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REVIEWS

Artemis Alexiadou, Geoffrey Horrocks & Melita Stavrou (eds.), *Studies in Greek syntax* (Studies in Natural Language and Linguistic Theory **43**). Dordrecht: Kluwer Academic Publishers, 1999. Pp. v + 283.

Reviewed by GEORGE TSOULAS, University of York

Studies in Greek syntax is a collection of 14 chapters (the first of which is the editorial introduction) dealing with various aspects of Greek syntax. The editors' objective in putting this book together is clearly stated on the first page of their introduction:

> It is the purpose of this book to present some of the results of recent work in a number of central areas of current interest and controversy, emphasising throughout the importance of the Greek facts for our understanding of the theoretical issues at stake and, even more importantly, for the development of theoretical linguistics. (1)

Interestingly, the next sentence in the introduction tells us that '[t]he thematic organization of this volume reflects the major characteristics of Greek' (1).

Unfortunately, the thematic organization of the volume fails to be reflected in the arrangement of the chapters, which turns out to be alphabetical. It may very well be that in the editors' minds there is some thematic organization in this volume but the reader is not helped by the fact that the chapters have not been arranged according to that thematic organization. However, despite the lack of a tangible reflex, the articles do fall into thematic units. These thematic units, again as defined by the editors, are:

1. word order patterns, problems of clausal and nominal structure;
2. clitics in standard Gr[eek] and its dialects;
3. the nature of sentential operators and the licensing of negative polarity items;
4. control and non-finite clauses revisited;
5. thematic roles and their grammatical realisation. (11)

Three articles address problems within the first theme: Artemis Alexiadou's 'On the properties of some Greek word order patterns'; Melita

Stavrou's 'The position and serialization of APs in the DP: evidence from Greek' and George Xydopoulos' 'Tense and temporal adverbials in Greek'. Alexiadou addresses the question of word order variability in Greek and she argues that the different word order patterns are specialized in their information structure. She further compares SVO and VSO patterns to their counterparts in English/French and Celtic/Icelandic (Transitive Expletive Constructions), respectively. She offers compelling arguments for the proposal that Greek SVO and VSO are different (SVO structures in Greek involve left dislocated subjects and VSO structures involve VP-internal subjects). However, the discussion gets considerably less clear when it comes to VOS structures. Here, her proposal is that the object moves out of the VP overtly because it is 'incompatible' with the focus domain (the object is unfocused). More obscurely though, it is proposed that, following Chomsky (1995), non-complex terminals cannot be ordered by the Linear Correspondence Axiom. The appeal to this proposal for these particular cases is rather puzzling for the following reasons: first, on the face of it, given Alexiadou's example,

(1) nikise   tus andipalus   O ALEKSANDHROS
     defeated the opponents-ACC the Alexander-NOM
     'Alexander defeated the opponents.' (59)

there is no clear sense in which *O Aleksandhros* ($[_{DP}$D NP]) is more complex than *tus andipalus* ($[_{DP}$D NP]). True, Alexiadou talks of 'final elements that have a complex structure, which focused constituents plausibly have' (59). It is very difficult to see what extra structure a focused constituent PLAUSIBLY has that a non-focused one lacks. But even if it does, the explanation still doesn't hold since *tus andipalus* is by no means a simple terminal (*him* is a simple terminal). Moreover, had the object in (1) remained in its base position, the subject and the object would not be in a mutual c-command configuration, a necessary condition for Chomsky's proposal to apply. Finally, if an ordering problem were the cause of the object moving as proposed in the paper, the grammaticality of (2) remains wholly mysterious.

(2) molis erikse i   gata   to potiri
     just   threw the cat-NOM the glass-ACC
     'The cat just dropped the glass.' (54)

From the above criticisms, it does not follow that focus is not a relevant property in accounting for the different word order patterns of Greek; the mechanics of the proposal, however, are clearly not on the right track.

   In her paper, Stavrou mainly examines the ordering of APs in the DP and argues that, at least for Greek, there is no need to postulate more than one functional head in order to host different types of adjectives.

She does, however, agree with the general conclusion in the literature that there is a limit to the number of non-coordinated adjectives and this is clearly linked to the number of projections available.

In the final paper within this theme, Xydopoulos examines the nature of Greek tenses within a neo-Reichenbachian framework and their mapping onto individual morphemes. The admittedly tenuous connection of this paper to the general theme 'clause structure' becomes apparent in the final couple of pages, where the author proposes that in order to derive the position of deictic temporal adverbs in Greek one has to allow both right and left adjunction to TP.

The second thematic unit in this volume concerns the syntax of clitics in standard Greek and its dialects. Four papers are grouped under this theme: Alexis Dimitriadis' 'On clitics, prepositions, and Case licensing in standard and Macedonian Greek'; Michael Hegarty's 'Clitic placement and the projection of functional categories'; Arhonto Terzi's 'Cypriot Greek clitics and their positioning restrictions'; and Ianthi-Maria Tsimpli's 'Null operators, clitics and identification: a comparison between Greek and English'. In his very interesting contribution, Dimitriadis argues that clitics contribute to the Case licensing capacity of verbs or locatives, which are characterized as DEFECTIVE CASE ASSIGNERS. He presents a well argued case that in obligatory clitic constructions involving indirect objects, the obligatoriness of the clitic can be accounted for as a Case mediator. Correspondingly, in clitic constructions involving direct objects, there is no such obligatoriness, since there is never a need to mediate accusative case assignment. He also shows that the different ordering between clitics and the verb in standard and Macedonian (Northern) Greek can be accounted for if we assume that the clitic is located in AGR_{IO} and the object moves to its specifier in order to be Case-licensed. Terzi's contribution also deals with clitics in varieties of Greek, in this case, Cypriot Greek. She shows that in Cypriot Greek, what can, on the surface, be mistaken for a second position restriction is in fact a restriction on the position of the clitic with respect to the finite verb, namely, that the clitic must always follow the finite verb whatever the position of the latter. As a result, she derives the different distribution of clitics in Standard and Cypriot Greek as a result of verb movement rather than it being dependent on the attachment site of the clitic itself. Hegarty's contribution concentrates on the distribution of clitics in finite and non-finite structures (proclisis vs. enclisis), and the account that he provides exploits the restrictions imposed by checking theory and the theory of locality of movement (essentially, the minimal link condition) concerning checking of N and V features by the verb and the clitic in order to derive the observed patterns.

The final paper in the clitic theme is Tsimpli's contribution. In one of the most intriguing contributions in this volume, Tsimpli discusses operator constructions in Greek and English and tries to account for the differences

in the two languages regarding the availability of an empty category or lack thereof. She claims that clitics are best analysed as feature identifiers sitting in AGR_O. The possibility of clitics appearing in operator constructions is tied to the nature of the operator and the features of the antecedent. The claim is that when a quantificational operator is present (e.g. matrix interrogatives, restrictive relatives) a clitic can appear only if the operator is compatible with a specificity index, the paradigmatic example here being D-LINKED *wh*-phrases. A crucial part of the empirical basis for the above claims is that accusative clitics are disallowed in matrix interrogatives in Greek, as is shown in (3).

(3)   *pjion ton   idhes
        whom CL-ACC saw-2SG
        'Who did you see him?'

Tsimpli acknowledges in a footnote that there may be some degree of dialectal variation with respect to this type of data, as for some speakers the sentence in (4) is grammatical.

(4)   pjia   pedhia (ta)   maloses
        which children CL-ACC scolded-2SG
        'Which children did you scold?'

The dialectal differences here may involve the extent in which D-linking is associated with certain types of *wh*-phrases ('which' vs. 'who'). There is, however, a different type of data which ought to be accounted for if Tsimpli's theory is to acquire full generality. Consider semi-rhetorical questions like (5).

(5)   pjion ton   dernoun ke tou   aresi
        whom CL-ACC beat-3PL and CL-GEN likes
        'Who is being beaten and enjoys it?'

The above sentence not only sounds fine with the clitic but in the absence of the clitic it becomes very awkward to say the least. This is not the place to suggest alternative accounts, of course, and the above type of example is only offered as an indication that there would be more to a complete theory of the occurrence of clitics in these constructions than the nature of the operator.

   Moving now to the third major theme of the volume, the nature of sentential operators and the licensing of negative polarity items, there are three papers addressing questions in this area: Yioryia Aggouraki's 'Propositional operators'; Anastassia Giannakidou's 'Weak and strong polarity: evidence from Greek'; and Anna Roussou's 'Modals and the subjunctive'. Aggouraki's paper argues that negation, question, focus, conditional, necessity and possibility 'operators' form a natural class, and

that they are one-place predicates. This is supported by the fact that these 'operators' share a number of syntactic and semantic properties, namely, that they modify the proposition, license polarity items, induce weak island effects, license root infinitives and license embedded interrogatives. Now, it may very well be argued that the fact that these elements share the above properties is significant, but arguing that they form a natural class is an altogether different matter. First of all, the notion of natural class is unclear (and undefined in the paper). Second, the semantics of interrogatives as currently understood cannot really be reduced to the presence of an operator in the above sense. Finally, the author insists that there should be a common syntactic analysis of these so-called operators, though it is rather unclear why. There is no denying that there are common syntactic patterns, but the absence of crosslinguistic variation that the author offers as a piece of supporting evidence for a syntactic analysis points more clearly in the direction of what is usually taken to be the invariant component: semantics.

In contrast, Giannakidou, in her paper, addresses the issue of polarity licensing and argues for a semantic characterisation of the class of licensors. She follows Ladusaw's distinction of weak vs. strong construals of negative indefinites. She proposes a classification of operators based on the notion of veridicality and shows that nonveridical operators in Greek are weak licensors and averidical operators can be either strong or weak licensors. One may take issue with various points in Giannakidou's argumentation (e.g. the syntax/semantics mapping and the proposal that the universal interpretation of negative indefinites is the semantic reflex of movement for the satisfaction of the NEG-criterion) but on the whole this is probably the most convincing paper in this collection.

In the final paper, dealing with the nature of operators, Roussou considers subjunctive complements in Greek and presents an approach that takes the subjunctive T to be licensed when bound by a sentential/intensional operator. She claims that epistemic predicates selecting the subjunctive have a modal reading which accounts for the subjunctive. Moreover, she shows that the Tense sequences observed in these cases are the same as with other epistemic modals.

The next-to-last major theme, control and non-finite clauses revisited, is addressed by a single paper, Irene Philippaki-Warburton & Georgia Catsimali's 'On control in Greek'. Now, the fact that only one paper addresses the issue of control in Greek might make the uninitiated think that this is a one man (or more accurately, two women) crusade. This is as far from the truth as can be. Control is one of those issues that arouses very strong feelings within the Greek linguistics community. Interestingly enough, the basic question is: is there in Greek anything approximating to the control constructions of English? Philippaki-Warburton & Catsimali rightly point out that the question has two logically independent facets: (i) Is the category

PRO part of the inventory of the empty categories of Greek? (ii) How does one deal with the obligatoriness of coreference in so-called control constructions? The authors of this chapter essentially set out to answer the first question, or more aptly, to provide further evidence for the answer that they have already provided in the past, i.e. no such thing as PRO exists in Greek. The bulk of the evidence concerns the fact that in subjunctive complements (the closest one can get to control complements in Greek), predicative adjectives, NP-modifiers, etc. always appear in the nominative. This, the authors take as evidence for the fact that the empty category in subject position is assigned nominative Case, and it is with this nominative marked element that the predicate-adjective, modifier, etc. agree. Being assigned Case is incompatible with characterizing the subject as PRO. They propose instead that it is *pro*. The paper does not offer much by way of a theoretical interpretation of the facts and only minimally addresses the question of coreference, where the authors suggest that the control interpretation (whether in Greek or in general is unclear) could be derived by appealing to the Gricean maxim of quantity. Quite obviously though, this is not an explanation; the question was, why is coreference obligatory in these constructions. The authors suggest very briefly that control is a lexical specification of the matrix verb. Again, there is no space here to go into any further details; suffice it to say though that this paper cannot represent the final word on control in Greek and that the control debate in Greek is probably here to stay. As for the Case-theoretic evidence in this particular paper, it is compelling and interesting, but one can't help thinking that if anyone ever had a use for a notion such as DEFAULT CASE, these contexts would be prime candidates.

The final theme addressed in this volume is thematic roles and their grammatical realization, and it is dealt with in two papers: Elena Anagnostopoulou's 'On experiencers' and Anna-Maria di Sciullo & Angella Ralli's 'Theta-role saturation in Greek compounds'. In her thorough, interesting and (as usual) particularly solid chapter, Anagnostopoulou deals with the discrepancy in the argument realization of psychological predicates (the 'fear' class and the 'frighten' class) in Greek. She considers two modes of linking θ-structure to syntactic structure, namely Baker's Universal Thematic Assignment Hypothesis (UTAH) and Grimshaw's Aspectual Prominence Hypothesis. The conclusion that Anagnostopoulou draws from a scrupulous analysis of the data is that the mapping between θ-structure and syntactic structure is determined by both thematic and aspectual properties. This is another one of those chapters which is very well constructed and argued, and although one can, as always, take issue with this or that minor aspect of the argument, the paper on the whole remains extremely worthwhile. The final chapter within this theme is the paper by di Sciullo & Ralli. This is the only paper that addresses morphological questions overtly but it addresses their syntactic aspect, namely, the question of θ-role

saturation in compounds. The position that the authors defend is that not only is it possible to saturate θ-roles inside compounds but also there is no principled restriction on the θ-roles to be found inside a compound. They relate the apparent restrictions to questions of rich/poor morphology, establishing thereby a link between morphology and configurational aspects of argument structure.

Up to this point, I have only discussed the papers in this volume in their own right, but what about the volume as a whole? In my opinion, what makes a collection of papers truly successful is when the collection as a whole is greater than the sum of its parts. The collection at hand doesn't strike me as one that achieves that level of integration. There is no doubt that there is a great deal to be learned from individual papers for both the Greek specialist and the theoretical linguist. The point of the volume as a whole though (if there was one) escaped me. However, no one should be deterred from reading this book by that final remark, nor should anyone be deterred by the rather alarming number of typos scattered around the book (do they have proof-readers at Kluwer?). As I said earlier, individual papers make good contributions, but the volume as a whole is slightly less than the sum of its parts.

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Pierrette Bouillon & Federica Busa (eds.), *The language of word meaning* (Studies in Natural Language Processing). Cambridge: Cambridge University Press, 2001. Pp. xvi+387.

Reviewed by KEN TURNER, University of Brighton

This is an interesting collection of (mostly) original papers on the Generative Lexicon (henceforth GL; see Pustejovsky 1995). The idea behind a GL is very simple: it is that word senses display variation, permeability and creativity and that therefore a theory of word senses must explain this variation, permeability and creativity and not merely classify it. A GL stands opposed to a Sense Enumerative Lexicon (henceforth SEL) in that it is designed to manufacture, or generate, senses and not simply to list them.

This idea is so simple that it comes as something of a surprise that no one has exploited it before, especially as a suitable intellectual framework was already available: as Pustejovsky says in the Preface to this book (xi–xii) (and elsewhere), the debt to the generative tradition in linguistics is obvious. In a GL, a word sense is an underspecified structure that is composed out of a number of other kinds of structure. These include argument structure, event structure, qualia structure (which itself makes reference to formal, constitutive, telic and agentive roles) and lexical inheritance structure. This underspecified structure is mapped onto a more fully specified structure by a number of operations – which Pustejovsky, in places, calls semantic transformations – that include type coercion, selective binding and co-composition. The net effect of these structures and operations is a small and compact lexicon which permits enormous generative potential. The overall theory is one of great subtlety and, especially, computational relevance. The attractiveness of a GL can be seen in some of the analyses contained in this book.

In 'Type construction and the logic of concepts', James Pustejovsky begins an investigation into conceptual category structure. He argues that conventional approaches to the design of knowledge bases are based on subsumption relations where concepts are specialized according to the needs of a domain. Such approaches are relatively liberal with respect to the formation of taxonomic structures but there is little agreement on what anything but the highest level taxa are. To replace these conventional approaches, Pustejovsky introduces a concept lattice that is structured into the three domains of entities, qualities and events and in which each domain is itself structured into natural, functional and complex types. The natural types are the most basic; the functional types are generated from qualia-based information from agentive and telic roles, and complex types are formed as the result of a specific relation between two types. These operations are the same as those employed in a GL and Pustejovsky concludes by saying that this analysis demonstrates that linguistic generalizations can satisfy metaphysical considerations: 'the combinatorics of semantic expressions is a reflection of the compositionality of thought itself' (93).

In 'Qualia and the structuring of verb meaning', Pierrette Bouillon & Federica Busa examine the French verb *attendre*. They argue that this verb is not ambiguous but that the different interpretations can be generated from the qualia-based semantic properties of its arguments. For example, the different complements (i) *pour* – VP and (ii) *que* – sentence/*de* – VP saturate different roles in the verb's qualia structure: (i) saturates the formal role in the scope of the telic; (ii) saturates the agentive role in the scope of the telic. Bouillon & Busa extend this argument to object NPs and to the distinction between Italian *aspettare* and *attendere*. This paper presents a clear and simple analysis of a small number of words and adequately displays the advantages that a GL has over an SEL.

