Is there life beyond generative syntax? [Review of: M. Everaert, H. van Riemsdijk (2006) The Blackwell companion to syntax]

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Link to publication

Is there life beyond generative syntax? Considering the study of syntax from a diachronic and semantic-pragmatic point of view

Review article of *The Blackwell Companion to Syntax. Vols I-V* (Blackwell Handbooks in Linguistics), edited by Martin Everaert and Henk van Riemsdijk, with the help of Rob Goedemans and Bart Hollebrandse. Oxford: Blackwell 2006, 3999 pp.

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This is the twenty-first volume in the valuable Blackwell Companion series of Handbooks in Linguistics, which since the late nineties has offered us insight into the state of the art of a wide range of subdisciplines within linguistics.[1] Most of these volumes are heavy tomes (usually about 800 to 900 pages), but this one caps them all with close to 4000 pages spread over five volumes (there is, however, some overlap, because the recommendations, contents, preface, index and to some extent the references are repeated in each volume). This enormous quantity is perhaps not surprising given that the study of syntax has been very much in the limelight since the Chomskyan 'revolution' in the 1960s, which shifted the centre of gravity in linguistics from the historical and mostly descriptive study of grammar – with language output or the 'performance' level firmly in a central position – to the development of a theoretical model, in which the speaker stands central; a model, in other words, in which the 'machinery' or the generation of sentences (i.e. 'competence') came to be seen as the object of study rather than the output itself. This, in turn, led to a strictly synchronic perspective being taken in grammar research.

This change in perspective was important and useful at the time because it linked the study of linguistics to other scientific domains such as psychology, neurology, cognitive science etc., and to all the recent new interdisciplinary research connected with the workings of the brain. It has created renewed interest in language and has resulted in a whole range of theoretical linguistic models being developed, some of which were variants on the generative model, such as Lexical Functional Grammar (cf. Vincent 2001), Head-driven Phrase Structure Grammar (cf. Pollard and Sag 1994), and the more recent Jackendoff (2002) model, while others represented a reaction to the strictly formal model of generative grammar, resulting in the creation of

functional models, such as Functional Grammar (cf. Dik/Hengeveld 1997), Cognitive Grammar (cf. Langacker 1987), Construction Grammar (cf. Goldberg 1995, Croft and Cruse 2004), Grammaticalization theory (cf. Hopper and Traugott 2003) and usage-based or memory-based models such as present in Hopper's (1998[1987]) Emergent Grammar, Tomasello (2003) and in the more formal analogical model of Skousen (1989, 1992).

The shift in attention also caused new interest in historical linguistics and language change, which up to then had been studied mainly from a phonetic-phonological and morphological point of view, and it revived the topic of language evolution, which had been virtually forbidden since the famous ban imposed on it by the *Société de Linguistique de Paris* in 1866. Thus, from the early 1970s, linguists such as Traugott (1972) and Lightfoot (1974, 1979) used the generative model to gain more insight into syntactic change, and the idea grew (cf. Kiparsky 1968) that a better understanding of change would also throw light on the synchronic system of grammar (including universal grammar) itself. Indeed, in the last few decades, the areas of synchronic grammar, language acquisition and language change have been studied more and more in relation to each other because it was realized that they all must make use of the same or similar mechanisms, which are ultimately all connected to the workings of the brain and our individual minds.

Compared to all other handbooks in the series, this multi-volume is set up quite differently. The previous issues were all divided up into coherent parts or sections, each dealing with a specific sub-area within the discipline (i.e. its history, its methodology/ies, the various existing theories, the analyses of the actual data etc.). Each one of these earlier volumes also contained a helpful and usually quite lengthy introduction setting the scene (and often the separate sections additionally had their own introductions). The present volume, on the other hand, in spite of its length, contains only a very brief preface of five pages, while its contents are presented in a strictly alphabetical order simply based on the titles of the papers. One would have expected in a companion volume such as this that the various syntactic theories current today would have been presented first, and it would have been useful to have had an introductory chapter discussing the different perspectives taken, and the merits of and agreements and disagreements between existing syntactic models.

Instead, what we get is a handbook that deals almost exclusively with generative syntactic research, making one wonder if this is the only existing model worth writing about. In addition all the contributors, with very few exceptions, are strongly allied to the generative school, and have not published about or worked with other models. Most of their publications can be found in generatively oriented journals such as *Linguistic Inquiry* or are connected with

the centre of generative publications, i.e. MIT. The synchronic, generative slant is also noticeable from the way other linguistic theories and linguists of other persuasions or from other domains are treated, or rather, I am sad to say, are not treated. A quick scan through the name and subject index shows that there are next to no references to the well-known theories already mentioned above, or to well-known functional and/or historical syntacticians such as Joan Bybee, John Haiman, Bernd Heine, Paul Hopper, Otto Jespersen, Frans Plank, Sandra Thompson, Elizabeth Traugott, not even to generative historical linguists such as Paul Kiparsky, Anthony Kroch and David Lightfoot (with the exception of what the latter have written from a purely synchronic point of view). Also topics which are important in other models, both synchronic and diachronic, such as grammaticalization, pragmatics, variation, frequency, analogy, iconicity, context, sociolinguistics, psycholinguistics etc. are not discussed or mentioned only in passing.

Strangely enough, this rather severe restriction to the generative model is only implicitly indicated in the preface and in the blurb, and likewise in the recommendations repeated in each volume and on the Blackwell webpage, almost as if it is taken for granted that syntax *is* generative syntax. Why the volume is sailing under false colours, and why its title is not simply *Generative Syntax* is a mystery. It would certainly have provided a much clearer indication of its contents, especially to linguists of different persuasions, who may not have any preconceived notions as to what constitutes 'syntax'.

The purely alphabetical set-up of the papers results in the unfortunate circumstance that related topics often do not appear together (especially if the titles themselves are rather fanciful) so that, for instance, contributions on adjectives and adjectival verbal constructions appear a long way apart (chapters 2, 3 and 48); similarly, clitics are dealt with in chapters 13, 14 and 67, ellipsis in both chapter 22 and 60, and topicalization or focus phenomena and related types of movement can be found in chapters 26, 37, 46, 62, 65 and 73. If there had been coherent sections, and proper introductions to each one of them, the relation between similar papers could have been made clear, and these introductions could also have pointed to further issues more indirectly related to the main one under discussion, or to papers where other properties of a category under discussion (e.g. properties of DP, VP etc.) are described.

One of the intentions of the volume is to offer the beginning linguist (which to my mind should include the interested, non-generative, outsider) a way through the labyrinth of publications in generative syntax because

[t]he data and analyses that at some point in the history of generative grammar played an important, sometimes even a crucial, role have a tendency to fade into the background rather quickly. This effect is particularly strong when the piece of theorizing they helped to establish becomes obsolete, but it is even true when the theoretical insight persists over time. As time went on, and as generative grammar … expanded to dozens of countries [and] hundreds of universities … , the muckheap of once useful but then discarded empirical material, continued to grow. Old hands in the field may still be served by a good memory …, but successive generations of young linguists simply don't have access to the wealth of data, generalizations, and analyses that might be terribly important to their research if only they knew about them (p. xix).

The use of the word 'muckheap' to refer to empirical material is telling; I will come back to that below.

The overall aim of the publication is given on p. xx. The idea is to produce

a well-delimited empirical area (both as to the construction(s) involved and as to the languages in which the phenomena are found) whose analysis has, at one time or another in the history of generative grammar, played an important role in the theoretical debates at that time [italics as in original].

Overall, give or take a paper or two which seem rather marginally relevant (e.g. chapter 7 on grammaticalized questions of the type *Does he like or not like this book* ─ the grammaticalization involves the reduplication of the first syllable of the main verb ─, which seem to occur only in Mandarin Chinese and to a limited extent in Singapore Teochew), it seems to me that this aim has been achieved – in spite of its restriction to what are considered important *generative* debates – in the sense that the most important and relevant topics and controversies are represented, so that the volume not only serves generative linguists but also offers food for thought to linguists with wider interests. What is indeed very useful is to be given an overview of the various descriptions and explanations that have been offered for the 'behaviour' of all the particular structures and categories under discussion, and most chapters offer useful historical summaries in this respect.

Another aim is to present the empirical material which forms the basis for the theorizing, next to the theoretical issues themselves, because "the potentially useful empirical material" (p. xx) is often flimsily treated in standard textbooks and is often "not part of the knowledge that

researchers and students in the field may be expected to possess" (*ibid*.). And even if it is treated, the titles of the articles

> are often unrevealing as far as the empirical domain is concerned, keywords are notoriously absent or unreliable, relevant data are scattered over several articles, and, most importantly, the presentation of the data in these articles is subservient to the theoretical aims that are made and hence not presented in succinct, didactically useful ways (p. xx).

I think that in this respect, the present volume could also have done better. Here too, not all titles are immediately clear, at least not to linguists who are not already well-read in generative theory. Some titles contain terms or pieces of jargon that are not transparent, such as, for example, the titles of chapter 7, 'A-not-A questions' or chapter 60, 'Sluicing' (which turns out to be a form of ellipsis). Some generative terms, of course, like 'preposition stranding' and 'pied piping' have become common knowledge in the course of time (maybe also because they are creative and transparent), but in cases such as 'sluicing', a glossary of terms would definitely have been helpful. In more ways than one, such a glossary would have been welcome because not every linguist would immediately know what Binding Rule B is (Chapter 55: 102) or the Complex NP Constraint (Chapter 11: 334). When it comes to clarity of presentation, in this handbook too, relevant or connected data are "scattered over several articles", which could have been solved (see above) by coherent sections with introductions; and as far as keywords is concerned, I found the index in places rather succinct; it does not contain all the references and subjects one would have expected to be indexed.[2]

Although the editors emphasize the importance of data and analyses (because "linguistics is a thoroughly empirical science", p. xix), they attach still greater importance to the fact that these data and analyses provide "tools to help us understand the structure and properties of the human language faculty" (*ibid*.). I agree. But in order to understand these properties we need good empirical underpinning, and, as I hope to show in the course of this review, this is not always provided. The data used are often the product of introspection, of the researcher's own intuitions about his or her language. Papers quite often present examples as if they are regular structures when they are, in fact, rare in natural language use, or are of a complexity that makes them difficult to understand or process in real time (which indeed makes one wonder whether

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[2] For instance the names of linguists are not always taken up. At a very cursory glance I missed references to, for instance, Bolinger and Pintzuk. Also, all the names which are referred to only in the footnotes seem to have been left out. Similarly, in the subject index I missed, among other things, quite a few of the references to pragmatics, and to the *that*-trace phenomenon.

they are ever used in reality). Hardly any of the papers make use of data drawn from corpora (even though there are now many large-scale corpora available, and even Google can be effectively used as a search machine, especially for the search of language utterances close to the spoken medium, cf. Meyer *et al.* 2003), nor do they take cognizance of the frequency or infrequency of a particular construction, even though it is well-known that frequency plays a crucial role in language acquisition and language change, i.e., in the choice of one variant over another. It seems to me that, generally speaking, only fairly frequently occurring variants should be accounted for *systematically*, because infrequent ones may well operate outside the system of grammar, i.e., they may be lexically isolated, and learned and produced as fixed formulae.[3] The same will be even more true for high-frequency items. It has been shown (cf. Bybee 1995, Bybee and Hopper 2001: 14ff., Corbett et al. 2001) that highly frequent collocations are often no longer analysed as constructions (i.e. produced by rule) but generated as holistic phrases, i.e. they have become lexicalized and opaque from a grammatical point of view. Clearly, the plea once voiced by Fillmore (1992) to combine armchair linguistics with corpus linguistics (because both approaches are necessary) has not been given ear to in these volumes.

