is grammaticalized into a definite article, its categorial structure is changed (e.g. from A to DET). Against this traditional view, I have argued that if a categorial continuum (e.g. an adjective-determiner continuum in this case) is assumed, then there is no need to posit an abrupt reanalysis. But Newmeyer seems to be unable even to conceive of a gradual change in word class:

“Indeed, the very *definition* of grammaticalization seems to imply a reanalysis, since we say that grammaticalization has taken place only if there has been a downgrading, that is a reanalysis, from a structure with a lesser degree of grammatical function to one with a higher degree.” (p. 244)

But this is only so if reanalysis is equated with categorial change *per se*, contrary to the traditional definition of reanalysis as the assignment of a new structural description made possible by surface ambiguity. The traditional concept of reanalysis presupposes that the new structural description is one that was possible in the earlier grammar as well, whereas categorial changes in grammaticalization can lead to truly new categories (e.g. a slight shift toward the determiner pole on the adjective-determiner continuum). Like most Chomskyans, Newmeyer seems to conceive of word classes as a small pre-established (i.e. innate) set of discrete categories (N, V, A, P, Q, D, etc.) from which languages (and language learners) have to choose. But if word classes are not pre-established and not innate, then there are many more possibilities. The typological literature on word classes (see Comrie and Vogel (to appear) for a recent sample) has provided ample evidence that the traditional view is a gross oversimplification and does not come close to doing justice to the great variety of phenomena found in the world’s languages.

## 5. Double standards

In several passages of Newmeyer’s book, he applies double standards to functionalist and Chomskyan linguistics, i.e. he criticizes functionalists for not meeting standards of methodological purity that no linguist would be able to meet. These criticisms are often very pertinent and useful, but they convey the erroneous impression that there is something specifically wrong with functionalist approaches.

A case in point is Newmeyer’s pessimistic assessment of the prospects for explaining grammatical patterns in terms of competing functional motivations: “It seems utterly hopeless, given the potentially open-ended number of factors that might plausibly be in competition with each other ... to ‘explain’ the full set of grammatical properties of the world’s languages in terms of the full set of competing motivations” (p. 153). I would be more hopeful, but in any event Newmeyer gives no reasons to believe that prospects are better for explaining the full set of properties of the world’s languages in terms of the full set of parametrized principles. Clearly, such maximalist demands are not very helpful in assessing the merits of rivaling approaches.

Another case that Newmeyer devotes much more space to is the study of cross-linguistic generalizations in grammar, or ‘language typology’ (Ch. 6). He mentions a number of well-known methodological problems in language typology, such as the selection of a representative sample, the quality of data from secondary sources and the identification of grammatical elements across languages. Again, his tone is very pessimistic in this discussion, while he does not seem to be worried about analogous methodological problems in the study of individual languages, such as the unnaturalness of artificially constructed examples, the representativeness of corpus data, the

frequent lack of agreement in grammaticality judgments, and so on. Newmeyer's concerns would be more understandable if typologists ignored these problems (as often seems to be the case in Chomskyan syntax, where textbooks generally don't discuss methodology in any detail), but in fact typologists worry about them a great deal and are constantly trying to improve their methodological tools.

Curiously, it is only on the last few pages of Ch. 6 that Newmeyer observes that all the 'difficulties' of language typology are difficulties for the Chomskyan approach to grammar as well, because cross-linguistic comparison has become crucial for this orientation as well after Chomsky's 'second conceptual shift' (even though Chomskians generally don't refer to cross-linguistic studies as 'typology', for purely historical reasons). Newmeyer discusses a few generative works (by Rizzi, Safir, and Kayne) that have a somewhat broader cross-linguistic orientation, showing that their sweeping claims do not seem to be supported by the cross-linguistic evidence. But he does not point out that the most prestigious type of study throughout the 1980s and 1990s has been the 'micro-typological' study which compares only a few languages at a time (often just English and the author's native language) and postulates a parametrized principle to account for the differences. However, these hypothesized principles and parameters invariably make (implicit) claims about many other languages as well, which are generally not put to the test. I find it difficult to see how this procedure is in any way more responsible methodologically than broad-based world-wide studies with all their problems. And yet few people would deny the value of these micro-typological studies, even though their results are usually obsolete as soon as a few more languages are considered.

