

SHORTER NOTICES

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Rens Bod, *Beyond grammar: an experience-based theory of language*. Stanford, CA: CSLI Publications, 1998. Pp. xiii + 168.

Reviewed by CHRISTOPHER D. MANNING, Stanford University

During the last 15 years, there has been a sea change in natural language processing (NLP), with the majority of the field turning to the use of machine learning methods, particularly probabilistic models learned from richly annotated training data, rather than relying on hand-crafted grammar models. Until recently, this revolution has had little impact within linguistics proper (as noted, but lamented, by Abney 1996), but this is now beginning to change, giving Bod's *Beyond grammar* particular relevance. This medium-length monograph is largely in the tradition of NLP research, but it is more interesting to linguists than most such work because it does spend time on foundational issues, arguing for an 'experience-based' model of language as an alternative to standard rule-based conceptions.

Indeed, it is useful to distinguish two parts to Bod's book. The larger, middle part is solidly NLP: statistical parsing, the formalization of various probabilistic grammar models and the evaluation of systems on parsing tasks. The beginning and end of the book address the big picture of how to approach human language processing.

The book's thesis is that the central issues in human language cannot be described via a competence grammar, but rather should be described via 'a statistical ensemble of language experiences' (145) remembered by each language user – a corpus, if you will – which the language user draws on and productively recombines to understand and produce new sentences. The central issue of linguistics should not be Universal Grammar but defining a Universal Representation suitable for this corpus – these representational issues are particularly acute once one moves beyond syntactic phrase structure to issues of semantic and discourse representation. In such a conception, linguistic competence and performance are inseparably intertwined. At a big picture level, I think Bod is right in a number of respects. He is right to emphasize that people are very sensitive to frequency when processing and producing language, and that this was for a long time ignored. He is right to believe that linguistics should be more engaged with modern machine learning research, and that it was a mistake to think that limitations on long-term memory are a major concern in models of human cognition. And I suspect that he is right in his scepticism toward traditional views of a strongly innate notion of knowledge of language.

The distinctive feature of Bod's Data Oriented Parsing (DOP) approach is to model sentence probabilities in terms of the previously observed frequencies of sentence fragments, including large fragments, whereas most other approaches work using just information in immediate local trees, with limited means of information percolation, in particular, use of head percolation in a manner familiar from theories such as Generalized Phrase Structure Grammar. The use of large and varied fragments to predict the probabilities of trees allows Bod to give a good account of processing idiom chunks of various sorts, and to explain non-head dependencies, such as between a superlative and a following PP – *the fastest woman in the world* – although whether there is a non-head dependency here depends on details of the assumed linguistic analysis.

This is not the appropriate venue for a detailed discussion of the technical NLP part of the book. To summarize very briefly, Bod's DOP model is one of a number of approaches to statistical parsing developed during the 1990s (see Manning & Schütze (1999) for general background and discussion of other approaches). Most of the book discusses DOP₁ or Stochastic Tree-Substitution grammars, which use conventional phrase structure tree representations. The emphasis on syntactic phrase structure trees is implausible from a psycholinguistic perspective, where most evidence suggests that humans rapidly forget words and syntactic structures and remember only meanings, but Bod justifies this from the practical perspective that the large structured corpora available provide only phrase structure trees. Later chapters discuss

extending the techniques to compositional semantic representations, discourse and recency effects, and non-context-free Lexical-Functional Grammar (LFG) representations. Bod's is one of the leading methods for statistical parsing. In particular, in more recent work he gets very competitive parsing results on standard test corpora (Bod 2001). However, there have been considerable concerns about the theoretical soundness and bias (in the technical statistical sense) of the estimation procedures used (Johnson 1998, Bonnema et al. 2000), and more foundational work on the framework is needed.

