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REVIEWS

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Douglas Biber, Stig Johansson, Geoffrey Leech, Susan Conrad & Edward Finegan, *Longman grammar of spoken and written English*. London: Longman, 1999. Pp. xxviii + 1204.

Reviewed by KEITH BROWN, University of Cambridge

The *Longman grammar of spoken and written English* (LGSWE) is based on data from a 40-million-word corpus of written and spoken British and American English, most of which was specially assembled for the project. The corpus represents four major registers (conversation, narrative fiction, newspaper writing and academic prose) and two smaller supplementary registers (non-conversational speech and non fiction).

The description is organised into five sections. Section A describes the structure of the corpus and the way it is used. The other sections are arranged along traditional, 'structural', lines. Section B lays down the basic grammatical framework: an overview of word, phrase and clause grammar. Section C describes the major word and phrase classes in more detail. Section D describes pre- and post-modification in the noun phrase, verb and adjective complementation, and adverbials. Section E looks at 'grammar in a wider perspective', examining a number of topics concerned with grammar 'at work' in texts: word order, 'stance', multi-word lexical expressions, and the grammar of conversation. It is easy to find your way around this grammar, the design and layout are clear and the tables of contents and indexes have the right amount of detail.

The 'grammatical framework of concepts and terminology' (viii) is largely, but not entirely, based on Quirk et al.'s *A comprehensive grammar of the English language* (CGEL). Like CGEL, its descriptive categories and structures are both formally and functionally defined and since the descriptive machinery of both grammars is compatible, the two grammars to some extent complement each other. CGEL is more detailed and has a more comprehensive coverage. LGSWE has more limited coverage but, by detailed examination of its corpus, goes well beyond CGEL in the 'exemplification and quantitative investigation of grammar across different language varieties' (viii) and, since it claims to be a grammar of use, of 'the linguistic patterns actually used by speakers and writers' (4).

Descriptions of the 'grammatical elements' cover their internal structure, distributional properties and semantic characteristics. So, for example, the

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description of word classes lists their morphological properties (adjectives are inflected for comparison); their distribution in functionally labelled structures (adjectives head adjective phrases, function as premodifiers in noun phrases and as predicatives in clauses); and the major semantic properties that affect grammatical structure (descriptive adjectives describe 'qualities' and are gradable; classifying, identifying and intensifying adjectives are normally non-gradable and not used predicatively). And similarly for other word and phrasal classes. Corpus findings provide frequency information and compare the distribution of the various items in different registers.

This is a 'descriptive' and not a 'theoretical' grammar. It correctly claims that it will be 'an important resource for investigating research questions' in functional linguistics and a wide range of other applied linguistic disciplines, including stylistics, dialectology, sociolinguistics, psycholinguistics and discourse analysis (45). As exemplified briefly above, the distributional properties of the 'grammatical elements' are described in 'flat' structures with functional labels in the manner of CGEL, rather than in 'hierarchical' configurational terms, and discussion tends to be focussed on distributional variation and commentary about their discourse function. Tree structures are called on only very occasionally and then only to clarify attachment problems (for example, to differentiate between predicate and clause adverbials on page 132) or to illustrate dependencies in complex structures (for example, to illustrate the difference between co-ordinate and embedded clauses on page 135). The functional terminology is clear and familiar but not pursued particularly rigorously: for example, 'corresponding to each type of lexical word, there is a major phrase type with the lexical word as head and a number of accompanying elements' (96); 'besides common nouns, noun phrases may also be headed by proper nouns, pronouns, and nominalised adjectives' (97); 'the term noun phrase … is frequently used more widely for any unit which appears in the positions characteristic of noun-headed structures (including clauses)' (97). Not all phrases are headed by lexical words: prepositions are classified as function, not lexical, words and the grammar recognizes prepositional phrases, though it is not clear whether these are headed by a preposition or whether they are to 'be viewed as a noun phrase extended by a link showing its relationship to surrounding structures' (103). The grammar also recognizes genitive and numeral phrases: genitive phrases are 'structures like noun phrases except for the addition of a genitive suffix [… which] marks a relation between two noun phrases in much the same way as a preposition' (108) and numeral phrases 'have special characteristics which makes it natural to treat them separately from noun phrases' (109). Unsurprisingly, there are no DPs, IPs or CPs: determiners are found in noun phrases as part of the premodification system of the noun and, as in CGEL, are classified as pre- and post-determiners, complex determiners and so on (§4.4); tense is a property of the verb and aspectual and modal

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auxiliaries are part of the verb phrase (§2.7.2); complementisers are subordinating conjunctions (85) and finiteness is a property of the clause. All this may not suit some contemporary theoretical preoccupations, but as a descriptive taxonomy it is clear, and clearly explained, and provides a relatively neutral framework for the discussion of a variety of descriptive problems, for example indeterminacy between word and other classes. Like CGEL before it, it will contribute to the empirical foundations essential for more theoretical investigations.

It has already been noted that the descriptive apparatus derives mostly from CGEL. It differs in some small terminological points, but the main differences stem from the attention the grammar pays to corpus findings on discourse and conversation: we will look at the treatment of word classes and clauses.

As noted above, LGSWE uses formal, functional and semantic criteria to distribute words into classes. It recognizes three general types of word (55 ff.): lexical words, function words, and inserts. Lexical words are 'the main carriers of meaning' (55), nouns, verbs, adjectives and adverbs. Function words are 'the mortar which binds the text together' (55), determiners, pronouns, auxiliaries and the like including, as noted above, prepositions. Inserts 'do not form an integral part of the syntactic structure, but are inserted rather freely [... and] characteristically carry emotional and interactional meanings' (56): they are subclassified according to their textual function as interjections, greetings, discourse markers of various kinds, attention signals and so on (93). This taxonomy works satisfactorily for all the text varieties in the corpus and, importantly, allows for a uniform description of them all, though there are of course substantial and significant distributional differences between the registers. Identifying a category of 'inserts' is a particularly useful innovation since it brings this varied class of words into the body of a description together with phrasal and clausal fragments instead of treating them as marginal. Units of this kind have been identified in descriptions for a long time as discourse markers, parentheticals, sentence fragments and the like. They occur in all registers; unsurprisingly, they are most frequent in conversation, which is notoriously full of partial and incomplete units, but they are also found in academic writing and in news. Bringing these diverse units together and treating them as a proper part of the grammar enables them to be given serious attention and will be helpful for those analysing other language registers characterised by fragments, syntactic blends and the like, for example, the language of poetry or the internet, and 'disfluent' language.

Sentences are not 'separately described, as it is debatable whether this notion is applicable to speech' (50). This insight is not original but is given powerful support by the corpus analysis in this grammar. Chapter 3, on the grammar of the clause, distinguishes between the clause, 'a unit structured round a verb phrase' (120), and 'non-clausal material', the inserts and

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fragments noted above 'that cannot themselves be analysed in terms of clause structure, and are not analysable as part of any other clause' (224). Identifying sentences in conversation is notoriously problematic, but it is interesting for it to be conclusively demonstrated that there is no difficulty in identifying clauses. It is also interesting that the analysis seems to work equally satisfactorily, and without loss of any significant generalisation, with written language. Clauses receive a fairly traditional analysis as independent clauses (simple sentences), largely dealt with in chapter 3, and subordinate clauses (complex sentences, etc.) in section D, noun complements in chapter 8, verb and adjective complements in chapter 9 and adverbial clauses in chapter 10. There is nothing particularly surprising about the grammatical analyses themselves and it is valuable to have statistics about the distribution of different types of complement clause, commentary on their textual functions and the useful sections detailing which nouns, verbs and adjectives take which forms of complementation. Some points of the analysis are, however, puzzling and the coverage is somewhat spotty. Since there is so much in common about the syntax of all complement clauses, it is not entirely clear why noun complement clauses are treated separately as noun postmodifers (604) whereas verb and adjective complement clauses are treated together as 'a type of dependent clause used to complete the meaning relationship of an associated verb or adjective in a higher clause' (658). Similarly, while there is a useful discussion about extraposed clauses, it is not clear why in *It's a wonder that …* it is 'the copula *be*, functioning as a predicate in combination with various noun phrases' (670), which takes an extraposed *that*-clause, when on the next page we find that in *It's nice that …* it is the adjective that controls the extraposed *that*-clause. As to the coverage, there are many types of subordinate clause that are mentioned but hardly discussed: there is, for example, not much on embedded subjunctives, concessive constructions are mentioned but hardly discussed and it would have been nice to see more on conditionals and relative clauses. It seems that this situation comes about partly because the constructions in question are insufficiently frequent in the corpus for them to yield significant statistical results and partly because so much of the available space is devoted to corpus findings on the more frequent constructions that there is little room for 'minor' constructions like these.

Descriptive sections are followed by 'corpus findings' in a standard sequence of expository subsections: first the 'raw' data, often in tables, showing the frequency of the relevant items in the different registers. These 'observed patterns of use' are then subjected to functional interpretation, involving 'the work that a feature performs in discourse, the processing constraints it reflects and the situational or social distinctions that it conventionally indexes' (41). The findings generally give an empirical reinforcement to traditional analyses, for example, of pre- and post-modification in noun phrases (chapter 8), the distribution of different types

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of complement clauses (chapter 9), the distribution of relativizers (§8.7.1) and many others. Usually the discussion focusses on differences between the registers and it is instructive to see that all the four registers yield to the same kind of analysis. Many of the distributional findings reveal facts that are simply not available to intuitive judgements. Some are a surprise: page 506, for example, shows the distribution of attributive and predicative adjectives across registers, revealing that attributive adjectives are more frequent than predicative in academic prose, reflecting 'the heavy reliance on noun phrases to present information' (506), and predicative and attributive adjectives are roughly equal in frequency in conversation 'in keeping with the general reliance on clausal rather than nominal presentation of information' (507). Others are more predictable: the fact that imperatives and interrogatives are more common in spoken than written language or that newspaper reports contain fewer first-person pronouns than conversation would seem to be accounted for by general principles of the different registers – conversation is interactive in a way that academic prose is not – the finding is unsurprising but it is useful to have it empirically confirmed. The analysis also provides much useful lexico-grammatical information on, for example, semantic classes of verbs and their syntactic patterning, on tense restrictions on classes of verbs, or on verbs, adjectives and adjectives that take sentential complements, together with a helpful discussion of the function of these complements. In a few cases we seem to have statistics for the sake of statistics: so, for example, pages 512–513 have a list of the commonest attributive adjectives across registers and page 561 has a list of the commonest adverbs followed by a commentary that describes their distribution.

Section E, 'Grammar in a wider perspective', is the most original section and is particularly interesting because it carries the analysis from the clause into text. Chapter 11 is 'concerned with the way clauses are adapted to fit the requirements of communication' (896) and deals with the way speakers/writers use grammatical means, usually involving word order, to control the flow of information, create focus, contrast and the like. There are sections on fronting, inversion of various kinds, the passive, existential *there* and clefting, and since all these processes are brought together, it is possible to see similarities and differences between them.

Chapter 12 pulls together sections throughout the book on ways in which grammatical devices are used to express personal evaluations, judgements and attitude, summarized here as 'stance' and referred to elsewhere as 'subjectivity', 'point of view' and the like. The chapter is a good summary of types of stance and the many grammatical systems (perhaps all) that can be exploited to express this, and demonstrates very clearly that semantics, pragmatics and syntax all interact closely in communication. The corpus analysis shows how pervasive stance markers are, and how surprisingly common they are even in academic prose.

Chapter 13 is a detailed account of 'lexical bundles'. These are

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distinguished from idioms and from 'multi-word expressions that function as a structural unit' (988), like phrasal and prepositional verbs. Although they are a kind of collocation, 'associations between lexical words that occur more frequently than expected by chance' (988), they are different from the familiar kind of collocation like *obvious difficulty/challenge/fact*, etc. 'Lexical bundles' are the ready-made phrases which constitute so much of communicative behaviour, 'sequences of word forms that commonly go together in natural discourse' (989). They occur in all the registers: examples in speech are … *do you want me to* … or … *I don't know what* …, and in academic discourse … *in the case of the* … or … *there was no significant* … . As the examples demonstrate, lexical bundles are not idioms and need not be complete structural units. They are, however, extraordinarily common. It is claimed (995) that almost 45% of the words in conversation and about 21% of the words in academic prose occur in a recurrent lexical bundle. It is important to have this brought to our attention.