In 'Event coreference in causal discourses', Laurence Danlos attempts to go a little beyond GL methods and exploit discourse information (Asher 1993) to distinguish two types of event causation. He demonstrates that resultative discourse relations generate a generalization relation, whilst explanation discourse relations generate a particularization relation. Another paper that seeks to employ discourse information (as defined by rhetorical relations) is 'Metaphor in discourse' by Nicholas Asher & Alex Lascarides. They acknowledge that they do not give a comprehensive theory of metaphor, and they could, in addition, acknowledge that they have yet to give a full theory of discourse structure (the inventory of rhetorical relations is just one area of uncertainty) but on present evidence (taken together with Asher & Lascarides 1995 and Asher & Sablayrolles 1995) the theory of the GL and segmented discourse representation theory seem at least not incompatible and a fertile area for continued examination. Both of these papers, in fact, are notable as attempts to go beyond the lexical semantic and it is to be anticipated that future GL studies will endeavour to examine other discourse and pragmatic effects on the structure and the generativity of the lexicon.

Not all of this collection's papers endorse the GL program. These papers are interesting and relevant, however, on the maxim that criticism is midwife to the growth of knowledge. In 'Underspecification, context selection, and generativity', Jaques Jayez argues that there are, in addition to generative operations of contextual specification, also complementary operations of context selection 'in which some lexical items impose constraints on the types of context in which they can occur' (125). He examines three French verbs, *faire penser à*, *suggérer* and *attendre*, and presents a plausible case. But his conclusion appears premature. He says, with reference to a GL,

> No system of principles can account for the distribution of lexical items at the level of detail that is considered as desirable in lexical semantics. Further, no magic can spare GL the trouble of dealing with irregularity in the lexicon. (145)

This conclusion is premature because it is hard to prove a negative. On the evidence of this collection, the theory of the GL is a progressive research program which is examining what questions it can address and attempt to answer. All that has been provided so far is a small number of fragments of GLs. It can only be conceded that there is a residue of 'irregularity in the lexicon' once the full resources of a particular GL have been exhausted.

Further critical fire comes from 'The emptiness of the lexicon: critical reflections on J. Pustejovsky's "The generative lexicon"' by Jerry Fodor & Ernie Lepore. They deny that there is any cogent reason for designing complex lexical entries and they argue that any semantic theory that takes the meaning of a linguistic expression to be constituted by some of its inferential relations cannot be sustained. They identify the GL as such a theory. They

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wish to replace the GL with a denotational lexicon (henceforth DL) which is designed on the assumption that lexical entries are typically atomic and so lack any internal structure. So the argument comes down to the simple matter of pitching a DL against a GL.

Fodor & Lepore put forward a number of criticisms against the GL and if these criticisms stand they claim that the DL must be preferred. First, they demonstrate that Pustejovsky slips between a discussion of denotations and a discussion of representations of denotations. Second, they argue that all cases of interlexical semantic relations that Pustejovsky examines are species of analyticity and so 'in effect, he is requiring that the lexicon reconstructs the notion of *analytic inference*' (32, italics in original). The problematic nature of this inference has been extensively advertised. They go on to say that, given the difficulty of establishing with clarity which interlexical relations are the semantically relevant, or how they should be individuated, a DL's inability to capture such relations should not be taken as a decisive argument against that kind of lexicon. Third, they deny that there exists a coherent notion of semanticality, or semantic well-formedness. Therefore, once again, the failure of a DL to legislate on the kind of examples that are usually presented as semantically defective cannot be taken as a decisive argument against that kind of lexicon. There is a number of other matters that Fodor & Lepore raise but these three should give a sense of the nature of their case. They conclude that none of the arguments for a GL can be sustained and therefore a DL is to be preferred.

Pustejovsky addresses these criticisms in 'Generativity and explanation in semantics: a reply to Fodor and Lepore'. His paper is a clear and helpful exposition of GL reasoning. He outlines how the theory behind the design of GLs derives from a combination of (i) early logical work on type-changing (e.g. Lambek 1958); (ii) more recent linguistic work on the design of more flexible interpretations of types for a variety of constructions (e.g. Partee 1992); and (iii) the unrelated but traditional concern of lexicographers with systematic polysemy (e.g. Bréal 1897). Characterized as such, a GL is concerned with the following problems: '(a) explaining the polymorphic nature of language; (b) characterizing the semanticality of natural language utterances; (c) capturing the creative use of words in novel contexts; (d) developing a richer, co-compositional semantic representation' (55). In short, logical and computational analysis meets lexicography and bears the GL.

One way of taking Fodor & Lepore's criticisms of the GL is to read them as specific forms of the following complaint: X is historically a notorious problem; the GL employs, or makes reference to, X; therefore, the GL must fail. One way of reading Pustejovsky's replies to these criticisms is to see them as specific forms of the following argument: X may have been historically a notorious problem; the theory of the GL employs, or makes reference to, X; therefore the theory of the GL must present hypotheses, employing tools of

logical, linguistic, computational and lexicographic practice, which present solutions to the problem of X. Exhibited in this way, it is clear which is the most fertile research program.

Pustejovsky's arguments are endorsed by Yorick Wilks in 'The "Fodor"–FODOR fallacy bites back'. Wilks argues that the Fodor & Lepore criticisms, although directed at the theory of the GL, have, as their final target, an approach to natural language processing that is based on symbolic representations. He says that these approaches are to be judged by their results – and he claims that some areas of machine translation and information extraction have delivered significant results – and that the standards adopted for evaluation in the case of machine implementation are quite different from those used to judge the philosophically defensible. Wilks is quite witty in further satirizing the "Fido"–FIDO fallacy which he sees Fodor & Lepore attempting to press back into service and, opposing this, he says that 'meanings depend crucially upon explanations and these, formally or discursively, are what dictionaries offer' (83). Taken with Pustejovsky's original reply, his paper further confirms that, contra Fodor & Lepore, the lexicon is not empty.

The collection concludes with three papers on computational implementations of various sorts. These papers are 'Generative lexicon and the SIMPLE model: developing semantic resources for NLP' by Federica Busa, Nicoletta Calzolari & Alessandro Lenci; 'Lexicography informs lexical semantics: the SIMPLE experience' by Nilda Ruimy, Elisabetta Gola & Monica Monachini; and 'Condensed meaning in EuroWordNet' by Piek Vossen. The volume also contains the following papers: 'Chomsky on the creative aspect of language use and its implications for lexical semantic studies' by James McGilvray; 'Sense variation and lexical semantics' by Patrick Saint-Dizier; 'Individuation by partitive constructions in Spanish' by Salvador Climent; 'Metaphor, creative understanding, and the generative lexicon' by Julius Moravcsik; 'Syntax and metonomy' by Jerry Hobbs; and 'Generative lexicon meets corpus data: the case of nonstandard word uses' by Adam Kilgarriff.

On a careful reading, this collection bristles with imaginative ideas about the lexicon. It receives this reviewer's recommendation. But it is a great shame that it contains so many typos.

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Lisa Cheng & Rint Sybesma (eds.), *The first* Glot International *state-of-the-article book: the latest in linguistics* (Studies in Generative Grammar **48**). Berlin & New York: Mouton de Gruyter, 2000. Pp. ix+410.

Reviewed by KERSTIN HOGE, University of Oxford

This book is a collection of the fifteen 'state-of-the-articles' (in revised form) which appeared in the first two volumes of *Glot International* (1995–1997). As their name implies, 'state-of-the-articles' tell us something about the state of the art in a particular linguistic subfield. In the editors' formulation, the task at hand is to give an overview of a topic by (i) outlining the original research questions, (ii) summarising and evaluating the main contributions which have been made, (iii) reviewing unresolved issues, and (iv) providing an extensive up-to-date bibliography. Ideally, a 'state-of-the-article' furnishes the reader with a roadmap to navigate her way through a considerable body of literature with diverse theoretical concerns and approaches. To this end, each article needs to be accessible to the non-specialist and, at the same time, informative to the specialist, which is compatible with revealing some of the author's own interests. The articles in this volume succeed in all these respects. Yet, it is to be emphasised that none of them can, nor is intended to, serve as a truly introductory reading – each assumes some familiarity with the basic concepts and theoretical apparatus of the relevant field.

The fifteen articles cover a wide range of topics which may be grouped under the following headings: (i) syntax (including the syntax-semantics interface): Teun Hoekstra, 'The function of functional categories'; Norbert Hornstein, 'Control in GB and Minimalism'; Alana Johns, 'Ergativity: a perspective on recent work'; Kyle Johnson, 'When verb phrases go missing'; Henriëtte de Swart & Helen de Hoop, 'Topic and focus'; Tom Cornell & James Rogers, 'Model theoretic syntax'; (ii) phonology: Luigi Burzio, 'The rise of Optimality Theory'; San Duanmu, 'Tone: an overview'; Geert Booij, 'The phonology-morphology interface'; Harry van der Hulst, 'Metrical phonology'; Curt Rice, 'Generative metrics'; Wendy Sandler, 'One phonology or two? Sign language and phonological theory'; (iii) language

acquisition: Lynn Eubank & Alan Juffs, 'Recent research on the acquisition of L2 competence: morphosyntax and argument structure'; Paula Fikkert, 'Acquisition of phonology'; and (iv) historical linguistics: Elizabeth Closs Traugott, 'Semantic change: an overview'. Some of the articles cut across the above categories. Thus, Burzio deals with constraint-interaction in both morphophonology and syntax; and Fikkert and Eubank & Juffs, as the titles of their respective articles indicate, consider the nature of (native/ second) language acquisition alongside theoretical issues in phonology and syntax.

Given the limited length of this review, in what follows I will concentrate on the articles by Hoekstra and Burzio, which seem to me to identify two overarching themes of this collection. Hoekstra discusses the development and role of functional categories in syntactic theory, and the editors did well to place it before the other syntax contributions, as these amply demonstrate the central position that functional categories have come to occupy in syntactic investigation. Thus, Hornstein discusses the way in which functional categories are exploited in movement theories of obligatory control. Johns informs us that current work on ergativity distinguishes a structural domain from a thematic domain (VP) and assumes there to be two functional projections above the VP, which can serve as landing sites for some or all of the argument noun phrases. Johnson's discussion of VP ellipsis shows that functional categories may be put to use in explaining cases where the antecedent does not match the elided VP as well as in analyses of pseudogapping as VP-ellipsis. Eubank & Juffs consider the debate regarding functional structure in early L2 knowledge. And even in the article by de Swart & de Hoop, the reader will discern the usefulness of functional categories, although the authors do not explicitly discuss approaches in which a discourse-semantic operator is associated with a functional projection of its own (e.g. Uriagereka 1988).

Hoekstra begins his study of the function of functional categories by discussing what motivated their introduction into generative grammar. Recapitulating the distributional evidence involving verb-second phenomena and 'short' and 'long' movement in French non-finite clauses, he shows that the introduction of functional projections follows from the structure preservation principle. If 'Move α may not create head positions, each occurrence of a head where it is not expected requires the presence of a head position where the displaced head has moved to' (4). X-bar theory ensures that each head will project a spec-head-complement configuration.

The use of an articulated phrase structure to account for language-internal word order patterns produced analyses which assumed that inflectional morphology was represented by means of functional heads, which in turn gave rise to the view that all lexical projections are dominated by a set of functional projections. The use of functional categories to account for cross-linguistic variation offered the possibility of a restrictive theory of language

variation, in which all variation resides in functional categories. Both developments resulted in a proliferation of functional categories. This enrichment of the phrase-structural inventory may have afforded analytic success but it also raised questions as to its explanatory adequacy. The central problem in the study of functional categories is, thus, their theoretical justification.

In the absence of a restrictive theory of functional categories, Hoekstra invites the reader to explore the position that functional categories are 'specific syntactic representations of semantic interpretations' (21). The idea is that each type of functional category is associated with a particular licensing role, e.g. the functional head Neg is involved in the licensing of negative heads and phrases (Haegeman & Zanuttini 1991). If we further assume that 'these licensing roles are uniformly required for each language' (7), it follows that (i) all languages have an identical set of functional categories; (ii) all languages have an identical ordering of functional categories (i.e. 'the relation between [two functional categories] is inherently given by the functional roles these play' (9)); (iii) variation can involve only the inherent properties of a functional head, such as strength; and (iv) given the Continuity Hypothesis (e.g. Pinker 1984), i.e. the claim that children's grammars can differ from adult grammar only in ways in which adult grammars can differ from each other, the full set of functional categories must be available throughout all stages of development.

The appeal to licensing roles as justification for functional categories is attractive insofar as it reduces the array of parametric options. There is no longer room for variation in terms of complement selection or absence/ presence of functional categories. On the other hand, the original problem, i.e. what theory constrains the postulation of functional categories, has simply been rephrased to ask which licensing roles are assigned to functional categories by Universal Grammar. Hoekstra recognizes the problem at various points in the text but does not present a proposal as to how we can identify a functional category to which is consigned a specific licensing role. He is, however, explicit about what will not do when he asks for a functional category to 'be motivated on grounds other than the interpretive effect itself which it seeks to capture' (7).

In this context, the article might have included a discussion of Chomsky's (1995) notion of interpretability of features, which, together with the Minimalist criterion of virtual conceptual necessity, ensures projective economy (and results in the elimination of Agr). In this approach, functional categories require justification by output conditions, i.e. they must provide 'instructions' at LF and/or PF, e.g. English C expresses force/mood and, if declarative, is pronounced as *that* (with a null option). Contrary to the position taken by Hoekstra, this approach does not subscribe to a universally fixed set of functional categories. It appears, then, that the very same question that was asked when functional categories were first introduced, viz.

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how and why languages can differ with respect to their functional category inventory, continues to be on the research agenda today.

Hoekstra's article is to be commended for clearly organising the different developmental strands in the field of functional categories and for providing an outstanding overview of the literature, even if the author chose not to include Chomsky's economy approach to functional categories or Rizzi's (1997) Split-CP hypothesis.

In the second of the articles that I will discuss in detail, Burzio outlines the evidence for a linguistic theory which allows for the parallel operation of an ordered set of constraints. This is, of course, the position taken by proponents of Prince & Smolensky's (1993) Optimality Theory (OT). Its pervasive use within phonology is apparent from most of the phonology contributions in this volume. Thus, van der Hulst discusses how parametric distinctions involving foot type and direction of footing can be expressed by means of different constraint-rankings. Rice shows how OT provides a framework for representing markedness in the domain of metrical poetry, so that optimal satisfaction of the constraint hierarchy can account for the statistical dominance of a particular metrical pattern in Arabic verse. Fikkert points out that 'OT allows for elegant accounts of phenomena that show the interaction of prosodic and segmental phenomena' (238), e.g. the alignment of place features to word edges, which is characteristic for a particular stage in child language. Sandler considers an OT approach to the phonological effects of pronoun cliticisation in Israeli Sign Language; and Booij ends his overview of the Lexical Phonology model with the suggestion that level-ordering effects are not necessarily incompatible with OT if constraint evaluation can take place at more than one level.

Burzio sees the main advantage of OT in that it espouses parallelism as one of its defining principles, which he argues is needed to account for effects of (i) globality over strings, (ii) globality over prosodic levels and (iii) globality of morphophonology. Parallelism requires that all constraints apply simultaneously to the input-output pair. Hence, constraints must be violable, i.e. 'well-formedness will become ''best''-formedness, alias optimality' (209).

After outlining the basic assumptions and workings of OT, Burzio turns his attention to the outstanding empirical and theoretical issues in OT. Empirical issues concern the 'derivational residue', i.e. the existence of effects which seemingly attest to the need for rule-ordering. Enrichment of either the representation or the derivation (e.g. by allowing for different co-phonologies) may be able to accommodate stratal ordering effects. Theoretical issues, which are open to discussion, include the classification and nature of constraints and the range of possible cross-linguistic variation in constraint-ranking.

Burzio then goes on to argue for an OT-type approach to syntax on the basis that it provides a natural way of representing scalar relationships,

which can be seen to play a role in pronominal selection. Thus, Burzio observes that morphological weight is inversely related to inherent reflexivity. In both Italian and English, predicates which are inherently reflexive, e.g. 'to open one's eyes' (one does not usually open somebody else's eyes) take a pronoun with lower morphological weight than those predicates which are not inherently reflexive, e.g. to cut one's hair, cf. (1)–(2) (215).

(1) (a) Gianni (*si)　　apre　gli　occhi.
　　　　Gianni　　to-self opens the eyes
　　　　'Gianni opens his eyes.'
　　(b) Gianni si　　　taglia i　　capelli.
　　　　Gianni to-self cuts　　the hair
　　　　'Gianni cuts his hair.'
(2) (a) Gianni opens his (*own) eyes.
　　(b) Gianni cuts　his　own　hair.

Burzio reasons that only by referring to the scalar relationships that hold between zero pronoun and overt clitic in Italian, and pronoun and pronoun-intensifier in English do we have a unified explanation for the two contrasts. If scalar relationships translate into constraints, the constraint preventing morphologically heavy items must be violable for the sake of satisfying the constraint that ensures full specification of the pronoun in the absence of an inherently reflexive predicate.

But the following German data may be problematic for this account. As (3) illustrates, German *sich* 'self' is optional with the non-inherently reflexive verb 'to comb one's hair'.

(3) Lorelei$_i$ kämmt (sich) ihre$_i$ Haare.
　　Lorelei$_i$ combs　self　her$_i$ hair
　　'Lorelei combs her hair.'