The success of another aim of generative syntax, which is an aim of this handbook too, i.e., "to go beyond observational and descriptive adequacy *to reach explanatory bliss*" (p. xix, emphasis added), depends on the empirical grounding, and it should not come as a surprise that the sounder this grounding, the better the explanation will be. One of the problems with respect to the generative approach, however, is the fact that the grounding is so narrow, relying very heavily on the *formal* contours of language. Context, i.e. discourse factors and pragmatics, is largely neglected; semantics has been given a place in the form of θ-theory, but this is rather static and restricted, linked as it is only to the lexicon, and not to the contextual (syntagmatic) situation as well, or to other (paradigmatic) analogous constructions that may influence the choice of arguments (cf. Hampe and Schönefeld 2003, and the approach taken in Construction Grammar). In other words, the socio-cultural factors surrounding language use are neglected, not just within the generative view of how language acquisition takes place but also with respect to research on language change.

In language change external factors play a crucial role, about which it has been stated that it is quite impossible to separate these clearly from internal linguistic factors (cf. Weinreich *et al.* 1968, Gerritsen and Stein 1992). Not taking such external or contextual influences seriously, means that the model also presents a distorted view of what the theory of grammar or the

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[3] According to some linguists a large portion of language consists of formulae, much larger than usually assumed, cf. Wray (1999), Wray and Perkins (2000).

grammar system looks like. In a similar way, children do not learn language in a vacuum; external circumstances aid their learning so that there is no need to accept the generative poverty-of-the-stimulus hypothesis, which postulates an innate universal grammar in order to explain why it is possible that children learn their native language in such a short period of time and in spite of often poor evidence.[4] General studies in language acquisition, which are not bound to a generative framework, show that children build up abstractions or schemas based on concrete linguistic signs used in concrete situations. By bootstrapping and with the help of analogy and general cognitive abilities, they can deduce by further abstraction the patterns of their native language, and thus acquire its conventional grammatical system without the need for an innate grammatical module (cf. e.g. Slobin 1987, Holyoak and Thagard 1995, Clark 2003, Tomasello 2003, Itkonen 2005, Wanner 2006, Fischer 2007).

Although another one of the volume's aims is to explain the existence of variants, the variants that are considered more often than not only present a small amount of the actual variation found. This is first of all because many of the variants that occur are seen as performance errors or 'merely' stylistic variants. Secondly, variants are considered mainly in formal terms, not also in functional terms (as is customary, for instance, in typological studies). Since each linguistic sign or structure has a form as well as a meaning (or communicatory function) (cf. Anttila 2003), it goes without saying that the consideration of variants should involve all structures that resemble the target structure both in their formal and functional aspects.

Variants may arise through borrowing from other dialects or languages, but they may also be products of diachronic change, whereby a particular variant may either lose or gain in frequency in the course of time, due to prestige or to the requirements of the conventional system of grammar that each child learns during language acquisition (i.e., a particular new variant may 'fit' that system with less effort, hence become more frequent, and then may replace an older variant). The way in which variants come and go or replace each other, especially in those cases where the pressure of the conventional system also plays a role, may give us insight into the mechanisms of change, and, given the window that change offers on the system of grammar, it should also give us more insight into this system, and therefore better (i.e., less likely to be circular) explanations than those given strictly within the confines of one model, as is the case here.

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[4] Not everyone agrees that this learning is remarkably fast and based mainly on poor material, see, for instance, Clark (2003), Lombardo Vallauri (2004), and the open peer commentary in *Behavioral and Brain Sciences* 12: 334-75 (1989) and 14: 597-650 (1991), as well as the debates in special issues of *The Linguistic Review* 19.1-2 (2002) and *Studies in Language* 28.3 (2004).

The mechanisms used in change should also be linked up with those used for language learning because it is unlikely that the learner would not use the same mechanism in language all through life. This does not mean that the effect of these mechanisms on the language output itself is the same. Children deduce their schemas and linguistic patterns on a smaller corpus of data, and, naturally, this will affect the outcome of their abstractions. They therefore tend to 'abduce'[5] more often than grown-ups, but many of these abductions will disappear again when their database becomes larger (cf. Bybee and Slobin 1982). Explanations offered purely within the rather narrow formal generative synchronic model, without recourse to evidence coming from diachronic or acquisitional developments, will often turn out to be circular. As Bybee (1988) and Faarlund (1990) have argued, an explanation is only an explanation if reference is made to another domain. In a similar vein, McMahon (2000: 146) writes with reference to biological evolution: "Constraints and general mechanisms, then, can help us delimit what is possible, but to *understand* actual organisms or genomes, 'we have to turn to the historical process that created them, in all its grubby particularity'(…)" (emphasis added).

Where diachronic evidence is neglected, there exists a strong tendency (as I will illustrate in more detail below) to posit (often rather ad-hoc) structural rules or constraints in order to allow or disallow a particular construction, without taking into account the possibility that these structures are left-overs of an earlier system, which may be on their way out, or which may have been preserved in its 'old' form because of its extremely high frequency. As already mentioned above, such items may live on as 'islands', i.e. as morpho-syntactically opaque forms. They may be memorized by the learner as formulae, rather than as the product of some more general rule or constraint.[6] As Derwing (1977), Joseph (1992), and many others have argued, speakers are not perfect linguists, they generalize only locally, i.e., their abstractions and reanalyses may be more opaque and less economical than a generative linguist might hypothesize, who tends to have a more global, and abstract (logical) view of the rules of grammar. Therefore, to posit a rule for such isolated structures would not be very economical from the system's point of view. Since the generative grammar system is supposed to be a biological system (cf. Chomsky *passim*[7], Lightfoot 1999), which is considered to be part of our brain or mind, it will have to take more notice of the way language is actually processed by our brains. The available evidence points

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[5] I.e., abduce from the point of view of the then conventional system.

[6] These are the frayed edges that Sapir (1921: 38) already referred to when he wrote that "all grammars leak".

[7] Chomsky's stance in this has become clearer over time, in *The New York Review of Books* (July 18, 2002, p. 64) he writes: "The long-term goal has been, and remains, to show that contrary to appearances, human languages are basically cast to the same mold, that they are instantiations of the same fixed biological endowment, and that they 'grow in the mind' much like other biological systems, triggered and shaped by experience, but only in restricted ways."

more and more to a certain fluidity in the way we learn, with patterns or schemas being formed analogically and locally (cf. Hofstadter 1995, Gentner *et al.* 2001), and there is also evidence that a large part of language processing is memory-based, making use of fixed formulae and influenced by analogous (neighbouring) signs in paradigmatic categories rather than that use is made of more general abstract types of rules familiar from generative grammar.

An additional problem as to explanation is that the solutions offered in terms of the rules and constraints operating within UG are often as short-lived as the rules themselves (cf. Faarlund 1990, Fischer 2007). Although it is inevitable that the theory and its hypotheses undergo change when new empirical facts are discovered, the danger of creating ad-hoc rules looms larger when the model used is highly abstract and autonomous and not responsible to other domains. As already stated, this may result in circular reasoning.

It may well turn out that the contributions in the volume under discussion that offer the most in terms of description (good examples are chapters 12 and 57 on Chinese *ba* and secondary predicates in Australian languages respectively)[8] will eventually turn out to be the more worthwhile chapters, while those papers that aim at the 'blissful' state of explanation by basing their analyses strictly on the current state of the theory, may turn out to be more ephemeral, and in the end less explanatory. Only those papers that also provide a good empirical description of the facts (of which fortunately there are still quite a few, since one of the aims of the handbook, as we have seen, is to newly provide the data on which previous analyses had been based) will escape becoming outdated.

For the review article for this journal, I was asked to consider especially what the five volumes have to offer the historical linguist. I think that, in spite of my reservations, they may offer a great deal because they provide diachronic syntacticians with ideas and hypotheses that can be tested in the area of language change and which may prove useful analytical tools for a deeper understanding of how language works. Especially interesting are those cases where generative syntacticians see links between existing synchronic structures, which may not be obvious at first sight, and which are explained and linked together in the model by a more abstract rule. In such cases it would be of great interest to find out whether such abstract links can be made more 'real' by looking at the historical developments of the constructions in question. It may turn out in some cases that they go back to a similar historical source, which would solidify the proposal for a synchronic rule binding them together. Many of the papers

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[8] It is interesting to observe that these chapters both deal with non-European languages, to which it is found to be less easy to apply the generative rules to explain their structures (these languages had little influence on the development of the generative model in its early stages), and that they are also more concerned with the pragmatics of the constructions.

included also provide evidence about similar cross-linguistic occurrences of the structures discussed,[9] which show us the possible variations that exist with respect to a certain structure, useful for both historical and typological linguists. The only problem may be that the historical or typological linguist (who are very often researchers making eclectic use of the various theories and models on offer[10]) does not understand all of the theoretical considerations that are offered due to the often jargon-like and theory-internal analyses.

The question the journal editors put to me could also be reversed, however. It might be of interest too to know what other domains, such as corpus linguistics, discourse pragmatics, historical linguistics, language acquisition and evolution etc., may offer to the generative linguist. In what way might these have contributed to a fuller explanation? In the following I have selected a number of chapters that I will look at in more detail. I have selected them on the basis of an index search, i.e. I had a closer look at those chapters that promised to be of interest because they refer to other works or linguists outside the strictly generative school, or which showed an interest in matters important for historical linguists such as grammaticalization (including semantic bleaching, clines), the use of context, and semantic-pragmatic factors. In discussing these chapters, I will especially point out what possible contributions the above-mentioned disciplines could have made in order to come closer to the experience of "explanatory bliss".