Chapter 14 is an account of conversation. The big surprise here is that there is nothing on intonation and paralinguistic features, which are not transcribed in the corpus. It is difficult to be entirely reassured by the claim that 'this makes comparatively little difference' (1042). This may be true of some of the more obviously 'grammatical' features of conversation, but is surely not true of many 'pragmatic' features, perhaps especially those affecting 'stance'. Intonation has been well studied: CGEL, for example, has good accounts of, inter alia, the intonation of tag questions and the way it is used in negation and it seems a pity that LGWSE does not make use of this. With this reservation, this is a good and balanced chapter. An impressive and comprehensive introduction sets the scene by summarising the salient properties of conversation – that it is interactive, takes place in a shared context and in real time, that it is expressive of attitude and the like. This is followed by a section on 'performance phenomena', hesitation, repetition and various kinds of disfluency. Next are sections on 'the constructional principles of conversation' (§14.3) and on a variety of topics in conversational grammar. Here the analysis of the earlier part of the grammar comes into its own. The identification of the clause and the distinction between clausal and non-clausal material is exploited in discussing and exemplifying 'prefaces, bodies and tags' and a variety of 'inserts', interjections, discourse markers, response forms, etc. Here the corpus has interesting material on differences between British and American English conversation. The section poses the question 'Is there a distinctive grammar of spoken language, operating by laws different from those of the written language?' (1038). The reassuring answer is that 'the same "grammar of English" can be applied to both the spoken and written language' (1038). That is, that conversation is not discontinuous from other forms of language use. It is, to be sure, different functionally and in terms of the frequency distribution of grammatical units. There are also some features to be found only, or practically only, in

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conversation. But all this turns out to be true of the other registers too. Novels, for example, are in many respects more like conversation than they are like academic prose. This should give pause for thought to those who claim a simple distinction between 'spoken' and 'written' language.

The introduction claims that this is 'a truly corpus based grammar' (viii) and this is right: even the bibliography limits itself largely to corpus-based works. This approach has strengths and weaknesses. The main strength is that it is hard to imagine how the frequency and distributional information so abundant here could be provided in any other way than by careful corpus analysis, and LGSWE is very good at this. The main weakness is that corpus analysis is not the same as linguistic analysis. Discussions of the corpus findings obviously, but silently, draw on insights developed over the years, in CGEL for example, and the discussion would often be stronger if non-corpus-based insights were more readily acknowledged and incorporated. The most egregious example is the failure to consider intonation mentioned in the previous paragraph, but there are others: the treatment of tense, aspect and modality, for example, where the grammatical analysis is less impressive than the material on frequency and distribution. It is difficult to see any good reason for not making use of non-corpus-based accounts in these and similar cases. To end on a positive note, this is an important book, demonstrating very clearly the significant contribution corpus linguistics can make to grammatical description.

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Noel Burton-Roberts, Philip Carr & Gerard Docherty (eds.), *Phonological knowledge: conceptual and empirical issues*. Oxford: Oxford University Press, 2000. Pp. x + 352.

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To the practitioners of other linguistic disciplines, phonology sometimes appears as a highly unified field where a shared body of ontological and epistemological assumptions makes cumulative progress possible (see e.g. Newmeyer 2000: section 1.1). The volume under review provides a most

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effective antidote to this misconception. It consists of an introductory essay by the editors followed by eleven specially commissioned chapters, two of which (the papers by Bromberger & Halle and Hale & Reiss) are revisions of earlier publications. The contributors – whose varied backgrounds include generative phonology, generative syntax, laboratory phonology, phonetics, psycholinguistics, sociolinguistics and philosophy – were all asked to reflect on the nature of phonology, and on the place and status of knowledge of linguistic sound patterns in relation to linguistic knowledge and nonlinguistic phenomena in general. The outcome of this exercise reveals a shocking lack of agreement: no consensus emerges as to what constitutes a phonological fact, what methods should be used for finding such facts, what theoretical entities should be postulated to account for them, and how such entities relate to those posited in other disciplines. Despite Newmeyer's (2000: section 1.1) rosy depiction of research practice in phonology, however, this result is not surprising: the relationship between the linguistic and the nonlinguistic remains the most contentious problem in contemporary linguistics, and its attendant perplexities are most acutely manifested in the problem of the connection between phonology and phonetics.

The volume has appeared at a specially opportune moment. During the past ten to fifteen years, several factors have conspired to exacerbate the controversy surrounding the status of phonology and its relationship with phonetics, particularly within the generative paradigm:

(i) With the rise of the Minimalist Program, mainstream generative syntax has witnessed a radicalization of the autonomy thesis: syntax is conceived of as wholly innate, strictly universal, and hermetically encapsulated. If this view is accepted, then the status of phonology within the generative theory of language becomes problematic, for it is clear that phonology will not share the radically autonomous nature attributed to syntax. In the volume under review, the papers by Noel Burton-Roberts ('Where and what is phonology? A representational perspective', 39–66) and Philip Carr ('Scientific realism, sociophonetic variation, and innate endowments in phonology', 67–104) directly grapple with this issue, which is also addressed in an appendix to the chapter by Janet Pierrehumbert, Mary E. Beckman & D. R. Ladd ('Conceptual foundations of phonology as a laboratory science', 273–303).

(ii) Nearly simultaneously, Optimality Theory (OT) has *de facto* become the dominant framework in generative phonology; but, by highlighting the grounded nature of many phonological constraints, the OT enquiry has further undermined the autonomy of phonology. The chapter by Mark Hale & Charles Reiss ('Phonology as cognition', 161–184) constitutes a violent reaction against this development.

(iii) Finally, experimental research has steadily eroded the empirical basis of generative phonology. In particular, a surprising amount of low-level nondiscrete sound patterning has been shown to be language-specific and

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therefore learnt, rather than the product of a 'universal phonetics' as assumed in SPE (Chomsky & Halle 1968). Gradient language-specific phonetics has also expanded at the expense of categorical phonology, as a host of phenomena previously thought to be discrete have proved upon experimental examination to involve continuous variables. This shift in the empirical foundations of phonology has accelerated dramatically since the first Conference in Laboratory Phonology was held in 1987. In the volume under review, the following articles rely on laboratory techniques: Pierrehumbert et al., which focuses on the nature of the laboratory phonology enterprise; Gerard Docherty & Paul Foulkes ('Speaker, speech, and knowledge of sounds', 105–129), which focuses on sociophonetic variation; Jennifer Fitzpatrick & Linda R. Wheeldon ('Phonology and phonetics in psycholinguistic models of speech perception', 131–160), which focuses on word recognition; Scott Myers ('Boundary disputes: the distinction between phonetic and phonological sound patterns', 245–272), which focuses on the distinction between categorical and gradient patterns; and Marilyn Vihman & Shelley Velleman ('Phonetics and the origins of phonology', 305–339), which focuses on the developmental study of phonological acquisition.

The chapters are arranged in alphabetical order by first author's surname. In the introduction (1–18), however, the editors do an admirable job not only of summarizing the content of each paper, but also of identifying and highlighting recurrent themes, and of locating each contribution within a taxonomy of approaches to the nature of phonology. In what follows I shall discuss a few of the leitmotifs running through the book. In the allocation of emphasis I will inevitably be guided by my own interests and concerns; this is the only practical expedient for a reviewer faced with a volume of such intellectual richness.

Several chapters touch upon the relationship between theory and data in the phonological enterprise. Docherty & Foulkes and Pierrehumbert et al. criticize research practice in mainstream generative phonology: they suggest that generative phonologists all too often advance far-reaching hypotheses on the basis of impressionistic evidence, fail to state the precise conditions under which those hypotheses would be falsified, and generally retain them in the face of major empirical difficulties. Carr redresses the balance by pointing out that generative practice can be exemplarily Popperian (74–75): bold (and, by the same token, potentially revelatory) conjectures are put to the test and, when clearly falsified, are discarded. Kiparsky provides a commendable example: in Kiparsky (1993) he resolutely abandoned the Strict Cyclicity Condition, despite having personally invested large amounts of research effort in it during the 1980s.

In contrast, Pierrehumbert et al. lay considerable emphasis on operational definitions and ancillary instrumental theories (281–282); they claim that

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agreement on their rôle binds the community of laboratory phonologists together, enabling progress. In this connection, however, Carr observes that such tools do not by themselves deliver a coherent account of the architecture of phonology and its place in human cognition (84). The point is well taken. Pierrehumbert et al., for example, assert that '[i]n so far as we know the denotation of the term "syllable", it is provided by work such as Bell & Hooper (1978), Derwing (1992) and Treiman et al. (2000)' (282). But, surely, having criteria for delimiting syllables in surface representations and assessing their relative phonotactic acceptability falls short of elucidating the rôle of syllable structure in phonology. Relatedly, Eysenck & Keane (1995: 466) note that in cognitive psychology a long experimental tradition has frustratingly failed to deliver overarching cognitive architectures. Pierrehumbert et al. defend the epistemological stance of laboratory phonologists with arguments from philosophers of science such as Laudan and Hull; in this they contrast with Carr's invocation of classical Popperianism, and with what they regard as the Kuhnian glamourizing of conceptual upheavals in linguistics (275). This shows that, whilst the philosophy of science can play a useful rôle in clarifying the intellectual position of individual linguists or schools, there is little chance of its being applied normatively so as to regulate the conduct of research.

Also worthy of comment is Docherty & Foulkes' specific complaint about the neglect of sociophonetic variation in the generative tradition. It is true that, insofar as variation is language-specific and therefore learnt, phonological theory must make provision for it. In addition, phonology must accommodate the fact that variation along certain dimensions may be – and often is – socially evaluated. Nonetheless, Docherty & Foulkes fail to justify their claim that specifically sociolinguistic variation is as revealing in respect of the nature of phonological representations and processes as lexical contrast. In particular, they do not address the observation that no particular dimension of variation can be predicted to bear social evaluation in any given language, given the fact that the link between linguistic variables and social values is culturally contingent.

Another thread in the fabric of the volume is the debate concerning the relationship between gradient and categorical sound patterns. In this area, the opinions voiced by some of the contributors are informed by their adoption of exemplar-based models of phonology, where lexical representations (and indeed phonological knowledge in general) inhere in the multiple traces of phonetic events stored in a pattern-associating memory. Exemplar-based models are explicitly endorsed by Docherty & Foulkes and Pierrehumbert et al. The latter specifically claim that categorical phonological patterns have no independent existence, but emerge from nonlinearities in continuous phonetic patterning (283–289). Vihman & Velleman state their position in more guarded terms. They highlight discontinuities in the phonetic behaviour of young children which, in their view, reflect the onset

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of phonological organization superimposed upon ongoing phonetic learning. However, they do not state whether such discontinuities involve the rise of discrete symbolic generalizations, or rather nonlinearities in the behaviour of neural networks.

It appears, though, that exemplar-based models, with their attendant dissolution of the gradient/categorical dichotomy, have not yet gained complete ascendancy among experimentalists. Notably, Myers mounts an admirably lucid defence of the classical modular approach, which assigns categorical patterns to the phonology proper and gradient patterns to a phonetic implementation module. First, Myers establishes that the choice between categorical and gradient analyses is experimentally decidable. Comparing two tonal rules in Chichewa, he shows that phrase-final high-tone retraction must operate categorically, since phonetic functions governing the timing of F_0 peaks perform measurably better if the high tone is assumed to shift phonologically onto the phrase-penultimate syllable (249–252); in contrast, a putative rule of forward high-tone spread proves to be an artifact of F_0 peak delay in syllables of relatively short duration. Having accomplished this, Myers then turns to a very useful checklist of diagnostics for categoricalness and gradience (259–267).