The optionality of *sich* is unexpected if non-inherent reflexive verbs universally require the morphologically heaviest form. It appears that in German the constraint preventing morphologically heavy items need not be violable even in the absence of an inherently reflexive predicate. An OT approach may come up with an analysis of these data which uses tied constraint ranking, thereby allowing for two optimal candidates in a given candidate set. Yet one should heed van der Hulst's warning of 'situations in which the very thing one wants explained is presented as an explanation itself' (322).

In conclusion, the fifteen 'state-of-the-articles' are an important resource for linguistic theory and their publication in one volume is welcome indeed. It is to be hoped that further volumes of *Glot International* 'state-of-the-articles' are in the making, and that these will be free of the editorial and/or productional glitches that can be found in some of the present articles. Yet,

the reader may rest assured that even these do little to detract from the benefits that can be reaped from this book.

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Michel DeGraff (ed.), *Language creation and language change: creolization, diachrony, and development*. Cambridge, MA: MIT Press, 2001. Pp. x + 573.

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This volume was originally published in 1999, but has now been reissued in paperback format. A low-budget edition is definitely welcome, since this is without doubt one of the most interesting publications in the field of creolistics for quite some time. Clocking in at almost 600 pages, the book is packed with data and analyses of utmost importance to any student of creole languages and related phonomena. DeGraff's editing – far better than much editorial work seen in other linguistic publications – also deserves praise.

The book is organised into five parts, comprising fifteen chapters (ranging from less than twenty to almost seventy pages) and a rather comprehensive index. It is introduced and concluded by a prolegomenon and an epilogue by the editor. Given the size of the volume, it is not possible to go into detail on all parts of the book so I will concentrate on a few chapters whose contents are the closest to my own area of expertise and/or which I find particularly worthy of comment.

Two particularly interesting studies are those by Elissa Newport on American Sign Language (ASL) and by Judy Kegl, Ann Senghas & Marie Coppola on the emergence of Idioma de Señas Nicaragüense (ISN – Nicaraguan Sign Language). Although both ASL and ISN, as well as (occasionally) other sign languages, have been present in creolistic discourse

before, these studies are especially welcome, since we are offered a great deal of data with the potential of making a significant theoretical impact on creole studies.

Newport discusses the language acquisition of deaf children of hearing parents (the overwhelming majority of deaf children) in the United States. Since the parents have acquired ASL only after the birth of their child, the situation is thus a relatively unusual one, where children acquire their native language from non-native users. Newport recognises, however, that some of the adult caretakers 'are more fluent [and use] structures [that] are more complex than those of pidgin speakers' (164). I cannot help but feel that the relevant comparandum is nativisation of, say, Hebrew and Esperanto rather than the exceptionally impoverished input suggested to have been offered to children of pidgin-speaking parents. Nevertheless, it is indeed interesting to note that while some of the features offered by the parents are probabilistic (i.e. not rule-governed), Newport's informant Simon restructures his input into a highly ordered language. In other words, he seems to create rules which are not offered by the input.

The same pattern is even more evident in Kegl, Senghas & Coppola's Nicaraguan study, somewhat puzzlingly entitled 'Creation through contact' (puzzling, since one of the main features of the birth of this variety is precisely the relative lack of contact with other languages). Although it might have benefitted from some shortening, it is the paper which I found the most stimulating in the entire volume. The birth of ISN is a story without any known parallels (they may exist, but are not documented). At least one pidgin language without a lexifier is attested, namely the interethnic sign language of the American Prairies, but the pidgin precursor of ISN seems to be one not only without a lexifier, but also without a real substrate! Moreover, it is one of the few languages for which we have documentation of both a pidgin and a creole stage, and where the development of a creole from a pidgin can thus be demonstrated.

The basic facts are as follow: before the Sandinista takeover in Nicaragua, there were no deaf schools. Deaf children existed, of course, but they were spread over the country, and communicated with caretakers and peers in home-made local signal systems of varying degrees of elaboration. In the late 1970s, a deaf school was founded, and it recruited students from all over the country. The older of these students did not create a stable and expanded means of expression, but the first younger children to grow up in this environment (so the authors claim), conventionalised and elaborated on the primitive contact language that had developed, and turned it into a true creole. Just like Simon, this new generation thus created rules and stability in excess of what their input had offered. The expansion, which took place in the 1980s, is said to have been rather abrupt (201–202).

Among several interesting features in ISN are serial verb constructions (215–219). As many readers are aware, such constructions have been the

focus of creole studies for a long time, and have been claimed (in particular by Bickerton, e.g. 1981, 1984) to represent a manifestation of innate language capacities surfacing in the transformation of pidgins into creoles. Yet, it has been demonstrated that serialisation is strongly present ONLY in creoles with serialising substrates (see references in Parkvall 2000: 70–77), which significantly weakens the nativist argument. Nevertheless, Kegl, Senghas & Coppola provide a strong case that serial verbs can emerge in a creolising language without any preexisting input.

ISN is thus a dream come true for anyone sympathetic to nativist accounts of creole genesis. Provided that Kegl, Senghas & Coppola's observations and analyses are correct, this must be considered proof that a grammar can be created on the basis of 'something mental' alone.

In his chapter, John Lumsden continues the beaten path of the 'relexification project' he and Claire Lefebvre were involved in for much of the 1980s and the 1990s. The aim of this project was to show that relexification, i.e. the equipping of an extant grammar with a new vocabulary, is one of the chief processes in creolisation. The test case is Haitian creole, whose structures are compared to Fon, a West African language suggested to be one of its major substrates. Given Lumsden's involvement with Lefebvre (whose account of creolisation is thoroughly outlined in Lefebvre 1998), it is perhaps all too tempting to invoke 'guilt by association'. I find Lefebvre's account highly unsatisfying, but I am happy to report that the Lumsden version as presented in this book is slightly less troublesome. As so many times before, there are annoying little errors such as the claim that French – contrary to both Haitian and Fon – lacks a progressive marker (149). This may be true for current standard French, but in all probability not for the variety which lexified Haitian. The use of the *être après faire qc* construction is amply attested in various dialects of France, and is so common in New World varieties that its absence from 17th century Haitian French would be surprising – all the more so since the Haitian progressive marker is arguably derived from *après*. There are methodological problems too – for instance, Lumsden uses the lack of subject-verb agreement in Haitian and Fon as an argument in favour of a genetic link (149). In a way that I see as typical of the relexificationist group, he refrains from taking into account how common such an absence is, and also that there is good evidence that it follows from pidginisation regardless of which substrate languages are involved. While his statement that 'there are many ways for a language to differ, but only one way for them to be identical' (153) is true, it can hardly be denied that certain similarities are of a rather trivial nature. If one wanted to establish a genetic link between, say, Haitian and Eskimo, and used as evidence the presence of both vowels or pronouns in both, few serious linguists would be convinced. Yet, there is nothing obvious in the Lumsden-Lefebvre methodology that would rule this out (for more detailed comments on this issue, see chapter 2 of Parkvall 2000). Among the more

absurd results are claims that e.g. Haitian *bwa-bannann* {wood-banana} 'banana wood' reflects Fon *kwékwé-xwÈlÈ* {banana-wood} rather than French *bois de bananier* {wood of banana-tree}. The difference between Haitian and French in this case is simply the dropping of a preposition and a derivational suffix, something that is hardly the unmistakable imprint of Fon influence. Simply considering the English translation – provided by Lumsden himself – should cast doubt on this and similar claims. Lumsden asserts that his claims are falsifiable (153), but it is not clear on what grounds this could be done. Lumsden points out that relexification is not the only process involved in the creation of a creole. The initial stages of creole genesis involve, they claim, relexification at an individual level, and the end-product is then the result of koinéisation (i.e. a compromise) between these idiolectal relexifications, together with other processes. While few people doubt that creoles do display a number of substrate features, Lumsden's explicit claim still is that 'Haitian Creole has inherited the syntactic properties of its functional categories from its substrate source languages' (149).

Another, albeit lesser, problem that I have with Lumsden, and the relexificationist theory in general, is the claim that creolisation begins as an attempted second language acquisition (e.g. page 134), which, because of the limited access to the lexifier (the alleged target language), fails, yielding a creole as a result. This assumption, reasonable though it may seem at first sight, has been questioned both by myself and others (see for example Baker 1990 and McWhorter 1999), and it would be desirable to at least see a comment on these criticisms from Lumsden (and other relexificationists).

The remaining chapters are loosely grouped into four themes: (i) creolisation and acquisition (Derek Bickerton, 'How to acquire a language without positive evidence', Dany Adone & Anne Vainikka, 'Acquisition of *wh*-questions in Mauritian Creole', Salikoko Mufwene, 'On the LBH: hints from Tazie' and John Lumsden's 'Language acquisition and creolization' mentioned above); (ii) acquisition under (other) 'exceptional' circumstances (Elissa Newport, 'Reduced input in the acquisition of signed languages' and Judy Kegl, Ann Senghas & Marie Coppola, 'Creation through contact' already discussed, together with Alison Henry & Denise Tangney's 'Functional categories and parameter setting in L2A Irish' and Rex Sprouse & Barbara Vance's 'An explanation for the decline of null pronouns'); (iii) parameter (re)setting in creolization, language change and language acquisition (Ian Roberts, 'Verb movement and markedness', Adrienne Bruyn, Pieter Muysken & Maaike Verrips, 'Double-object constructions in the creole languages' and Viviane Déprez, 'The roots of negative concord'); and finally, (iv) commentaries and epilogue (David Lightfoot, 'Creoles and cues', Luigi Rizzi, 'Broadening the empirical basis of UG models' and the editor's 'Epilogue').

Among the few problems that I have with the volume in general is its high concentration of generatively oriented studies. Arguments which, regardless

of their intrinsic qualities, are clad in Chomskyan terminology often have the effect of alienating the non-believer. This may be more of a problem to Europeans than to those from the parts of the United States where derivatives of GB remain the predominant creed.

While the formalist framework and the assumptions entrenched in it may be bothersome to many, the Chomskyan orientation is possibly responsible for the fact that many of the contributors dare to use the term 'language bioprogram hypothesis', and moreover, they do so without being overtly scornful. Since the demise of Derek Bickerton's (1981, 1984) attempts at coupling innateness theories to creole language genesis, this has been almost taboo in creolistics. Few people would doubt that the stronger formulations of this theory are downright incompatible with the data at hand, but I increasingly feel that significantly weaker versions of it may, as a consequence, have been insufficiently considered.

As already mentioned, DeGraff's editing is impressive, and one gets the impression that he has put a good deal of energy into his editorial work. Yet, I find the preference for endnotes rather than footnotes highly disturbing. To the reader, this means a copious amount of flipping of pages and searching for notes, which may or may not be rewarding.

The very latest developments in creolistics offer an interesting perspective on DeGraff's two contributions – the prologue and the epilogue already mentioned. He basically delivers a state-of-the-art description of creolistics as it looked in the early 1990s, which could serve well as an introductory text in courses dealing with language contact issues. What makes it particularly interesting is the polemic nature of his recent writings where he has been hard on anybody sticking to the description of creoles as nativised pidgins. None of this vitriol is present in the prologue, where he states that

> [A]rguably, a creole is a pidgin […] that has become a full-fledged language by stabilization and expansion, as the pidgin acquires native speakers and/or becomes the primary language. (5)

The paperback edition, however, has been endowed with a new preface, which reflects DeGraff's new position, where he accuses those who share his own 1999 views of reviving 'nineteenth-century notions of language evolution'. For UG 'offers no conceptual room', he states, for an 'opposition between (the diachronies of) Creole and non-Creole languages' (viii). I do not mind DeGraff changing his mind – on the contrary, that can be taken as a sign of intellectual development, even though he happens to be heading in a direction opposite to my own. What bothers me is that he attacks his opponents with such frenzy, without even commenting on the fact that he himself until recently shared the views which he now sees as downright racist.

The shortcomings, however, are more than compensated for by the many other merits of *Language creation and language change*. If the majority of the

publications in creolistics were as stimulating as this volume, the subdiscipline would be a lot healthier than it currently is.

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Laurence R. Horn & Yasuhiko Kato (eds.), *Negation and polarity: syntactic and semantic perspectives*. Oxford & New York: Oxford University Press, 2000. Pp. ix+271.

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Horn & Kato (henceforth H&K) bring together contributions from 'prominent senior specialists' (2) or, if you would rather, 'internationally renowned scholars' (14) investigating the formal and functional complexities of negative polarity/concord/inversion and the contrast between sentential and constituent negation, primarily in syntactic and semantic terms, but also from a pragmatic perspective. Indeed, at least half the contributions (Haegeman, Kato, Progovac, Portner & Zanuttini and, to a lesser extent, Hoeksema) are couched within recent Minimalist/Principles-and-Parameters syntax (even if, in places, these authors venture into semantic and/or pragmatic considerations). Not surprisingly, Horn and Ladusaw are more clearly semantic in outlook although, here too, syntactic and pragmatic issues are not ignored totally. Finally, as we shall see below, Yamanashi's contribution fits less well within the context of the collection as a whole.

The editors' dramatically titled 'Introduction: negation and polarity at the millennium' (1–19) provides a useful contextualization and overview, going back to the landmark contributions of Jespersen (the cyclic behaviour of negative-polarity items (NPIs) and negative concord) and Klima (the first

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generative study of negative scope). H&K chart approaches to sentential negation *qua* a single, universal phenomenon in terms of a one-place propositional operator or an operator on predication. With respect to the licensing of negative polarity, H&K consider this as a distinct syntactic/ semantic/pragmatic phenomenon or just one aspect of a broader phenomenon (cf. reflexive licensing).

Liliane Haegeman's 'Negative preposing, negative inversion and the split CP' (21–61) addresses the contrast in (1a, b).

(1) (a) [PP With no job] would Mary be happy. (NEG1-PP: FOCUS)
    (b) [PP With no job], Mary would be happy. (NEG2-PP: TOPIC)

Haegeman suggests that the wide-scope, inversion-triggering, inner-island-inducing PP in (1a) (NEG1-PP) is focalized new information (paralleling fronted *wh* XPs) and that it occupies a different structural position – Rizzi's (1997) SpecFocusP – from the one – Rizzi's SpecTopicP – occupied by the narrow-scope, non-inversion-triggering, non-inner-island-inducing PP in (1b) (NEG2-PP), which is topicalized old information. (The distinctive properties of the two are usefully tabulated on pages 38–39. See also Haegeman 2000.)

The parallel between NEG1-PP fronting and *wh* fronting, as well as their mutual incompatibility, is accounted for by a well-formedness (*wh*/NEG) criterion obliging an appropriate operator (a *wh*/NEG XP) and head (a [+WH]/[+NEG] finite auxiliary) to appear in an (adjacent) spec-head configuration. Fronted *wh*/NEG XPs in matrix clauses target a unique SpecFocusP position, and the attendant inversion takes the auxiliary to Focus^0; matrix negative and *wh* inversion are, therefore, mutually exclusive. As for NEG2-PP fronting, since it targets (Spec)TopicP, it is compatible with matrix *wh* inversion (as, indeed, it is with NEG1-PP inversion).

The breakdown, in embedded contexts, of the parallel (NEG inversion takes place; *wh* inversion does not), and the fact that negative (NEG1) and *wh* fronting can co-occur in embedded contexts, are explained if embedded interrogative clauses, but not embedded negative clauses, are selected. In a (selected) embedded interrogative clause, a *wh* feature occupies Force^0; no inversion is triggered; the preposed *wh* XP occupies SpecForceP (rather than SpecFocusP). In a (non-selected) embedded negative clause, in contrast, the pattern of behaviour is identical to that found in matrix negative clauses: preposed NEG XPs occupy SpecFocusP and inversion is triggered. Since *wh* fronting and negative (NEG1) fronting 'happen' at different levels in embedded contexts (ForceP and FocusP, respectively), co-occurrence is predicted to be possible. The general theme of Haegeman's paper is a reinforcement of the cross-linguistic parallels – familiar for some time – between the syntax of interrogation and negation.

Yasuhiko Kato's 'Interpretive asymmetries of negation' (62–87) uses a bare-phrase-structure approach (Chomsky 1995) to structure building and

formal features (that is, without Haegeman's *wh*/NEG criterion) to address contrasts between Japanese and English: (a) in English, but not Japanese, there is a subject–object asymmetry with respect to NPI licensing; (b) in English, but not Japanese, NPIs are licensed in non-negative contexts. Echoing some of the concerns of Haegeman, Kato distinguishes between the notions of a negative phrase (a clause-internal XP bearing the feature [+NEG]) and a clause with negative polarity (a CP bearing the feature [+NEG]) to address the kind of contrast in (1a, b). Surface position aside, the difference between NEG1-PP and NEG2-PP is seen as derivational: NEG2-PP in (1b) is an adverbial merged directly in sentence-initial position; NEG1-PP in (1a) is a complement of *happy*, merged lower in the structure and moved to sentence-initial position. (See the similar distinction made by Martín-González 2000.) This gives Kato an immediate explanation for the (un)availability of preposition stranding illustrated in (2a, b) (Kato's (29), (30)): (2b) is ungrammatical because the PP is not first merged in the necessary low position.

(2) (a) [No job]$_i$ would John be happy [with t$_i$].
    (b) *[No job]$_i$ John would be happy [with t$_i$].