Chapter 1 by Jeffrey T. Runner deals with 'The Accusative Plus Infinitive Construction [a.c.i.] in English'. As the title indicates, this paper deals only with English a.c.i's (of the type: *I believe Jean to have arrived*), apart from a minimal reference to French complement types, which only allows the a.c.i. construction when the embedded subject has been extracted by *wh*-movement as in *Le garçon que je croyais être arrivé*. The chapter offers a satisfactory account of how this construction has been dealt with in the history of generative grammar. It discusses the various proposals, i.e., those proposals that consider the NP *Jean* [henceforth the 'accusative NP'] to be semantically a subject but syntactically an object (the Raising-to-object analysis, which is also followed in lexicalist frameworks), thus emphasizing its relation to the 'equivalent' *that*-clause (*I*

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[9] Many papers indeed do this, but by no means all. Some rather parochially refer only to one language, or discuss constructions that seem to be prominent only in one language. This language is often from the Germanic or Romance family. This shows the general bias towards European languages, which may deeply have influenced the form and shape of UG in generative theorizing.

[10] This is because the historical linguist or philologist is first and foremost interested in giving an explanation as to why the surface data have changed and what they mean. S/he is not primarily interested in developing a grammatical model. In order to explain change, s/he needs to take account of all the facts available, the spelling, the variants, the context, the meaning, the style of the text, the age, the social-cultural circumstances etc. Consequently, s/he will make use of both formal and functional models, and consider both internal and external factors.

believe that Jean has arrived), and those that see *Jean* as essentially a subject (The Exceptional Case Marking (ECM) approach, which makes use of S-bar deletion to enable this subject to receive oblique case from the finite verb – because a non-finite verb cannot assign case to its subject).[11] The article concludes that even though the ECM rule made more correct predictions in the older Government and Binding model, the Raising-to-object rule is now considered to be more "consistent with [the] new architecture of the theory" (Chapter 1: 14). This indicates that the state of the theory has a big impact on the description (explanation?) offered.

The analyses suggested are descriptive rather than explanatory. Analysing something as S-bar deletion or Object-raising does not explain the nature of this construction, the variants used (infinitival structures vs. finite *that*-clauses), and the constraints on it (see below). It also does not explain why the construction occurs in English and not in other Germanic languages like Dutch and German, even though the construction was foreign to earlier Germanic. Finally, it does not explain why the same construction with *wh*-movement (as in the French example above), or with NP-movement in the form of a so-called second passive (as in *He was believed/said to be a real gentleman*), is more wide-spread; the latter occurring also with verbs like *say* in English (the regular a.c.i. is not possible after *say*: **They said him to be a real gentleman*), and in Dutch and German. The description offered is not explanatory because the rules suggested for the derivation of the a.c.i. look ad-hoc, their application being merely stipulated, for those verbs that allow it, in the lexicon.

Also rather unsatisfactory is that the 'accusative NP' in fact does have properties that are both object- and subject-like at the same time, in syntactic as well as semantic terms. In that respect the Object-raising rule is more appropriate than the ECM solution. If corpus evidence or naturally occurring examples had been studied more carefully, it would have been clear that there are severe constraints on the use of the a.c.i. construction, in the sense that they only occur when the 'accusative NP' indeed *also* functions as an object of the finite verb. (The reason, of course, why this NP cannot be both object and subject at the same time in a generative framework, is because θ-theory does not allow this.[12]) Bolinger (1967) has shown that the

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[11] This is according to the theory of Case, which stipulates that any phonetically realized NP must have Case (cf. the Case filter, Chomsky 1981:175, and this volume, Chapter 51: 654), and that only finite verbs (or Tense) can assign nominative Case (Chomsky 1981: 50; this volume, Chapter 11: 367). Chomsky notices that there are problems with this in some languages (1981: 140, note 25) but he does not elaborate on it. It is well-known, however, that there are languages that allow double case assignment, e.g. Japanese and some Australian languages (as acknowledged and discussed in the present handbook in chapters 11, 20 and 57 respectively), and see also Plank (1995). Similarly, there are languages that allow a non-finite verb to govern nominative case, e.g. Spanish (cf. Bolinger 1967: 52ff.) and Middle English (cf. Fischer 1988).

[12] Schultze-Berndt (Chapter 57:186) points to double case marking in Australian, but does not discuss how this might be allowed for within a generative model.

acceptability of a.c.i. constructions heavily depends on the presence of a *semantic* relation between the finite verb and the 'accusative NP'. Thus, examples like (1) are fully acceptable and occur regularly, while examples such as (2) are not:

(1) I believe John to be a man of integrity (Bolinger 1967: 48)

We believe these ideas to be constructive and helpful (*ibid*.: 51)

(2) ? I believe the rain to be falling (*ibid*.: 48)

? They believe that judge to accept bribes (*ibid*. 52)

Bolinger argues that there are two factors that play a role in the acceptability of the construction, one semantic and one stylistic: the 'accusative NP' and its infinitival predicate must refer to 'information' (so the above clauses must be interpretable as 'I believe in John', 'I believe in these ideas', cf. '*I believe in the rain', '*I believe in that judge';[13] i.e., the object functions as an argument (receives a θ-role) of the finite verb), and secondly, there must be a certain amount of formality (the clauses have a "legalistic flavor", *ibid*.: 51). Noël (1997: 282), using a corpus-based approach, adds further, discourse-type constraints distinguishing a.c.i.'s from *that*-clauses: "the subjects of the infinitival complements have almost always recently mentioned referents, while the subjects of *that*-complements often introduce unmentioned referents", and secondly, "the infinitival pattern occurs most often in subordinate constructions, whereas the pattern with *that* usually operates independently" (the latter no doubt, to avoid a rather awkward double *that*-clause). None of these facts or variations can be accounted for within the proposed generative rule model.

To understand these constraints, it is also interesting to look at how the a.c.i. construction arose historically in English. It is now generally accepted that the construction did not occur in native Old English (cf. Fischer 1989),[14] where a.c.i.-type of constructions were only allowed after physical perception verbs and causatives and only with bare infinitives (as in *I saw him cross the road, I let her do it*). This was in fact the situation in all the Germanic languages. A number of factors are involved which caused its development in the Middle English period: (1) the influence of Latin,[15] (2) analogical extension (the construction spreads by lexical diffusion from physical perception verbs to mental perception verbs), and (3) a change in word order from

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[13] It is for this reason that the construction occurs most frequently with *be* or some other empty verb, which does not alter the interpretation of the clause as much as fully referential verbs like 'accept', see also below.

[14] It does occur in texts heavily influenced by Latin, which are awkward as translations in all respects, not only with reference to the a.c.i. constructions, cf. Fischer *et al.* (2000: 222ff.)

[15] This factor also explains why the use of the a.c.i. is much more frequent still in more formal discourse than e.g. in the spoken language.

basic SOV to SVO (this paved the way for the construction, see below). It is interesting to note that the first factor was also present in German and Dutch in the Renaissance period, when Latin influence was strongest, and presumably analogical extension (factor 2) may have caused the a.c.i. to develop there too. This accounts for the fact that the Latin-type a.c.i. constructions occur in German and Dutch in this period (cf. Krickau 1877, Fischer 1994). However, after the Renaissance, they disappear again as quickly as they came. This indicates that the word order change may have been the crucial factor in the more permanent position the a.c.i. acquired in English. This idea is further strengthened by the fact that (3), the word order change, did not affect either German or Dutch, which remained basically SOV (for more details, see Fischer *et al.* 2000: 211ff.).

Another detail that is often forgotten is that the a.c.i. constructions in English are most frequent with *be* as an infinitive, and this too can be linked up with its historical development. *Be*-infinitives are also the most frequent verbs found in Renaissance Dutch and German a.c.i.'s (as shown in Fischer 1994). In addition, examples where the 'accusative NP' has been moved into first position (by *wh*-movement or NP-movement, as in the French example above and in second passives respectively) are also more frequent in Renaissance Dutch and German and in the earlier English examples. This is, as shown by Warner (1982), because these structures either resemble already existing small clauses of the type *I believe **him innocent*** (the small clause or secondary predicate is given in bold), or because they avoid the new, and therefore awkward, NP to V surface structure. Warner (1982: 150) refers to this as the strategy of "least salien[cy]" or "minimal alteration"; this is a strategy that often seems to be followed in syntactic change, no doubt to avoid problems in communication (cf. Aitchison 2001: 101-2).

This relation with small clauses (also called 'predicative complements' or 'secondary predicates') is of double interest because, as Schultze-Berndt shows in Chapter 57 of the present volume, a number of Australian languages show double case marking in constructions with a secondary predicate (as in *That man eats food* [***like***] *a horse*, where the secondary predicate, *a horse*, receives a similative marker as well as an agreement marker (Chapter 57: 186). If it can indeed be shown that there is a historical relationship between modern English a.c.i. constructions and small clauses, then there is no reason not to accept a similar analysis for the Australian small clauses and the English a.c.i. constructions for the predicative NP, i.e., there is no reason not to accept double (abstract) case marking for the 'accusative NP' in the a.c.i.[16] If generative grammar wants to be a truly universal system, then such links should be made. It is

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[16] This, of course, would mean that the present Theory of Case (cf. footnote 11) would have to be changed too.

clear, however, as in this case, that these links may not always be synchronically discoverable. This strengthens the idea that historical evidence may help to provide clearer and more insightful explanations. Wanner (2006: 5) indeed questions "the quality of explanation afforded by an approach that sidelines the diachronic phenomena characterizing language".

The next two chapters I will consider in more detail from an 'extra-generative' point of view are Chapter 3 by Daniel Valois entitled 'Adjectives: Order within DP and Attributive APs', and Chapter 48 on '(Past) Participle Agreement' by Adriana Belletti. Both deal with adjectival features. Chapter 3, in spite of its general title, concentrates only on *event* nominals and adjectives, presumably because this aspect has been mostly the subject of discussion in the generative literature. It is, however, somewhat disappointing that adjective position and adjective order in general is not discussed in more detail, especially since there is a lot of literature available on this subject, but indeed mainly outside the generative paradigm. Section 2 in this chapter points to the fact that the pre- or postposition of the adjective may be related to their attributive vs. predicative nature (this difference, as e.g. Vincent (1988) and Fischer (2000, 2006) have shown is related to information structure), while section 3 merely indicates that "adjectives seem to be ordered according to semantic criteria" (p. 62). Both sections are brief, and offer no further discussion of how this should be dealt with in terms of rules, presumably because, within the generative model, it is difficult to account for these particular facts syntactically. Discussion only starts in sections 4, 5 and 6, all dealing with adjective placement in event nominals. Here, Valois discusses parallel constructions relating the position of adjectives in event DPs to that of adverbs in clauses. In accordance with this, derivational rules are suggested which makes this parallelism, believed to be present at a deeper level, explicit.