Bod's central empirical result is that 'any systematic restriction of the fragments seems to jeopardize the statistical dependencies that are needed' (144). His experiments show that large sentence fragments help in parsing: accuracy is harmed by any method that excludes large fragments. In large part this is true: for all sorts of reasons of genre, topic, style, syntactic dependencies, and so on, there are going to be statistical dependencies between different sentence elements in many configurations, and models that make false independence assumptions will be hurt by dependencies they do not model. On the other hand, Bod's (DOP₁) tree fragment models are particularly simple, and need large fragments because they do not attempt to model the structure of the domain. Grammatical frameworks like Generalized Phrase Structure Grammar or Head-driven Phrase Structure Grammar turn long distance tree dependencies into local dependencies by percolating relationships such as head relationships so that there is a local dependency between the head of the subject noun phrase and the head verb of the sentence, and most state-of-the-art probabilistic parsing models (including Bod's LFG-DOP models) do the same. Since Bod's DOP₁ models do not do this, large fragments are needed to capture these dependencies. Similarly, no attempt is made to incorporate into the model explicit notions of style or genre, so any effects of these will appear as occasional dependencies smeared over large sentence fragments.

The distinction that Bod wishes to make between using a grammar and using a corpus of stored linguistic representations for language processing ends up being rather unclear. In the end, the way his various models work can be described via sufficient statistics that can be computed from the corpus. And to all intents and purposes these sufficient statistics can be thought of as a probabilistic grammar inherent in the corpus. The approach is therefore less distant from work overtly using probabilistic grammars (with the parameters estimated from corpora) than Bod makes out. Bod's use of large fragments leads to connections between his work and example-based or nearest-neighbour approaches (which compute no aggregate statistics over corpora, but just reason from most similar stored exemplars), but in this respect his approach differs somewhat: rather than using individual examples, statistics are aggregated over all fragment analyses.

In summary, this is a useful book for introducing linguists to statistical approaches to language and a new way of thinking about language, but the reader would be well advised to also look at other material for different approaches and further technical background.

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David Crystal, *Language death*. Cambridge: Cambridge University Press, 2000. Pp. x + 198.

Reviewed by ALEXANDRA Y. AIKHENVALD, La Trobe University

The problem of the encroaching dominance of majority languages all over the world, and the disappearance of minority languages, is one of the 'hottest' issues in modern linguistics. 'Will English become the world's only language before too long? How many languages die each day? What does it mean when a language dies?' – these are typical questions journalists or even just lay people ask linguists. The answers Crystal's book provides are accessible, not too technical and yet not overly simplistic. This book offers the general English speaking public the basics – what language death is about, why it is important to preserve linguistic diversity, and how this can be done. Quotations from all sorts of literary and other sources help Crystal prove his points – among them, T. S. Eliot, Rudyard Kipling and George Steiner. So, the general reader – who considers himself sophisticated enough to appreciate literary and philosophical allusions – will rest assured that preserving linguistic diversity is an important and urgent task.

A popular book on language death has been long overdue. And this is especially true in the light of strong tendencies against multiculturalism and in favour of encroaching monolingualism. Crystal summarizes this kind of attitude at the beginning of chapter 2, entitled 'Why should we care?'. A nice and catchy title, reminiscent of Dixon's 'Why bother' (see section 9.1 of his 1997 book dealing with the same issues).

Unlike most linguists who write on the issue of language death and the loss of linguistic diversity – like R. M. W. Dixon, Nancy Dorian, Michael Krauss, Marianne Mithun, to name but a few – Crystal has never done any fieldwork on an endangered language himself. Thus, he relies heavily on other sources – and succeeds in providing a careful, well-balanced summary of the issues of language loss, importance of linguistic diversity and what can be done about it. Many of the points he makes can be traced to numerous other sources – Dixon's *The rise and fall of languages* (1997) is drawn on copiously. The book resembles a patchwork quilt of quotations – in places, this makes it look more like a popular anthology than a single author volume.

Crystal starts by discussing the very notion of 'language death', addressing the issue of how many languages there are and looking at the proportion of 'large' and 'small' languages around the world. To some, the range of 5,000–7,000 as Crystal's 'lower and upper bounds' (11) may appear exaggerated; I find it very useful to have a statement of the problem of 'how to count' languages in a popular book. A word of caution is in order: Crystal's figures are based on tables and data found in *Ethnologue* (Grimes 2000) – widely acknowledged as a source of variable reliability. That is, his calculations based on the *Ethnologue* data are valid only as a first approximation.

Crystal has a difficult task – to maintain a delicate balance between being simplistic and being too technical. In most cases, he succeeds. But I was surprised, in a book like this, to find no references to the established literature on what happens to the structure of languages which undergo obsolescence and attrition, and finally die out. If Crystal had included a brief mention of seminal work in the field, especially Campbell & Muntzel (1989), he would have avoided simplistic statements concerning 'threatened languages incorporating features from the contact language(s)' (22).