In 'Co-ordination, c-command and "logophoric" n-words' (88–114) Ljiljana Progovac starts from the observation that some of the unexpected patterns of behaviour of NPIs and n-words parallel those of reflexives, and argues that a parallel treatment should be found. For example, reflexives, which normally need to be licensed, can sometimes appear in unlicensed adjoined contexts as logophors; alternatively, in conjoined contexts, where the first conjunct might be expected to c-command the second, they cannot. Similarly, in languages where n-words normally need to be licensed, they can sometimes appear in adjoined contexts, but fail to appear in conjoined contexts. From this, Progovac concludes that reflexives and n-words have a number of properties in common (specifically relating to formal licensing), and that the relevant adjunct/conjunct contexts mentioned above have some special properties which 'suspend' those licensing requirements.

In 'Negative-polarity items: triggering, scope and c-command' (115–146) Jack Hoeksema offers a useful potted history of NPI phenomena, then concentrates on the contexts in which NPIs are licensed and on the nature of scope. Given the unclear correlation between scope and c-command (e.g. in co-ordinate structures – see above), Hoeksema concedes that there is much work still to be done on the notion of scope, and wonders whether scope (more specifically, the relationship between a trigger and an NPI) isn't a semantic, rather than a syntactic, notion. Relevant here is the fact that different kinds of NPI demonstrate different sensitivities: verbal NPIs seem to be semantically, rather than syntactically, triggered; predicative NPIs appear insensitive to scope issues; adverbial NPIs demonstrate a wide range

of topicalization behaviours. And even the familiar subject–object asymmetries are not as clear-cut as often believed: the empirical facts can be complicated by the nature of the predicate, the embedded/non-embedded distinction, as well as the nature of the trigger.

Laurence Horn's highly descriptive and data-oriented 'Pick a theory (not just *any* theory): indiscriminatives and the free-choice indefinite' (147–192) starts by considering the contrast between (non-presuppositional, scalar) *not just* and (presuppositional, optionally scalar) *not only*, attributing the semantico-pragmatic difference to the distinct structures the constructions have: *not* [*just* P] versus [*not only*] P. In the former, *not* takes wide scope over P; in the latter, its narrow scope is restricted to *only*. (The differences are usefully summarized in (13) on page 152.)

Horn goes on to consider *any*, more specifically, whether NPI *any* and free-choice *any* really are two distinct items, as often assumed. He ultimately rejects the view that *any* is a quantificational indefinite at all. As he himself puts it, his stated aim is to 'hammer one more nail in the coffin of the traditional approaches to *any* as a universal and/or existential quantifier, supporting instead the revisionist line …, on which *any* is analysed as a non-specific indefinite involving scalar end-points, widening and/or indiscriminacy' (183).

In their contribution 'The force of negation in *wh* exclamatives and interrogatives' (193–231), Paul Portner & Raffaela Zanuttini (henceforth P&Z) are interested in the notion of 'expletive' negation and contrast the use of the Paduan clitic negative marker *no* in exclamative contexts, where negative force is lost, and interrogative contexts, where it is maintained:

(3) (a) Parcossa no ve-to   anca ti!?   (interrogative)
        why   NEG go-SUBJ.CL also you
        'Why aren't you going too?'
    (b) Cossa no ghe dise-lo!   (exclamative)
        what NEG him say-SUBJ.CL
        'What things he's telling him!'

What is specific about the Paduan clitic negative marker *no* is that, in addition to introducing negation, it triggers a scalar implicature. (Another, non-clitic, negative marker *no* introduces negation, but triggers no scalar implicature.) While the 'same' clitic negative marker *no* appears in (3a, b) above, the interaction between the semantic properties of exclamation or interrogation, on the one hand, and the scalar implicature triggered by clitic *no*, on the other, makes it hard to detect the presence of negation in the former, but not the latter, context. More specifically, P&Z follow Grimshaw (1979) in assuming that exclamatives are crucially factive, while interrogatives are not. The relevance of this distinction is that, if the propositional content of an exclamative is presupposed, the formal presence of negation is

irrelevant. Thus, the negative marker in (3a) can be deemed to be the same as the one in (3b).

William Ladusaw's contribution, 'Thetic and categorical, stage and individual, weak and strong' (232–242), is a reprinted article from the *Papers from the Fourth Annual Conference on Semantics and Linguistic Theory*, 1994 (SALT4), and is included here since it is felt not to be sufficiently widely accessible. The paper deals with the ambiguity of indefinite nominal expressions, namely, the familiar strong/weak distinction and the way this potential ambiguity interacts with the stage-versus-individual-level (state-versus-property) distinction for predicates: weak indefinites cannot be the subject of individual-level (property) predicates.

Probably the contribution which fits least well in the collection, Masa-aki Yamanashi's 'Negative inference, space construal and grammaticalization' (243–254), considers, from a cognitive perspective, the development of Japanese grammatical markers like *nai*, in particular, the evolution of spatial terms into negative markers. Yamanashi suggests that negation is not in fact a primitive concept, but rather one that is derivative of other cognitive concepts involving individuals' bodily and/or spatial experience.

The book is rounded off, unusually for a collection of essays, by a very useful thematic bibliographical section 'Further reading' (255–264). The material included ranges from the general to the specific (including other collections of papers) and, surprisingly perhaps for this particular volume, there is even a section devoted to acquisition and processing.

H&K tell us (14) that the volume was originally conceived as a festschrift to Akira Ota, then later reworked as a more general collection of papers on negation and polarity. It is unclear why the standing of the contributors is emphasized; it is, after all, irrelevant, assuming the proper mechanisms of refereeing, which in the case of the present volume led to two contributions being excluded. That said, it is a pity that two other heavyweights, originally expected to contribute papers, were not ultimately able to do so.

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Krasimir Kabakčiev, *Aspect in English: a 'common-sense' view of the interplay between verbal and nominal referents*. Dordrecht: Kluwer, 2000. Pp. xii + 348.

Henk J. Verkuyl, *Aspectual issues: studies on time and quantity*. Stanford, CA: CSLI Publications, 1999. Pp. x + 266.

Reviewed by Henriëtte de Swart, Utrecht University

Tense and aspect were important themes in the field of semantics in the 20th century, and still are in the 21st. Generative linguistics made it possible to gain new insights into the syntactic structure of verbs, verb phrases and sentences. Montague Grammar has highlighted the importance of the compositional mechanisms that are behind the construction of the temporal and aspectual meaning of sentences. These two major developments have created new ways of looking at language and languages. The tension between 'universal' features shared by all languages and typological variation across languages is particularly acute in the domain of aspect, where we see wildly diverging linguistic systems that nevertheless seem to have interesting areas of overlap and similarity. I am invited to review two books that reflect the state of the art at the turn of the century. In his collection of papers written between 1991 and 1998, Verkuyl tries to marry generative syntax and model-theoretic semantics in order to solve problems in combining verbal and nominal expressions. With this 'Western' background, he looks, among other things, at aspect in Slavic languages. Kabakčiev's book was originally written in Bulgarian, in an attempt to explain the aspectual feature of a 'non-aspect' (i.e. non-Slavic) language to a Slavic public. When it was rewritten in English, the presentation of the book was slightly changed, but it still emphasizes the comparison between English and Slavic (mostly Bulgarian), and offers a 'Slavic' perspective on the phenomenon of aspect in English. Kabakčiev acknowledges the influence of Verkuyl (1972, 1993) on the development of aspectual theory, and discusses his theory at length (chapter 3). So his appreciation of Verkuyl's work is well known. Verkuyl's (2002) review of Kabakčiev's book provided me with some insights from another angle. My role in this review is that of the 'outsider' who tries to compare the two views, and the influence of each writer on the other's analyses. I will also bring in a third language family, Romance – both Verkuyl and Kabakčiev establish comparisons with the Romance languages, without working out a full analysis.

   The differences in point of departure are visible in the main divisions the authors draw. For Verkuyl, the distinction between terminativity and

durativity is the basic aspectual distinction, and the perfective/imperfective contrast that we find in Slavic, and to some degree in Romance languages, is somehow subsumed under this distinction. It is considered to be morphological 'glue', whereas languages like English only use syntactic 'glue' (the progressive), and other Germanic languages basically have none (5). For Kabakčiev, the perfective/imperfective contrast is basic, and he proposes a reinterpretation of the Vendlerian aspectual classes in these terms (chapter 2). Simplifying a bit, he claims that non-Slavic languages express with determiners what Slavic languages express with prefixes (chapter 4). A closer comparison of the two theories reveals some interesting similarities and differences.

Interestingly, both authors return to the insights of traditional aspectual theories in their recognition of the importance of the verb, albeit in a 'modern' version in which compositionality is a crucial ingredient of the analysis. For Verkuyl, the verb does not play its full role until it combines with an inner argument, and builds a VP that constitutes a predicational path for the external argument (chapter 1). The notion of path, and the asymmetry of subject and object bring Verkuyl to an in-depth study of multiple quantification (chapter 6) and scope ambiguities (chapter 8), and of issues surrounding collectivity and distributivity (chapter 7), which we will leave aside in this review for lack of space. For Kabakčiev, the constraints on the combination of inner arguments and verbs with a (perfective) prefix are at the heart of the discussion. Kabakčiev claims that we need to give a temporal (not spatio-temporal) interpretation to the NP to flesh out the meaning of the combination of the verb with its (inner) argument (chapter 6).

For both authors, the notion of compositionality of aspect is at the core of the matter, but they seem to disagree on the content of this notion. Verkuyl (1993) implements a Montegovian view on compositionality, at the heart of which are function-argument structures, and in which function application is the only mode of composition. For languages like English, Dutch, etc., he proposes that non-stative verbs (i.e. [+ADD TO] verbs) introduce an enumeration of indices, modelled on the natural numbers. The combination with the inner argument maps the set of indices onto a path denotation, which consists of a set of pairs of indices i and locations p. If the argument is an NP like *a book*, it has the feature [+SQA] (i.e. specified quantity of A), and the path is finite. If the argument is an NP like *books*, it has the feature [−SQA], and the path is non-finite. Because of this distribution of labor between the verb and the NP argument, a terminative VP can only come about as the result of combining a [+ADD TO] verb and a [+SQA] NP (chapters 1 and 4). In chapter 5, this leads to an analysis of the English progressive in terms of an aspectual operator that is situated in a position between S and INFL (133, 134). Accordingly, the English progressive is analyzed as a modifier of the predicate-argument structure built up by the (untensed) sentence (cf. Smith 1991, de Swart 1998 and others for similar

analyses). Its semantic value is $\subset$, by which Verkuyl means that the progressive picks out a subinterval of the interval connected to the untensed sentence, and it is this subinterval that is actualized in real time by the tense operator. Thus, we obtain a two-tier system, in which grammatical aspect is built on top of aspectual class in an interpretation that is directly derived from syntactic phrase structure.

Verkuyl extends this analysis to the French Passé Simple/Imparfait (138–141), which are traditionally viewed as the perfective and the imperfective past tense. The aspectual value of these tenses is given as = (for the Passé Simple) and $\subseteq$ (for the Imparfait), by which Verkuyl means that the Passé Simple commits the speaker to the truth of the whole interval as actualized in real time, whereas the Imparfait is indeterminate in this respect. The same aspectual values are claimed to play a role in Slavic (144–148). Verkuyl views the perfective/imperfective prefixes in Slavic languages as aspectual operators, albeit at a lower level in the tree. In his view, they are located in an Asp phrase, in between the VP and the S (144), where they 'secure' the terminative/durative interpretation of the VP (131). The Slavic imperfective can have either a $\subseteq$ or a $\subset$ value (145, 146), whereas the perfective has an = value (147).

According to Verkuyl (2002), Kabakčiev fails to establish a distinction between 'inner aspect' (everything up to the combination of the VP and the external argument) and 'outer aspect' (aspectual adverbials, etc.). I partially agree with that criticism, insofar as Kabakčiev's insights about aspectual adverbials (*for-* and *in-*adverbials, frequency adverbs, negation and the like, cf. chapters 11 and 12) are not revolutionary in my opinion, and do not come out that clearly in his conceptual system. Presumably, these could indeed benefit from an analysis in which these expressions are analyzed as aspectual operators (cf. de Swart 1998; de Swart & Molendijk 1999). But that should be set apart from the main insight that Kabakčiev is trying to formulate, and which bears on the domain of inner aspectuality, namely, his rejection of the two-tier system that is the result of the combination of a distinction between aspectual class and grammatical aspect on the one hand, and the identification of compositionality with function application on the other. Kabakčiev does not want to 'wait' until the VP level to interpret the contribution of the English progressive or the Slavic perfective/imperfective prefix. Instead, he builds a mapping system in which either the NP can impose boundedness/unboundedness on the verb or the verb can impose boundedness/unboundedness on the NP (chapter 6). He analyzes non-English Germanic sentences without aspectual morphemes as examples of the former category, and English sentences with the progressive and Slavic sentences with perfective/imperfective prefixes as instantiations of the latter type (chapters 7 and 8). The fact that both verbs and NPs are taken to have a temporal denotation makes it possible to develop mappings in either direction, whereas Verkuyl's theory only allows a mapping in one

direction. Kabakčiev refuses a formalization of his approach: he considers the conceptualization of his mapping principles to be the main contribution of his book.

Although I respect Kabakčiev's position vis-à-vis formalization, I regret his decision very much. The result is that he posits an exciting concept, but does not clarify the syntax of aspect, or the relation between syntax and semantics. Verkuyl makes an important contribution to aspect theory by formalizing the mapping in one direction, based on a syntactic structure in which terminativity is defined from the level of the VP onwards. The intuition that the NP imposes boundedness on the verb is thus formalized at the level of the combination of the two expressions. It seems difficult to derive an interpretation in which the verb imposes boundedness on the NP argument from this structure along these Montagovian lines. This is why Verkuyl analyzes the perfective/imperfective prefix in terms of an aspectual operator that 'secures' the terminativity of the VP. How exactly this works remains somewhat vague but, clearly, this analysis does not match Kabakčiev's intuition about the semantic contribution of the prefixes. Kabakčiev's intuition and the claim that the mapping of boundedness values can proceed in either direction is a crucial insight that raises a challenge for a 'true' compositional aspect theory, and that takes into account the relation between syntax and semantics. I certainly wish Kabakčiev had taken up this challenge rather than refusing a formalization, for this is an unsolved problem, as far as I know.

Kabakčiev emphasizes the role of the lexicon in aspectual theory (chapters 9, 13, 14). He claims that there are three classes of verbs: those that are disposed to get a perfective meaning, those that are disposed to get an imperfective meaning, and those that can go either way. Of course, verbs that naturally 'explicate' perfective aspect (in Kabakčiev's terminology) have well-formed imperfective variants, but these are to be considered as derived meanings (chapter 9). I think that Kabakčiev is right that we need more than a feature [± ADD TO] to describe the aspectual contribution of the verb. The problems raised by *push* verbs (+ ADD TO verbs where the object is unable to create a terminative meaning for the VP, as in *push the cart*, *stroke the cat*, etc.) could be addressed within a more fine-grained classification. We certainly need a deeper study of the characterization of the different verb classes, but Kabakčiev's classification has a potential for new insights, especially because Slavic and English pattern along similar lines, due to the influence of 'knowledge of the world' (chapter 14).

A question that comes up in both books is the relation between so-called 'primary' and 'secondary' imperfectivization. All Slavic languages have perfective and imperfective prefixes. Most Slavic languages have some examples where a verb with a perfective prefix can be imperfectivized again by adding a suffix, but in Bulgarian this system is the most elaborate, as pointed out by both Verkuyl (118) and Kabakčiev (6). Their observations

about the meanings of sentences involving secondary perfectivization/ imperfectivization are very similar. The 'default' is that an imperfective prefix combines with an imperfect past tense, and a perfective prefix combines with the aorist. Interesting meaning effects arise when we combine an imperfective prefix with the aorist (atelic action, bounded in time, e.g. by an adverbial like *for three hours*), or a perfective prefix with the imperfect (unbounded (habitual) iteration of a bounded action), cf. Verkuyl (118) and Kabakčiev (7, 8 and appendix). According to Kabakčiev, the distinction between the aorist and the imperfect is not to be compared to the perfective and imperfective prefixes. The former distinguish actions with respect to termination and lack of termination, whereas the latter involve completion and non-completion, in ways similar to the different past tenses in Romance (5).

At first sight, there is a sharp contrast between Kabakčiev's observations and Verkuyl's analysis of all these notions in terms of aspectual operators with the values $\subseteq$, $\subset$ and $=$. However, Verkuyl treats the difference between primary and secondary (im)perfectivization in terms of scope: the aorist/ imperfect distinction is located in the higher Asp phrase projection, whereas the perfective/imperfective prefix distinction is in the lower Asp phrase. Note that Verkuyl also situates the contrast between the French Passé Simple and Imparfait in the higher Asp phrase, so this comes close to Kabakčiev's claim that the aorist/imperfect distinction is to be compared to the aspectual contrast between different past tenses in Romance. But somehow this reviewer feels that this is not the end of the story.