Fitzpatrick & Wheeldon argue for a psycholinguistic model of spoken-word recognition where, at an initial stage, discrete features are extracted from the acoustic stream and directly mapped onto an underspecified lexicon. Interestingly, a subsequent stage in the recognition process involves the grammar (conceived very much in orthodox generative terms) assigning full phonological representations to activated lexical entries.

A third strand of argument in the volume concerns the substantive content of phonological representations and the phonetic grounding of phonological patterns. The chapters by Sylvain Bromberger & Morris Halle ('The ontology of phonology (revised)', 19–37) and John Harris & Geoff Lindsey ('Vowel patterns in mind and sound', 185–205) uphold the traditional position that distinctive features, though internal mental objects, have substantive phonetic content: articulatory in the case of the binary features assumed by Bromberger & Halle (in the SPE tradition), auditory in the case of the vocalic elements with stand-alone interpretability proposed by Harris & Lindsey. Ontologically, Bromberger & Halle describe features as constituting 'mnemonic elements' (30) in the context of underlying representations and 'phonetic intentions' (25) at surface level; for Harris & Lindsey features are 'auditory images' (195, 203).

In contrast, Hale & Reiss defend a more emphatically autonomist position in their article, which, alongside the expected tirades against OT, includes a striking repudiation of chapter 9 of SPE (168–169). Hale & Reiss acknowledge that, through the mediating rôle of transducers, phonological representations enter into nonarbitrary relationships with phonetic events; but they insist that the computational system treats features like arbitrary

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symbols. Skilfully wielding Occam's Razor, Hale & Reiss argue that the grammar should not incorporate markedness statements, which redundantly duplicate the effects of processes of misacquisition and change caused by phonetic factors. This highly effective argument constitutes a valuable contribution to the debate surrounding OT: as Hale & Reiss show, it is incumbent upon the proponents of OT to demonstrate that an adequate account of phonological acquisition and change cannot dispense with cognitive representations of markedness.

Other aspects of Hale & Reiss's thought-provoking chapter are far less successful. When it comes to identifying phonological facts whose explanation lies in arbitrary formal properties of UG, they have nothing of interest to say (177–179). At other times, they are borne aloft on the tide of their own shrill rhetoric to transparently fallacious conclusions. Thus, they suggest that a hypothetical 'disfunctionalist' version of *Con* made up of constraints promoting unfaithfulness (OBFUSCATE) and markedness (NOPAIN–NOGAIN) would produce the same effects as standard faithfulness and markedness constraints. This is plainly not the case: the ranking OBFUSCATE ≫ NOPAIN–NOGAIN, for example, predicts a system where the most marked feature occurs everywhere (neutralizing underlying contrasts) except where it would faithfully realize input specifications.

Harry van der Hulst ('Modularity and modality in phonology', 207–243) arrives at a similar conclusion: phonological entities have no intrinsic content. He argues for this view on the grounds that phonological theory must accommodate both spoken and signed languages, and so the elements of phonological representation cannot be modality-specific; *a fortiori*, they cannot have phonetic substance. Van der Hulst claims that phonological representations consist of the abstract primes 'C' ('dependent') and 'V' ('head') arranged in hierarchical structures governed by the same principles as syntactic objects (e.g. X-bar theory). However, without hard empirical constraints on the possible phonetic instantiations of these structures, van der Hulst's intriguing idea runs the risk of degenerating into Procrustean slot-filling: any segment type that is handy can be claimed to instantiate some configuration of abstract primes.

Both Hale & Reiss and van der Hulst can be read as reacting to the anomalous position which a phonology endowed with phonetic substance occupies in the Chomskyan paradigm, where autonomy is the hallmark of linguistic knowledge. A clear diagnosis of this problem is provided in Burton-Roberts' chapter, which sets the frame of reference for the entire volume. Burton-Roberts draws a fundamental distinction between two conceptions of Language and of UG. In the 'generic' conception, Language is a theoretical construct incorporating the properties shared by all particular languages; as an abstract type, Language does not exist independently of the tokens by which it is instantiated. In the 'realist' conception, in contrast, Language is (in Chomsky's terms) 'a real object of the natural world': an

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innate module of mind present in all humans. Burton-Roberts shows that, under 'realist' assumptions, Language cannot contain a phonological component, for phonological systems are conventional, language-specific, and learned. What, then, should be the status of phonology in the Chomskyan paradigm? In answer to this question, Burton-Roberts proposes his Representational Hypothesis: phonology consists of a set of cultural conventions for the external representation of linguistic (syntactic-semantic) objects, the latter being nonconventional and strictly internal; phonology, in short, exists outside UG. This claim is staked on purely conceptual grounds, and takes for granted a radical version of the autonomy thesis (see above); if its premises are accepted, however, the Representational Hypothesis offers an effective solution to some of the tensions that riddle the Minimalist Program.

Developing the implications of the Representational Hypothesis, Carr asserts that phonological acquisition must be conceived of in thoroughly Empiricist terms (85ff.). This poses intriguing questions. Carr does not openly reject opaque generalizations, which hold at relatively abstract levels of representation but not on the surface, nor does he explicitly deny the existence of covert phonological structure lacking phonetic exponence. In the absence of a phonological component to UG, however, both opacity and covert structure raise Plato's Problem. Carr holds that an Empiricist position is compatible with the deployment of nontrivial innate cognitive capacities in phonological acquisition, provided that such capacities are not specific to language; this would include, for example, the ability to normalize and idealize sensory stimuli (88). In Fodorian terms, however, it is hard to see how such merely 'horizontal' faculties could be instrumental in the acquisition of opaque processes and covert representational structure. An intriguing possibility which Carr does not raise is that phonological acquisition commandeers the resources of nonlinguistic 'vertical' faculties or modular 'input analysers': one could, for example, suggest that the acquisition of metrical structure relies on the contribution of a nonlinguistic rhythm module that is domain-specific but cross-modal.

All in all, I would not recommend this book to persons of delicate sensibilities or impressionable disposition: for the uninitiated, the contributors' bewildering variety of opinion and their often acerbic tone may prove disconcerting. Colleagues will probably find that the volume delights and infuriates, enlightens and obfuscates, in equal measure. It is a microcosm of the loose federation (one dare not say community) that concerns itself with linguistic sound patterns.

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William Croft, *Explaining language change: an evolutionary approach* (Longman Linguistics Library). London: Longman, 2000. Pp. xv + 287.

Reviewed by DAVID W. LIGHTFOOT, Georgetown University

William Croft offers another 'evolutionary approach' to language change. There have been many. Nineteenth-century linguists formulated sound changes and sought to explain them as instantiating general directions to change: Rask (1818) thought that there was a built-in tendency for languages to become simpler. Schleicher (1848) identified a progression from isolating to agglutinating to inflectional types, although this was said to hold for preliterate societies while Rask's drive to simplicity was relevant for literate societies. Darwin (1874) followed Max Müller in seeing languages progressing to shorter, easier forms. This historicist paradigm – the notion that there were laws of history to be discovered, which would account for a language's development – was largely abandoned in the early twentieth century. Indeed, there was a virulent anti-historicism among some of the early structuralists (Lightfoot 1999: 40–41). However, the typologists of the 1970s used Greenberg's harmonic universals to resume the search for universal diachronic trajectories and they were no more successful (Lightfoot 1999: chapter 8).

The desire to predict how languages change is, though, resilient and recent work has turned to biology. Bauer (1995) follows the evolutionary approach of Bernard Bichakjian, whereby the direction of change is rooted in human biology: languages evolve in the direction of features that are acquired early.

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Other evolutionary approaches invoke natural selection to predict the direction of change. This is true of Deacon's (1998) e-language based approach, for example. For him, languages exist out there, extra-human entities, and they have evolved in a 'flurry of adaptation' (109) that has 'been going on outside the human brain' (109). They have become 'better and better adapted to people' (107) and attach themselves to children like viruses. For Deacon, languages have evolved, not brains. People have not changed but languages have, and they have changed selectively, but the selective principles driving language change are purely stipulative. Haspelmath (1999) also invokes adaptation gratuitously. He argues that certain languages become 'optimal' in some fashion, and pleads that this must reflect adaptation and natural selection, but he offers no argument. The simple fact that a language is 'optimal' in some way does not automatically implicate natural selection. But Haspelmath goes even further and sets up universal opposing tendencies, so that any change is adaptive with respect to one of these. For good discussion, see Dresher & Idsardi's (1999) commentary.

Croft's new evolutionary approach also uses selection but in a different way: speakers select utterances. He follows Dawkins (1976) on biology and Hull (1988) on scientific development in adopting a general theory of selection, and he applies it rather literally to language, seeking equivalents to genes, DNA and other biological constructs. The analogies often raise more questions than they illuminate, particularly with regard to the role of selection. He takes the speaker as the unit of selection and linguistic structures that can be inherited in replication as the units of replication; these are 'linguemes' (28), analogous to Dawkins's memes. They have linguistic structure and are parallel to the gene as the basic replicator in biology. Utterances are equivalent to DNA (12).

> The equivalent to alleles of genes are VARIANTS of a lingueme, that is, alternative structures used for a particular structural element, such as alternative phonetic realizations of a phoneme, alternative words for the same meaning, or alternative constructions used to express a complex semantic structure such as comparison. (28)

Linguistic innovations emerge from the remarkable complexity of communication in social interaction. 'Once innovations occur, they are propagated through the equally complex social structures of the speech communities we participate in' (cover blurb). This is where Croft's Theory of Utterance Selection comes in, and 'utterance selection is the primary locus of language change' (30): change happens when speakers select different utterances. He presents four major 'theses', summarized in chapter 1 and never stated precisely:

1. Altered replication results from the gradual establishment of a new convention through language use. An example of a convention is placing

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a *wh*-phrase at the front of a sentence, something that others might see as a property of mental grammars.
2. Altered replication results from speakers adjusting the mapping from language structure to external function, what others might see as interface requirements, e.g. 'the mapping from phonological structure to phonetic reality' (8).
3. Mechanisms for propagation of change are social, the kinds of things discussed by sociolinguists.
4. External (contact-induced) sources of language change become more similar to internal sources once one recognizes that all speakers command multiple varieties or codes.

The theory lacks specification but it is comprehensive and Croft tackles many aspects of linguistics: grammatical change, pragmatics, social variation, language contact and 'genetic linguistics'. However, the reader must struggle to understand. Croft emphasizes a distinction between a linguistic system (2) and a mental grammar, but I cannot give a coherent account of that distinction. He discusses a wide range of literature and sometimes gives accounts which defy my reading. For example, on page 10, he characterizes Chomskyan linguistics as taking a 'literal approach' to an evolutionary model of language change: it claims 'a biological basis for the universal properties of languages' and therefore, astonishingly, that 'differences among languages reflect genetic differences among their speakers', which is, Croft notes, 'patently false'. Elsewhere he discusses the notion of UG parameters, so here and elsewhere I alternated between understanding assent and befuddlement.

There is much to agree with. Croft adopts a social view of languages (chapter 2), analogous to population definitions of species (17), healthily different from Deacon. Children are exposed to a population of 'utterances in a speech community' (26), since there is no coherent definition of a language. Furthermore, a grammar is a 'real, individual, psychological entity' (27), a 'cognitive structure in a speaker's mind that contains her knowledge about her language' (26). All of this is plausible enough. He distinguishes himself from Kirby (1997), Haspelmath (1999) and others who take functional explanation of language change to be analogous to adaptive explanations in evolutionary biology (39). Furthermore, Croft's notion that it is linguistic structures which are the units of replication seems rather similar to ideas about cue-based acquisition: that children scan the mental representations resulting from understanding utterances and identify elements of grammars, the cues (Lightfoot 1999).