But in fact, the results of language typology are not nearly as problematic as Newmeyer suggests. The two main reasons for my confidence in typology are as follows. First, poor data in a typological study have the same effect as noise in the transmission of a radio signal. If there is too much noise, we should find a random distribution of features, yielding no pattern. So if despite all the difficulties we do detect significant correlations, this in itself proves that the quality of the data is sufficient, because otherwise we would have seen no pattern.

And second, if Newmeyer were right that (macro-)typology is on shaky foundations, we would expect many premature generalizations to be subject to revision after a deeper investigation. And to be sure, there are some generalizations that have been revised (e.g. the correlation of adjective-noun order with object-verb and genitive-noun order, cf. Dryer, 1988), but such revisions have been much less common than in generative micro-typological studies. For instance, one might have expected Greenberg's (1963) universals, which were based on a biased convenience sample of 30 languages, to be totally obsolete by now. But in fact, most of them have stood the test of time amazingly well. What typically happens in world-wide typology is that somebody proposes a universal on the basis of a few dozen languages, and once the universal has become more widely known, individual counterexamples turn up here and there. But usually they do not threaten the generalization as a statistical universal, and functionalists have no problem with statistical universals (see Dryer, 1997 for arguments that they are actually to be preferred over exceptionless universals).

On the contrary, the existence of exceptions shows that a universal is not due to some innate inviolable constraint and thus points to the need of a functional explanation of the universal.

## 6. The challenge of grammaticalization

In Ch. 5, Newmeyer discusses grammaticalization and the claims that functionalists have made about it. On the one hand, his goal is apologetic: showing that the phenomena cited in grammaticalization research do not pose a threat to the Chomskyan program:

“[I]t cannot be denied that generative grammar per se has had little to contribute to an understanding of most of the historical changes that fall under the rubric of grammaticalization. In particular, the semantic changes and phonetic reductions are explained by theories of meaning and processing respectively, not by the set of theoretical conceptions identified with the generative enterprise ... But crucially, there is nothing that we find in grammaticalization that is incompatible with any well established principle of generative grammar.” (p. 292)

As we have already seen that there are no unbridgeable differences in the basic assumptions (cf. Section 2 above), Newmeyer’s position is not surprising, and I see no reason to contest it. Again, the difference lies in the research strategies, not in the basic assumptions. Chomskyans generally adopt the strategy of studying competence in isolation from performance, and synchrony in isolation from diachrony. This strategy seems counterproductive to functionalists, and this feeling is reflected in functionalist statements such as the following: “[T]he study of grammaticalization challenges the concept of a sharp divide between *langue* and *parole* ...” (Traugott and König, 1991: 189). But to be fair to Newmeyer, vague statements like this are all too common in the functionalist literature, and they invite Newmeyer’s interpretation. In any event, functionalists will be gratified to see that Newmeyer has read the grammaticalization literature so carefully, has accepted many of the individual proposals and even provides his own functional explanation for the rarity of degrammaticalization (pp. 275–278).

On the other hand, Chomskyans who are skeptical of grammaticalization studies will happily note the title of the chapter (‘Deconstructing grammaticalization’) and may nod in agreement over Newmeyer’s more aggressive passages. His main points of criticism are (i) that grammaticalization is not a distinct process, (ii) that the unidirectionality claim is false, (iii) that grammaticalization is not a theory, and (iv) that reconstructed forms should not be used as evidence for grammaticalization. The latter two are not particularly serious. Against the term ‘grammaticalization theory’, Newmeyer objects that what is really meant is “the set of independently-needed theories that are relevant to the explanation of the phenomena” (p. 240). I think he is right here, and the term ‘grammaticalization theory’ is not unlike ‘evolutionary theory’ in this respect. This usage was perhaps prompted by the enormous prestige in contemporary linguistics of the word ‘theory’, whose inflationary use is also found in generative linguistics (cf. ‘theta theory’, ‘binding theory’, etc.). As far as the use

of reconstructed forms as evidence is concerned, Newmeyer's methodological scruples strike me as exaggerated. I trust that everybody knows that reconstructed forms can only serve as weak evidence supporting a claim, and can never serve as counterexamples to a claim. Grammaticalization is so ubiquitous and attested examples are so plentiful that it would not be necessary to use reconstructed forms for illustration, and most examples could easily be replaced by attested cases. Newmeyer suggests that 'mainstream historical linguistics' does not use reconstructed forms as evidence (p. 279), but this is simply not true (cf. Harris and Campbell, 1995, who are critical of grammaticalization studies, but make extensive use of reconstructed data in their argumentation).<sup>5</sup>