The question again arises, however, to what extent these parallels are also 'real' for the language learner or the language user, or whether they typically present the kind of global parallels foregrounded in a formal, rule-based type of grammar. The parallel is described ('explained') in terms of (partial) N-movement, i.e., it is suggested that head-movement may take place within the DP so that the DP structure can be linked to clause structure, creating a parallel between the relation of adjective and head noun on the one hand, and adverb and verb on the other. What exactly causes the head movement remains unclear,[17] and possible semantic-pragmatic differences between pre- and postponed adjectives remain unsaid. Of course, if this

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[17] It is stated that such movement or "partial movement" is held to be "responsible for the possibility for an attributive adjective to appear in pre- or postverbal position"; in other words, the introduction of a movement rule *itself* seems to function as an 'explanation' for the choice in position (Chapter 3: 70).

position is dependent, as suggested above, on discourse or information-structural factors,[18] it would be difficult to account for within a strictly sentence-based syntactic model. Another generative solution put forward here to explain the different adjective positions is "to posit two homophonous adjectives that are base-generated in different positions within the DP" (Chapter 3: 77). This of course ignores the fact that the referential meaning of the adjective is the same whether pre- or postponed; in other words, this proposal misses an important (albeit semantic) generalization, and is therefore not a very economical or elegant one.

The more general drawbacks of the generative approach mentioned above in the introductory section are again visible here. First, as already indicated, the rules suggested in this chapter to explain the distribution of adjectives with event nominals come across as rather ad-hoc because the evidence for the parallels between DPs and clauses is not very explicit, while the meaning differences that may exist between pre- and postponed adjectives are not taken into account when it comes to the formulation of rules. In addition, knowledge that can be gained from studies of language change is ignored. The fact that position may become conventionalized historically, whereby remaining instances of an older position may become isolated and henceforth generated as fixed formulae (as happened in English, for instance, with some French adjectives) is not considered, nor the way in which linear order or particular stress patterns may modify the interpretation of the adjective (this would be in accordance with a more general cognitive or iconic factor, cf. Bolinger 1952).[19]

Secondly, there is no corpus back-up as far as the use of data is concerned. On p. 68, a number of examples are given that should support the parallel syntax that has been suggested between DPs and clauses, and the consequent need for an N-movement rule. These examples are said to bear out the predictions made by the suggested model. Thus the phrases in (3) are given as grammatical:

(3)   a   The probable complete invasion of China

        b   The frequent complete invasion of China

        c   The probable frequent invasion of China

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[18] This naturally only works in languages where there *is* variation in position, as e.g. in Italian and Spanish. In the case of French or English, adjective position has become mostly conventionalized to post- and preposition respectively so that any pragmatic-semantic differences, which may have been present at earlier stages of the language when variation was still the rule, will have been lost (cf. Fischer 2006).

[19] Bolinger refers to this as the 'principle of linear modification', which claims that a preposed element will tend to modify everything that follows it due to the fact that language users process language in linear fashion, with the result that an adjective + noun combination is interpreted almost as a compound, constituting one information unit, while an noun + adjective constitutes two separate information units (cf. also Fischer 2001: 252-57). In the former the adjective is attributive, in the latter predicative. In a similar way, a *stressed* preposed adjective could signal a new information unit phonologically (cf. Langacker 1997: 22).

while those in (4) are said to be ungrammatical:

(4)   a   *The complete probable invasion of China

        b   *The complete frequent invasion of China

        c   *The frequent probable invasion of China

While most of these examples are hardly frequent (not a single example with these combinations of adjectives could be found in the large British National Corpus, nor in the ICE-GB corpus), it is interesting to observe with a quick Google search (accessed October 17, 2006) what could in fact be found with these very same adjectival groups there. For (3a to c) the respective instances in Google were: 89, 178, 99, and for (4a to c): 91, 285, 6. In other words, the ungrammatical (4b) with 285 instances is in fact much more frequent than the grammatical (3b) with 178 instances. The predictions made by the generative rule seem to be somewhat more correct for (3c) and (4c): (4c), with only 6 attested examples, is clearly more rare than (3c), which has 99 examples. Overall though, the instances are rare and with the possible exception of (4c) do not obey the postulated rule; the result is either indifferent (as to 3a and 4a), or shows a clear majority of the non-expected form. French is said to show the same distributional constraints, but here, again, I suppose some more research into these constraints (or rather into the examples used as data) might well be necessary. In the chapter itself, it is not mentioned on the basis of what evidence the assignment of (un)grammaticality has been made.

Chapter 48 on past participle agreement [henceforth PPA] discusses the (adjectival) gender and number agreement markers that are found on some past participles in the Romance languages, concentrating on standard French and Italian. The use of agreement markers in these languages is not straightforward since they do not always appear on the participles, and there are also differences in this respect between French and Italian. In French the phenomenon has been much reduced, and for a large part is only visible in the spelling (if it is no longer pronounced, it could be argued that it has in fact been lost, with the spelling only reflecting an older stage). In this chapter, PPA is considered to be Spec-Head agreement, and according to Belletti it is "a consequence of passing through the Spec of the past participle projection of an element, typically the direct object, *moving to some other position* in the clause" (Chapter 48: 498, emphasis added). Cases where PPA does not apply, and cases where it applies optionally, are then seen as involving a special kind of movement (e.g. "improper movement", to prevent (5a) from occurring in French as against (5b), p. 501), or, as in the case of Italian (6a,b), they are viewed

"as a function of object movement and syntactic verb movement" (p. 503) to allow the optional presence of the agreement marker,

(5)   a   *Combien a-t-il conduit**es** de voitures?

               How many has he driven [fem/pl] of cars [fem/pl]

      b   Combien de voitures a-t-il conduit**es**?

               How many of cars has he driven [fem/pl]?

(6)   a   Maria ha conosciut**e** le ragazze

               Maria has known [fem/pl] the girls [fem/pl]

      b   Maria ha conosciut**o** le ragazze[20]

               Maria has known [masc/sg i.e., the neutralized marker] the girls

The question arises whether these variants (and others, see below) really need to be accounted for by specific movement rules. It should be noted, in addition, that the movement that takes place is of an abstract kind, and not visible on the surface. The picture becomes rather complex this way, and a lot of rules are needed to get the right results.

If the situation is viewed historically, the different variants might be understood more naturally as the slow spread of a grammaticalization process (with different speeds and different conventionalizations[21] in the different Romance dialects), in which the originally adjectival past participle slowly develops into a fixed, purely verbal element (i.e. without its adjectival inflexion) as part of a new periphrastic verb construction (e.g. a new perfect, a new passive, a new impersonal passive etc.). Indeed the more verbal the past participle has become, the more likely it is that it will have dropped its agreement marker. The presence of the marker also seems to depend on the position of the subject or object NP with which the past participle agrees. It is only in positions where the NP precedes the verbal group that agreement may still be found; this is in accordance with the position the NP originally had before the verbal group grammaticalized (see below).

Taken this way, most examples of variation fall out naturally, i.e., the more adjectival the participle is, the more it shows agreement, made possible by the regular system of adjectival

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[20] (6a) was possible in earlier standard Italian (cf. Chapter 48: 515, fn. 12); it is still possible in stylistically marked Italian, and in some other Romance dialects like Occitan (*ibid*.: 502). Standard Italian normally uses (6b).

[21] The conventionalization of a particular structure may also be influenced by standardization. Thus different rules in neighbouring languages and dialects may be the result of a different timing with respect to standardization. For a good example of this, see Detges (2001).

inflexion used elsewhere in the grammar.[22] We could then do away with all the complicated movement rules (for which there is little evidence on the surface, and which will make them hard for children to learn), and simply indicate that children would learn to mark only those participles that still look most like adjectives (which of course are marked in the same way). It goes without saying that if adjectival marking itself is lost, then agreement marking on past participles would be lost too (as was the case in English), so there seems to be a clear connection between adjectivalness and agreement marking on past participles. Let us have a look at how this may work in practice.

We will first turn to the development of the periphrastic perfect. In the older stages of the Romance languages (and this is also true for the development of the perfect in English, and for the Germanic languages in general, and in fact for most European languages, cf. Heine and Kuteva 2002: 245 and studies quoted there; van der Horst forthcoming, Vol. I), the verb *habere* (*avere/avoir*) 'have' etc. still functioned as a main verb with the meaning 'possess', and would be followed by an NP indicating the object that was possessed, as in *I have a book*. This object could be accompanied by a past participle that functioned as an adjective and was declined like an adjective, as in *I have a book written* (i.e., 'I possess a book', and that book is in a 'written' state). The construction would thus be similar to a construction with a postposed (predicative) adjective as in *I have a book new* 'I have a book (that is) new'. A similar story applies to the perfect with intransitive or mutative verbs, which was construed with *essere*. Here, too, the past participle functioned like an adjective when the existential verb *essere* began to function like a copula. The construction is similar to one where the copula is followed by an adjective, compare *I am arrived/gone* and *I am happy*.

Since there was often a pragmatic inference that the main verb and the past participle had the same syntactic subject, it became usual to associate the two, and if, as was the case in English and Romance, the object next moved to a position behind the verb 'have' so that 'have' and past participle became adjacent resulting in a fixed order (in the case of *essere* the main verb and past participle would already be adjacent), this would also speed up the grammaticalization process.[23] When we look at the examples used in this chapter, a range of constructions based on such a grammaticalization cline soon becomes visible:

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[22] Wanner (2006) argues that agreement marking is often rather dysfunctional in language in the sense that it does not carry crucial syntactic functions by itself. He also shows that, where there is agreement marking, it is often in the form of left-overs of an earlier system; as a system it often is not very logical in and by itself.

[23] This happened only with full NPs in the Romance languages, the pronouns remained clitics (unlike in English where their clitic status was lost, cf. van Kemenade 1987), and as such stayed on in the original position. This is probably the reason why (preverbal) pronominal objects still require agreement marking on the past participle unlike (postverbal) full NP objects.