If this is a correct reading of Croft, then one can ask what the biology talk amounts to for the purposes of the linguistic analyses. Croft assumes that there is a UG from which particular grammars may be drawn by children. In his terminology, children SELECT linguemes, hence grammars, on exposure to

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elements of linguistic structure. This selection terminology has nothing to do with adaptive accounts of natural selection, but it is a way of seeing the child, plausibly, as an agent in the acquisition process, very different from Deacon's children. Under this view, changes emerge as children are exposed to different triggering experiences and that happens through social change of some kind. Others have tried to describe changes in terms of changes in parameter settings, changes in the distribution of cues, or in any of the myriad e-language based accounts. For Croft, linguistic selection is governed by social forces, and this is what the book seems to be about. Croft has little interest in the properties of UG or even in the properties of grammars, which for him are 'conventions', as noted. So one turns to the case-studies to see what is offered.

He discusses variation in childhood experience such that it triggers new grammars at certain points. He is surely right to discuss this in social terms, but there is nothing particularly innovative here, and the social factors are not spelled out. Croft is keen on taxonomies and he classifies types of grammatical change: hyperanalysis, hypoanalysis, metanalysis and cryptanalysis (chapter 5). They are changes in conventions, and Croft treats some familiar cases: the loss of governed oblique case (121–124), instances of grammaticalization (156–165), etc. The case studies offer only a rudimentary re-statement of a few facts, no analysis. For example, on the loss of governed oblique case, Croft points out that in earlier Russian certain verbs assigned genitive, dative, or instrumental case to NPs that they governed, whereas some of those verbs now assign only accusative case, an instance of 'form-function reanalysis' (121) and 'hyperanalysis of the oblique case' (123). Something similar happened in Germanic and Icelandic. Semantic irregularity and semantic overlap constitute conditions for hyperanalysis (122). No analysis is offered, there is no discussion about the relevant sociolinguistics, and discussion is concluded in less than three pages.

I am left not knowing what the triggering experiences consist of: are the linguemes unanalyzed e-language expressions or are they abstract structures? If so, what? The analyses are too sketchy to tell. The changes reflect social variation, to be sure. In none of the case-studies can I see what precisely has changed in such a way that a different grammar is selected, nor what the relevant social factors were. Even less can I see why the changes vary in some direction. We are given no idea, and one is left wondering where the 'evolutionary approach' of Croft's subtitle is. He tells us that 'the proper objects of the study of language are actually occurring utterances and an actual speaker's knowledge about her language' (229), a short list which does not include UG, social factors or anything about diachronic trajectories. He concludes his book by saying

> The evolutionary framework for language – EVOLUTIONARY LINGUISTICS – will require a rethinking of the model of grammatical knowledge

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> developed by grammatical theory. The result of that rethinking will look quite different from most contemporary grammatical theories (see Croft to appear). (232)

Maybe the future work will have the details but the sketchy case studies here look rather conventional. We have taken a difficult and somewhat belabored tour through some rather suspect analogies with Dawkins and Hull and learned nothing new about language change, least of all what Croft's new EVOLUTIONARY approach is.

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Joseph H. Greenberg, *Indo-European and its closest relatives: the Eurasiatic language family*, vol. 1: *Grammar*. Stanford, CA: Stanford University Press, 2000. Pp. xiv + 326.

Reviewed by DON RINGE, University of Pennsylvania

One is seldom asked to review a book that proves to contain nothing of value, but that is unfortunately true of this volume (hereafter IECR).[1] In what follows I will briefly discuss some of the book's fatal shortcomings. The list is NOT exhaustive; valid objections to IECR can be multiplied almost *ad libitum*.

After a brief overview of previous work seeking to demonstrate a relationship between Indo-European (IE) and other language families (1–23), IECR contains two substantive chapters, one discussing 'Some aspects of the comparative phonology of Eurasiatic' (24–60), the other offering 'Grammatical evidence for Eurasiatic' (61–239). There is also an appendix on 'Ainu vowel alternations' (241–277), a table of classification (279–281), and the usual back matter.

Astonishingly, most of the chapter on comparative phonology discusses alternations which are PHONETICALLY similar in different families; Greenberg makes no attempt to find alternations which exhibit parallel distributions or similar grammatical functions. For instance, the widespread word-final alternation between stops and nasals in Eskimo languages is compared to a few isolated Aleut facts, the Uralic 2sg. suffix *-t/-n*, and 'a few other scattered traces of a similar alternation elsewhere' (31). Since regular sound change is much more likely to obscure the phonetic content of alternations than their distributions, and since Greenberg proposes to recover information about a prehistoric language much older than Proto-Indo-European (PIE), the strategy of this chapter seems misconceived. The plausible alternative explanation of purely typological similarity is not even mentioned.

One might hope that the chapter on grammatical evidence would attempt to reconstruct at least one coherent paradigm or inflectional system for the protolanguage (cf. Goddard 1975). Instead we find a list of 72 morphemes, some bound and some free, with their supposed reflexes in the various daughter languages, including three different first-person markers, three second-person markers, five plural markers, three absolutive markers (and two accusatives – surely not part of the same proto-system?), two datives, six

[1] I am grateful to Bill Poser for helpful discussion of this review, especially of the mathematical arguments. Responsibility for its contents rests solely with me.

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locatives, four negatives, and five interrogatives. A well-known critique of Greenberg's earlier work applies to this chapter as well: 'comparisons are almost all of isolated morphemes, not substantial portions of paradigms' (Poser & Campbell 1992: 217). In effect, the chapter is yet another example of the multilateral comparison of lexical items and is vulnerable to all the criticisms that have been levelled at earlier examples. In particular, the combinatorics of multilateral comparison ensure that as more and more languages are added to the comparison the probability of chance resemblances continues to rise, until it becomes practically impossible to demonstrate that the similarities one has found are in fact greater than would be expected by chance (Ringe 1999). As in all his previous publications, Greenberg simply ignores the problem; it seems clear that he just did not understand it.

Moreover, chance resemblances between items of the kind that Greenberg lists in this chapter are especially likely, because the items are very short (a large majority consist of a single segment each), the formal criteria for a match are lax – that is, exact sound correspondences are not required – and the range of meanings or functions of the supposed reflexes in the daughter languages is wide. For instance, under 'interrogative K' are listed interrogative pronouns (and forms plausibly derived from them, such as interrogative adverbs), indefinite pronouns, suffixes used to form indefinites from interrogatives, sentential interrogative markers, relative pronouns, and coordinating conjunctions; under 'locative M' we find endings for locative, dative, and instrumental cases, as well as prepositions, derivational suffixes of various kinds (forming nouns of location, adverbs of time, nouns of instrument, etc.), and nouns meaning 'earth' and 'place'.

The number of linguistic units compared is also effectively larger than appears at first glance because, instead of comparing protolanguages, Greenberg 'reaches down' into the established families to find items for comparison which are actually attested only in particular subfamilies. Ostensibly only twelve uncontroversial linguistic units are being compared, namely IE, Uralic, Yukaghir, Turkic, Mongolian, Tungus, Korean, Japanese, Ainu, Gilyak, Chukotian and Eskimo-Aleut. (Etruscan is claimed to be a member of the Eurasiatic family (22–23), but Etruscan material is not used in the chapter on grammatical evidence.) But Turkic, Chukotian and Eskimo-Aleut are deeply divided into two subfamilies each, and since Greenberg feels entitled to use comparanda found in only a single subfamily, we must add three to the number of units compared. Further, Uralic and IE are each split into a number of divergent subfamilies, and it seems clear that we must add another dozen units to account for that fact. The result is that the effective number of families compared is not 12 but about 27.

To understand what these numbers mean, consider a search for potential cognates exhibiting any of five meanings or functions in any of the 27 linguistic units under investigation. Whatever word of the first language

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examined we choose to compare, we will have five chances to find a suitable word for our 'cognate set' in the second language (or rather at least five, since synonyms do occur). In the third language we will also have five chances; and since it does not matter whether the meaning of a 'cognate' found in language #3 is the same as that of a 'cognate' found in language #2 or #1 (so long as it's on the list of five acceptable meanings), there are 25, or 5^2, potential word-matchings – 25 chances to find a 'cognate set' in terms of which words are chosen. For the same reason, adducing a fourth language will give 125, or 5^3, chances to find a 'cognate set' in terms of words chosen, and so on as languages are added. If we are comparing 27 languages we have 5^{26}, or about 1.49×10^{18}, chances to find a set; even if only 12 languages are compared, we have 5^{11}, or 48,828,125, chances to find a 'cognate set' in those terms. Nor can the method be salvaged by requiring relatively large 'cognate sets' because as the size of the sets increases the number of chances to find a set in terms of the LANGUAGES chosen also explodes. Let K be the number of languages compared, N be the number of acceptable meanings exhibited by the members of a 'cognate set', and L be the size of the set required minus the member from language #1 (which is given). The number of different ways to construct a cognate set both in terms of the words chosen and in terms of the languages chosen will then be given by the expression

$$\binom{K-1}{L} \times N^L$$

(where the first expression, read 'K minus 1 choose L', is the number of different ways of choosing L members from a total of $K-1$). Let us set K at 27 and N at 5, as above. For a two-member 'cognate set' L is 1, and since there are 26 ways to choose a single language out of 26, there will be 130 (i.e. 26×5) ways to obtain a two-member set. For a three-member 'cognate set' L is 2, there are 325 ways to choose two languages out of a total of 26, and there will be 8,125 ways to obtain a three-member set (325×5^2). For a four-member 'cognate set' L is 3, there are 2,600 ways to choose three languages out of 26, and the number of ways to obtain a four-member set will be 325,000(!) – and so on. There are actually more ways to construct a 22-member 'cognate set' (more than 35 quintillion!) than a set of any other size. It should be obvious that, unless the probability of particular types of consonants or vowels appearing by chance in a given item is vanishingly small, we are virtually certain to find chance resemblances wherever we look.

Strictly speaking, the foregoing discussion is a complete refutation of IECR; since Greenberg makes no attempt to circumvent the crippling mathematical shortcomings of multilateral comparison (and thereby to prove the reality of Eurasiatic), we are under no obligation to take the book seriously. Unfortunately it would not be responsible to end the review here, because in fact IECR is WORSE than that, and for the simplest of reasons: the

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discussions of at least the IE data are full of errors. The following paragraphs will point a few of them out; a complete list would be many times as long.

Greenberg's 'conative SK' (206–209) includes the PIE present-forming suffix *-sḱé- ~ *-sḱó- and supposed cognates in other families. He appears unaware that the PIE suffix almost certainly formed iteratives and habituals (cf. now Zerdin 2000, especially chapters 2, 5 and 10); that is its productive function in Hittite and in Ionic Greek, and the seven stems most securely reconstructable for PIE can all be interpreted as iteratives or habituals:

root	present (i.e. imperfective) stem
*g^{w}em- 'step'	*$g^{w}m̥$sḱé/ó- 'walk' ('step repeatedly')
*ǵneh$_3$- 'recognize'	*ǵn̥h$_3$sḱé/ó- 'know' ('recognize each time')
*h$_1$es- 'be'	*h$_1$(s)sḱé/ó- 'be (customarily), used to be'
*h$_2$eys- 'seek'	*h$_2$i(s)sḱé/ó- 'search for' (repetitive action)
*meyḱ- 'combine'	*mi(ḱ)sḱé/ó- 'mix' (repetitive motion)
*peh$_2$- 'protect'	*ph$_2$sḱé/ó- 'pasture (animals)' ('habitually protect')
*preḱ- 'ask'	*pr̥(ḱ)sḱé/ó- 'keep asking'

Greenberg's characterization of this suffix as 'conative' depends on his equation of it with a Tocharian verb meaning 'try, attempt, strive' which he lists as Tocharian A '*skai*', Tocharian B '*ske*'. In fact, the verb is A *ske*-, B /skaya-/ (sic!), reflecting Proto-Tocharian *skaya-, with a two-syllable sequence which cannot be accounted for if one attempts to connect it with the PIE present-forming suffix. (Greenberg is also unperturbed by the fact that the suffix itself survives in Tocharian B as a productive causative formation.) Nor do Tocharian A causatives in *-s-* (rather than *-sk-*) suggest that the PIE suffix is analyzable; they arose by a simple sequence of sound changes and paradigm levelling (Ringe 1989: 42 fn. 7).