The reasons for the necessity of maintaining language diversity, given in chapter 2, are just right. Linguistic diversity has to be maintained 'because we need diversity', 'because languages express identity', 'are repositories of history', and 'contribute to the sum of human knowledge', and because 'languages are interesting in themselves'. This last point probably requires slightly more refinement; otherwise it sounds like praise for 'butterfly collecting' – accumulating facts for their own sake. The reason why languages are interesting in themselves is that they offer 'a unique window' into studying how humans construct representations of the world and encode them into their languages. An apparently 'exotic' category interesting 'in itself' is evidentiality (an illustration Crystal uses, following Dixon 1997). But its interest could go beyond its 'exoticism' – the cultural correlates of this may have to do with people's attitude to knowledge and the importance of being precise in relating information (for fear of being accused of sorcery).

Here Crystal could have made more of the Sapir-Whorf hypothesis (for some reason 'backgrounded' in footnote 63, page 54).

The discussion of pronominal systems with inclusive-exclusive distinctions is quite neat. But the reader should be warned – the main example used is Tok Pisin, an English based creole which is now the main language of Papua New Guinea. It is far from being endangered – quite the opposite: in fact, it is sweeping aside a number of indigenous languages of New Guinea (see Dixon 1997: 110).

Then Crystal proceeds to examine the reasons for language death, and what he says is mostly correct. The seminal classification of 'kinds' of language death suggested by Campbell & Muntzel (1989) would have come in handy here.

At least five kinds can be distinguished:

1. 'Sudden death', or 'linguicide': when all the speakers are simply killed or die as the result of an illness, as was the case with Yana;
2. Radical language death – when a language stops being spoken within one generation;
3. Gradual language death: gradual shift to a 'dominant' language;
4. Bottom-to-top language death: language survives in elevated ritual contexts or 'literate registers'; and
5. Emblematic language maintenance, as secret codes; this was the case with Krekonika, a secret language of Peloponnese masons with Greek grammar and Arvanitika Albanian vocabulary.

These distinctions are important for understanding what type of language knowledge may survive, and, consequently, what the role of a linguist could be.

Chapters 4 and 5 discuss this last issue: 'where do we begin' and 'what can be done'. Though somewhat programmatic, these chapters summarize a number of important issues already discussed in the existing literature. And it may be true that an endangered language can only survive if the speakers increase their prestige (economic, or educational), their self-esteem and their legitimate power within the dominant community. However, the example of the Pirahã community from southern Amazonia shows that an endangered language may survive without being written down; it is their 'inward-looking', self-centred attitude that keeps the community and the language alive (Dixon 1997: 82). Crystal's hypothesis (141–2) that 'an endangered language will progress if its speakers can make use of electronic technology' is hard to accept. Once speakers of a language have access to internet and other electronic facilities they have already been affected by globalization to such an extent that their language and their culture are extremely threatened.

The question of whether language shift can be reversed, or whether an endangered language can be revived, is also controversial. It suffices to say that the Maori which is being 'revived' is structurally very different from the traditional Maori (see Dixon 1997: 110–1). Along similar lines, the 'revived' Kaurua (162) – a language known from detailed materials collected 100 years ago which probably ceased being spoken around 1900 – is a new 'constructed' language, quite different from what the original language must have been. Here, Crystal proceeds with due scholarly caution. He points out that the contentious example of Hebrew revival (where there has been significant continuity in writing and other areas of language use since Classical Hebrew) cannot be used as a model for language revival in other circumstances. This is a good lesson which should be learnt by currently misinformed scholars (for example, Amery 2000).