(7)   a   Maria è partit**a** (*perfect with intransitive verbs*)

           Maria is departed[fem/sg adjectival marker]

        b   Maria ha parlat**o** (*perfect of transitive verb without an object*)

           Maria has spoken[neutralized marker]

        c   Maria ha lett**o** un bel romanzo (*perfect of transitive verb with full NP object*)

           Maria has read[neutralized marker] a beautiful novel

        d   Le ho parlat**o** (p*erfect of transitive verb with clitic indirect object pronoun*)

           To-her I-have spoken[neutralized marker]

        e   Ne ho lett**a**/lett**o**? la metà (*perfect of transitive verb with partitive object clitic pronoun*)

           of-it I-have read[fem/sg] the half[fem/sg] ('I have read half of it')

        f   Le ho lett**e** (*perfect of transitive verb with clitic object pronoun*)

           them I-have read[generalized adjectival pl marker]

        g   Ho lett**o** i libri

           I-have read[neutralized marker] the books (*as in (f) but with postverbal NP object*)

        h   I libri che ho lett**o** (*with* wh-*movement of NP object*)

           the books that I-have read[neutralized marker]

        i   Maria è stat**a** assunt**a** (*passive*)

           Maria is been[fem/sg] hired[fem/sg] hired ('Maria has been hired')

        j   Ci siamo guardat**e** allo specchio (*perfect with reflexive verbs, clitic reflexive object*)

           ourselves[pl] we-are watched[pl] in the mirror ('We watched ourselves in the mirror)

        k   Maria si è lavat**a** i capelli (*reflexive*)

           Maria herself is washed[fem/sg] the hairs ('Maria has washed her hair')

        l   Si sono costruit**e** molte case (*impersonal passive*)

           themselves are built[pl] many houses[pl] ('Many houses have been built')

        m   Arrivat**a** Maria, …. (*past participle non-finite clause with intransitive verb*)

           arrived[fem/sg] Maria ('Maria being arrived'= 'Maria having arrived')

        n   Conosciut**a** Maria, ….. (*past participle non-finite clause with transitive verb*)

           known[fem/sg]]Maria ('Maria being known'= 'having known Maria')

What becomes clear from these Italian constructions is that those constructions in which the subject and the main verb depict a stative situation (evident from the use of *essere* in perfects with intransitive verbs and in passives, i.e. (7a) and (7i)) show agreement in the past participle because these past participles behave very much like adjectives. When the perfect is a combination of *avere* with a transitive verb, there are various possibilities. The closer the construction is to the original construction consisting of *avere* 'possess'+ NP object + infinitival

adjunct, the more likely it is for the agreement marker to be still present (cf. footnote 22). In other words, here the position of the object plays a role. A preverbal object tends to take an agreement marker on the past participle. Since the pronouns remained in this position as clitics, the markers used in (7e) and (7f) can be explained this way, while with postposed objects the relation with earlier 'possession' has become weakened, as in (7c) and (7g). If there is no object at all, as in (7b) and (7d), then naturally, there cannot be any possession, so naturally these constructions do not take agreement markers. Such constructions are generally, in the languages investigated, of a later date. If the object is preposed by a movement rule, then it is also more likely that the agreement marker is lost, as in (7h). The reflexive cases with *essere* (7j,k,l) must presumably be seen as similar to stative constructions.

Concerning the constructions in (7m) and (7n), it seems to me that these are similar, despite what Belletti (Chapter 48: 503) writes. She argues that Maria in (7n) is an object, while it is a subject in (7m), so that the agreement in (7n) is unexpected because normally there is no agreement when the object is a postverbal NP. From a historical point of view, however, both constructions are the same. They are originally ablative absolutes in which the NP in the ablative is combined with an inflected participle. Gildersleever and Lodge (1895: 264) note that Latin had no perfect participle, and therefore it is historically incorrect to describe (7n) as a transitive perfect in which *Maria* is object, as Belletti does. Originally *Maria* was a subject, just as it is in (7m), and this explains the presence of the agreement marker. Woodcock (1959: 34) also makes clear that in such ablative absolute constructions the participle was used predicatively, i.e., like an adjective, which similarly accounts for the presence of the agreement marker. So we do not need a complicated rule that ensures past participle agreement in (7n), because it is not (pace Belletti p. 503) "a genuine case of agreement with the object". The agreement found there is, like in (7m), with the *subject*. It is possible, of course, that, synchronically in present-day Italian, speakers do see this as a perfect. In that case, the agreement marker should be seen as an anachronistic left-over of an earlier stage; if this is so, it is uneconomical and unnecessary to explain this remnant by a productive synchronic *movement* rule.

Chapter 4, by Denis Delfitto, is on 'Adverb Classes and Adverb Placement'. This is an informative chapter; the discussion here is more interdisciplinary than in most of the other chapters, with reference being made, for instance, to other studies outside the formal model. This makes the chapter more readable (less jargon-like) and more accessible to non-generative linguists. This different approach may be partly due to the fact that much less has been written on adverbials within the generative model than on other linguistic categories or constituents, presumably because the adverbials occur in may different positions, which are often tied up with

meaning and/or function. Another reason for the different approach may be that adverbials are difficult to classify because of their ambiguous categorial status, i.e., they do not share a common set of syntactic features: they are in fact easier to classify semantically than syntactically (cf. Chapter 4: 86-89).

In a way, this is admitted via Delfitto's reference to Cinque's "hierarchy of adverbs" (*ibid.*: 95), in which the different types of adverbs (various types of sentence adverbs and VP adverbs) are described syntactically in terms of hierarchically ordered functional projections. Although these projections look structural and hence syntactic (the adverbials have fixed positions in the tree), they are in fact based on semantic information, i.e., it is the semantics that explains their position, not the syntax. A clear drawback of Cinque's proposal, however, is that the tree structure has now been provided with a large amount of functional projections, most of which remain empty in any given derivation.

It is clear that, in order to understand the placement of adverbials, we need to have recourse to semantic information. And here, too, a historical perspective may be rewarding, especially in the case of sentence adverbs. As is well-known, sentence adverbs may occur in many different positions in the sentence without any change in meaning, in contrast to VP adverbs of manner or degree. For instance, when in English a manner adverb is used preverbally rather than postverbally (its usual place), it in fact begins to behave more like a degree adverb; in other words, position changes meaning, as shown in (8) (cf. Chapter 4: 96),

(8)   a   He told them clearly
        b   He clearly told them

In (8), the adverb *clearly* functions like a manner adverb postverbally and as a degree adverb preverbally, with the latter expressing the degree of the subject's 'telling' (parallel to 'he did *not* tell them, he *half* told them' etc.). This is not the case with sentence adverbs, where the semantics (the focus may be different) of the clause remains the same in spite of a change in position:

(9)   a   Evidently John has eaten the beans
        b   John evidently has eaten the beans
        c   John has evidently eaten the beans
        d   John has eaten the beans, evidently (examples are from Chapter 4: 97)

The reason for this is that the category of sentence adverbs historically arose out of full clauses (cf. Fischer 2007), and like full clauses they can occur in all the positions indicated in (9) (i.e., in all cases *evidently* can be replaced by a full clause such as *It is clear*).[24]

In the generative framework observed here, the position with respect to sentence adverbs is that "the very same structural position should be involved … since the interpretation is essentially the same in all cases" (Chapter 4: 97). (This is in contrast to the generative position on the VP-adverbs in (8), where the adverbial adjunct should be generated in different positions because of their clear differences in meaning.) This position leads automatically to the positing of all kinds of movement rules, which must ensure the correct surface order. However, what is said to be moved is not the adverbial, but the constituents around it. Thus we get suggestions such as the one by Belletti (quoted in Chapter 4: 98) that (9b) is "transformationally related to [(9a)] by means of left-dislocation of the subject", or that (9c) "involves recursion of the Agr(eement)-node", the proposal being that "S-adverbs are … uniformly generated as adjoined to the sentential AgrP node [so that w]henever an auxiliary is present, Agr-recursion is assumed to take place, with obligatory movement of Aux to the higher Agr-node". These descriptions are not very explanatory. I fail to see, for instance, why the subject should be seen as being moved when the movable element is in fact the ellipted clause or sentence adverb.

An interesting aspect of adverbs, not discussed in this chapter is the fact that the position of the adverb (and its meaning) may also depend on larger discourse factors. This only becomes clear when one considers actual examples from a large corpus. Thus in English, manner adverbs that appear postverbally and which are found with all kinds of syntactic subjects, animate agentive as well as inanimate NPs, may appear in preverbal position when the subject is presented as agentive, giving the manner adverb a sense of purposefulness, of deliberation (in other words coming close again to intensity and degree, cf. (8)). Compare the following examples all collected with Google (accessed November 14, 2006) from the web:

(10)   a   **The moon moved slowly** across the sky. Laura kept listening, staring out into the darkness and waiting. Then the night began to change. **...** http://www.domestic-church.com/CONTENT.DCC/20030101/STORIES/chapter15.htm

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[24] In Fischer (2007: chapter 6), I show how the earliest sentence adverbs derived mostly from full clauses. Once the category of sentence adverbs had been established, however, new sentence adverbs of a similar nature also came to be formed and used in similar positions *analogically*, without their being clausal first. This happened, for instance, with *unquestionably, presumably, admittedly* (cf. Noel 2005, Fischer 2007: section 6.2).

b   That night as **the moon moved slowly** across the small, barred window that lie [sic]
         above where Jack lay chained on the cold, damp floor, he thought of many ...
         http://www.tarotpassages.com/swordsstory-dd.htm

c   **The moon moved slowly** on its path to morning, but the emptiness stayed with him. It
         was warm out and he had no intentions of leaving. ...
         http://www.lonestar.texas.net/~rtremper/sands.html

d   As time passed, **the Moon slowly moved** away from the Earth to its current orbit, taking
         about 27 days to circle the Earth. ...
         www.thursdaysclassroom.com/15jun00/article1.html

e   **The moon slowly moved** over the sun, its curve forming a dark silhouette across the
         bright face, moment by moment darkening the sky. ...
         http://www.horrorlibrary.net/index.php?page=php_view_story&author_id=64

f   **The moon slowly moved** further and further from the Earth and in fact still recedes to this day.
         It moves away at a rate of 3.8 centimeters per year. ...
         www.csphysmath.com/physics/astronomy/solarsystem/moons/earthsmoon.html

In (10a-c) the description of the moon functions as background information in the story, the
moon is not itself the subject of the discourse. This is different in (10d-f), where the narrative is
centered around the moon itself, causing the moon to be seen as a more active agent. These
different positions of the adverbial in a narrative text can be linked up with other linguistic
discourse features, such as the use of the imperfect tense in the Romance languages in purely
descriptive passages against the use of the perfect or aorist to indicate plot development. It is
hard to see how a sentence-based model like generative grammar, including Cinque's hierarchy,
can account for this type of variation. Stipulating different types of syntactic movement or
functional projections does not explain the variation found.

I selected Chapters 7 and 12 for further discussion because they make reference to areas
not usually covered in generative grammars, such as pragmatics and grammaticalization, and
they show more interest in variation. Chapter 7 by Paul Hagstrom (already briefly mentioned
above) deals with 'A-not-A Questions'. It is perhaps a little surprising to find this topic included
in the handbook because this type of question only seems to have been grammaticalized in
Mandarin and Singapore Teochew (with some parallel constructions using a particle in one or
two other Chinese dialects); in other words, cross-linguistically it is not a very current
grammatical structure. Although the chapter provides us with a lot of data, it does not really
clarify the relation between the different variants that are used in Mandarin. Apart from
indicating structural differences and how they may be derived within a formal model, it would

have been helpful to refer to the process of grammaticalization in order to discover how one variant arises out of another, steered by factors such as frequency and subsequent phonetic reduction and semantic bleaching. It would explain, for instance, why the fuller (i.e. less grammaticalized) alternative construction with *haishi* 'or' in (12) is "syntactically relatively unconstrained" (Chapter 7: 181) compared to the reduplicative or partly reduplicative constructions shown in (11a) and (b) respectively,

(11)   a   ta xihuan-bu-xihuan zheben shu?

             He   like-not-like   this book

       b   ta xi-bu-xihuan zheben shu?