Newmeyer's main criticism of grammaticalization studies is that they portray grammaticalization as a 'distinct process', i.e. "an encapsulated phenomenon", "driven by a distinct set of principles governing the phenomenon alone" (pp. 233–234). He argues that this is not the case, and that there is nothing special about grammaticalization: It is simply a kind of change that happens to involve 'downgrading reanalysis', phonetic reduction and semantic bleaching (or whatever semantic change one associated with grammaticalization). Each of these three kinds of change, Newmeyer notes, are attested in non-grammaticalization changes as well. But these observations do not contradict any claims that have been made by students of grammaticalization: Nobody has ever claimed that grammaticalization is a phenomenon "governed by its own set of laws" (p. 234). Postulating special "grammaticalization laws" would be completely against the spirit of the functionalist orientation, and nobody will be embarrassed to learn from Newmeyer that grammaticalization is an "epiphenomenon". On the contrary, it would be truly astonishing if anyone claimed seriously that there are "autonomous diachronic processes" that are not epiphenomenal of something else.<sup>6</sup>

So this part of Newmeyer's critique is based on a misunderstanding of what grammaticalization is claimed to be. And it is not difficult to see how this misunderstanding arose: Grammaticalization changes represent the most salient case of a pervasive regularity of language change, and regularities of change are quite puzzling for the Chomskyan orientation. In this perspective, the most natural way to conceive of language change is as "essentially a random 'walk' through the space of possible parameter settings" (Battye and Roberts, 1995: 11), so when somebody claims that

<sup>5</sup> In this context it is surprising, to put it mildly, to see that Newmeyer himself makes liberal use of reconstructed forms in his argument against unidirectionality (e.g. Anatolian subject clitics, Indo-European indefinite clitics, Semitic case markers). The problem is that these cases are presented as counterexamples to a well-motivated generalization, not merely as illustrations of a rampant phenomenon. Moreover, the case of the Semitic case markers is not merely reconstructed, but wildly speculative (Orin Gensler, p.c.), and hence totally useless.

<sup>6</sup> Newmeyer accuses functionalists of conceiving of language change as processes independent of the minds and behaviors of speakers (p. 238–239), as betrayed by metaphors such as grammatical forms 'traveling along developmental pathways', 'bleaching', 'being fossilized', and so on. Maybe such colorful terminology can confuse some naive readers, but I trust that all linguists who use these terms are aware that they are not more than metaphors – just like the movement and deletion metaphors in Chomskyan linguistics that nobody would take literally.

there are pervasive regularities of change, it is difficult to see how that could be without a special set of autonomous forces that cause the change.<sup>7</sup> However, as theorists of change such as Lüdtke (1980), Keller (1994) and Croft (to appear) have shown, regularities of language change can be derived from the complex interplay of various general constraints on performance. An explanation of grammaticalization in this general perspective is offered by Haspelmath (to appear). But Newmeyer's error is not only due to his own Chomskyan perspective: His attempt at criticizing grammaticalization studies was not made easy by the current literature on the topic, which too often remains at the fairly shallow level of grammatical forms and too rarely tries to link observed changes directly to cognitive and social events.