Crystal argues for the extreme importance of documenting languages and doing fieldwork before it is too late. This advice could not be more welcome. In order to become a competent linguist, one has to go through an apprenticeship, undertaking original fieldwork on a previously undescribed, or poorly known, language, analysing texts, observing day-to-day usage and coming up with a grammar, a dictionary, and a volume of texts. In other words, following the Boasian tradition of language documentation. With the current tendency towards detribalization and urbanization all over the world, doing fieldwork may not necessarily involve living in a mud hut and sacrificing one's own needs for comfort to the needs of linguistics. However, not too many people recognise the need for 'getting one's feet dirty in the field' (65). Only very few people employed by departments of linguistics work on actual languages. And of those who do, only a few bother to write up the results of their fieldwork – this is the case with the majority of linguistic authorities cited in Crystal's book (Dixon, Marianne Mithun and a handful of others being notable exceptions). Besides a great mass of formalists – barely interested in actual languages – the 'armchair' typologists produce slight generalizations based on superficially gathered information. Why do they not document languages – as Crystal suggests?

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The appendix presents a short list of relevant organisations responsible for the 'salvation' of endangered languages. Interestingly, all of them are located in Japan and the Western countries – the USA, Germany, Belgium, France, Spain. One could add just a few academic centres located elsewhere – the Museu Paraense Emilio Goeldi in Belém (Brazil), or the Colombian Centre for Study of Indigenous Languages in Bogotá, or the Academy of Mayan languages in Guatemala (mentioned on page 101). It is a sad fact that despite the high degree of language endangerment in Latin America, many South American linguists are only interested in applying the latest North American formalisms to Spanish or Portuguese, or – at best – to a tiny bit of an indigenous language. My sincere hope is that linguists all over the world will answer Crystal's appeal – to go out and work on languages before it is too late. This useful and accessible book will no doubt alert the general public, beginning linguists – and maybe even funding agencies – as to where the priorities lie.

A final note – all the royalties from the sale of the book will be transferred to the Foundation for Endangered Languages. This is a small but generous gesture.

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Gjert Kristoffersen, *The phonology of Norwegian* (The Phonology of the World's Languages). Oxford: Oxford University Press, 2000. Pp. xvi + 366.

Reviewed by TORE NESSET, University of Tromsø

In this volume, Gjert Kristoffersen aims at providing 'a comprehensive analysis of the phonology of the variety of Norwegian spoken by the majority of the inhabitants of the most densely populated area of Norway' (v). Kristoffersen's analysis is couched in Lexical Phonology, and in the preface the author admits that for this reason it may appear 'somewhat outdated' already (v). It is hard to disagree. I believe the book would have benefited substantially from incorporating insights from Optimality Theory (OT). As it is, OT is only mentioned in passing a handful of times in the text, and OT alternatives are never explored in any detail even in places where the author himself notes the problems of rule-based approaches (e.g. 93, 101, 337).

However, given that linguistic theories tend to be rather short-lived, any linguistic analysis is bound to become theoretically obsolete in a short time. The most important aspect of a book of the type under review is therefore, in my opinion, to what extent it will prove successful as a reference book for future students of Norwegian phonology. In this respect, I find the book very promising. First of all, Kristoffersen not only gives examples as illustrations of his analyses, but provides extensive sets of data with precise information about pronunciation variants and exceptions. He often goes into phonetic detail (e.g. 16f., 236ff.) and offers useful statistics on the type frequency of various patterns (153ff., 254). This makes it easy for students of the relevant topics to consider the viability of alternative analyses. Let me add that as a native

speaker of the variety of Norwegian described in the book, I have few objections to acceptability judgments and transcription. (I am not convinced, though, that the consistent inclusion of an onset in the second syllable of words like *duell* 'duel' (20) and *koala* 'koala' (35) adequately reflects the most widespread pronunciation in coherent speech.) Another aspect of the book that will make it useful as a reference book is the copious bibliography and the many good summaries of earlier work on Norwegian phonology. Since much of this work is published in Norwegian, Kristoffersen makes a substantial body of research available for the international linguistic community.

After a lucid introduction to the linguistic situation in Norway in chapter 1, Kristoffersen explores the segment inventory and phonotactic constraints in chapters 2 and 3. The discussion is thorough, but I found the exposition of phonotactics in chapter 3 somewhat cumbersome. The proposed constraints are fairly complex, and it is not always clear whether they refer to accidental or true gaps. The missing asterisk in the 'Dorsal/strident + palatal constraint' (50) represents one of the few typos likely to confuse the reader.