             He li-not-like   this book

             'Does he like or not like this book?' (Chapter 7: 174)

(12)   [wo qu Meiguo haishi bu qu Meiguo] bijiao hao? (*ibid*.: 181)

             I   go America or   not go America more good

             'Is it better that I go to America or that I not go to America?'

(12) shows that the *haishi* variant is less subject to constraints because it can be used "inside islands without difficulty" (*ibid*.: 181), which is not possible with the more grammaticalized constructions in (11).

      Other constraints are found with respect to focus marking, such as restrictions on the combination of the N[egative]Q[uestion marker] and the focus marker, as shown in (13) (*ibid*.: 185ff.), which are not found with e.g. the *haishi* structure,

(13)   a   ta hui-bu-hui zhi   shuo Yingyu?

             he can-not-can only[focus marker] speak English

             'Can he only speak English?'

       b   *ta zhi   hui-bu-hui shuo Yingyu?

             he only can-not-can speak English

             'Can he only speak English?' (*ibid*.: 186)

Again, this can be shown to be related to the degree of grammaticalization; (13) is more deeply grammaticalized: it presents a structure in which the reduplicated A-not-A question is seen as a single unit. The fact that (13b) is impossible follows from the degree in which it has been grammaticalized, and also from purely pragmatic factors. In (13a) the focus is on whether English is the only language that the subject can speak, a more accurate translation therefore

would be 'Can he or can he not, only speak English?' (i.e. 'is it the case or not the case that he only can speak English'), whereas in (13b) the A-not-A verbal group itself is focused (*zhi* being placed in front of it), which does not make sense pragmatically because one does not normally focus on an alternation; a focus singles out and implies a contrast, and can therefore only focus on *one alternative.* Because Hagstrom offers the same translation for both, the suggestion is raised that the difference is a purely structural one. With the *haishi* construction *zhi can* be placed immediately before the main verb (i.e., the *haishi* equivalent of (13b) *is* possible) because here the alternative questions are framed as two separate clauses, each of which can be separately focused. Other restrictions found, e.g. with adverbs (*ibid.*: 190ff.) can be similarly explained. In other words, a purely "structural explanation" (*ibid.*: 190) provides only half the story.

Chapter 12 by Yen-hui Audrey Li presents a (very extensive[25]) discussion of a specific Mandarin construction: 'Chinese *Ba*'. It represents a more satisfactory treatment than the one in Chapter 7 because its author takes great pains to show the historical development of this construction (i.e., how grammaticalization has affected it), which more fully explains its use and its many variant forms. The construction in question involves the verb *ba,* which developed from a full verb meaning 'to take/hold', accompanied by an NP_1 subject and NP_2 object, and followed by another verb and some extra element. This complex structure evolved in the course of time into a variety of structures with their own meanings, ranging from serial verb constructions (a first step on the grammaticalization cline), to constructions where the NP_2 functioned no longer as a direct object but as an instrument or locative, or as part of a predicative complement, and many others. In addition, unusual for this handbook, a lot of attention is paid to semantic and pragmatic factors. The data given in this chapter are copious and highly valuable because it enables the linguist to form his/her own opinions about the structures and the way they are used. The space devoted to capturing the variation in terms of formal rules and constraints (as e.g. in Chapter 7) is minimal compared to the space devoted to a description of usage, to the many semantic-pragmatic constraints, and the constraints linked up with the historical (grammaticalization) development. The structural explanations that have been provided in the

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[25] It contains almost a hundred pages, which seems a little excessive considering the fact that the construction is peculiar to only one language (even though it is "extremely prominent" (p. 375) there), and little effort is made to tie it to other typologically similar structures cross-linguistically. This could easily have been done because the structure has clear ties with transitivity phenomena occurring elsewhere and shows similar constraints. However, because the treatment in this handbook is purely structural and not typological (where similarity in function clearly also plays a role), such links are, unfortunately, ignored so that the non-formal linguist has to discover these by him/herself.

literature so far are also discussed and evaluated, but it is emphasized all along "that a clearly defined structural account" (Chapter 12: 454) is not possible.

Other chapters that are of interest in terms of grammaticalization and are close in nature to Chapter 12, are Chapter 31 by Tara Mohanan on 'Grammatical Verbs (with Special Reference to Light Verbs)', and Chapter 59 on 'Serial Verbs' by Pieter Muysken and Tonjes Veenstra. One of the well-known problems with light verbs is that they are not easy to define. The reason for this is again that they are on a cline from fully referential verbs, to serial verbs, to empty grammatical function words. Mohanan tries very hard to make a "principled distinction" between the various possible categories on the basis of empirical evidence (of both a semantic and a morphosyntactic nature) and cross-linguistic generalizations, but admits that this will in most cases lead to only "partial answers" (Chapter 31: 464). Particularly difficult to categorize are those light verbs that are somewhere halfway down the grammaticalization cline, like e.g. 'make' in a construction such as 'make the claim', which can be both a separate verb, with 'claim' as a separate object, as well as a complex inseparable V. This situation is of course typical in grammaticalization processes (the old and the new structures appear side by side), but it is impossible to account for within a formal grammar system that needs to make clear categorial choices. So we get a "mismatch between the structure in terms of grammatical categories and in terms of grammatical functions" (*ibid*.: 467). I think it is important to note that this mismatch is due to the model, and that it is not a problem in language use or for language users. If the same example would be considered in terms of another model, such as for instance Construction Grammar, there would be no mismatch at all. In this connection Mohanan (*ibid*.: 470, and see also p. 484ff.) asks the following questions:

> How must the compositionality of complex predicates be formally expressed such that the semantic and syntactic representation of the light verb and its host, together with the structural relation between the two, derives the semantic and syntactic properties of the complex predicate? What kind of syntactic and semantic theory can describe this compositionality? While framework-internal analyses of different aspects of different types of complex predicates are available, covering rich data with recurrent clustering of properties, answering the above questions requires bringing together all of the fragments into a comprehensive picture. We do not as yet have such an integrated theory of complex predicates.

My answer to this would be that it is impossible to give a *coherent* synchronic account because the synchronic variants found are part of a long-term development, the forms (and attached

meanings) of which exist side by side. Only a Construction Grammar approach or a memory-based grammar can do justice to these facts. "Formal expression" by means of an integrated theory, as Mohanan aims for, will not be possible. In this connection it should also be noted that many light verbs and their hosts do not grammaticalize, but instead lexicalize, a similar process which leads to new lexical items rather than to new grammatical functions (cf. Himmelmann 2004). Which of the two processes occurs in each case depends to a large extent on the overall grammatical system of the language in which the light verbs and their satellites function.

In Chapter 59, Pieter Muysken and Tonjes Veenstra refer to this distinction between lexicalization and grammaticalization implicitly. The authors note that the way in which a complex serial verb construction develops, all depends on "how their lexical and syntactic properties interact" (Chapter 59: 236). It seems clear, but this is not discussed, that this is influenced by many factors, such as the shape of the overall system in which the complex predicate develops and functions, the frequency of the lexical items involved, the semantic reference of the items in question (general or specific), and, very importantly, the adjacency or non-adjacency of the elements in question. Thus, some complexes may become idiomatic expressions and lexicalize (illustrated in sections 3.3 and 3.4 in Chapter 59), while others, due to the greater frequency and generality of one of its verbs, may form more general grammatical structures whereby e.g. the first verb in the complex becomes a grammatical function word: an auxiliary, a preposition/case affix, an aspect marker or a complementizer (as illustrated by the authors in sections 3.1 and 3.2). Adjacency plays an important role here; thus, in Saramaccan, the reduced form *káá* from *kabá* 'to finish' turned into an aspectual adverb (i.e. it lexicalized) rather than a verbal aspectual affix (i.e. a grammaticalization process which theoretically would have been a possibility) probably because the verbs *jabí* and *káá* are not adjacent:

(14)   Mi jabí dí dóo káá
         I open the door finish
         'I already opened the door' (*ibid.*: 247)

while in,

(15)   Mi wáka lóntu dí wósu
         I walk surround the house
         'I walk around the house'(*ibid.*: 245)

the verb *lóntu* can begin to function like a preposition (i.e. grammaticalize) because it regularly stands in front of the object NP that it governs, i.e., in a slot already in use for prepositions within the system of Saramaccan as a whole. Presumably, if Saramaccan had been an SOV language rather than an SVO language, this development would not have happened; the second verb might have become a case affix or postposition instead, again depending on whether the language in question already possessed these morphosyntactic categories.

Finally I will briefly consider two more articles, especially with respect to the question whether the use of relevant (corpus) data, of the semantics-pragmatics of the context, and of the historical development may make a difference as to the kind of structural explanations offered for the synchronic facts. Chapter 21 by Joseph Emonds and Rosemarie Whitney deals with 'Double Objects'. The authors discuss two possible structures for double object structures occurring in clauses such as *I gave him a present/I gave a present to him.* Either there is true 'dative alternation', i.e., the bare indirect object NP alternates with a PP (as in English), or the bare indirect object is in fact a PP at a more abstract level (in which case it can only surface as a bare NP if it is VP internal, as in Dutch). On p. 76, the authors indicate that they suspect "that double objects involving truly bare indirect object NPs are limited to head-initial systems". Dutch, which doesn't have a head-initial system, is exceptional in allowing bare NPs but, because this has to be VP-internal according to the authors, Dutch does not allow the bare indirect object to passivize or topicalize without a P resurfacing (*ibid.*).[26] A look at Google instances containing standard Dutch examples soon shows, however, that this cannot be upheld. Bare indirect objects are found both in passives (but see note 26) and in topicalized position as (16) shows:

(16) a **Hem werd gevraagd** dit te ontkennen (http://nl.wikipedia.org/wiki/Paus_Zosimus)
         him   was   asked   this to deny
         'he was asked to deny this

    b   dat **hem werd gevraagd** het voorzitterschap van het bestuur van het nll op zich te nemen
         (http://www.delftintegraal.tudelft.nl/info/index00d5.html?hoofdstuk=Artikel&ArtID=2361)
         that him   was   asked   the chairmanship   of the board   of   the nll on him to take
         'that he was asked to accept the chairmanship of the board of the nll'

    c   **Gerrit-Jan werd gevraagd** wanneer hij nu de plaats van vader Evert zou gaan innemen.

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[26] It seems that the authors consider 'hem werd gevraagd' a true passive, which is a little unusual, but I follow them in this here. Historical linguists would call this an impersonal passive, a passive in which dative case has been preserved rather than becoming nominative. Note that Dutch did not share the development of English, where the dative of the older (Old English) construction *Him was asked* changed into a nominative: *He was asked.*

Gerrit-Jan was   asked   when   he now the place of father Evert would go take-in

'Gerrit-Jan was asked when he would go and take his father Evert's place'

d　　**Hem heb ik regelmatig** vragen gesteld over wie mijn moeder was en wat   ze deed

him have I regularly questions asked about who my mother   was and what she did

'him I have regularly asked questions about who my mother was and what she did'

d　　Toen heb ik nog om een priester gevraagd en **hem heb ik ook mijn verhaal** gedaan

then have I still for a   priest   asked   and him have I also my   story   done

'then did I still ask for a priest and him I have told told my story'

This makes one wonder whether the suggestion that the Dutch bare indirect object is underlyingly a PP (cf. p. 76) necessarily holds.