Newmeyer devotes many pages to listing counterexamples to the claim that degrammaticalization does not exist (also called the 'unidirectionality hypothesis'). The larger purpose of this enterprise is not quite clear, though Newmeyer relates it to his basic point that grammaticalization is not a distinct process: If grammaticalization were a distinct process, its unidirectionality would be as unremarkable as the irreversibility of aging, geological erosion, and biological evolution (p. 262).<sup>8</sup> Counterexamples to strict irreversibility have been noted in the grammaticalization literature, but they have not been seen as affecting the main generalization that grammatical changes are overwhelmingly in the direction of a greater degree of grammaticalization. After reading Newmeyer's section on degrammaticalization, I see no reason to revise that assessment. Some of his examples are all too familiar from the earlier literature (e.g. English/Scandinavian genitive *s*), some are based on uncertain reconstructions (cf. footnote 5), some are erroneous,<sup>9</sup> some have nothing to do with degrammaticalization,<sup>10</sup> and some are valid newly discovered exceptions. In the absence of reliable quantification, it does not make much sense to quarrel whether one feels that degrammaticalization is "rampant" (p. 263) or extremely rare, whether grammaticalization occurs "at least ten times as often" (p. 275) as degrammaticalization or rather a hundred times as often (as I would estimate). What counts is that there is a significant skewing that needs to be explained, and this is not contested by Newmeyer. In fact, he provides a functional explanation of his own for

<sup>7</sup> Similarly, Lightfoot's (1999) main point is that there are no autonomous principles causing grammatical change, and he attributes such a view to functionalists. See Haspelmath (1999b) for detailed discussion of Lightfoot's position.

<sup>8</sup> I frankly do not see the point of subsuming as diverse phenomena as aging (a programmed process), erosion (due to the simple addition of many near-identical events) and evolution (a complex interaction of differential replication and environmental selection) under the same rubric of 'distinct natural process'. Certainly there are reversible 'natural processes', e.g. the movement of sand dunes, the melting of ice, and many sound changes (both [o] > [a] and [a] > [o] have been found to occur).

<sup>9</sup> For instance, the replacement of English *hastow*, *wiltow* by *hast thou*, *wilt thou* (p. 270) simply represents the loss of the cliticized postverbal pronoun construction, not its decliticization. The constructions *hast thou*, *wilt thou* were presumably always available.

<sup>10</sup> For instance, the derivation of a verb *to down* from the 'preposition' *down* in English (p. 273) is perhaps remarkable in that a function word serves as the basis for a verbal derivative, but it is not degrammaticalization: (De)grammaticalization changes always involve a construction whose identity is preserved. In transpositional (i.e. word-class-changing) derivation, a stem is inserted in a completely different (morphological) construction.

“why unidirectionality is *almost* true” (p. 275), in terms of the principle of least effort:

*“Less effort is required on the part of the speaker to produce an affix than a full form ... All other things being equal, a child confronted with the option of reanalyzing a verb as an auxiliary or reanalyzing an auxiliary as a verb will choose the former.”* (p. 276)

This particular explanation does not work, because there is no known mechanism by which the production effort in performance might have an influence on the child’s analytical decisions in building up her competence. But as Haspelmath (to appear) observes, functionalists have not made much of an effort to explain the asymmetry in directionality, and I certainly do not want to fault Newmeyer for not being a good enough functionalist. The point is that Newmeyer’s critical remarks by no means amount to a ‘de(con)struction’ of grammaticalization, but can to a large extent be seen as useful sympathetic comments on an important strand of research, despite the occasional rhetoric that suggests the opposite. In the last section of the chapter, Newmeyer agrees with Haspelmath (1998) that the only serious generative treatment of grammaticalization, Roberts (1993), clearly falls short of providing a story that can supersede the functionalist accounts.

What Newmeyer has not appreciated at all, unfortunately, is the explanatory potential of grammaticalization changes, i.e. the possibility that many grammatical structures can to a large extent be understood through the regularities (and the accidents) of diachronic change. One passage is particularly telling in this respect: “... only a small percentage of auxiliaries and other functional category members are known to have originated as verbs and other lexical category members” (p. 259). This is strictly speaking true, but it is also true that 100% of the function words and the overwhelming majority of affixes whose origin is known have originated as lexical category members. Thus, it is a fair guess that almost all grammatical items have ultimately arisen through grammaticalization, and there are countless examples of synchronically puzzling behavior that can be diachronically illuminated.

## **7. Typology, functional explanation and formal description**

As in the case of nondiscrete word classes and grammaticalization, in Ch. 6 Newmeyer notes the correlation between broad typological interests and the functionalist orientation, but he makes no attempt to explain it. Again, a plausible hypothesis is that the focus on internal explanation makes it difficult for Chomskyan linguists to make the idealizations that are necessary for world-wide cross-linguistic studies. As has often been noted, such broad cross-linguistic work is necessarily shallow, but these losses in depth are compensated by corresponding gains in breadth. While Chomskysians insist on the necessity of idealizations (e.g. with respect to the homogeneity of the speech community), they have found it difficult to accept the idealizations made in cross-linguistic studies.