Verkuyl does not work out the semantics of sentences with two aspectual operators that have a conflicting aspectual value, so we don't quite know how he obtains the habitual iteration of bounded events from the combination of the two values $\subseteq$ and $=$ in the order perfective prefix– imperfect, and a bounded interpretation of a durative situation in the order imperfective prefix–aorist. The problem seems to be that the syntactic structure is hierarchical, but the semantics is 'flat', so it is unclear how a difference in the order of application of the aspectual operators leads to differences in meaning. Moreover, Verkuyl claims that 'higher' aspect somewhat redundantly confirms the aspectual nature of the untensed sentence in the default cases – that is, the combination of the aorist with a perfective prefix, for instance, contributes an $=$ value which is already present in the sentence (147). But this raises the question as to why a language like Bulgarian would have vacuous aspectual operators in so many of its sentences. Furthermore, Kabakčiev's intuition is clearly that the perfective prefix and the aorist do not contribute the same aspectual value.

I wonder whether the two views can be reconciled in the three-tier system that I developed in de Swart (1998). I argue there that we need to recognize the aspectual potential of tense operators as distinct from grammatical aspect and aspectual class. I analyze the French Passé Simple and Imparfait as

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aspectually sensitive tense operators: both are past tense operators, but one applies to events (the Passé Simple), and the other applies to states/processes (the Imparfait). Crucially, this maintains Verkuyl's scopal distinction between 'lower' and 'higher' aspect, but with an even higher projection (namely Tense) where aspectual information might be relevant. I work out the semantics of this idea in the framework of Discourse Representation theory (Kamp & Reyle 1993). The analysis is set up in such a way that intermediate levels are visible in the meaning construction, which allows aspectual information at different levels to have a different impact on the meaning of the sentence, as opposed to the 'flat' representation we find in Verkuyl. In that paper, I tentatively suggest that this approach might be extended to the Bulgarian aorist/imperfect distinction, and Kabakčiev's observations give me the impression that this might be on the right track.

This brings me to a final issue, which is, sadly, missing from both Verkuyl and Kabakčiev, namely, the contribution of their different analyses of aspect to a better understanding of the temporal structure of discourse (narrative and otherwise). As pointed out by Kabakčiev (15, 16), most verb–aspect combinations are grammatical, but usually only one possible combination is the right choice in a particular context. He illustrates this claim with the translation of an English text into Bulgarian, with manipulations of the aspect forms. He complains that there is no good analysis of the contextual values of aspectual forms, but unfortunately does not make a contribution himself. Verkuyl seems to dismiss the question, because most discourse-oriented semantic analyses of tense and aspect are based on events. Although he admits that events might be useful at the 'macro-level' of discourse, he does not find the Davidsonian or neo-Davidsonian view attractive for the 'micro-level' of inner aspectuality that he is interested in (70–72). I would like to end this review with a plea for the study of the contextual value of aspect forms. Some attempts have been made for English (Kamp & Reyle 1993, Lascarides & Asher 1993 and others) and Romance (cf. Borillo et al. (to appear) for an overview) but I am unaware of any advanced studies of the contribution of Slavic aspect to the temporal structure of narrative discourse. Bulgarian especially, with its layered aspectual structure, could provide interesting contributions to our general understanding of these issues.

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Philip Lieberman, *Human language and our reptilian brain: the subcortical bases of speech, syntax, and thought*. Cambridge, MA & London: Harvard University Press, 2000. Pp. 221.

Reviewed by Michael A. Arbib, University of Southern California

The premise of this book is that comprehension and production of spoken language is a learned skill, based on a functional language system (FLS) in the human brain which includes cortical areas and subcortical structures such as the basal ganglia. However, to call the basal ganglia (BG) 'our reptilian brain', as Philip Lieberman does, is as useful as calling the spinal cord 'our fish brain'. So much had to change to go from circuitry that exhibits the traveling wave of neural activity required for swimming to, for example, the spinal cord of salamander that in addition supports the standing wave seen in walking (Ijspeert 2001) – let alone bipedal locomotion and dextrous manipulation. In the same way, the BG of humans has many specializations, which distinguishes it from the BG of any reptile.

This is a book on language and not linguistics, focusing on the physical mechanisms of both vocal tract and brain that equip humans for language. But it does not ask: 'As the vocal tract evolved in complexity, what co-evolution of the brain – neocortex as well as basal ganglia – must have occurred to equip humans for language?'

A particular bête noire of Lieberman is the notion of an innate Universal Grammar. The term has been watered down to the point where some authors describe it as simply 'the learning mechanism that makes it possible for a child to learn a language', reducing claims of innateness to tautology. What Lieberman disputes is closer to the Principles and Parameters (P&P) view that innate knowledge of language takes the form of a Universal Grammar which comprises general syntactic rules which contain some parameters, and that the child's task in learning a language reduces to recognizing values of

parameters from the utterances it encounters. Contrary to this, Lieberman reminds us that human children make use of statistical learning to master the syllabic structure of words and the patterns of syntax, and relates this to the general process of 'automatization' by which animals, including humans, learn to rapidly execute skilled motor control programs without conscious thought.

Chapter 2 shows that lesions of Broca's area not only may affect syntactic processes but may result in oral apraxia or interfere with speech perception. The great variation in the effects of both damaging and electrically stimulating Broca's area suggests that the brain's language mechanisms are acquired phenotypically in a manner similar to the neural bases of various aspects of motor control. Lieberman emphasizes the subtle timing problems in phonation, as laryngeal activity transforms the flow of air outward from the lungs, with consonants generated as air is forced through constrictions higher in the supralaryngeal vocal tract (SVT). The details are given a thorough exposition. For example, the sound [b] is produced when phonation occurs near in time to the release-burst that occurs when the speaker's lips open, [p] when phonation is delayed. Voice-onset time appears to be an important feature of all human (spoken) languages.

The human SVT is essentially a tube whose shape and length can be continually modified as we move our lower jaw, tongue, lips, larynx and velum, and the velum can switch the coupling of the nose into the system. The 'motor theory of speech perception' (Liberman [not Lieberman] et al. 1967) proposed that decoding the acoustic signal involves resolving it into the articulatory patterns that generated it. This interests me because of its relation to the claim that mirror neurons may play an essential role in the human brain's mechanisms for language (Rizzolatti & Arbib 1998). Each mirror neuron for grasping, found in the premotor cortex of monkeys, is active both when the monkey executes a specific action and when he sees a human or monkey execute a similar action. Brain imaging suggests that such a system also exists in the human Broca's area, fueling the hypothesis that language evolved via manual gestures (protosign, if you will) so that speech gestures, too, would be mediated by a mirror neuron system. Note that the mirror system claim is not that Action A has the same neural code in individuals X and Y, but rather that the neural code for A is the same in X whether X is doing action A himself or perceiving Y perform action A. Similarly, it is no argument against the motor theory of speech perception that different people use different motor control patterns to achieve the 'same' acoustic goal.

On this view, we perceive speech by subvocally modeling it without producing overt articulation. How, then, does the child acquire the community's stock of phonemes, given that its own voice is more highly pitched than those of adults? Lieberman argues that speech perception involves reference to knowledge of, for example, the length of the SVT. A

more plausible account (consistent with Lieberman's positive account of adaptive artificial neural networks) is that we have a network that uses the pitch and timbre of recognizable elements as an input to affect the transformation of the overall auditory input to the 'speech code'. Other processing can transform the input into an output encoding speaker identity. Such identification may correlate with SVT parameters but does not involve knowledge of them.

Chapter 3 turns to 'The lexicon and working memory'. Lieberman argues that the human lexicon must code linguistic distinctions such as the argument structure of verbs as well as 'semantic' knowledge. He follows Baddeley (for instance, 1986) in viewing verbal working memory as the neural 'computational space' in which the meaning of a sentence is derived, taking account of syntactic, semantic, contextual and pragmatic information. This raises the question of how all this information is represented, and of the relation between the mechanisms of perception and production. Lieberman stresses the lability of the functional language systems (FLS): children may suffer large lesions to cortical 'language areas' yet still recover language abilities. However, among people whose brains have not suffered major damage or loss of sensory input, evidence can be found of a fairly standard architecture for the FLS. Generation of color words selectively activates a region in the ventral temporal lobe just anterior to the area involved in the perception of color, verb retrieval is more impaired than noun retrieval in aphasics with left frontoparietal damage, and naming deficits associated with persons, animals and tools occurred in the temporal pole, the anterior inferotemporal region and the posterior inferotemporal region, respectively. (Of course, data showing that a brain region is the one most active in a particular task does not imply that it is the only region involved in that task, nor that it is involved in no other tasks.)

Lieberman stresses that syntactic computations differ for individuals who have different working-memory capacities for language (Just & Carpenter 1992), citing their conclusion that 'the syntactic processing of a person with a small working memory capacity is encapsulated only by virtue of a capacity constraint, not an architectural constraint' (1992: 126). Indeed, migration of more and more information into the lexicon as Chomsky's generative program has undergone radical transformations via Government and Binding to the Minimalist Program makes it unclear to what extent modularity is really a viable concept even for generativists. The constraints in the lexicon at times seem as much semantic as syntactic, and the boundaries become further blurred as we try to understand what sort of performance model might yield such a competence model as an abstraction. I am tempted to see this work as owing more to Generative Semantics than to *Aspects* (Chomsky 1965) in its genesis!

There is increased metabolic activity in Broca's area when subjects read sentences that contained a center-embedded relative clause compared to

sentences that contained a right-branching clause. Lieberman suggests that this activity correlates with working-memory load which 'necessarily increases because resolution of the initial noun must be delayed until the intervening clause is processed' (78). Lieberman reviews imaging studies showing that verbal working memory involves a distributed system including Wernicke's area, Broca's area, other cortical areas, and subcortical structures. Regions of the frontal lobes implicated in abstract reasoning and planning are recruited as task difficulty increases. However, the term 'verbal working memory' seems inadequate for so distributed a system, which invokes semantic, contextual and pragmatic information far richer than the string of words being interpreted. (Another intriguing challenge is to reconcile the limited working memory used in parsing a sentence with the 'unlimited' ability to build an increasing 'narrative frame' as we conduct a conversation or read a novel.)

Chapter 4 focuses on the basal ganglia (BG), 'the structures of our reptilian-amphibian brain' (82). However, Lieberman does not ground his analysis of BG in data on reptiles but rather starts with rats, which make use of a 'syntax', regulated in the BG, for 'well-formed grooming programs' (87). This usage obscures crucial differences from the syntax of human languages bound to a compositional semantics for the unbounded creation of meaning. Thus, while welcoming this chapter's excellent review of the involvement of BG and related cortical areas such as the supplementary motor area in sequential, self-paced, manual motor control tasks as well as a variety of language tasks in human subjects, I am concerned that Lieberman does not attend to the question 'What changes in the BG readied the human brain for language?'.

Diseases such as Parkinson's disease (PD) associated with major damage to the BG yield tremors, rigidity and repetitive movement patterns. However, subcortical diseases also cause linguistic and cognitive deficits, including syntax comprehension deficits that cannot be attributed to non-linguistic motor deficits. Since PD subjects exhibit voice onset time sequencing deficits as well as sentence-comprehension deficits similar to Broca's aphasia, Lieberman argues that disruption of BG circuits may be the common basis for such deficits. However, Lieberman is untrue to the distributed nature of the FLS when he tries to place BG at center stage. It makes far more sense, given the available data, to suggest that the evolution of the human brain was such that any evolutionary changes in neocortex provided the basis for changes in basal ganglia and cerebellum, and vice versa – just as changes in body structure made it possible for certain changes in the brain to become selectively advantageous in ways that were not possible before, and vice versa. Indeed, even while repeatedly stressing the importance of BG, Lieberman does note that the FLS also includes Broca's area, Wernicke's area and adjacent regions of the neocortex implicated in working memory and executive control, as well as premotor cortex, supplementary motor area,

and areas of posterior cortex associated with perception and phonologic association. Moreover, lesions in the anterior cingulate gyrus (which, rather than Broca's area, is the homologue of the vocalization area of monkeys) can result in mutism.

Chapter 5, 'The evolution of the functional language system', starts with the observation that humans and chimpanzees have in common maternal care, toolmaking ability, territoriality, hunting and warfare. Fully bipedal locomotion is absent in chimpanzees, but its evolution can be studied because the fossil record preserves skeletal features needed to support the biomechanics of bipedal locomotion. But what of language? Vargha-Khadem et al. (1998) show that the linguistic deficits related to a genetic anomaly in three generations of a large extended family (Family KE) are not purely syntactic but involve a severe impairment in sequential articulation, with related impairment in nonspeech movements. MRI scans of the affected family members' brains revealed some cortical damage and bilateral reduction in the volume of the caudate nucleus of BG. Lieberman thus sees the language deficits of Family KE as having a genetic basis very different from that posited for a P&P-style Universal Grammar.

The lexical and syntactic ability in chimpanzees raised by humans and in (young) human children are roughly equal. However, a 'naming explosion' occurs concurrently with increased grammatical ability at age 2 or so in humans, but never in chimpanzees. Lieberman then asserts that

> [s]ince chimpanzees possess limited syntactic ability, it would be surprising if early hominids such as the Australopithecines lacked any syntactic ability … [and it is unlikely] that advanced syntactic ability was possessed only by anatomically modern *Homo sapiens* 150,000 years or so in the past. (135)

This is unconvincing, for two reasons: first, it is dangerous to conflate what chimpanzees can learn from humans with what they possess in the wild, let alone what Australopithecines had 3 million years ago. After all, chimpanzees can also be taught to wear modern clothing! Second, even if Australopithecines had the 'syntax' of a two-year-old human, there would still be much to explain in how the brain changed to make a subsequent 'naming explosion' possible. Moreover, since I find it plausible that the first *Homo sapiens* still had holophrastic utterances, I see the ability to name as quite separate from the possession of a syntax which supports a compositional semantics. Thus, much remains to be understood regarding what, rather than an innate P&P Universal Grammar, renders the human brain capable of feats of language learning that are closed to other species. Indeed, Lieberman mentions a 'gradual development of syntactic ability over the course of hominid evolution' (135) but gives no evidence that this involved biological changes prior to the emergence of *Homo sapiens* rather than a process of non-biological change that occurred during the history of *Homo sapiens*.

Lieberman has done valuable research on the SVT of humans and hominids. The human SVT enhances speech production but makes swallowing liquids and solid food more risky. He thus argues that the restructuring of the human SVT to enhance the perceptibility of speech would not have contributed to biological fitness unless speech and language were already present in the hominid species ancestral to modern *Homo sapiens*. However, Fitch & Reby (2001) showed that lowering of the larynx has occurred in other species. For example, lowering of the larynx in the red deer may have been selected to deepen the animal's roar so that the animal would seem larger than it was. Thus the lowering of the larynx in humans or pre-human hominids might have served a similar purpose – without denying that further selection could have exploited the resultant increase in degrees of freedom to increase the flexibility of speech production. Moreover, this selective advantage would hold even for a species that employed holophrastic utterances devoid of syntax. Thus, I cannot agree with Lieberman that speech and language (as distinct from some simpler form of vocal communication) must have already been present in *Homo erectus* and in Neanderthals.

Many human languages make use of controlled variations of the fundamental frequency of phonation (F0) to produce 'tones' that differentiate words. Lieberman notes that since apes possess laryngeal anatomy that can generate F0 contours, early hominids must have had this ability, so that 'the roots of speech communication may extend back to the earliest phases of hominid evolution' (142). However, if these contours are adaptive for chimps, they must be more generic than 'just' speech markers. Lieberman notes that the vocalizations of frogs, which are 'ancestral to both birds and mammals', are regulated by one hemisphere of their brains, which 'explains why both birds and mammals … have lateralized brains' (145). But this would simply show that lateralization has nothing to do with what distinguishes language from other motor activities, including other forms of vocal communication.

Lieberman offers a useful review of the evidence from fossils of a number of hominid species, with special attention to *Homo erectus* and to its probable descendants, Neanderthals and modern humans. He argues that the skeletal morphology of the basicranium of skulls dated to 100,000 years BP could have supported only a modern human SVT. He further asserts that the human FLS already existed at that time since the migration of humans from Africa and their subsequent dispersal throughout most of the world occurred over the next 50,000 years, so that otherwise we would find differences in the linguistic capabilities of human children native to different parts of the world.

Virtually all aspects of human behavior would be affected if we could not use our hands to touch, hold and manipulate. Manual sign languages can convey subtle linguistic distinctions. Nonetheless, Lieberman dismisses the

view that the initial stages of hominid language were crucially dependent on manual gestures (Rizzolatti & Arbib 1998 offer a recent example, more neurally grounded than most). He asserts that '[t]he neural bases of manual sign language clearly are more primitive, since chimpanzees can master childlike ASL [American Sign Language]' (152). This confuses the dexterity of making signs with the linguistic richness of full ASL, which chimps lack. As a result, Lieberman fails to explore arguments showing that circuits supporting voluntary speech may have evolved as concomitants to earlier systems for 'protosign' as a basis for 'protospeech'. Lieberman sees as crucial the transition from the rodent BG that regulates submovement sequencing of an innate grooming 'program' to a monkey BG which supports the learning of submovements of complex motor activity, with corresponding changes in the prefrontal cortex and cerebellum, but he offers no insight into what changes in the brain may have accompanied the changes in the SVT that equipped human beings for speech. We thus have a book which is important in its demonstration that changes in brain and body must go together in the evolution of language, and that the basal ganglia is a crucial part of the FLS, but which does little to make clear what distinguishes the human brain from that of other animals in 'equipping' it for language and how it got that way.