Another claim made with respect to data concerns "the cross-linguistic absence of both double accusatives (outside of predicate nominal agreement) and double genitive complements" (p. 80). Again the data seem to be a little more refractory in that double accusatives are found, for instance, in Old English (cf. Mitchell 1985: §1083) with the verbs *læran* 'teach' and *biddan* 'ask', while there are also a number of verbs with double accusatives in Latin (cf. Gildersleeve and Lodge 1895: 215ff.).

Concerning the semantic-pragmatic or discourse circumstances in which the double object constructions are used, the authors indicate that any claims made to the effect that facts such as definiteness may distinguish the two versions of the dative alternation are not relevant since "such claims seem concerned at most with language use rather than syntax"; they further add that "syntactically, dative alternations are equally well-formed with definite or indefinite objects in either position" (p. 83). This well-formedness, however, is not tested on discourse but on made-up examples.

Another discourse factor that is ignored but which may well be relevant is the information status of the indirect object NP. The *to*-NP is used in discourse when the information conveyed by it is rhematic, while the bare NP is used when it is thematic (this is also the reason why the bare NP almost always surfaces as a pronoun, which is, by its very nature, anaphoric or thematic, unless stressed and used for contrast). This would explain in a natural and straightforward way why there is a restriction on A'-extraction of the bare indirect object, as noted by the authors, and which they try to account for, structurally, discussing a number of generative proposals (cf. Chapter 21: 95-98). In discourse terms the solution would be that a

clause such as *Which sister*ᵢ *shall we send* tᵢ *a present,* is unlikely to occur because in this case the bare NP is typically rhematic and needs to be represented by a *to* NP. Also from an iconic point of view, it is not at all surprising that a rhematic constituent receives more explicit coding than a thematic one, cf. Givón's (1995: 49) 'principle of quantity'.

Other semantic or discourse factors that seem to influence usage can be drawn from the BNC corpus, which shows that, for instance, the verb *deliver* does not normally take a bare full NP, i.e., cases such as given on p. 93: *We delivered Mary the package* seem to be rare. The bare indirect object NPs found in the BNC with *deliver* are all pronouns. More interestingly, all the relevant examples with *deliver* do not have the original (concrete) sense of 'to take goods to a particular person/place', but simply replace 'give' in a more formal or jocular style:

(17)   a   Charles Howard had just delivered me a colossal snub (BNC, AOF)

        b   He delivered her a long look, then smiled. (BNC, H97)

In other words, their usage is clearly influenced by the regular double object structure of the verb *give*, and the process at work here is the spread of a structure by lexical diffusion rather than direct generation by a rule of grammar. Again, this can be more easily accounted for in a Construction Grammar approach.

This also leads me to a historical point. It is well-known that dative alternation in English does not apply to all verbs that can take a direct as well as an indirect object. Many verbs only allow an 'indirect object' introduced by a preposition (so in fact only a *prepositional* object). Such facts are difficult to explain synchronically, unless one proposes separate lexical entries for those verbs that do allow dative alternation (this is one of the solutions proposed in the generative literature, see the discussion in section 2 of Chapter 21).[27] This is obviously not a welcome solution since the verbs in question carry the same meaning, whether they take a bare or prepositional NP. Historically, it can easily be shown that the bare construction is the original one, and that the prepositional NP only came to be introduced when English was losing its case inflexions. It is this fact (and the possibility of lexical diffusion referred to above) that explains the present-day distribution for the larger part. The verbs allowing dative alternation are mostly

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[27] The authors of this chapter reject this lexical solution, mainly because they prefer a more 'elegant' and economical solution in terms of the grammar; thus, they are "convinced that (i) a structure dependent theory of abstract case marking [whereby one oblique case is a structural PP] provides superior accounts of double object constructions, and (ii) a null PATH affix akin to overt applicative affixes found outside Indo-European is a good basis for an account of English and Mainland Scandinavian 'double accusative objects'" (p. 130). It is to be noted first of all that such a structural solution is only possible on the basis of greater abstraction within the model, and secondly that a structural PP is dictated for theory-internal reasons, because a verb can only assign one case according to the theory of Case (see the discussion on p. 87 of Chapter 21).

verbs of Germanic origin and they are highly frequent. It is the frequency of these verbs with the old construction which enabled them to preserve this structure against the tendency to replace dative NPs by prepositional ones, helped by the fact that this former dative was also easily recognized because it only occurs in double NP structures involving a pronoun with a fixed order of indirect and direct object;[28] single datives did not survive anywhere except possibly as idioms. In other words, this structure was robust enough to survive even though it no longer obeyed the current rules of grammar.

The last chapter I would like to pay attention to is Chapter 23 on 'Embedded Root Phenomena' by Caroline Heycock. This chapter deals with transformations which normally only occur in root (main) clauses, but which sometimes also show up in subordinate clauses. The main topic of discussion is the exceptional use of the main-clause V2 rule in some subordinate clauses. Section 4 of this chapter discusses an explanation of the phenomenon in semantic-pragmatic terms based on the work of Hooper and Thompson (1973). It is again one of the rarer occasions in the handbook where room is made for and serious attention is being paid to other than structural factors. However, even here preference is given to a structural solution because the semantic-pragmatic account in terms of the semantic concept of 'assertion' and the 'pragmatic incompatibility of emphasis with non-assertion' is found to be vague and circular. The author notes more generally that "[i]t is a general problem for work in this area that definitions given are vague and independent evidence for the validity of the concepts used are often weak" (Chapter 23: 190).

I am not sure, however, that the accusation is entirely valid. Heycock provides one piece of counterevidence to Hooper and Thompson's proposal that assertion is a necessary and sufficient condition for the occurrence of root phenomena and that the pragmatic incompatibility of emphasis with non-assertion explains why root transformations do not occur in most subordinate clauses. This is because most subordinate clauses are by their very nature non-assertive, being reduced or presupposed; only some types of adverbial clauses can be assertive. Heycock notes that clefting is another way to create emphasis and that, therefore, if Hooper and Thompson are correct, clefting should also not occur in non-assertive contexts. She illustrates this with the following example: *We regretted that it was precisely this book that had been destroyed* (p. 191), arguing that this shows clefting in a non-assertive or presupposed clause, and therefore shows that emphasis and non-assertion do occur together. Note, however, that *regret* is

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[28] The fixed order is a later development, which helped to preserve the bare NP structure. In Old English the two objects may occur in any order when they are full NPs. When one NP is a pronoun, the preferred order is for the pronoun to precede the nominal NP, while with two pronouns the order already is as good as fixed , cf. Koopman (1990).

here not used in the usual sense of 'to feel sorry about something you have done wishing you had not done it", but in the sense 'to *say* that you are sorry'. This puts the verb as used here in Hooper and Thompson's class A predicates (containing verbs like *say, report* etc.), which (exceptionally) take assertive complements (as also indicated by Heycock, pp. 188-89). In other words, this example is fully explained by Hooper and Thompson's hypothesis, and cannot be used as a counterexample.

Heycock does indeed come to the conclusion later, after a discussion of the structural solutions offered, that a unified phenomenon in purely structural terms is not possible and that "progress on this question is likely to come from work on the syntactic encoding of discourse semantic concepts" (p. 203). I can only agree, in the sense that an explanation of these facts should combine formal as well as semantic-pragmatic factors. In her conclusion she also notes in passing that it might well be profitable to compare the cases of V2 in embedded clauses to the "style indirect libre" (p. 203). It is quite noticeable that indeed all the examples of this phenomenon constitute cases where the embedded clause, although syntactically embedded by a complementizer, is in fact in other respects like a main clause. This would show that the phenomenon started off as something quite marginal, and stylistically coloured. One may well wonder whether such a phenomenon deserves to be derived in rule-like fashion from the system of grammar.

Some concluding words are pertinent. I fully realize that in this article I have not given enough consideration to the enormous amount of work produced in these five volumes. It goes without saying that these volumes will be invaluable to the generative school of linguists and their followers, and to many other linguists who are interested in finding out what has been written on a great number of syntactic topics from a generative point of view. To find all these topics together, complete with a history of the various proposals made to describe and (possibly) explain them, and the data on which these descriptions are based, is extremely useful. But there remains a sense of regret that most of the contributors have not used this opportunity to discuss other proposals also in existence, coming from different schools of thought. This would have made the discussion much richer, and would have led, it is my firm opinion, to more satisfying explanations. I have wanted to make clear in this review article that the explanations now offered may, on paper, seem to provide the expected "bliss", but that taking historical and semantic-pragmatic circumstances into account would have led to even greater "explanatory bliss".

References

Adamson, Sylvia. (2000). "A Lovely Little Example: Word Order Options and Category Shift in the Premodifying String". In *Pathways of Change. Grammaticalization in English*, edited by Olga Fischer, Anette Rosenbach and Dieter Stein, 39-66. Amsterdam: Benjamins.

Aitchison, Jean. (2001). *Language Change. Progress or Decay?* 3rd ed. Cambridge: Cambridge University Press.

Anttila, Raimo. (2003). "Analogy: The Warp and Woof of Cognition". In *The Handbook of Historical Linguistics*, edited by Brian D. Joseph and Richard D. Janda, 425-40. Oxford: Blackwell.

Bolinger, Dwight. (1952). "Linear Modification". *PMLA* 67: 1117-44.

———. (1967)".Apparent Constituents in Surface Structure". *Word* 23: 47-56.

Bybee, Joan L. (1988). "The Diachronic Dimension in Explanation". In *Explaining Language Universals*, edited by A. Hawkins John, 350-79. Oxford: Blackwell.

——— . (1995) "Regular Morphology and the Lexicon". *Language and Cognitive Processes* 10: 425-55.

Bybee, Joan L., and Paul Hopper, eds. (2001). *Frequency and the Emergence of Linguistic Structure*. Amsterdam: Benjamins.

Bybee, Joan L., and Dan I. Slobin. (1982). "Rules and Schemas in the Development and Use of the English Past Tense". *Language* 58: 265-89.

Chomsky, Noam. (1981). *Lectures on Government and Binding*. Dordrecht: Foris.

Clark, Eve V. (2003). *First Language Acquisition*. Cambridge: Cambridge University Press.

Corbett, Greville, Andrew Hippisley, Dunstan Brown, and Paul Marriott. (2001). "Frequency, Regularity and the Paradigm: A Perspective from Russian on a Complex Relation". In *Frequency and the Emergence of Linguistic Structure*, edited by Joan L. Bybee and Paul J. Hopper, 201-26. Amsterdam: Benjamins.