A standard argument against broad cross-linguistic studies with their necessarily superficial treatment of individual languages is that in the absence of in-depth stud-

ies of these languages, one cannot be sure that the comparison is valid because similar surface structures might turn out to be quite different at a higher level of abstraction. The implicit assumption is that the abstract structures arrived at by the generative method not only serve to express the relevant generalizations, but are also closer to the truth. Newmeyer states this quite bluntly (in the context of a discussion of the formal description of indirect objects):

“[A]ny external function-based account of why particular structures cross-linguistically carry the semantic and/or pragmatic functions associated with indirect objects had better have a compelling characterization of just what those structures *are*. In other words, formal analysis precedes functional explanation.” (p. 341)

But this view is naive. For one thing, forty years of Chomskyan linguistics should have taught us that it is unlikely that we will ever arrive at a consensus of what *the* correct formal analysis of a structure is. There are just too many conceivable possibilities, and to a large extent, whatever consensus has existed has been due more to the social prestige of the analyst (in many cases, Chomsky himself) than to the compelling nature of the arguments in favor of the analysis. But even more importantly, if functional explanations play the important role that Newmeyer accords to them, then not only does formal analysis precede functional explanation, but also vice versa, functional explanation precedes formal analysis. Arguments in favor of certain formal analyses invariably take the form that a generalization would be missed if a different analysis were adopted. But if the generalization is due to external functional factors, then there is no need to state it in the grammar. Consider the example of article-possessor complementarity, i.e. the ungrammaticality of *\*the Joan's book/\*the my book*. This has been explained since Bloomfield's days by a structural position 'determiner' that can only be filled once, by the article or by the possessor. Now Haspelmath (1999a) provides an economy-based functional explanation for article-possessor complementarity and points out that many languages show article-possessor complementarity also when the two elements do not compete for the same slot (e.g. Hebrew, where the possessor is postposed and the article is preposed). If this functional explanation is correct, a large part of the motivation for the determiner analysis in English disappears. Thus, it is not possible to arrive at the correct formal description without a prior cross-linguistic and functional analysis. Newmeyer has completely overlooked this consequence of accepting functional explanations. Eventually, his program would have far-reaching consequences for Chomskyan syntax as it exists now. Recall that Newmeyer accepts Hawkins's (1994) explanation of word order regularities in terms of parsing efficiency. But to a large extent, the edifice of assumptions in contemporary Chomskyan syntax rests on generalizations concerning word order. If Hawkins is really right, then much of the empirical motivation of government-binding theory and subsequent developments disappears (cf. also Hawkins's (1999) parsing explanation of extraction restrictions, another central topic of Chomskyan theory).

Furthermore, Newmeyer suggests that “deep grammatical analysis” helps solve the problem of identifying the same categories across languages (p. 342–343). Practicing typologists are acutely aware of this as a serious practical problem, and the

usual solution is to apply semantic/functional criteria. Newmeyer objects that semantic criteria should not be used to identify syntactic elements, but again, his proposed solution is fairly naive. He suggests that “linguistic theory itself provides the means for identification” (p. 342), so for instance subjects are identified as the highest argument position in VP at D-structure, which may or may not raise to a higher structural position at S-structure. In this sense, too, deep grammatical analysis is a prerequisite to cross-linguistic comparability. But Newmeyer does not ask how VPs, NPs and arguments are identified in the first place: In the absence of cross-linguistically invariable formal features, nouns and verbs cannot be identified across languages without recourse to semantic criteria (cf. Croft, 1991). And it seems extremely unlikely that researchers working on diverse languages would have come up with the VP-internal subject hypothesis independently. More realistically, generative grammar works as follows: A prestigious analysis is developed for a particular language (generally English). Then someone identifies a comparable phenomenon in another language on the basis of semantic or intuitive criteria, and proposes a formal analysis in similar terms. The analysis only uses purely formal categories, so the impression can arise that it was arrived at completely on language-internal grounds and without using semantic criteria. But this is an illusion. Newmeyer’s discussion thus indirectly illustrates the fact that generative syntacticians, unlike functionalist typologists, have consistently ignored the methodological and theoretical problem of identifying categories across languages.