Greenberg's treatment of the first-person pronouns is equally defective. He continues to cite the obsolete reconstruction '**eǵ(h)om*' for the 1sg. nominative (62), though it is clear that the aspiration occurs only in Indo-Iranian and that *-óm (or *-ém) is an emphasizing suffix, found on numerous other forms in Indo-Iranian and Italic. His citation of the root-extension *-k-* in a discussion of 1sg. forms (67) glosses over the fact that it is found in second- and third-person forms as well (and note that Greek *éthēka* is an aorist, NOT a perfect!). The suggestion that 1pl. nominative '**n̥smes*' (sic; the reconstructable form is *wéy) arose by dissimilation from '**mes-mes*' (70) leaves the oblique enclitic *nos (also with *n) unexplained. It is not surprising that Greenberg is unfamiliar with Katz (1998) – the best recent discussion of PIE pronouns – but he appears not to know even Schmidt (1978).

The treatment of the oblique nonsingular cases in *-bh- and *-m- (139–140, 144) is confused and out of date; see Katz (1998: 248–251) with references for a more up-to-date discussion.

Particularly egregious is Greenberg's treatment of 'vocative E' in PIE (164). He admits, in a convoluted fashion, that thematic voc. sg. *-e is simply

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the thematic vowel – part of the stem – with a zero ending. 'However that may be', he continues, 'forms corresponding to the *-e* vocative are found in several branches of Eurasiatic'. He might as well have said, 'I admit that the analysis of *-e as an ending is incorrect, but I still wish to treat it as an ending for comparative purposes'.

Such misanalyses, or at any rate inexact analyses, are rife in IECR. Nominative plural endings are treated as 'plural' morphemes; the hic-et-nunc particle *-r of the mediopassive endings is vaguely equated with a similar-looking 3pl. ending (110–112); stem-forming suffixes are compared with case endings of various kinds (128, 131), and so on.

Limitations of space forbid a more extensive discussion of Greenberg's handling of the IE data, but my overall assessment can be summarized very briefly. I find Greenberg's treatment of IE material incompetent; even if his methodology were not worthless, he could not possibly prove anything about IE without a far better grasp of the data. Since it is relatively easy to find reliable information about IE languages and PIE, specialists in the other uncontroversial language families treated in IECR should certainly scrutinize those data as well.

But the most lamentable thing about IECR (and Greenberg's similar recent work, especially Greenberg 1987) is the timing of its publication. It is true that Greenberg pursued antiscientific linguistic comparisons for most of his career, insisting that they would still give acceptable results because they had done so in the 19th century (an assertion which is demonstrably false; see Poser & Campbell 1992). His early attempts, however, attracted relatively little notice. By contrast, his recent 'long-range' comparative work has provoked a storm of controversy and seriously compromised his reputation among mainstream linguists. Since Greenberg is unfortunately no longer with us, there is a real danger that he will be remembered chiefly for his indefensible 'long-range' work, and that his interesting and useful work in other areas of linguistics will be forgotten.

It would be much better if IECR were forgotten instead.

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Reviewed by JAKLIN KORNFILT, Syracuse University

The Turkic languages, with their more than 100 million speakers, form one of the major language families of the world. It is surprising, therefore, that there have been only a few works that have brought them together in one volume. Two existing volumes on Turkic (Deny et al. 1959 and Menges 1968) are still useful, but not current enough. Since their publication, more field work has been carried out in more of the 'smaller' Turkic languages, and more in-depth studies have been published even about some of the 'old' languages (cf. e.g. Erdal 1991 on word formation in Old Turkic). Thus, there was a need for a new volume on the Turkic languages, accessible to Turkologists and general linguists alike. This point is amplified by the editors of the volume (henceforth J&C), who stress in their preface that their volume differs from previous work on Turkic in this respect, and that it is intended to be accessible to a wide readership without any previous knowledge of Turkology. They further state that the work is also intended as a textbook for undergraduate or graduate courses in Turkology and Linguistics, and that they hope that general linguists, typologists and historical linguists will also find valuable information in this work. Finally, they state that they have tried to ensure that the book provides an up-to-date survey of current knowledge in the entire field covered.

Any evaluation of this volume would have to assess to what extent these aims have been met successfully.

The coverage of the volume under review is impressive. In addition to a useful and detailed 36-page chapter on the structure of Turkic in general, and another equally useful 45-page chapter on the history of Turkic, there are 15 chapters on modern Turkic languages, 4 chapters on 'old' Turkic languages, one chapter on Turkish dialects, one chapter on language policy and on the Turkish language reform, one chapter on the writing systems of the Turkic

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languages (and an appendix with some of the alphabets used), two chapters on the speakers of Turkic languages (one chapter concentrating on the speakers of contemporary languages, and the other on the history of the Turkic peoples), and one chapter on the reconstruction of Proto-Turkic. Thus, while the emphasis is on modern languages and on linguistic description, it is a welcome feature of the book that it also offers some historical, sociological and ethnological background.

In addition, the book includes a map of the Turkic languages, explanatory notes on the transcriptions and symbols used, a list of abbreviations of grammatical terms and an index.

The chapters dealing with language description are structured along very similar lines. First, phonetics and phonology are discussed, with vowel and consonant inventories, and, as these are phenomena typical for the Turkic languages, assimilation and harmony rules. Then comes morphology, which is a particularly long section in most chapters, as necessitated by the extremely rich morphology of these languages. Derivational morphology, as pertinent to different categories, receives particular attention, focussing on the most productive suffixes in each language. Verbs and their suffixes are typically studied under the headings of 'finite' and 'non-finite', and the latter type finds extensive discussion in most chapters – again, as required by the fact that most Turkic languages use non-finite predicates in most of their subordinate clauses. Syntax is discussed last. This section starts in most chapters with a subsection on nominal phrases. Given the just-mentioned tendency of these languages to have non-finite subordination, some aspects of embedded clauses are discussed under this heading. The issue of word order and how it is affected by the notions of topic and focus is also one that comes up repeatedly in a number of the language descriptions.

This standardized structuring of most of the chapters makes the reader's task easy when looking up specific details and also comparing corresponding properties in the languages discussed.

The fact that the authors of the individual chapters are specialists in the respective languages, documented by their publications, gives a seal of authority to the descriptive and analytical parts of the texts.

The chapter on the speakers of contemporary Turkic languages, by Hendrik Boeschoten, points out that there is no clear match between individual languages and the ethnic groups that speak them. We learn that groups speaking similar languages or dialects may be located in different regions. It is good to find a section on the Karaim, as a chapter on their language could not be included in the book. Together with its table of speakers, this is a very informative chapter.

Peter Golden offers a condensed history of the Turkic people which is informative, especially for readers not familiar with this language family.

The chapter on the structure of Turkic by Lars Johanson is one of the most extensive ones, and sets the stage for the language chapters that follow.

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It is very thorough and it offers, for each topic it discusses, first the most generally found properties, and then the instances where individual languages diverge from the 'norm'.

Since Johanson, in this chapter, also offers the first instances of the technical terms used throughout the volume, I would like to mention just a couple of terms which diverge from usage which I think is more generally accepted by general linguists (and this not only within the generative tradition).

One term which is used in some descriptive traditions but not in others is that of ACTANT. Johanson seems to use this term to mean, roughly, 'argument to which some thematic role is assigned', as do most of the other authors in this volume, as well. However, this usage isn't always clear. What is, in particular, a FIRST ACTANT? Johanson uses this term at least sometimes in the sense of (VP-) external argument in syntactically derived structure, to judge by the fact that he characterizes a configuration with special nominalization suffix for Turkish relative clauses with subject targets as the one where 'the first actant of the relative clause is coreferential with the head' (61). Here, the FIRST ACTANT cannot be the argument to which the highest thematic role of the predicate is assigned because, at least in Turkish, the same special suffix is found on the nominalized predicate of the modifier clause (i.e. in Johanson's terminology, on the participle) when the target of the relative clause is the DERIVED subject in a passive, and thus would have not the highest thematic role (most typically that of ACTOR), but some lower thematic role – most typically that of THEME or PATIENT. It appears, then, that ACTANT is mostly used as a thematic notion, but sometimes as a notion of (surface) grammatical relation; this should have been made clear, and a definition of the term should have been provided.

Similar remarks can be made about other terms, too. Terms as such might not matter much. But they have to be clarified and defined, especially where they are not very generally used ones. Some of the most widely used dictionaries of linguistics (e.g. Crystal 1991, Trask 1993) don't include the term ACTANT. The index does not help us in this matter, either. What a volume which is supposed to be a handbook certainly needs is a glossary of the terms used.

Despite these shortcomings, Johanson's chapter on the general properties of the Turkic languages is very rich as well as clear and well-structured. The same is true of Johanson's chapter on the history of Turkic, which is even longer and more detailed. It offers a huge wealth of information about sound inventories and sound changes, as well as of morphological changes – not just in the shapes of individual morphemes themselves, but also of morphological classes. Issues of classification are addressed as well as those of contact phenomena. In addition, this chapter has a long reference list with entries which are relevant for Johanson's earlier chapter on the structure of the Turkic languages.

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The chapter by Róna-Tas on the reconstruction of Proto-Turkic and on the possible genetic relationships of Turkic to other language families is very clear and informative, and the author is to be commended on the objectivity with which he discusses a number of previously proposed relationships (e.g. with the Uralic, the Indo-European, and the Mongolian languages). He addresses the issues of the Altaic hypothesis (which, in its more extensive form, would include Turkic, Mongolian and Tungus as well as Korean and Japanese) and the Nostratic hypothesis, and he cautions against sweeping generalizations not warranted by sound scientific principles and sufficient data.

The language chapters are, in general, quite detailed and clear. Their coverage of the respective languages should suffice for usage of this volume as a handbook.

The modern languages treated are: Turkish (Csató & Johanson, the editors of the volume), Azerbaijanian (Claus Schönig), Turkmen (Schönig), the Turkic languages of Iran (Khorasan Turkic with its dialects and Khalaj, by Gerhard Dörfer), Tatar and Bashkir (Árpád Berta), West Kipchak languages (Kumyk, Karachay-Balkar, Crimean Tatar, and two varieties of Karaim, by Árpád Berta), Kazakh and Karakalpak (Mark Kirchner), Noghay (Csató & Birsel Karakoç), Kirghiz (Kirchner), Uzbek (Hendrik Boeschoten), Uyghur, Yellow Uyghur and Salar (Reinhard Hahn), South Siberian Turkic (a number of 'small' languages, e.g. Altay, Khakas, Shor, Tuvan, Tofa, and Chulym Turkic; the chapter is by Schönig), Yakut (Marek Stachowski & Astrid Menz) and Chuvash (Larry Clark).

In addition, there is a short chapter (six pages) by Bernt Brendemoen on Turkish dialects, i.e. the contemporary dialects of the Turkish Republic.

The 'old' languages discussed are Old Turkic (Marcel Erdal), Middle Kipchak (Árpád Berta), Chaghatay (Boeschoten & Marc Vandamme) and Ottoman Turkish (Celia Kerslake).

In addition to using similar guiding topics, which is a plus in this instance, given the many similarities among the Turkic languages, these chapters also include references for further reading and study, as well as a variety of tables, both for morphology (e.g. case suffixes, pronouns, tense and aspect markers, agreement markers etc.) and for phonetics (e.g. vowel and consonant charts for a variety of languages). These tables enable the reader to compare similar paradigms across languages quickly and efficiently.

It should be mentioned, however, that in most cases, the data in these chapters would be quite unusable and unintelligible for non-Turkologists without these tables – and it is only up to a certain point that the material in the tables does help. This is mainly because the examples in most chapters are translated, but not glossed. (Two notable exceptions are the chapter on Turkish and the one on Noghay, where glosses are indeed given, but are not aligned under the examples.) This appears to be a bad habit not only in Turkology, but also in philological work on other languages and language

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families. In this instance, it is quite a grave obstacle to the book attaining the success it deserves, or to its even reaching its goal of becoming a reference tool for general linguists or a textbook for general linguistics.