Croft, William, and Alan Cruse. (2004). *Radical Construction Grammar: Syntactic Theory in Typological Perspective*. Oxford: Oxford University Press.

Derwing, Bruce L. (1977). "Is the Child Really a 'Little Linguist'?" In *Language Learning and Thought*, edited by John MacNamara, 78-84. New York: Academic Press.

Detges, Ulrich. (2004). "How Cognitive Is Grammaticalization? The History of the Catalan Perfet Perifràstic". In *Up and Down the Cline -- the Nature of Grammaticalization*, edited by Olga Fischer, Muriel Norde and Harry Perridon, 211-27. Amsterdam: Benjamins.

Dik, Simon C. (1997). *The Theory of Functional Grammar, parts 1 and 2* (edited by Kees Hengeveld), Berlin: Mouton de Gruyter.

Faarlund, Jan Terje. (1990). *Syntactic Change. Towards a Theory of Historical Syntax*. Berlin: Mouton de Gruyter.

Fillmore, Charles J. (1992). "'Corpus Linguistics' or 'Computer-Aided Armchair Linguistics'". In *Directions in Corpus Linguistics: Proceedings of Noble Symposium 82, Stockholm, 4-8 August 1991*, edited by Jan Svartvik, 35-60. Berlin: Mouton de Gruyter.

Fischer, Olga. (1988). "The Rise of the *for Np to V* Construction: An Explanation". In *An Historic Tongue. Studies in English Linguistics in Memory of Barbara Strang*, edited by Graham Nixon and John Honey, 67-88. London: Routledge.

———. (1989). "The Origin and Spread of the Accusative and Infinitive Construction in English". *Folia Linguistica Historica* 8, no. 1-2: 143-217.

———. (1994). "The Fortunes of the Latin-Type Accusative and Infinitive Construction in Dutch and English Compared". In *Language Change and Language Structure. Old Germanic Languages in a Comparative Perspective*, edited by Toril Swan, Endre Mørck and Olaf Jansen Westvik, 91-133. Berlin: Mouton de Gruyter.

———. (2000). "The Position of the Adjective in Old English". In *Generative Theory and Corpus Studies: A Dialogue from 10 ICEHL* , edited by Ricardo Bermùdez Otero, David Denison, Richard M. Hogg and C.B. McCully, C. B., 153-81. Berlin: Mouton de Gruyter.

———. (2006). "On the Position of Adjectives in Middle English". *English Language and Linguistics* 10: 253-88.

———. (2007). *Morphosyntactic Change. Functional and Formal Perspectives*. Oxford: Oxford University Press.

Fischer, Olga, Ans van Kemenade, Willem Koopman, and Wim van der Wurff. (2000).*The Syntax of Early English*. Cambridge: Cambridge University Press.

Gentner, Dedre, Keith J. Holyoak, and Boicho N. Kokinov, eds. (2001). *The Analogical Mind. Perspectives from Cognitive Science*. Cambridge, Mass.: MIT Press.

Gerritsen, Marinel, and Dieter Stein. (1992). "Introduction: On 'Internal' and 'External' in Syntactic Change". edited by Marinel Gerritsen and Dieter Stein, 1-15.

Gildersleeve, B.L., and G. Lodge. (1895). *Gildersleeve's Latin Grammar*. 3rd ed. London: MacMillan.

Givón, Talmy. (1995). "Isomorphism in the Grammatical Code: Cognitive and Biological Considerations'. In *Iconicity in Language*, edited by Raffaele Simone, 47-76. Amsterdam: Benjamins.

Goldberg, Adele E. (1995). *Constructions: A Construction Grammar Approach to Argument*

Hampe, Beate, and Doris Schönefeld. (2003). "Creative Syntax: Iconic Principles within the Symbolic". In *From Sign to Signing. Iconicity in Language and Literature 3*, edited by Wolfgang G. Müller and Olga Fischer, 243-61. Amsterdam: Benjamins.

Heine, Bernd, and Tania Kuteva. (2002). *World Lexicon of Grammaticalization*. Cambridge: Cambridge University Press.

Himmelmann, Nikolaus P. (2004). "Lexicalization and Grammaticalization: Opposite or Orthogonal?" In *What Makes Grammaticalization? A Look from Its Fringes and Its Components*, edited by Walter Bisang, Nikolaus P. Himmelmann and Björn Wiemer, 21-42. Berlin: Mouton de Gruyter.

Hofstadter, Douglas. (1995). *Fluid Concepts and Creative Analogies*. New York: Basic Books.

Holyoak, Keith J., and Paul Thagard. (1995). *Mental Leaps. Analogy in Creative Thought*. Cambridge Mass.: MIT Press.

Hooper, Joan and Sandra Thompson. (1973). "On the Applicability of Root Transformations", *Linguistic Inquiry* 4: 465-497.

Hopper, Paul J. (1998). "Emergent Grammar (Revised Version of 1987)". In *The Psychology of Language: Cognitive and Functional Approaches to Language Structure*, edited by Michael Tomasello, 155-75. Mahwah, New Jersey: Erlbaum.

Hopper, Paul. J., and Elizabeth C. Traugott. (2003 [1993]). *Grammaticalization*. 2nd ed. Cambridge: Cambridge University Press.

Horst, Joop M. (forthcoming) *Geschiedenis van de Nederlandse Syntaxis. Vols I-III.*

Itkonen, Esa. (2005). *Analogy as Structure and Process*. Amsterdam: Benjamins.

Jackendoff, Ray. (2002). *Foundations of Language. Brain, Meaning, Grammar, Evolution*. Oxford: Oxford University Press.

Joseph, Brian D. (1992). "Diachronic Explanation: Putting Speakers Back into the Picture". In *Explanation in Historical Linguistics*, edited by Garry W. Davis and Gregory K. Iverson, 123-44. Amsterdam: Benjamins.

Kemenade, Ans van. (1987). *Syntactic Case and Morphological Case in the History of English*. Dordrecht: Foris.

Kiparsky, Paul. (1968). "Linguistic Universals and Linguistic Change". In *Universals in Linguistic Theory*, edited by Emmon Bach and Robert T. Harms, 171-202. New York: Holt, Rinehart and Winston.

Koopman, Willem F. (1990). "The Order of Dative and Accusative Objects in Old English". In *Word Order in Old English. With Special Reference to the Verb Phrase*, edited by Willem F. Koopman, 133-223. Amsterdam: University of Amsterdam: Faculty of Arts.

Krickau, C. (1877) *Der Accusativ mit dem Infinitiv in der englischen Sprache, besonders im Zeitalter der Elisabeth*. PhD dissertation, University of Göttingen.

Langacker, Ronald W. (1987). *Foundations of Cognitive Grammar*. Vol. I: Theoretical prerequisites. Stanford: Stanford University Press.

———. (1997). "Constituency, Dependency, and Conceptual Grouping". *Cognitive Linguistics* 8: 1-32.

.Lightfoot, David W. (1974). "The Diachronic Analysis of English Modals". In *Historical Linguistics*, edited by John Anderson and Charles Jones, 219-49. Amsterdam: North-Holland.

———. (1979). *Principles of Diachronic Syntax*. Cambridge: Cambridge University Press.

———. (1999). *The Development of Language: Acquisition, Change, and Evolution*. Oxford: Blackwell.

Lombardo Vallauri, Edoardo. (2004). "The Relation between Mind and Language: The Innateness Hypothesis and the Poverty of the Stimulus". *The Linguistic Review* 21: 345-87.

McMahon, April. (2000). *Change, Chance and Optimality*. Oxford: Oxford University Press.

Meyer, C.F., R. Grabowski, H-Y. Han, K. Mantzouranis, and S. Moses. (2003). "The World Wide Web as Linguistic Corpus". In *Corpus Analysis, Language Structure and Language Use*, edited by P. Leistyna and C.F. Meyer, 241-54. Amsterdam: Rodopi.

Mitchell, Bruce. (1985). *Old English Syntax*. 2 vols. Oxford: Oxford University Press.

Noël, Dirk. (1997). "The Choice between Infinitives and *That*-Clauses after *Believe*". *English Language and Linguistics* 1: 270-84.

———. (2005). "The Productivity of a 'Source of Information' Construction: Or, Where Grammaticalization Theory and Construction Grammar Meet". Paper presented at the *Fitigra Conference*. University of Leuven, Belgium.

Plank, Frans. (1995). *Double Case: Agreement by Suffixaufnahme*. Oxford: Oxford University Press.

Pollard, C. and Sag, I. A. (1994). *Head-Driven Phrase Structure Grammar*. Chicago: Chicago University Press.

Sapir, Edward. (1921). *Language*. New York: Harcourt, Brace and World.

Skousen, Royal. (1989). *Analogical Modeling of Language*. Dordrecht: Kluwer.

———. (1992). *Analogy and Structure*. Dordrecht: Kluwer.

Slobin, Dan I., ed. (1985). *The Crosslinguistic Study of Language Acquisition. Vol. 2. Theoretical Issues*. Mahwah N.J.: Erlbaum Associates.

Tomasello, Michael. (2003). *Constructing a Language. A Usage-Based Theory of Language Acquisition*. Cambridge Mass.: Harvard University Press.

Traugott, Elizabeth C. (1972). *A History of English Syntax*. New York: Holt, Rinehart and Winston.

Vincent, Nigel. (1988). "La Posizione Dell'aggettivo in Italiano, 181-195". In *Tema-Rema in Italiano. Symposium, Frankfurt Am Main, 26/27-4-1985*, edited by Harro Stammerjohann. Tübingen: Gunter Narr.

---- (2001). "LFG as a Model of Syntactic Change". In *Time over Matter. Diachronic Perspectives on Morphosyntax*, edited by M. Butt and T. Holloway King,1-42. Stanford: CSLI Publications.

Wanner, Dieter. (2006). *The Power of Analogy. An Essay on Historical Linguistics*. Berlin: Mouton de Gruyter.

Warner, Anthony R. (1982). *Complementation in Middle English and the Methodology of Historical Syntax*. London: Croom Helm.

Weinreich, Ulrich, William Labov, and Marvin I. Herzog. (1968). "Empirical Foundations for a Theory of Language Change". In *Directions for Historical Linguistics*, edited by P. Lehmann Winfred and Yakov Malkiel, 95-195. Austin: University of Texas Press.

Woodcock, E.C. (1959). *A New Latin Syntax*. London: Methuen.

Wray, Alison. (1999). "Formulaic Language in Learners and Native Speakers". *Language Teaching* 32: 213-31.

Wray, Alison, and Michael R. Perkins. (2000). "The Functions of Formulaic Language: An Integrated Model". *Language and Communication* 20: 1-28.

Yngve, Victor H. (1996). *From Grammar to Science: New Foundations for General Linguistics*. Amsterdam: Benjamins.