## **8. Conclusion**

In the concluding chapter, Newmeyer reasserts his main point, that the generative and functionalist approaches are both valuable, and that linguists of both orientations should pay attention to what the other side is doing. But will the book be useful in furthering more mutual comprehension? It might, especially if readers restrict themselves to the first two chapters, on autonomy and explanation. But the last three chapters, on discrete categories, grammaticalization and typology, are so negative in their evaluation of the functionalist approach that few Chomskyans will bother to read them, and most functionalists will get away with the impression that Newmeyer’s main purpose is functionalist-bashing. And indeed, to a large extent these latter chapters appear like lists of problems that Newmeyer sees in functionalist work from his perspective. He does not confine himself to defending Chomskyan linguistics against attacks by functionalists, but goes on to make critical remarks on all kinds of topics (such as unidirectionality, the use of reconstructed forms as evidence, the sampling problem and the data problem in typology) that have little to do with the fundamental issues. As a result, the book is less coherent and integrated than it could have been. The remarks that are critical of generative grammar are sporadic and mostly hidden away in the last few pages of the typology chapter, so I fear that the book will not be seen as a major challenge in Chomskyan circles.

By emphasizing the common ground of the two orientations, Newmeyer misses the chance of explaining why the gulf is so huge in practice. Maybe that was not

among his goals in the first place, but I feel that it should have been. Or perhaps he thinks that his chapter on autonomy is sufficient to explain the practical differences, but I have given my reasons for thinking that Chomskyans and functionalists differ primarily in their goals, not in their basic assumptions. I see no other way of explaining why Chomskyans are so consistent in ignoring functional explanations, even when their validity is evident.

One of the most disappointing aspects of the book is that Newmeyer has not realized that the discreteness of word classes that he defends in ch. 4 is in no way intrinsically connected to the generative enterprise. The generally unquestioned assumption of discrete categories in Chomskyan linguistics seems to be based on a prejudice inherited from a venerable tradition. There is nothing wrong in principle with prejudices (or unquestioned assumptions). It is impossible to question all assumptions simultaneously, so we must work with some unquestioned assumptions. But ideally we should be aware of our prejudices, and especially a philosophically oriented book should make them explicit (even if they turn out to be correct, as Newmeyer argues for discrete categories).

Despite these various criticisms, I still regard *Language form and language function* as an excellent book, and I recommend it to every linguist. Newmeyer's argumentation is very clear, and the mix of philosophical passages and concrete data and analyses should make it attractive to a wide audience. Newmeyer discusses many important foundational issues in linguistics which will be with us for a long time to come, even though many of them are not currently fashionable. The range of his topics and the references cited is simply stunning, and Newmeyer does not sacrifice accuracy for breadth.

A final point: Newmeyer admits that he has omitted phonology from consideration, although "[e]very major point of difference between the two camps with respect to the analysis of syntactic structure has a homologue in the analysis of phonological structure" (p. xi). However, the gulf between functionally and formally oriented approaches in phonology seems to be not nearly as wide as in syntax. The description of a phonology workshop at a recent GLOW (Generative Linguistics in the Old World) conference (Potsdam, 1999) reads as follows:

"The workshop is broadly concerned with the relevance of articulatory and perceptual facts for phonological theory. More specifically, it intends to focus on such questions as the extent to which functional factors determine phonological grammars, the status of the distinction between phonological representation and phonetic implementation, the issue of multiple (articulation-based as well as perception-based) phonological representations, and the universality and 'groundedness' of phonological constraints."

So is phonology different? Perhaps the current polarization in syntax has to do with the fact that formalist syntax is still dominated by Chomsky, a radical anti-functionalist,<sup>11</sup> whereas generative phonology has no such unique leading figure. So I am hopeful that the ultimate reason for the current schism in syntax is to be found in the historian's, not the philosopher's realm.

<sup>11</sup> See Newmeyer (1998) for a critique of Chomsky's anti-functionalist stance on the issue of the biological evolution of language.

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