Another drawback of this book is the fact that we are not given the sources of most examples. This is a serious flaw, if this book is to serve as a work of reference for the scholar, whether a Turkologist or general linguist. (For a textbook, this would be less serious.) It is possible that, at least in the instance of the modern languages, the data were based on field work (which they might be in some instances, especially where data from dialects are concerned), or else were based on the authors' own native intuitions. However, we are not told explicitly about field work as providing examples. Furthermore, given that, as far as I can tell, not one of the authors is a native speaker of the languages s/he presents here, we must also exclude the possibility of native intuitions. How valid, then, are the data? And even if they were valid, it is questionable how useful the examples are to the scholar if neither written nor oral sources can be explicitly referred to. (I should mention that Erdal is a native speaker of Turkish, and it is surprising that he was not included in the preparation of the chapter on Turkish. Incidentally, it is also surprising that no other linguists who are native speakers of Turkish were included in this work. In the case of other modern Turkic languages, it might have been difficult to find qualified linguists who are also native speakers of the respective languages. This is certainly not the case for Turkish, however, as a number of highly trained and productive linguists who are native speakers of the language would have been available for work on such a chapter.)

When the data are either not fully reliable or not fully usable, due to lacking source information, then one tends to suspect the generalizations which are based on them – perhaps unfairly so; obviously, it is also possible to draw wrong, misleading, or inexact generalizations from correct and reliable data. I would like to discuss briefly one instance of the latter type, and I shall limit myself to Turkish, given my own expertise.

The discussion of case marking of the subjects of adverbial clauses (221) is based on correct data, but results in partially inexact generalizations. We are told that subjects of ADVERBIAL clauses are normally in the nominative, which is correct. This is surprising, because in ARGUMENT clauses of similar shape (i.e. nominalized, gerundive-like clauses with nominal agreement markers), subjects bear genitive (rather than nominative) Case. Whatever the explanation, this is true as a generalization only in a limited sense, holding of clauses headed by the *-DIK* nominalizer; adverbial clauses termed 'clauses based on infinitives' by the authors (230), i.e. those with the *-mA* nominalizer, all have genitive subjects.

Now this fact might appear to fall under the authors' generalization that 'adverbial clauses based on postpositions may also behave like nominal clauses with regard to case assignment of the subject' (221). This is not so, however. Adverbial clauses with *-DIK*, even when functioning as the

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complements of postpositions, have nominative subjects. They can have genitive subjects only if they can be analyzed either as free relatives clauses or as comparatives, in other words as operator-variable constructions. The two examples of genitive subjects in *-DIK* adverbial clauses discussed by Csató & Johanson are of just this kind; the first is a comparative complement of a postposition (where the postposition *kadar* means 'as much as, as far as'), and the second is a free relative complement of a postposition (where the postposition *gibi* means 'like, in a similar way as'). On the other hand, adverbial clauses with *-mA* always have genitive subjects. Here, we have a discussion where the crucial data, while correct, are incomplete, and where the generalizations are not quite right. The attempt at an explanation, formulated as '[t]he syntactic choice between the nominative and the genitive may correspond to semantic differences' (221), is too vague to explain anything.

Another surprising drawback of this chapter is that it does not include a good deal of work done on Turkish by Turkish linguists; only one doctoral thesis on Turkish is included in the references, yet there have been a number of thorough and influential dissertations by Turkish native speakers that have come out in the last twenty to twenty-five years. Most of these were written in English and thus are accessible to a general readership. Likewise, a number of articles addressing a variety of topics and addressing Turkish in particular have been published in easily accessible journals like *Linguistic Inquiry*, *Natural Language and Linguistic Theory* and the like; none of this work is mentioned in this chapter or listed in the references.

One positive feature of this chapter is that it does include a relatively long and detailed section on syntax. The same is true of the chapter on Yakut by Stachowski & Menz, and, most notably, of the chapter on Old Turkic by Erdal – the latter to be especially commended for detail and clarity. The chapter on Chuvash by Clark also includes some interesting discussion of syntax. However, most of the other language chapters have only very brief and sketchy syntax sections, concentrating instead on phonetics, phonology and morphology. Neglect of syntax and focus on morphology are found in Turkological studies in general; however, this is a drawback in the volume under discussion, if it wants to be different from Turkological usage and if it is to be used as a handbook for general linguistics.

Brendemoen's chapter on Turkish dialects is, despite its brevity, one of the most interesting ones in the volume, based as it is on the author's own field work. The chapter focusses on phonetics and phonology. A shorter section on morphology has some fascinating data, showing that some dialects have 'flipped' the Standard Turkish suffixes for accusative and dative. Brendemoen argues convincingly that this cannot be explained by phonetics or phonology. The briefest section is on syntax, but it also includes discussion of interesting differences between northeastern dialects and Standard Turkish. For example, while Standard Turkish does not allow

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focussed constituents in postverbal positions, these dialects apparently do. Furthermore, while Standard Turkish is a null subject language, these northeastern dialects do use subject pronouns in discourse situations where Standard Turkish would omit them, despite rich agreement morphology on the predicates. Clearly, this is a fascinating area which should be investigated, both for the sake of Turkish/Turkic studies and for syntactic theory at large.

Overall, this is an impressive as well as useful book. However, its usefulness is somewhat marred by generalizations that are not fully reliable, by the fact that the provenance of most examples is not stated, that most of the data are not glossed, and that some technical terminology is not widely used in general linguistics. Relatively scanty discussion of syntax in most of the chapters is another drawback in a volume that is to be used as a handbook. Nevertheless, together with other studies on specific languages and/or specific topics, this volume can serve as a good guide, both for work in research and in pedagogy. In this sense, the volume has reached its main goals, albeit in a more modest fashion than stated in its preface.

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Andreas Kathol, *Linear syntax*. Oxford: Oxford University Press, 2000. Pp. xi + 315.

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One of the most important achievements in the study of pre-generativist Germanic syntax was the observation that the clause is partitioned into a number of fixed positions or areas, the so-called TOPOLOGICAL FIELDS (in

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German *topologische Felder*).[1] The root clause of Dutch and German, for instance, is partitioned from left to right into a pre-field, a position for the finite verb, the middle-field, another position for non-finite verbal material, and a final field which hosts extraposed material such as clausal complements. In dependent clauses which are introduced by a complementizer, the finite verb appears in the clause-final verbal complex.[2] With the development of transformational grammar into the EST-framework and X′-theory, the descriptive insights of the purely linear topological fields model could be used to argue that material in the pre-field is transformationally related to positions in the middle field or further to the right, and that the finite verb in second position is transformationally related to the final position of the finite verb in the COMP-initial clause. Pioneering work in this area by Hans den Besten, Arnold Evers, Jan Koster, Craig Thiersch and others has in the meantime had an influence on developments in syntactic theory which goes far beyond Germanic syntax. It seems to be fair to say that through this work and its successors the fields theory has indirectly fertilized later developments such as the BARRIERS framework and the MINIMALIST framework. However, given modern phrase structure theories and the integration of functional heads into X′-architecture, the linear organization of the clause as conceived in the fields theory appeared to be an epiphenomenon of something more universal. Thus, the slogan could have been: Out of the fields! Andreas Kathol (henceforth AK) scrutinizes this development and comes to the conclusion that the generalizations of the fields theory cannot be adequately derived with the tools of current syntactic theories (not only transformational but also monostratal ones), and that therefore central concepts and notions of the fields theory must be reintroduced into syntactic theory. In a condensed formula, AK's suggestion thus amounts to the slogan (which is actually the title of one of the papers quoted): Back into the fields!

AK's framework is Head-driven Phrase Structure Grammar (HPSG), and some of the criticism that is launched against the transformational reduction of fields to positions and landing sites in tree structures is certainly rooted in the more general debate between derivational and representational theories. Thus, although various parts of the book discuss ways of improving existing HPSG accounts by adopting field features which map unordered lexical structures onto linear order, the theory is clearly interesting for a wide

[1] I want to thank Ellen Brandner, Uli Lutz and Susanne Trissler for helpful discussion and comments.

[2] In the book under review the following abbreviations are used: *vf* (pre-field), *cf* (left position for verb or COMP), *mf* (middle field), *vc* (right position for verb(s)), and *nf* (final field). Examples for root and dependent clauses would be (i) and (ii), respectively:

(i)	[$_{vf}$	Lisa]	[$_{cf}$	hat]	[$_{mf}$	dem	Vater	gestern]	[$_{vc}$	gesagt]	[$_{nf}$	daß	es	regnet]
		Lisa		has		the	father	yesterday		told		that	it	rains
(ii)	[$_{cf}$	ob]			[$_{mf}$	Lisa	dem	Vater	gestern]	[$_{vc}$	gesagt	hat]	[$_{nf}$	daß	es	regnet]
		whether				Lisa	the	father	yesterday		told	has		that	it	rains

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linguistic audience. The title of the book, *Linear syntax*, may evoke the idea that conventional constituent structure can be reduced or given up entirely in favor of linear precedence principles. This, however, is clearly not the case. Throughout, the HPSG representations make reference to conventional notions such as 'head', 'NP', 'VP', 'S', etc., which had no technical status in the invoked ancestral theory. Thus, AK's claim cannot be that linearity in terms of topological fields should REPLACE something in existing theories; rather, linearity in this sense has to be ADDED to the usual modules. Let us therefore see on what basis AK argues that – at least for German and other Germanic languages – topological-field linearity is indispensable. There are three constructional domains for which enough empirical background is reported to make the essential contribution of topological fields theory visible: verb fronting, the left periphery and the verbal complex. In what follows, I will comment on selected aspects of AK's proposals concerning these areas.

VERB FRONTING. The standard assumption in generative syntax is that the finite verb moves to the head position, the C(OMP) position, of a CP, if this position is not occupied by a lexical (or zero) complementizer. This serves as the background of the presentation which appears in chapter 5 of the book. A transformational relation between the two positions of the finite verb in the clause is rejected, whether it is expressed in terms of movement or in terms of the GPSG/HPSG-type SLASH convention. It is also denied that there is a featural relation between C and the finite verb, as has been argued on the basis of data that show the phenomenon of inflected lexical C. AK's suggestion is that inflected C can be reduced to cliticization and morphological change that turns, for instance, *wenn du* ('if you') into *wenn-ste* in analogy to *kenn-st du* ('know-2sg you') which becomes *kenn-ste*. Nevertheless, his topology-based representations assign the finite verb in the root clause to exactly the same place as the lexical C. Thus, the issue reduces to the question of whether the finite verb moves to the C-position or is base-generated there. Although most syntacticians believe it moves (in one technical form or another), AK tries to argue that this is wrong. One interesting issue is Reconstruction, i.e. the phenomenon whereby the fronted verb is semantically evaluated in its purported base position rather than in the position where it appears in phonetic form. I agree with the author that certain arguments in favor of reconstruction which are based on judgements about scope cannot be maintained. For instance, in *Du* ***sollst*** *nicht töten* ('You must not kill') as well as in the V-final clause *daß du nicht töten* ***sollst***, the modal appears in the scope of negation, but as he points out, it is equally true that in the pair *Du* ***mußt*** *nicht töten* ('You need not kill') and *daß du nicht töten* ***mußt*** the preferred order is reversed. So nothing about reconstruction from second position can be deduced. AK's suggestion is to simply remain in the descriptive tradition of the topological fields approach, and to merge the finite verb exactly where we see it in phonetic form. In my

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view this is nevertheless the wrong approach. There are observations not reported in the book which suggest that the contentive part of the verb is NEVER evaluated in the fronted position. If only the features of finiteness (and perhaps illocutionary force) are responsible for V-fronting, the contentive part of the verb appears in fronted position for reasons of morphological integrity. The most striking example is the behavior of the German verb *brauchen* ('need'), which in its modal use is a negative polarity item (NPI), and as such must be in the scope of negation. Notice now that, with negation present, both (1) and (2) are fully grammatical, despite having opposite linear orders.