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Mark Steedman, *The syntactic process* (Language, Speech, and Communication). Cambridge, MA: MIT Press, 2000. Pp. xiv + 330.

Reviewed by MARTIN JANSCHE, The Ohio State University, and SHRAVAN VASISHTH, University of the Saarland

The syntactic process (SP) presents major developments in so-called COMBINATORY CATEGORIAL GRAMMAR (CCG) and summarizes Steedman's thoughts on a CCG framework that addresses issues as diverse as intonation and meaning, human sentence processing and computational efficiency.[1] There are 10 chapters, grouped into three parts. Chapter 1 summarizes the main claims and the contents of the book.

Part I, 'Grammar and information structure', starts with chapter 2, 'Rules, constituents, and fragments', which outlines some of the basic goals and assumptions of CCG: the desire to recursively derive semantic representations directly from syntactic expressions, a non-traditional notion of constituents and the need to assign semantic representations to them, issues of simplicity and parsimony, and the desire to capture proposed linguistic universals. Chapter 3, 'Intuitive basis of Combinatory Categorial Grammar', introduces applicative categorial grammar, then adds a co-ordination rule and additional rules motivated by combinators borrowed from combinatorial logic. Chapter 4, 'Explaining constraints on natural grammar', introduces constraints on allowable combinatorial rules and discusses several consequences of the CCG formalism that are partly contradicted by empirical data, involving phenomena of English grammar such as subject extraction and heavy NP shift. It also explores how traditional configurational notions such as control, binding, quantifier scope and so-called donkey pronouns can be addressed at the semantic level in CCG. Chapter 5, 'Structure and intonation', argues that the rules of CCG can be used to derive Surface Structures, which for Steedman fully determine intonational structure, compatible with intonation contours that have been proposed in the literature. Semantic notions such as Theme, Rheme, Focus and Background are introduced and integrated into CCG in a framework called Combinatory Prosody, which relies on prosodic marking of syntactic categories.

Part II, 'Coordination and word order' – comprising chapter 6, 'Cross-serial dependencies in Dutch' and chapter 7, 'Gapping and the order of

constituents' – presents case studies of a number of challenging syntactic phenomena and explores the technical ramifications for a CCG analysis that aims to account for them, expanding on Steedman's earlier work on these topics.

Part III, 'Computation and performance', starts out with chapter 8, 'Combinators and grammars', which studies issues of expressivity and complexity: the connection between combinatorial expressions and closed λ-terms is discussed, as well as issues of generative power of CCG grammars, which is strictly greater than that of context-free grammars. The last section and the appendix discuss options for making the directionality of combinatory grammar rules a property of the selected category, as opposed to a property of the rules that combine categories. Chapter 9, 'Processing in context', deals with issues of sentence processing under strict assumptions about the relationship between competence grammars and human parsers. The so-called Strict Competence Hypothesis that Steedman proposes states not only that the human parser directly utilizes competence grammars, but furthermore requires that all structures produced by the parser are constituents of the competence grammar. The consequences of these assumptions for a proposed nondeterministic parsing algorithm and the oracle that guides it are discussed, as well as its relation to other models of human sentence processing. A short tenth chapter, 'The syntactic interface', summarizes and repeats earlier discussions of language acquisition and the role of the Strict Competence Hypothesis in sentence processing.

Two existing reviews, on the *Linguist* mailing list and in *Computational Linguistics*, summarize the contents in detail and highlight the many good things about this book. In this review, we will, however, focus on its more problematic aspects, in the hope that they will be addressed in future work.

First, according to the book, a CCG theory consists of: a set of basic categories (which may be complex in nature to deal with agreement phenomena), with categories defined as finite terms formed from basic categories and connectives; a lexicon, which assigns to each basic syntactic expression one or more categories; a set of rule schemata together with notational conventions and a set of constraints on rule applications in order to block undesirable instantiations of some rules. Especially that last ingredient is somewhat unusual, and the constraints for the fragment of English discussed in SP are never made fully explicit. The use of a rule schema for type-raising is somewhat awkward, perhaps simply because the notational conventions are confusing when multiple type-raised constituents combine, as in the analysis of medial gapping in English (183). This could be formalized more elegantly and perspicuously in terms of second-order quantification. Furthermore, it is not clear what formal pitfalls end up being concealed behind Steedman's use of the untyped λ-calculus together with a vaguely circumscribed notion of semantic types.

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Steedman comes very close to discovering the concept of proof normalization when he notes that alternative derivations for the same utterance 'form semantic equivalence classes' (65). However, CCG differs markedly from Lambek Categorial Grammar since it does not make those alternative derivations formally equivalent, but instead attributes intonational significance to them.

The use of combinators is a defining characteristic of CCG and, at the same time, a bit of a puzzle, since it is never justified properly, neither formally nor empirically. Extensive justification of the choice of combinators might have brought out clear advantages for CCG over other syntactic frameworks. The combinators are used in CCG mostly to name the rules of inference; the combinators themselves, surprisingly, do not occur in the rules of inference that bear their names. Instead, a separate principle is formulated (36) that essentially defines a homomorphism from syntactic types to semantic types while leaving open the choice of semantic terms associated with a rule of inference. This means that, theoretically, it is possible to have a rule of inference called B with a term annotation that shares with the B combinator only its type. In actuality, however, Steedman ends up using precisely the λ-terms corresponding to the combinators. If this choice is fixed, the combinators could have been incorporated directly into the formulation of the rules of inference.

The inclusion of the non-linear S combinator sets CCG apart from most other variants of categorial grammar, and seems to be entirely unjustified. The only justification given in chapter 3 (49) is for capturing so-called parasitic gap constructions, which actually fall into two major classes. First, adjunct parasitic gaps can be captured without the S combinator, by adding suitable lexical entries for the heads of the adjunct phrases in which parasitic gaps can occur. Second, subject parasitic gaps, not discussed at all in SP, pose an immediate problem for an analysis in terms of the S combinator: in phrases such as *executives who even friends of consider backstabbers*, the S combinator does not suffice to derive *friends of consider backstabbers* as S/NP, given reasonable assumptions about the lexicon.

The major problem with SP is its casual attitude towards empirical data in the form of judgements of grammaticality, semantic plausibility, availability of 'readings', and phonetic naturalness, to mention just a few. Nowhere does Steedman give direct evidence for why there is a star, question mark or hash mark in front of any particular example. Although most syntactic research suffers from similar problems, recent work, e.g. Schütze (1996) and Cowart (1997), shows the dangers of such an approach to data-gathering and develops a more scientific methodology.

To repeat a common exercise (Manning & Schütze 1999), the reader may wish to judge the following sentences from SP with respect to grammaticality and then compare her results with Steedman's, given below:

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(1) 'Three cats', ejaculated Tom, prematurely, 'in ten prefer corduroy'. (22)

(2) articles which I will read your instructions before filing (49)

(3) I wanted to, and you actually expected to, try to write a play. (94)

(4) a book which I hope that I will meet the woman who wrote (17)

(Solution key according to Steedman: *, #, ?, *, respectively)

Some of the empirical arguments that Steedman attempts to use in order to argue for or against particular choices he makes in designing a CCG fragment for English simply do not hold up to closer scrutiny. For example, he claims that the special treatment of subject extraction in English (61) is 'supported' by evidence from language acquisition, which suggests that embedded subject questions are 'among the very last details of English grammar' (62) a child learns. However, the support for his theory that Steedman is looking for only follows if one is prepared to make very specific assumptions, in addition to the Strict Competence Hypothesis, about the relationship between time of acquisition of a phenomenon and its representation in a linguistic competence grammar.

Occasionally, empirical argumentation in support of theoretical claims is rather odd. For example, Steedman suggests (58) that it may not be necessary to distinguish between nominal subjects and pseudo-subjects in order to rule out undesirable conjunctions such as in example (5).

(5) [Dexter], and [I wonder whether Warren], is a genius.

Instead, he considers explaining their oddity by a semantic incongruity akin to zeugma or syllepsis. To illustrate this, he exhibits the following sentence, which he describes as a 'real-life example' (58):

(6) This flour is suitable for vegetarians, freezing, pizza dough, and home bread-making machines.

However, this example seems to be quite different from the kind of conjunctions involving nominal and pseudo-subjects that a simple-minded CCG theory of English might fail to rule out. Moreover, the very fact that it occurred in 'real life' strongly suggests that it is not nearly as unusual as Steedman would like it to be in order to argue that example (5) can be ruled out for similar reasons.

Another important aspect of CCG is the major role that the so-called rule-to-rule hypothesis plays. Yet the formal and empirical justifications given in SP are rather problematic. Steedman claims that 'logics and programming languages exhibit a very strong form of the rule-to-rule relation between their semantics and the syntax' (11). While this may be true of most well-studied logics and programming languages, it is not the norm, especially at times when the semantics of those artificial languages is not understood properly, such as during the early days of modal logic and of unstructured

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programming languages. 'This condition in', Steedman continues, 'its most general form means simply that there is a functional relation mapping semantic rules and interpretations to syntactic rules and constituents' (11). Traditionally, the direction of that mapping is reversed; moreover, it is not clear why that relation should be functional in the case of natural languages. All of this leads up to the claim that in 'the natural system, we must therefore expect to find a similarly direct relation between syntax and semantics, for it is hard to imagine any evolutionary pressure that would force it to be otherwise' (11). This statement suggests that carefully designed properties of artificial languages can be found in natural languages and that it is known what the syntactic and semantic units and rules of combination are in natural languages. This hypothesis is not backed up by empirical data, nor does it leave room for the possibility that the syntax/semantics mapping may be highly complex yet learnable, just like most other aspects of natural language.

The psycholinguistic claims suffer from several problems. First, Steedman's theory in its current form does not provide fine-grained processing time predictions for every stage of the parsing process. Rather, processing slowdown is predicted at a specific region in both garden-path sentences and in unambiguous but hard-to-comprehend sentences. Such a radical restriction in explanatory precision and coverage contrasts with other highly articulated accounts of human sentence processing, such as Gibson (1998) and Lewis (1993), making a fair comparison impossible.

A related problem concerns Steedman's Strict Competence Hypothesis. He assumes that the constraints on the grammar itself drive the parsing process. It is not clear whether he assigns any significant role to general constraints on cognition, e.g. working memory. If the claim is that general constraints do not play a significant role in parsing, decades of research suggesting the opposite would at least need to be addressed; for example, see Just & Carpenter (1992) on individual working memory capacity affecting comprehension ability. If no such claim is made, then the question is: what is the relative importance of such general cognitive constraints compared to those imposed by the competence grammar? Other theories that assign a central role to, say, working memory would need to be shown to be empirically inadequate before they can be abandoned in favour of Steedman's view. Steedman appeals to evolutionary grounds and Occam's razor in order to argue for the Strict Competence Hypothesis, but the same arguments can be turned around to argue against this view: why would an information processing system such as the brain abandon general constraints only in the case of language? Appealing to Fodor's modularity hypothesis does not help, since what needs to be shown (empirically) is that language, even though it must pass through the same channels in the brain that other information does, remains largely unaffected by independent constraints on these channels.

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The above discussion illustrates a general problem with Steedman's approach: he merely takes evidence in favour of his theory as confirming it, and makes no attempt to rule out alternative accounts. To give another example, Steedman presents a principle of parsimony, which says that 'the analysis whose interpretation carries fewest unsatisfied but accommodatable presuppositions or consistent entailments will be preferred' (238). Until very recently, the significance of this principle appears to have been underappreciated in psycholinguistic research on scrambling. However, Steedman implies (240) that a paper by Sedivy & Spivey-Knowlton (1993) provides evidence in support of his principle of parsimony. Although the paper presents evidence partly supporting Steedman's theory, one of the main results of that paper (pages 458–459) is that theories such as Steedman's FAIL to fully account for some important experimentally determined processing facts. To be fair, this may be just a citation error, but the general tendency to gloss over problematic aspects of the theory are disconcerting, and misleading to the unwary reader.

SP summarizes and expands on past research on CCG, and contains many interesting and promising proposals which are worth exploring. This book will be easily accessible to anybody with considerable background in traditional syntactic theory and a strong interest in CCG, although the above-mentioned problems regarding formalization and methodology may disturb some. Readers without enough background in traditional syntax may not pick up on the significance of many of Steedman's arguments. The formal underpinnings of CCG are not explored far enough in SP, and anyone interested in the logical aspects of categorial grammar in general may gain more from reading Carpenter's (1998) book. Nor are the practical computational aspects of CCG treated in enough detail to be relevant for computational linguists working on practical systems. The book touches on many complex notions, including the λ-calculus, parsing algorithms, combinatory logic, the question of the non-context-freeness of natural languages, evolution, tree adjoining grammars, and many more. Readers with sufficient background in a relevant subset of these areas will get the most out of this book. Its most damaging weakness, however, is the unsatisfactory data-gathering methodology together with the tendency to view any data seemingly consistent with the proposed claims as evidence in favour. Remedying these two problems is not difficult, and may not necessarily have any adverse consequences for CCG *per se*, but needs to be done before any syntactic research (not just CCG) can develop along scientific lines. In sum, SP is an unconventional book, but may not be unconventional enough.

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Rint Sybesma, *The Mandarin VP* (Studies in Natural Language and Linguistic Theory **44**). Dordrecht: Kluwer Academic Publishers, 1999. Pp. xi + 230.

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As a detailed examination of the syntax of the Mandarin VP, this book is an absolute must for anyone working in the field of Chinese syntax. For anyone already acquainted with Sybesma's work, the research questions are familiar, and much of the data and some of the argumentation are not new, but in a number of places Sybesma introduces new data and draws new conclusions. The footnotes to each chapter make explicit the relationship of the chapter to any previous publications. The book pulls together much of Sybesma's previous research into a single extended piece of argumentation culminating in an account of the *ba* construction and conclusions for both theta theory and theories of event structure.

The central claim defended in the book is that all of the material appearing after the verb in a Mandarin sentence forms a single constituent and that that constituent is the complement of the verb. This is Sybesma's reformulation of the Postverbal Constraint, which he discusses in chapter 1. The Postverbal Constraint refers to the assumption that where other material appears after the verb, the object is forced to move leftward to some preverbal position. The problem that Sybesma addresses in his reformulation is that there are a

number of constructions for which the Postverbal Constraint as traditionally articulated does not appear to hold. In double object constructions, resultative constructions and with duratives and frequentatives, the object is NOT forced to move out of its base postverbal position. A large part of Sybesma's book is devoted to arguing that in these constructions, the apparent object in fact forms a small clause with the other material in the VP and it is the small clause that is the complement of the verb. More precisely, the apparent object is argued to be the subject of the small clause with, for example, the resultative attribute as the predicate. With this line of argumentation in place, Sybesma then proposes that in the *ba* construction, *ba* heads a causative phrase and the NP following *ba* is the leftward extracted subject of the small clause complement of V.

Sybesma makes extensive use of Teun Hoekstra's work on small clauses. He begins in Chapter 2 by applying the small clause analysis to Mandarin resultative structures, such as in (1).[2]

(1) (a) Shoujuan   ku-shi-le.
        handkerchief cry-wet-LE
        'The handkerchief got wet from crying.'
    (b) Zhangsan ku-shi-le   shoujuan.
        Zhangsan cry-wet-LE handkerchief
        'Zhangsan cried the handkerchief wet.'
    (c) Zhejian shi   ku-lei-le   Zhangsan.
        this-CL matter cry-tired-LE Zhangsan
        'This matter got Zhangsan tired from crying.'

The most straightforward is the transitive resultative in (1b) which is analysed as having the underlying structure given in (2a) and made concrete in (2b).

(2) (a) NP [$_{VP}$ V [$_{SC}$ NP XP]]
    (b) Zhangsan [$_{VP}$ ku [$_{SC}$ shoujuan   shi-le]]
        Zhangsan   cry   handkerchief wet-LE

The head of the small clause then incorporates into the matrix head to give the final surface word order in (1b). This is a direct application of Hoekstra's analysis of Dutch and English, except that Sybesma fails to explain why in Mandarin there is obligatory head movement whereas in Dutch and English the head of XP in the small clause obligatorily remains in base position.
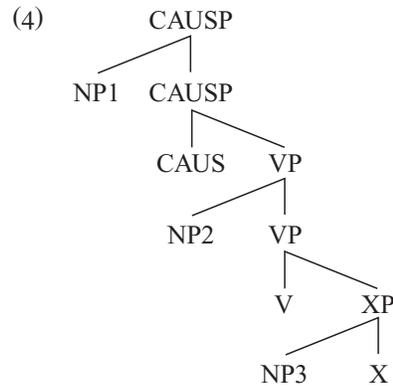
For intransitive resultatives as in (1a), the only difference is that the matrix subject position is left empty, as in (3):

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[1] In the glosses, LE is an aspect marker, CL is a noun classifier and BA is a causative relation marker. The particle glossed as DE in example (8) is traditionally described as a resultative particle introducing a resultative phrase or clause, although Sybesma briefly argues for an alternative analysis of *de* as the head of a functional projection Extent Phrase.