In chapter 4 Kristoffersen treats voice assimilation, spread of apicality in coronals and lowering of /e/, before turning to syllabification and stress in chapters 5 through 8. Although in general the book is well organized, the devotion of as much as four chapters to syllabification and stress is perhaps somewhat out of proportion. As for stress, the argument against an alternative OT analysis proposed by Rice (1999) is surprising (162). While Kristoffersen may be right in pointing out that words like *armé* 'army', with stress on a final open syllable, may be problematic in Rice's approach, this does not constitute a strong argument in favour of Kristoffersen's own analysis, insofar as he himself seems to assume exceptional lexical marking in cases of this type (159).

My favorite chapter is number 9, on tonal accents. Here Kristoffersen offers excellent overviews of the geographical distribution and historical development of tonal accent in Scandinavian, and gives detailed illustrations of the two most common phonetic realizations in Norwegian, before embarking on a detailed analysis of the East Norwegian pattern. Contrary to the traditional view, he shows that the tonal melodies consist of three tones that each have different functions and pertain to different levels of grammar. The well known minimal contrast between words like *bokser* 'boxer (dog)' and *bokser* 'boxer (athlete)' is accounted for in terms of the presence of a high 'lexical tone' in the latter, but not the former. An important contribution is Kristoffersen's demonstration that the presence of this tone is predictable in many cases (253ff.). Several of the proposed generalizations involve a disyllabic domain with a schwa at the right edge, so one wonders whether the relevant statements could have been conflated into one overarching generalization. This would have represented a gain in generality and simplicity.

Chapter 10 gives a very readable introduction to Randi Alice Nilsen and Thorstein Fretheim's so-called Trondheim model of East Norwegian intonation. The book ends with a fairly long chapter on postlexical phonology and a short overview of the Norwegian Bokmål orthography. These chapters reinforce my general impression of an extremely thorough but in some places somewhat cumbersome analysis. In conclusion, Kristoffersen clearly achieves the goal cited in the beginning of this review. While the almost total neglect of OT is regrettable, *The phonology of Norwegian* is nevertheless a book that no future student of Norwegian phonology can afford to ignore.

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Guido Mensching, *Infinitive constructions with specified subjects: a syntactic analysis of the Romance languages* (Oxford Studies in Comparative Syntax). Oxford: Oxford University Press, 2000. Pp. ix + 267.

Reviewed by CARLO CECCHETTO, Università degli Studi di Milano-Bicocca

This book on the topic of infinitive constructions with specified (that is, phonetically realized) subjects explicitly assumes a generative approach, although generative theories contain a ban against specified subjects in this type of construction (a ban accomplished by various techniques, for example by using the PRO theorem as in Chomsky 1981 or by using the Null Case approach as in Chomsky 1995). Accordingly, infinitive clauses in which an overt subject is present are treated as special cases in the generative framework. However, Mensching uses extensive diachronic and synchronic data to show that specified subjects are a common rather than an exceptional property in Romance infinitival constructions and this leads him to revise the generative theory of nonfinite clauses.

The book is organized as follows. The first part (chapters 1 and 2) is mainly descriptive. The properties of infinitival clauses in Italian, French, Spanish, Portuguese, Galician and other Romance varieties are reported together with a description of their diachronic evolution. The second part (chapters 3 and 4) summarizes the main ideas concerning Romance syntax in the generative tradition and the explanations that have been proposed in this tradition for the presence of overt subjects in infinitival clauses. This part makes the book accessible, at least to a certain extent, also to non-generative scholars. The third part contains Mensching's original contribution on two key issues: the structural position occupied by specified subjects in infinitival clauses (chapter 5) and their Case properties (chapter 6). Since Mensching's analysis is carried out in the Government and Binding framework, which is not the most recent version of the generative approach, the last chapter of the book (chapter 7) contains a reshaping of Mensching's ideas in terms of the Minimalist Program that Chomsky has been proposing in the last decade or so.

It is not possible to summarize Mensching's analyses in a few lines. However, at least one central aspect of his account should be mentioned. He observes a robust correlation between the position of the overt subject with respect to the infinitival verb and its Case. Typically, preverbal subjects in Romance infinitival constructions carry the accusative and postverbal ones carry the nominative (this is by no means exceptionless but Mensching discusses the exceptions). Mensching therefore proposes two different mechanisms of Case assignment, each of which capitalizes on the position that the infinitival subject occupies.