(1) daß du dieses Stück *(**nicht**) zu üben **brauchst** NEG < NPI
that you this piece not to practise need
'that you don't need to practise this piece'

(2) Du **brauchst** dieses Stück *(**nicht**) zu üben NPI < NEG
'You don't need to practise this piece.'

If reconstruction of the contentive part is mandatory, (2) is a well-behaved example. In AK's base-insertion approach to V2 it is instead a surprising exception to the otherwise attested form of NPI-licensing. There are more tests, which for reasons of space cannot be presented here, but all of them show that the verb itself is never evaluated in fronted position, so that I am almost certain that AK's proposal of direct generation of the verb in front position (in his terminology '*cf*') cannot be maintained.

THE LEFT PERIPHERY. In chapter 6, AK takes issue with the GB-inspired standard view that *wh*-phrases move to SpecCP while complementizers and finite verbs are heads that move to or are generated in C. This approach leads to well-known problems. For example, *wh*-phrase and C seem to enter into coordination in [[***daß***] *und* [***in wen***]] *er sich verliebt hat* ('that and in who he REFL in-love-fallen has', i.e. 'that and with whom he has fallen in love'). Similarly, the distinction of head- versus phrase-status seems to be suspended in dialectal cases of so-called 'inflected COMP'. In Bavarian, the morpheme for 2nd person appears either on C or on SpecCP, as in *daß-**ts*** ('that-2.PL') and *von wem-**ts*** ('from who-2.PL'). AK's conclusion is that *wh*-phrase and C form a natural class by virtue of sharing the same position (*cf*) in subordinate clauses, while the *wh*-phrase is assigned to the pre-field in root clauses ('*vf*' in his terminology). His approach is standard in establishing a non-local association for [wh] by means of the SLASH convention. Along with the idea that *wh*-phrases may be either in *vf* or in *cf*, *cf* must now also be able to host a filler. Against the background of the extensive argumentation against verb fronting in the previous chapter, *cf* again looks less homogeneous, and because both allow a *wh*-filler, *cf* and *vf* become less distinguishable than expected under the classical topological theory. It seems to me that the price being paid for a surface-oriented solution of some problem cases is rather high. If the head/non-head distinction is neutralized in one context, similar

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effects are expected to appear elsewhere too, and if they don't, the theory has to clarify why not.

Unfortunately, here and elsewhere the author is quick to draw far-reaching conclusions from surface similarities. In chapter 7, which is devoted to sentence types, he observes that non-referential *es* ('it') in the pre-field, as in *Es sah jemand die Blume* ('it saw someone the flower'), is semantically empty and is only present in order to signal declarative status. From the fact that the complementizer *daß* is equally devoid of meaning and signals subordinate status, he concludes that *es* and *daß* share a single syntactic category.[3] This conclusion is especially astonishing as it was previously observed that *daß* appears in coordination with *wh*-PPs. Note also that *es* cannot appear in coordination at all as shown by the ill-formedness of **Er hat* [***es*** *und* ***die Bücher***] *zurückgegeben* ('he has it and the books returned').

THE VERB CLUSTER. Chapter 8, which at 72 pages is one of the largest in the book, offers a novel analysis of the verbal complex in German and Dutch. It also covers dialectal and non-standard variations such as VERB PROJECTION RAISING, LONG-DISTANCE PASSIVES and the so-called THIRD CONSTRUCTION, and it contains an excursion into consequences of the analysis for the topicalization of 'incomplete' VPs. In general topological theory, the verb cluster or verbal complex is assigned to the right sentence bracket ('*vc*' in AK's terminology). This position can be segmented into at least two more sub-fields, the 'upper field' and the 'lower field'. While the lower field retains the typologically expected left branching, the upper field follows right branching. Thus, instead of *daß er das Buch kopieren lassen müssen* ***haben wird*** ('that he the book copy let must have will') one may get *daß er das Buch* ***wird haben*** *kopieren lassen müssen*. In comparison with other empirical domains, syntactic theory has to date failed to arrive at a convincing standard account of such constructions. AK offers a fresh look at these phenomena by rejecting any kind of displacement operations. Following his general WYSIWYG strategy, he develops a descriptive account in which the V-cluster and all permissible permutations are accepted directly, i.e. without restructuring operations such as literal Verb (Projection) Raising and without operations of linear reordering. Lexical rules which generate coherent constructions are supplemented with rules of linear precedence that allow mixed directionality as in the standard 'double infinitive' construction ***wird*** *finden können* ('will find can'). This requires that verbs or V-clusters may look for their governing verb on either side. Nevertheless it is assumed that there is a 'canonical' head-final order. Deviations from that order as in the double infinitive construction involve the government feature [GVOR $\leftarrow$], which is interestingly assigned to the dependent category. If this feature

[3] In footnote 103 (150), the fact that *daß* derives historically from a demonstrative pronoun is adduced as supporting evidence. With this logic, however, one can also argue that *alarm* is a PP because it derives from 'to-the-weapon'.

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appears on the complex *finden können*, the governor *wird* must appear in the upper field. 'Broken' V-clusters as in *finden* ***wird*** *können*, which have troubled transformational analyses, are captured by assuming that linear precedence is independent of the adjacency of the parts of the governee; in other words, the governed V-cluster *finden können* can be linearly separated by the governing verb *wird*. AK claims (211) that generally unacceptable V-clusters such as **können finden wird* or **können wird finden* are excluded in his theory, but I did not see a principled reason for their exclusion. If *finden* is assigned [GVOR←] and *können* is assigned [GVOR→], both orders should be accepted.

To capture V-Projection Raising as in *daß du uns* [$_{vc}$ *hast* [*die Schlacht gewinnen*] *helfen*] ('that you us have the battle win helped'), the field *vc*, which normally hosts only verbal heads, is allowed to also host phrasal chunks. For reasons which remain unclear, the material in *vc* is still taken to be 'lexical', however. This leads to the consequence that a [LEX+] category may dominate a [LEX−] i.e. phrasal category, a relaxation that is certainly not desirable as a general option.

Linear syntax is a book that covers a lot of ground of German and Germanic syntax in a strictly non-derivational format that adopts the topological fields organization of the clause as the basis of its linear precedence rules. The criticism which is directed against earlier derivational approaches is often justified. The conclusions which are derived from this criticism seem to be less often justified. The adoption of field features as syntactic primitives adds a layer of representation which may not be as strongly warranted as the author suggests, and which can be shown to yield new problems. This does not diminish the book's general value as an important contribution to the exploration of German(ic) syntax within the theoretical assumptions of HPSG. Thanks to chapters 2 through 4, which introduce, respectively, the HPSG framework, formal accounts of discontinuous structures and the topological fields theory, the book builds a bridge of communication between different research camps and gives linguists without a Germanic background access to the classical insights of topological theory, whose merits, as we see, have come an exceptionally long way.

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Roger Martin, David Michaels & Juan Uriagereka (eds.), *Step by step: essays on minimalist syntax in honor of Howard Lasnik*. Cambridge, MA & London: MIT Press, 2000. Pp. xxiv + 357.

Reviewed by EMILY M. BENDER, Stanford University/YY Technologies

This book contains 10 essays written in honor of Howard Lasnik, as well as an introduction by two of the editors, a list of Lasnik's publications, a list of dissertations supervised by Lasnik, and an index. The dust jacket describes the contents as 'an up-to-date overview of research in the minimalist program of linguistic theory'. As such, this review considers the book as a window onto current minimalist research.[1]

The quality of the writing in this book varies enormously, from Chomsky's self-described 'first part of an unfinished manuscript' (89), which reads as (brilliant) stream-of-consciousness, through Willim's thorough but dense and hard to follow overview of kinds of evidence for DP, to some very clear papers, such as those by Bošković, Demirdache & Uribe-Etxebarria, and Takahashi. The introduction by Martin & Uriagereka is also generally clear, but not obviously about linguistics. In the course of trying to prove the scientific legitimacy of the Minimalist Program, Martin & Uriagereka make reference to physics (including magnetism, thermodynamics and fluid dynamics), chemistry and biology (including anatomy and evolution). No reference is made, however, to psychology, psycholinguistics or neuro-linguistics. In general, the presentation would have been made clearer if the papers had more structure (sections and subsections) and authors provided more tree diagrams. Finally, using footnotes instead of endnotes would have enhanced readability.

In his chapter (chapter 3), Chomsky lays out what he calls the 'strongest minimalist thesis':

(1) Language is an optimal solution to legibility conditions. (96)

Chomsky presents this thesis as something that would be surprising, and therefore interesting, if true. And indeed, it does seem highly implausible. (For an extended discussion of its implausibility, see Johnson & Lappin 1999.) However, science often proceeds by researchers pushing implausible ideas to their limits, either proving them true or learning more about the domain of inquiry in the process of proving them false. We are a long way

[1] I would like to thank Jeff Good, Jeffrey Parrott, Susanne Riehemann, Ivan Sag and Tom Wasow for helpful discussion in the preparation of this review. They do not necessarily agree with the opinions expressed here.

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from seeing the minimalist thesis proven true, but in the meantime, we can ask whether we are finding out interesting things about the domain of inquiry. In this light, let us examine the questions that are asked in the other chapters of this book.

Andrew Barss (chapter 1) investigates which interpretation mechanisms are needed for wh in situ: LF movement to scope position, existential choice functions, or both? Željko Bošković (chapter 2) asks how we can account for the distribution of wh movement across question types and across languages, and what theoretical implications the distribution has. This question leads to the observation of an interesting correlation: phonologically null complementizers correlate with required wh movement in embedded contexts but no wh movement in matrix contexts. In chapter 4, Hamida Demirdache & Myriam Uribe-Etxebarria ask how X′ theory can provide a compositional representation of tense/aspect systems. Kyle Johnson (chapter 5) investigates which kind of overt movement quantifier scope is most similar to: scrambling or extraposition from NP. Keiko S. Murasugi (chapter 6) asks whether Japanese has relative clauses, and if not, why not?

Javier Ormazabal (chapter 7) looks into whether movement out of VP can be motivated by something other than Case. More specifically, he explores an account for gaps in the paradigm of ditransitives in Romance and Basque, without appeal to Case. Mamoru Saito & Hiroto Hoshi (chapter 8) explore how to account for the properties of Japanese verbal nouns in minimalist terms, and what they tell us about the nature of the faculty of language. In chapter 9, Daiko Takahashi asks whether, under Move F, empty subjects get pied-piped when their features get attracted. This leads to interesting observations about control of open adjuncts in various constructions and about kinds of arguments in Japanese. Finally, Ewa Willim (chapter 10) explores whether there is any evidence in Polish for a DP projection.

How interesting one finds these questions probably depends on how one defines the domain of inquiry. The Minimalist Program (henceforth MP) defines the domain of inquiry as the nature of the faculty of language (FL), and patterns within and across languages are seen as a window onto FL. Outside the MP, there are linguists who share the goal of eventually understanding the human capacity for language but take the view that we do not yet know enough about the (neuro-)psychological mechanisms involved to ask such questions directly. For such linguists, the current domain of inquiry is the patterns within and across languages. The results of this work will (hopefully) form the foundation of inquiry into the nature of the cognitive systems behind language acquisition and use.

Unsurprisingly, the questions asked in this book are more likely to be interesting to linguists working within the MP, although linguists who take the second view described above might find the papers by Bošković and Takahashi interesting for the patterns they observe. (In the case of Bošković's paper, however, the correlation only obtains with some fairly substantial

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abstraction. In particular, movement of some wh words in Serbo-Croatian is interpreted as PF movement, and subject-auxiliary inversion in English is counted as a phonological realization of a complementizer.)