(3) (a) e [$_\text{VP}$ V   [$_\text{SC}$ NP XP]]
    (b) e [$_\text{VP}$ ku [$_\text{SC}$ handkerchief shi-le]]
           cry   handkerchief wet-LE

Again, this follows Hoekstra's (1988), and Hoekstra & Mulder's (1990) analysis for Dutch and English, involving an unaccusative matrix verb with a small clause complement. The criteria for unaccusativity in the Dutch examples, e.g. choice of auxiliary, apply straightforwardly to the matrix verb in the context of a resultative complement. The main criterion for unaccusativity in Chinese, on the other hand, is the licensing of postverbal subjects. Thus, it is a more indirect type of argumentation, namely that the resultative cluster behaves as an unaccusative by licensing a postverbal subject, and hence the matrix verb in the intransitive cluster must be unaccusative.

The second argument for unaccusativity relates to the third type of resultative construction, namely the causative resultative in (1c). Again, the analysis draws on Hoekstra's work, namely his (1990a, b) analysis of the familiar *break–break* unaccusative–causative alternation. The analysis involves a higher abstract predicate CAUS, which theta-marks the surface subject (the causer) and embeds an unaccusative resultative. Thus, the argument is that if the intransitive resultative can be embedded into a causative resultative, it is unaccusative. The structure posited is given in (4) below. As the structure for (1c), NP1 is the base position for the surface subject *Zhejian shi*. The apparent object *Zhangsan* begins as the subject of the small clause XP [Zhangsan lei] embedded under the unaccusative matrix V *ku*. Verb movement produces the cluster *ku-lei-le* which raises still further to head CAUSP.

(4)

```
                CAUSP
              /
      NP1   CAUSP
              /
          CAUS   VP
            /      /
        NP2     VP
                 /
               V    XP
               /     |
           NP3     X
```

This is the structure that Sybesma also arrives at through independent argumentation for the *ba* construction. Adopting the same underlying structure for both causative resultatives and the *ba* construction allows him

to account for the fact that all causative resultatives have a counterpart with *ba*, as illustrated in (5).

(5) (a) Zhejian shi   ku-lei-le   Zhangsan.
        this-CL matter cry-tired-LE Zhangsan
        'This matter got Zhangsan tired from crying.'
   (b) Zhejian shi   ba   Zhangsan ku-lei-le.
        this-CL matter BA Zhangsan cry-tired-LE
        'This matter got Zhangsan tired from crying.'

The head of CAUSP is filled either by verb raising or by insertion of *ba*, which is a dummy with no independent semantics. Thus, any of the interpretative properties of the *ba* construction, such as the definiteness and affectedness of the NP following *ba*, cannot be attributed directly to *ba* but must be derived from abstract CAUS or the embedded predicate. Furthermore, *ba* and the NP following *ba* do not form a constituent since the apparent object of *ba* originates as the subject of the embedded small clause and moves to the NP2 position as specifier of the matrix V.

Examples like (5b) are labelled causative *ba* sentences, since the subject (*Zhejian shi*) does not appear to have any thematic relationship to either of the verbs in the predicate. Rather it is interpreted as the cause or initiator of the overall event denoted by the *ba* sentence. Sybesma argues that not just causative *ba* sentences, but also canonical *ba* sentences such as (6), in which the subject appears to be thematically related to the matrix verb, should have the structure in (4).

(6)   Zhangsan ba shoujuan   ku-shi-le.
      Zhangsan BA handkerchief cry-wet-LE
      'Zhangsan cried the handkerchief wet.'

Sybesma then argues that since all *ba* sentences have non-*ba* counterparts, the *ba*/non-*ba* alternation should be accounted for by the choice between head movement or *ba* insertion as ways of assigning a phonological matrix to the head of CAUSP. This brings him to the claim that since the non-*ba* counterparts (and presumably also the *ba* sentences themselves) are accomplishments, all accomplishments are underlyingly causative.

There are a number of problems here. Firstly, we find Sybesma abandoning without discussion the distinction between transitive and causative resultatives that was developed in chapter 2. While this might seem well motivated by the *ba*/non-*ba* alternation, Sybesma has in earlier work argued explicitly that the non-*ba* transitive resultative does NOT have a causative interpretation. In other words, there is a shift in interpretation that comes with the *ba* alternation, as illustrated in (7), that cannot be accounted for if the transitive resultative has the same underlying causative structure.

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(7) (a) Zhangsan qi-lei-le   ma.
         Zhangsan ride-tired-LE horse
         'Zhangsan rode the horse and IT GOT tired.'
     (b) Zhangsan ba ma   qi-lei-le.
         Zhangsan BA horse ride-tired-le
         'Zhangsan rode the horse and GOT IT tired.'

This difference in interpretation thus also poses problems for Sybesma's claim that ALL accomplishments are underlyingly causative since (7a) is clearly an accomplishment but not causative in the sense of (7b). Of course, the response might be that the relationship between the two subevents of riding and being tired in (7a) is still one of causation. However, Moens & Steedman (1988) show that the relation between the subevents of an accomplishment is neither directly temporal nor causal but rather a relation of contingency.

Sybesma's claim is not just that all accomplishments are causative but, more specifically, that they all have the underlying structure in (4). This claim also entails that the external argument of accomplishments is not thematically related to the matrix verb, or even the embedded predicate where there is one, but receives a semantic role, Initiator or Cause, directly from CAUSP.

Apparent thematic relations with the lexical predicate, he argues, are the result of what he calls 'shadow interpretation', following Hoekstra (1988). Hence, where the external argument appears to have an agentive interpretation, it is not because it receives an Agent role from the matrix verb, but because our knowledge of the world tells us that if the initiator of an event is animate, it is likely to also be the Agent. In other words, we interpret *Zhangsan* as the rider of the horse in (7) because our world knowledge tells us that if someone causes a horse to be tired from riding then they are likely to have done the riding. On the other hand, *zhejian shi* 'this matter' in (5), being inanimate, is not a possible argument of either of the lexical predicates *ku* 'cry' or *lei* 'tired', so our knowledge of the world forces us to pick out another candidate.

Sybesma defends this view using the ambiguity in (8). Both the example and the observation of its ambiguity are attributed to Huang (1990: 39, fn. 14).

(8) Zhangsan ku-de   Lisi hen shangxin.
     Zhangsan cry-DE Lisi very sad
     Either: 'Zhangsan cried so much that Lisi got very sad.'
     Or: 'Zhangsan caused Lisi to cry so much as to become very sad.'

The second reading is only available when *Zhangsan* refers not to the person himself but to an event related to him, such as his death. The problem is that if Sybesma is right and the agentive interpretation is a matter of shadow interpretation, the second intepretation should also be available even where

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Zhangsan does refer to the person himself, because our knowledge of the world also tells us that this is an equally legitimate scenario. Similarly in (7), our knowledge of the world tells us that it is quite possible for Zhangsan to cause a horse to be tired from being ridden by someone else (for example where Zhangsan owns a stable or is a trainer). However, only the agentive reading where Zhangsan does actually ride the horse is available. Hence, Sybesma's application of the notion of shadow interpretation neither explains the absence of a non-Agentive interpretation in (7), nor the restriction to an inanimate interpretation in (8).

Sybesma also runs into problems with his own analysis of examples such as (9).

(9)   Baoyu qi-lei-le   nei-pi ma.
      Baoyu ride-tired-LE that-CL horse
      Either: 'Baoyu rode that horse and got tired.'
      Or: 'Baoyu rode that horse and it got tired.'

The problem Sybesma addresses is the first interpretation where the resultative predicate appears to apply to the matrix subject. This would be a violation of Simpson's Law (Simpson 1983: 144), which states that 'resultative attributes are predicated of OBJECTS, whether surface OBJECTS or underlying OBJECTS'. Sybesma preserves Simpson's Law by arguing that where (9) has the former interpretation, *lei* 'tired' is a two place predicate and the matrix verb *qi* 'ride' is actually unaccusative, the agentive interpretation of Baoyu presumably being arrived at by shadow interpretation. In other words, he assigns the following underlying structure:

(10)   qi [Baoyu lei   nei-pi   ma]
       ride [Baoyu tired that-CL horse]

According to Sybesma's own discussion of Hoekstra's typology of events (14–15), this is clearly an accomplishment. However, he does not assign it the structure in (4). Furthermore, if he were to assign it the structure in (4), then he would have to sacrifice Simpson's Law, which he is clearly unwilling to do.

I have summarised and reviewed only some of the main claims of Sybesma's book. Chapter 3 includes a very detailed examination of the aspectual particle(s) *le* and convincing argumentation for differentiating both between sentence-*le* and verb-*le*, and between two different verb-*le*s. Chapters 4 and 5 are much briefer, more speculative discussions of double object constructions and duratives and frequentatives. Sybesma's argumentation typically relies on fine grained examination not just of grammaticality judgements but also interpretative differences over an impressively large body of data. While this makes for extremely insightful discussion of the data, it makes it very difficult to do justice to the argumentation in a review of this length. Sybesma's discussion of the data is very persuasive; however, the implementation of his analysis is not. In sum, I am not persuaded by the

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overall conclusions that Sybesma draws on the basis of his examination of the Mandarin VP. Nonetheless, the value of Sybesma's work is in the very exacting, theoretically driven examination of large amounts of data which present a real challenge to mainstream generative theory.

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Carol Tenny & James Pustejovsky (eds.), *Events as grammatical objects: the converging perspectives of lexical semantics and syntax* (CSLI Lecture Notes **100**). Stanford, CA: CSLI Publications, 2000. Pp. x + 510.

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The idea that the logical representation of sentences contains reference to events via quantification over an event variable has gained acceptance among logicians and linguists, and has found a wide range of applications in linguistics and logic. Philosophers have focused on questions of metaphysics and ontological commitment, while more linguistically oriented studies have focused on questions such as: what is the syntactic expression of the event variable? How is this argument discharged and where in the syntactic tree is this accomplished? What syntactic constructions are best explained by assuming the existence of this event argument?

  The volume under review is one in a series of recent edited volumes devoted to events in linguistic theory (two others are Higginbotham, Pianesi & Varzi 2000 and Rothstein 1998). While there is considerable overlap in the topics covered in all these volumes, what sets the present volume apart is its focus on what the editors call in their introduction ('A history of events in

linguistic theory') the notion of a 'grammaticalized event' (4), which at least some of the contributors take to be encoded in the morphosyntax of languages. The assumption seems to be that a subset of the possible semantic properties of events is relevant to the morphosyntax, and that the explication of the semantics–syntax interface is facilitated by postulating the notion of a grammaticalized event which encodes just these properties, structured in a particular way. The research questions which follow from the postulation of a grammaticalized event focus on the internal structure of this grammaticalized event, how it corresponds to morphosyntactic structure, and how operations on this structure may be morphologically signalled. Since verbs are predicates of events, we can ask how the meanings of individual verbs are distinguished according to the linguistically relevant semantic properties of events they encode and the event structures they are associated with. The present volume, then, pays more attention to morphosyntactic and lexical semantic concerns than the other volumes and therein lies much of its interest.

The fourteen papers in the book are arranged in three sections. The last section deals primarily with the traditional concerns of event-based semantics and its relation to grammar. The first section deals with the morphosyntactic expression of event notions and the second section deals with the relation between phrase structure and event structure. Because of space limitations, I focus in this review on those papers which reflect the specialized interest unique to this volume.

Assuming a notion of grammaticalized event, there are a number of ways in which it could interface with morphosyntax. There could be an autonomous event structure representation, which is constrained to correspond to morphosyntactic structures in a particular way. Another possibility is that the grammaticalized event is encoded directly and read off of the morphosyntax. Both views are represented in this book by different authors.

In order to appreciate the differences between these two views, and what sets both these 'grammaticalized event' views apart from the more traditional event-based approaches, we can contrast the ways in which the different approaches deal with the phenomenon of aspectual shifts. It has been known since the earliest linguistic studies of aspect that properties of the direct object can affect the aspectual classification of a sentence. The best studied example of this is the effect of the (non)quantized nature of the direct object on the telicity of the sentence (*eat apples* vs. *eat an/the apple*). Studies in formal event-based semantics (e.g. Krifka 1992) take the aspectual properties of the sentence to be compositionally determined by the interaction between the interpretive properties of the verb and those of the direct object, where the referential properties of the direct object (i.e. its (non)quantized reference) are carried over to the predicate through the mediation of the thematic relation between them.

The question which work in this tradition typically does not ask is why the direct object – and not arguments bearing any other grammatical role – is privileged in being able to affect the aspectual classification of the sentence (*People ate the cake* is telic despite the nonquantized nature of the subject). The exact nature of the relation between the direct object and telicity is still a matter of debate, and indeed some linguists have called into question the idea that only the direct object can show this property (Dowty 1991; Jackendoff 1996). However, the broad association of the direct object with telicity is a robust phenomenon which needs to be accounted for, a task that syntacticians have recently turned to. Tenny (1994), perhaps the first syntactically oriented study to deal with the phenomenon, accounted for this relation by means of a set of constraints on the mapping between lexical arguments and syntax. One such constraint ensures that an argument serving as a MEASURE (akin to 'incremental theme') must be a direct object. She does not say how aspectual shifts of the type mentioned above are effected, presumably leaving this to be carried out by the semantics. She therefore represents the first of the views set out above.

Semanticists have also noted that there are morphosyntactic markings, in languages, which appear to signal some of the traditional aspectual distinctions, such as telicity. The relation between the semantics of aspectual distinctions and morphosyntactic categories such as perfective and imperfective is a complicated issue. An excellent and careful study of these issues appears in Hana Filip's article, 'The quantization puzzle'. These studies take the aspectual properties of sentences to be handled by the semantics, with the morphology signalling certain of the aspectual distinctions.

In contrast, papers by Elizabeth Ritter & Sara Rosen (henceforth R&R; 'Event structure and ergativity'), Angeliek van Hout ('Event semantics in the lexicon-syntax interface') and Lisa Travis ('Event structure in syntax') attribute a greater role to the syntax and morphology in determining interpretive properties, thus representing the second view. Instead of deriving aspectual properties from the process which compositionally integrates the semantic properties of the direct object with that of the predicate, with the morphosyntax signalling this shift and principles of mapping constraining the measure to direct object, this approach has the morphosyntax effect the change itself.

R&R argue that there is a syntactic notion of event, presumably a grammaticalization of the conceptual notion, the main components of which are the initiation and termination points, represented in the functional projections of Agr-s and Agr-o, respectively, through case and agreement. The special role played by the direct object in aspectual composition stems from the fact that Agr-o is associated with the interpretive value of delimitation (telicity) and the direct object, when it moves into [Spec, Agr-o], receives the role of the delimiter of an event. Thus, movement into the specifier of the functional projection is what determines telicity, rather than

merely signalling telicity. Van Hout also attributes an active role to the morphology, 'positing that telicity is introduced in the syntactic computation as an interpretable feature that needs to be checked in AgrOP, thereby triggering movement of the object to the specifier of AgrO' (252).

What is the empirical evidence for the claim that semantic properties related to event structure are configurationally determined by the position an argument occupies in syntax? R&R's own words are revealing. They suggest that since the classification of events is determined compositionally, event structure should be syntactically encoded 'because such compositionality is best operationalized in the syntax' (194). This is an obvious way for syntacticians to capture this compositionality, but I see no EXPLANATION here for why accusative case and direct objecthood are so frequently involved in the calculation of telicity. The complex syntactic operations are a way of stating generalizations concerning the relation between accusative case, direct objecthood and telicity, but there are other ways of dealing with this compositionality, as Barbara Partee insightfully points out in her paper ('Some remarks on the linguistic uses of the notion "event"'). Partee notes that 'the generative tradition in semantics as well as syntax tends to assume that any linguistically significant syntactic or semantic property has to be overtly represented as some element in a representation' (490). She stresses that it is possible to express significant semantic properties without them being represented as 'pieces' of meaning, deriving them instead from the application of some semantic operation.

Travis offers evidence for the syntactic encoding of event structure from certain interesting morphological co-occurrence constraints in Tagalog and Malagasy. She suggests that these morphological constraints can be explained by appeal to independently motivated syntactic constraints, if it is assumed that the relevant morphemes head phrasal projections which correspond to subeventual constituents. The best way to argue for encoding event structure in syntax is indeed to show that constraints on event composition are explained by appeal to independently motivated syntactic constraints. But, however insightful Travis's analysis may be, the idea that the morphological patterns she describes are best attributed to syntactic constraints will convince only those who are already predisposed to assume that event structure is encoded in phrase structure and signalled by the morphology.

My impression is that the identification of event structure with phrase structure is motivated largely by the association of transitive verbs with accomplishments. Transitive verbs are indeed often accomplishments, and accomplishments are often analysed as having a bi-eventual analysis. Recent syntactic analyses have motivated the introduction of two VPs in the syntactic representation of transitive verbs, and so it is a small step to identifying the higher VP as the outer event of an accomplishment and the lower VP as the inner event. Explicit links of this sort are found in Travis's

article and in the editors' introduction to the volume. However, there are many transitive verbs which are not accomplishments (transitive activities such as *play the piano*, *jiggle the handle*), and many accomplishments which are not transitive (such as *run to the store* and *roll open*). And, in general, this correlation between syntactic configuration and aspectual classification is far from being as regular as the theories under discussion would lead us to believe. But syntacticians rarely delve deeply enough into the intricacies of aspectual properties of verbs in their various frames to appreciate this. It is not surprising, then, to find syntacticians misanalysing or failing to furnish the appropriate evidence for the aspectual properties of the sentences they provide. R&R illustrate what they consider a process which converts an inherently delimited predicate to one that is non-delimited with the verb 'kiss' in a regular transitive construction in West Greenlandic Eskimo, and with the same verb in the antipassive, which indeed has an atelic or imperfective reading (190). But 'kiss' is not a typical accomplishment or telic verb, and they give no evidence that the transitive form of 'kiss' is telic in West Greenlandic Eskimo. Henry Davis & Hamida Demirdache in their contribution (about which below) state without argumentation that the root meaning 'to hit with a stick or a whip' is a causative accomplishment, though this classification is not at all obvious.