Regardless of what one thinks of the questions asked, the papers in this book suggest that work in the MP is generally hampered by the fact that the program is anchored neither in a systematic body of verifiable empirical observations nor in a worked-out formal theory. As a result, the authors end up reasoning about reified metaphors and/or producing or trying to argue against unfalsifiable claims.

The lack of verifiable empirical observations is the result of the data collection methodology. In these papers, the data appears to be collected in an off-hand, unsystematic way, with unconfirmed questionable judgments often used at crucial points in the argumentation.

For example, the punch-line of Takahashi's paper is the lack of ambiguity in examples involving open adjuncts controlled by quasi-argument subjects. Such judgments appear quite subtle, and the paper (already one of the best in the book) would be much stronger if such conclusions were supported by evidence of systematic data collection (e.g. survey results or at least some indication of how many speakers the author consulted).

Barss's paper is similarly concerned with extremely subtle judgments, for example, whether or not each of (2a, b) have both of the readings in (3a, b)

(2) (a) [Which professor]$_2$ did everyone tell e_2 to grade [which student]$_1$ strictly?
(b) [Which student]$_1$ did everyone tell [which professor]$_2$ to grade e_1 strictly?

(3) (a) which pairs $\langle x,y \rangle$, x a student & y a professor, are such that for every person z: z told y to grade x strictly
(b) for every person z: which pairs $\langle x, y \rangle$, x a student & y a professor, are such that z told y to grade x strictly

In this case, the author reports on only his own judgments. There is really no data to account for until such judgments have been replicated by other speakers not working on the problem and not alerted to what the author is researching.

Johnson provides several instances of questionable data collection, remarking with respect to some German examples, 'With some grilling, however, I have managed to squeeze from speakers approval of examples such as (38)' (202). In addition, on page 205, he bases some arguments on the readings available for the sentence in (4), which he rates as ??.

(4) ??It's Saturn$_1$ that I dissected [an alleged insect from t_1].

He is reporting his own intuitions here, but it is not clear how he could simultaneously have the intuition that the sentence is just about unacceptable and have clear intuitions about which scopal ambiguities it has.

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This kind of collection and presentation of the data is presumably common only because readers (and reviewers) don't demand better. The assumption that readers will take the data as given and focus on the argument is also apparent in Chomsky's contribution. On page 103, he cites a patently ungrammatical string of English words (*there read-pl the book* (*never*) *any students*) as grammatical. Only in endnote 36 does the reader learn that this example is actually an English gloss of an Icelandic sentence.

Such cavalier treatment of the data is an embarrassment to the field and should be embarrassing to the authors. Why should other sciences pay attention to the 'results' of linguistics (as Martin & Uriagereka (27) hope) when we handle our data so irresponsibly?

Such examples are hardly restricted to minimalist work, and indeed predate the MP (see Schütze 1996 and references cited there for discussion, and Cowart 1997 for a suggestion on how to do better). However, the MP does encourage lack of concern for the data, above and beyond what is unfortunately already the norm in formal syntax, because the connection between analysis and data is allowed to be remote. For example, the papers in this book often gloss over such details as the word order of the sentences being analyzed, even when the argumentation concerns movement of various elements of those sentences.

Ormazabal, arguing for replacing Case-driven movement with movement driven by a feature [+animate], has nothing to say about the word order differences in these two Spanish examples that he cites:

(5) (a) Te la llevamos.
CL.2DAT CL.3ACC brought.1PL.NOM
'We brought her to you.' (247)
(b) Te llevamos (a) ésta/ésa.
CL2.DAT brought.1PL.NOM (to) this/that.FEM
'We brought this/that one to you.' (249)

Of third person clitics (such as *la*) and demonstrative pronouns (such as *ésta*/*ésa*), Ormazabal writes:

> [They] are not [+animate] and consequently cannot check the relevant feature with T; they must therefore remain in their base-generated positions unless, of course, they are attracted to check some other features (e.g., a *wh*-feature). (250)

Since *la* and *ésta*|*ésa* appear in different positions in (5), either they have different base-generated positions or one but not the other is moving to check some other feature. Without any discussion of what's behind this difference, it is difficult to evaluate Ormazabal's claim quoted above.

More generally, the analyses in this book are often incomplete or underformalized. For example, one aspect of Bošković's analysis relies on a particular version of the Head Movement Constraint which hasn't been

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integrated into the general system that Bošković assumes (that of Chomsky 1995). Murasugi concludes her study of whether Japanese has relative clauses with the statement:

> [T]he most distinctive property of Japanese then is that pure sentential modifiers can modify the nominal heads in ways that are not allowed in other languages. (231)

She doesn't provide any indication of how the structures allowed in Japanese would be ruled out in another language. Since the MP assumes general crosslinguistic similarity, any statement of this kind must be backed up with a formal way of making the distinction for the analysis to be complete. Finally, Demirdache & Uribe-Etxebarria (163 ff.) assume that arguments of one abstract head (T or Asp) appear in the specifier position of the complement of that head, with no explanation of how this fits in with general assumptions.

In another kind of incomplete analysis, the authors assume something to be true by fiat, without making sure the formal system actually enforces the conditions assumed. For example, Bošković assumes that there always has to be a complementizer in any wh question, but doesn't specify what would happen otherwise. Demirdache & Uribe-Etxebarria propose a 'Constraint on Aspect Recursion' (173), but don't discuss how such a constraint could be implemented. Saito & Hoshi argue that '[t]he only requirement on θ-roles … is that they be properly assigned at LF' (268). They provide no discussion of how this requirement is enforced. Saito & Hoshi also don't discuss what formal status θ roles have in the MP. Are they a species of feature? Is θ role assignment some kind of feature checking? How is the information contained in a θ role – which semantic argument a syntactic argument is associated with – preserved and interpreted?

In general, much of the theoretical discussion in this book appears to take place at a metaphorical level. Metaphors are certainly useful as heuristics in generating new hypotheses to explore. However, in order for a community of researchers to collaborate in building a large theory, the metaphors need to be grounded in some descriptive system whose properties are more readily agreed upon and less open to interpretation. This is particularly important in enterprises like the MP, where the nature of the computational system is the main focus of inquiry. When most of the technical proposals are stated in terms of metaphors, researchers don't necessarily interpret the properties of the operations, constraints and formal entities proposed in the same way. Without an agreed-upon set of properties, the discourse about the proposals becomes disjointed, and researchers seeking to construct an argument about some aspect of FL have little to base their argument on but the properties of the objects to which the theoretical constructs are metaphorically related.

Saito & Hoshi provide a clear example of this, in the following argumentation about chains:

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> Let us assume that a chain consists LITERALLY of the head and its copies. It is then possible that an operation on the head also automatically affects the other members of the chain, which are by definition copies of the head. On the other hand, an operation on a copy is illegitimate, since it does not affect the ''original.'' (284, emphasis added)

Here, they are making an argument for one interpretation of chains based on properties of 'copies'. These properties of copies are not agreed-upon properties of the formal entities, but rather properties of things that go by the same name in other (non-linguistic) domains. The use of the word 'literally' signifies a reified metaphor: the word 'copy' was chosen for this theoretical construct based on some similarity between the theoretical 'copies' and other kinds of copies (say, photocopies). However, nothing in that naming should license the inference that theoretical 'copies' have any particular properties of photocopies.

Note that Chomsky in fact directly contradicts Saito & Hoshi's interpretation of the metaphor: regarding the use of the term 'copy' for non-head elements of a chain, he writes: 'The terminology is misleading … each of the elements is a ''copy'' of the other.' (114)

The hypothesis Saito & Hoshi are putting forward about chains is a perfectly legitimate one. It is not, however, supported by the reasoning about properties of other kinds of copies. Progress toward the goals of the MP might be faster if technical proposals were accompanied by more precise descriptions of the formal properties involved. Ideally, this would make it easier for researchers to apply and/or argue against each other's proposals.

A common style of argumentation in these papers is to make a proposal at the outset (e.g. insertion of complementizers at LF (Bošković) or LF incorporation of Japanese verbal nouns into light verbs (Saito & Hoshi)), build an analysis incorporating that proposal, then conclude that since the analysis is successful in accounting for the data, the original proposal is supported. In the abstract, there's nothing wrong with this style of argumentation, especially since the authors don't claim to have proven anything. In the details, however, it turns out to be quite unsatisfying.

For one thing, such argumentation is convincing to the extent that there is substantial, accepted data for the analyses to account for. Given the dubious treatment of data in our field and in MP work in particular (see above), the empirical base required for this style of argumentation is rickety at best and most likely illusory. Furthermore, the range of data considered is usually extremely narrow, so that a few linguistic contrasts end up supporting a disproportionately large number of theoretical constructs and claims.

A second problem is that the analyses always include a host of other assumptions along with the proposal being argued for: in Saito & Hoshi's case, those assumptions include the stipulation that LF incorporation

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licenses accusative Case on the incorporated noun, the assumption that LF incorporation is driven by the requirement of the noun to assign its θ roles, and the assumption that nouns need not discharge their external θ role. In Bošković's case, the ancillary assumptions include the idea that some phonologically 'overt' complementizers take the form of 'phonological affixes', the assumption that Merge is exempt from Procrastinate (or that Procrastinate is eliminated from the formal system altogether), and the assumption that some operation other than wh movement can move wh elements to the beginning of a sentence, at least phonologically.

The success of the analysis only lends support to the combination of assumptions used in the analysis. This decreases the apparent confirmation of any one of those assumptions: with so many variables in play, it seems extremely likely that another analysis that differs in one or more assumptions would be equally successful. And there is no apparent difference between the assumptions foregrounded as the 'proposal' and the other ancillary assumptions, other than theoretical interest.

A third problem is that the analyses offered (whose success is meant to support the original premise) are often incomplete (see above). In effect, this means that no account of the facts incorporating the theoretical proposal has been provided, for an incomplete analysis can't be said to account for anything.

Thus the arguments from the success of the analysis end up far from convincing. However, since Minimalist work is proceeding with neither a strong connection to data nor a well worked-out formal theory, arguments from the success of the analysis are often all that is available. When authors try to construct convincing arguments for or against any particular theoretical position, they are often impeded by the abstractness of the program, which allows too much wiggle room. This, in turn, means that many minimalist claims are effectively unfalsifiable.

For example, on page 74, Bošković summarizes the intersecting factors in his analysis: apparently null complementizers might really be null or actually be phonological affixes (for example, things that trigger subject-auxiliary inversion), and fronting of wh words may be wh movement or some other operation. Add in the possibility of covert movement, and it's hard to see how Bošković can be certain of the correlation he reports: that phonologically null complementizers correlate with required wh movement in embedded contexts and lack of wh movement in matrix contexts.

Willim's paper is a remarkably thorough search for possible evidence for a DP projection in Polish. However, the effort is significantly hampered by the wiggle room provided by the theory. She states that even if D has no phonological content in a language, its presence could be known if it were syntactically active. Finding no evidence of overt N-to-D movement, she concludes that there is no 'evidence for a phonologically unrealized but syntactically active D head in the language' (330). In a less abstract

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framework, this would be reasonably convincing. Given usual minimalist assumptions, however, one is left wondering if there is any evidence of COVERT N-to-D movement. Since such evidence is necessarily indirect, it is extremely difficult to anticipate and argue against.

Later on, Willim argues that there is no evidence for a syntactically active [Spec, DP] in Polish either, on the basis of the lack of noun phrases with two genitive arguments (332). Her explanation for this fact is that the absence of the DP layer leaves the second genitive argument no place to go to be 'licensed and identified' (333). However, Willim has already argued that Polish nominals involve multiple KP projections, and it is not clear why those layers wouldn't serve the purpose as well as a DP layer. With this loophole still open, Willim has no real account of the facts she's interested in, and therefore her argument can't serve as evidence against DP in Polish.

In summary, in order for the MP to achieve its goals, a balance needs to be found between the important work of making new proposals about the nature of FL and the equally vital work of fleshing out those proposals in terms of defining the formal system and connecting theoretical ideas to a strong empirical base. In this book, there is too much emphasis on the former, and its success is impeded by the lack of attention paid to the latter.

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