There is also a fact which emerges from the experiments reported in van Hout's paper which deserves some attention. Van Hout has found that, in Dutch, if there is no overt marking for telicity on the verb (in the form of a particle), a transitive verb with a quantized direct object is not necessarily interpreted as telic by adult subjects in a significant percentage of the responses. The same appears to be true in English, and many linguists point out that *John read the newspaper* can have a telic or an atelic reading. The ambiguity is absent, apparently, in parallel sentences in languages such as Finnish or Estonian, which have direct objects which are obligatorily marked as either accusative or partitive. Here, I think, the morphology of the language does play a significant role, but in a way which is not reflected in current analyses. These data are suggestive of the structuralist idea that the range of interpretations a linguistic unit can assume is determined by the forms it stands in contrast with. In English and Dutch, where sentences like *John read the newspaper* do not contrast with a morphologically distinct form, the quantized direct object can still be part of an atelic reading. In Finnish and Estonian, where the accusative necessarily contrasts with the partitive, the quantized object must be part of a telic reading.

Carol Tenny, in her contribution ('Core events and adverbial modification'), maintains her earlier less radical position about the relation between event structure and syntax. She argues that there is a limited correspondence between what she calls semantic zones of composition and syntactic categories encoded in functional projections. She identifies units of event structure and distinguishes different classes of verbs according to what units

of event structure they encode. The focus of her study is a set of adverbials, often lumped among the VP adverbials. She shows how the different adverbials diagnose certain elements of event structure associated with different verbs. Verbs whose meaning involves the achievement of an end state have what she calls a core or inner event, and a subclass of these verbs also contain a path or measure. She argues that measure adverbs like *partway* or *halfway* participate in the semantic composition of a gradable endstate (*to close the door partway* is to say that the door has reached a state of being *partway closed*), and are compatible only with verbs that have a measure or a path (corresponding to the gradable scale defining the endstate). Restitutive *again*, in contrast, takes scope over the entire core event, and is, thus, compatible with any verb that has an endstate, gradable or not. Finally, she gives interesting evidence that the purported scopal ambiguity with *almost* is really not a case of scopal ambiguity, but of different ways of resolving a vagueness associated with *almost*. She then shows that these semantic differences among adverbial types are reflected in syntactic differences between them. The adverbs which compose with the endstate are restricted in their position to being closest to the verb, restitutive *again* must be syntactically outside the measure adverb, and *almost* is syntactically further away from the verb. These differences support a theory which assumes that verbs have internal event structure and that depth of embedding in event structure is reflected in the syntax. This is, perhaps, syntactic evidence that the internal structure of the event is reflected in its correspondence with the syntax.

An article which pays careful attention to the lexical semantics and its relation to syntactic projection and aspectual classification is Liina Pylkkänen's contribution ('On stativity and causation'). It deals with the well-studied argument expression properties of psychological predicates, drawing most of the data from Finnish. The challenge of this class of predicates is well-known: the existence of two classes of predicates, one which maps the experiencer argument to subject (*fear*, *admire*), and one which maps the experiencer argument to direct object (*frighten*, *worry*). Recent studies take the experiencer object verbs to be causative and experiencer subject verbs to be stative, and attribute the differences in mapping to this distinction. Pylkkänen draws attention to the existence of verbs which bear overt causative morphology in Finnish, but are clearly stative, a problem for this approach. Traditional aspectual classifications do not recognize a class of causative statives, and repeatedly characterize causatives as including a change of state event (see, even, the editors' introduction to this volume, 'A history of events in linguistic theory' (for instance, page 7)). But Finnish has pairs of verbs both members of which are stative, where one bears overt causative morphology and maps the experiencer to direct object while the other lacks causative morphology and maps the experiencer to subject. The question arises as to the semantic

difference between the causative stative and the noncausative stative, which the difference in mapping can be attributed to. These pairs can even be based on the same psychological state encoded in the very same root, with one member causative and the other noncausative, as in the Finnish translations of *Mikko finds mosquitoes disgusting* (noncausative), versus *Mosquitoes disgust Mikko* (causative). Pylkkänen suggests that morphologically causative psych predicates are complex stage-level predicates, while noncausative ones denote properties of simple individual level states. The causative morphology, then, signals the derivation of a stage-level predicate from an individual level one.

While many have noted that there are psych predicate minimal pairs, such as *fear* and *frighten* in English, it has gone largely unnoticed that there are actually very few such pairs both based on the same psychological state. There must be a lexical semantic element to the explanation for this fact, and Pylkkänen suggests (429) that there are perhaps some mental states that cannot be conceived of as episodic, and so will not appear in the causative form. More generally, we can feel encouraged that we are identifying the appropriate features of event structure which are relevant to the morpho-syntax, if the features we identify help explain the distribution of various basic verb meanings in the different morphosyntactic realizations. Pylkkä-nen's contribution, then, both provides new insight into the ontology of event types, and also contributes to our understanding of the semantic properties of event types which are morphosyntactically relevant.

Davis & Demirdache (henceforth D&D; 'On lexical verb meanings: evidence from Salish') study the relation between lexical semantic classes and the morphosyntactic derivation of verbs in St'at'imcets, in an article rich with fascinating data. They show that in this language morphologically simple roots for all verbs appear to take one single internal argument, where all other forms and valencies must be morphologically derived by affixation to the root. This pattern of morphological derivation is markedly different from that of other languages and D&D attempt to lay out the implications this may have for theories of lexical semantic representation and its relation to morphosyntax. However, it appears to me that D&D draw somewhat premature conclusions from some of the data they present. They assume that all roots are syntactically unaccusative but semantically causative, but seem to be confusing causativity with transitivity. All the arguments they provide for the causative analysis of the roots in St'at'imcets seem to be an argument for their basic dyadic status. Because all roots come with a single argument with an internal-type semantic role, they conclude that all roots are unaccusative. They then argue that since unaccusatives are telic (based, I imagine, on conclusions drawn from other languages) then all roots in the language are telic. However, they provide no real lexical semantic analysis to support the classification of roots in these ways. This paper is rich in interesting data, and indeed addresses important questions about the relation

between event types and morphological derivation, but more careful lexical semantic analysis needs to be done before any preliminary conclusions can be drawn from the data.

Space has prevented me from reviewing all of the many interesting and important issues raised in the papers of the volume. But this brief review will give the reader an idea of the range of topics covered and the potential for fruitful interaction between researchers interested in different aspects of the way events are represented in language.

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Richard Wiese, *The phonology of German* (The Phonology of the World's Languages). Oxford & New York: Oxford University Press, 2000. Pp. ix + 358.

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In this book, Wiese discusses almost all aspects of Standard German phonology in a very accessible way. He gives an account of the major phonological and morphophonological phenomena found in this language, relying on the theories of Lexical Phonology and Morphology, Feature Geometry, and Radical Underspecification.

The phonology of German was first published in 1996. This paperback edition contains only a few changes to the 1996 edition. Some errors have been corrected and the spelling of German words has been changed in accordance with the reformed orthography of German. The most important change is the addition of a 'Postscript 2000', following the concluding chapter. This postscript pays tribute to the major change in phonological

theorising which has taken place in the last decade. Optimality Theory (OT, Prince & Smolensky 1993) has become one of the major phonological theories. Of course the discussion of the phonology of German has not been unaffected by this paradigm shift and Wiese provides a short overview of recent OT accounts of aspects of German phonology and morphology. In this overview he thematically follows the overall structure of the book. Given this change of perspective by many phonologists (including the author of this book), the question arises what value a book has which explains the phonology of a language on the basis of theoretical assumptions that are by and large incompatible with those of OT. In this review, I will first give an overview of the book's content and then come back to this question.

The book is structured into 9 chapters plus the Postscript 2000. This is followed by an appendix containing a list of all the German words found in the book together with their English glosses and a reference to the page where the word is discussed. This appendix is followed by the references and a subject index.

Chapter 1 gives a comprehensive introduction to the aims and purposes of the book as well as to basic conventions of the notation applied.

The book is very well structured in that the first chapters introduce the key concepts which are used in further analysis of the patterns found in German. After introducing the phonemic system of German in chapter 2, Wiese goes on to provide a feature geometric account of segmental structure in chapter 3. Chapter 3 also introduces the notions of skeleton and syllable, as well as the foot, the phonological word, the phonological phrase and the intonational phrase. Wiese not only introduces these categories but also gives evidence from German for each of them. For instance, he argues for the prosodic category of the foot on the basis of glottal stop insertion and plural formation. He shows that the left edge of the foot is the most important context for glottal stop insertion. This elegantly explains the intricacies of glottal stop epenthesis in German.

These well-motivated prosodic concepts are then used in the following chapter, on prosodic morphology, to explain the distribution of the -*heit*/-*keit* allomorphs, the participial prefix *ge*- and the prefix *be*-, and a range of other issues. This is Wiese's strongest chapter, in which he demolishes the boundaries between phonology and morphology. The two above-mentioned prefixes are only found preceding words starting in a foot. The past participle, for instance, is argued to require words starting in a weak syllable. If none is automatically supplied by the foot structure built up on the word, the epenthetic morpheme *ge*- is inserted to fulfil this condition on participles. I will come back to this issue later.

Chapter 5 gives an account of the interaction of morphology and phonology in terms of Lexical Phonology and Morphology. Each affix is assigned to one of three lexical levels which are captured as root, stem, and word level, thus referring to lexical categories rather than completely abstract

levels of derivation. Arguments for the allocation of particular affixes to particular levels are supplied from various directions such as their behaviour in syllabification, stress assignment, and their phonetic content.

In chapter 6, Wiese gives an underspecification account of the German sound system. This feeds into an analysis of various segmental alternations in the next chapter. For instance, the low vowel a is analysed as underspecified for height. This then explains the behaviour of a in umlaut. The vowel changes in height when umlauted, as in *f*[a]*hren* 'to drive' vs. *f*[ɛ]*hrt* 'drives' (3rd person singular). This underspecification analysis of a also helps to give a systematic account of the diphthongs. German has the three diphthongs aɪ, aʊ, ɔʏ. On the basis of the height underspecification of a together with a rule on rounding assimilation triggered by front vowels, Wiese analyses the diphthongs as underlyingly /aɪ/, /aʊ/ and /aʏ/, respectively. The first vowel in the last diphthong is changed to ɔ by a rounding assimilation rule, which is triggered by front ʏ. This also explains the behaviour of the diphthong aʊ under umlaut, which is fronting of the last vowel in Wiese's account. In words like *H*[aʊ]*s* 'house', the diphthong becomes [ɔʏ] in the plural (*Häuser*). After the umlaut rule (fronting) has applied to /ʊ/, resulting in a front rounded vowel, the context for the application of the rounding rule is created, which then changes a into ɔ. The result is, of course, [ɔʏ].

Chapter 7 discusses various other well-known and controversial issues of segmental alternations in German, such as final devoicing, *g*-spirantization, dorsal fricative assimilation, nasal assimilation, *g*-deletion, degemination and consonant epenthesis. The chapter starts out with segment-related phenomena, then discusses syllable-related processes and ends in a discussion of phonotactics.

Finally, chapter 8 is concerned with stress, starting with simplex words and then moving on to increasingly larger domains, of which the last one is that of the phonological phrase.

Wiese's analyses strongly depend on the assumption of ordered and cyclic application of rules, as well as on the assumption of underspecified underlying structures. Therefore, in the course of reading the book, the impression arises that German is a language which defies an OT analysis, given the assumptions of parallelism and 'freedom of the base' at the heart of this theory. In OT, output forms are chosen from a set of output candidates by parallel comparison of all members of this set with respect to their performance on an ordered set of constraints on surface structures. The necessity of stepwise derivation of output forms from abstract underlying representations is largely disfavoured (though for a different view, see Kiparsky 2001). The 'freedom of the base hypothesis' claims that the underlying representation of a form is rather irrelevant. Instead, the grammar has to produce the right results irrespective of underlying forms. Pairing these assumptions with the theory of lexicon optimization actually

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results in a concept of grammar that determines almost all underlying representations as fully specified structures.

However, many of Wiese's generalisations give insights that stand beyond the choice of a particular theory. For instance, to account for the occurrence of fricatives preceding consonant clusters in syllable onsets (as in [ʃtʀ]*aße* 'street'), Wiese proposes to analyse these as suffricates, the mirror image of affricates, which can be regarded as filling one segmental position only. This saves him from assuming a prependix to the syllable that can be filled only with voiceless coronal fricatives.

An even more intricate case is Wiese's observation on the role of the foot in German phonology and morphology. The environment for glottal stop epenthesis is the left edge of the foot in his account. This implies the creation of degenerate feet in words such as *Idee* 'idea' to account for the occurrence of the glottal stop as the onset of the first syllable. The first syllable of *Idee* contains a short vowel only, and is thus not binary on any level of analysis. Even if the first syllable can be analysed as long, the foot structures in (1) still result in a stress clash, which is usually avoided in German (see Wiese, section 8.5.2 on stress shift, and Alber (2001: 18) on possible foot parsings of words like *Idee*).

(1)

F   F

σ   σ

ʔi   deː
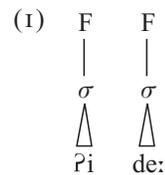
Alber (2001) provides an OT analysis of glottal stop insertion in German, which relies on the assumption that the glottal stop is inserted at morpheme boundaries. To account for morpheme-internal epenthesis in various varieties of German, as in the adjective [ka.ˈʔo.tiʃ] 'chaotic', related to the noun [ˈka.ɔs] 'chaos', she has to assume a positional markedness constraint demanding an onset for stressed syllables. Since the onset of a stressed syllable coincides with the left edge of the trochaic foot, her analysis confirms Wiese's generalisation.

The occurrence of the prefix *ge-* in past participles is also determined by the foot. If a word starts in a stressed syllable, it takes the *ge-* prefix; if stress is found on a later syllable in the word, *ge-* does not occur. The examples below are taken from Wiese (90). I have added English glosses, and stress is indicated.

(2) (a) ge-ˈredet 'talked'   (b) disku'tiert 'discussed'
        ge-ˈsucht 'searched'   ver'sucht 'tried'
        ge-ˈfallen 'fallen'   kra'keelt 'roistered'

Wiese proposes to analyse *ge-* as an epenthetic morpheme to provide a weak syllable at the left edge of the past participle.

In a tentative OT analysis, this behaviour of the prefix could be accounted for by the assumption of an alignment constraint which aligns the right edge of the prefix with the left edge of a foot. (On alignment see McCarthy & Prince 1993.)

(3) ALIGN (*ge-*, R, foot, L): Align the right edge of *ge-* with the left edge of a foot.

If this constraint outranks the faithfulness constraint that demands surface realization of underlying material (MAX-IO), violation of the former will be avoided by omission of the prefix. Additionally, the alignment constraint has to be outranked by a constraint against insertion of material into underlyingly contiguous strings of segments, CONTIGUITY. The latter constraint is also one of the driving forces behind the distribution of glottal stop epenthesis in Alber's account.

(4) A tentative OT approach to the distribution of *ge-*

(i) /ge + disku't-ier-t/	CONTIGUITY	ALIGN	MAX-IO
a. ge.dis.ku.'tiert		*!(dis) *(ku)	
b. dis.ku.ge.'tiert	*!		
☞ c. disku'tiert			*

(ii) /ge + 'red-t/	CONTIGUITY	ALIGN	MAX-IO
☞ a. ge'redet			
b. 'redet			*!
c. 'redetge		*!*	

Contrary to Wiese's assumption (and a range of other authors'; see Wiese (91)), the prefix is assumed to be the underlying affix for past participle here. To avoid analysis of the past participle formation as an instance of a circumfix, the OT account could draw on Output-Output correspondence (Benua 1997, Kenstowicz 1996, and many others) of the participle with either the third person singular of the present tense form (as in *geredet – sie/er/es redet*) or with the infinitive (as in *gefallen – fallen*).

Though tentative, such an analysis would avoid the assumption of an epenthetic morpheme and confirm the prosody-driven nature of the alternation.

In conclusion, this book gives a thorough account of most phenomena of German phonology, insightful descriptions of the facts and many insights which stand beyond the choice of a particular framework. Besides this, Wiese always discusses the literature on the issues he is concerned with, and thus gives an excellent overview of the field. Every chapter (except chapters 1 and 2) ends with a little conclusion. In chapter 5 this is headed 'On some open problems', and in chapter 6, 'Some open questions'. Here the author points to further strands of investigation. *The phonology of German* must be regarded not only as a valuable source for everybody who wants to be informed about the state of the art in German phonology, but also as the starting point for every investigation into almost any aspect of German phonology.

This book deserves a place in the shelves of every 'Phoni' and every 'Morphi'. And if you can't make sense of the word 'Phoni' I recommend consultation of Wiese (62ff.).

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