With respect to nominative subjects, Mensching extends the approach originally proposed by Roberts (1993), arguing that the properties of functional categories are parametrized. In particular, in some languages the category (T)ense can assign nominative to a noun phrase it governs irrespective of its finite or nonfinite character. In other languages only the finite inflection, formed by tense AND AGREEMENT features, can assign nominative, and does so in a Specifier-Head configuration. Most Romance varieties that allow nominative subjects in infinitival clauses belong to the first group of languages and this explains why subjects tend to be postverbal. Postverbal subjects are shown to sit in their base position (Spec, VP) and in this position they are governed by the category T. At least one of the exceptions to this trend that Mensching discusses must be mentioned, namely, the incidence of nominative subjects in languages like Portuguese or Galician, in which the infinitive is inflected for agreement. Not surprisingly, the nominative subject tends to precede the infinitival verb in these varieties, a distribution that is explained by the fact that nominative is assigned by the inflected infinitive in a Specifier-Head configuration, by analogy with what happens in finite clauses. All in all, Mensching can derive the distribution and the properties of nominative subjects from a limited set of assumptions, much in the spirit of the generative approach that takes the syntax of natural languages to be the result of the interaction of universal principles and a few language specific parameters.

Accusative subjects are the hardest task for Mensching. Based on the fact that they tend to precede the infinitive, he extends to them the Exceptional Case Marking (ECM) mechanism originally proposed for English examples like *I believe him to be stupid*. In ECM sentences, the

main verb is taken to assign the accusative to the embedded subject. However, there are cases in which extending the ECM analysis to Romance is difficult because, as Mensching discusses (cf. section 6.1.2.1.), accusative subjects can also be found in the subject clause of unaccusative or passive verbs (a string of this type in English would be something like [*him to be stupid*] *was believed*). Unaccusative and passive verbs, under standard assumptions, do not assign accusative, so no obvious assigner of the accusative is available. Mensching handles this problem by assuming that unaccusative or passive verbs can assign accusative in principle and indeed do so in the counterpart of the [*him to be stupid*] *was believed* string, although they don't ordinarily do this. In particular, in a string like *Me was seen* the passive verb does not assign accusative because the noun phrase *me* needs to move to the preverbal subject position (Spec, AgrP) anyway in order to satisfy the requirement that a clause must have a subject. In (Spec, AgrP) nominative is assigned (cf. the grammaticality of *I was seen*) so if the passive verb assigned Case, there would be a Case conflict under the plausible assumption that a noun phrase cannot be accusative and nominative at the same time. This explanation seems problematic both for reasons that Mensching mentions and for other reasons that might be advanced: for example, what is wrong with the string *me to be seen is strange*, in which *me* is assigned accusative by the passive verb in the complement position and later moves to the subject position, in which no further Case can be assigned by the infinitival inflection? One can think of possible answers, but the problem of explaining accusative subjects definitely requires further scrutiny.

This book is bound to become a reference text for anyone interested in the properties of infinitival clauses in Romance. It is a valuable source of data and also advances our understanding of an interesting class of phenomena that before this book did not receive enough attention in the literature.

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Robert McColl Millar, *System collapse, system rebirth: the demonstrative pronouns of English 900–1350 and the birth of the definite article*. Oxford: Peter Lang, 2000. Pp. 371.

Reviewed by MARGARET J.-M. SÖNMEZ, Middle East Technical University

Using a corpus of 11 Old English texts, this book attempts to trace not just how but also why the demonstrative systems of Old English changed as they did. Although the stated focus is on the birth of the definite article, the development of a separate distal demonstrative is seen as a process both parallel to and connected with that of the definite article. The book is generous with its data and ambitious in its aims but possibly over-stretched in its methodology.

That the definite article has its roots in the demonstratives of Old English is uncontroversial; this book offers a discussion based upon the details of all the demonstrative forms in selected texts from what Millar calls the 'transition' period (27). A strength of the book is the writer's decision to present his data in transparent form, so readers can see for themselves what his searches found. In addition, all of the conventional paradigms discussed are provided, so that the reader can easily compare the Old Germanic demonstrative paradigms, and others such as the paradigms for Old Norse enclitic determiners.

Millar introduces his work with a statement of intention to 'demonstrate the breakdown of

the inherited case- and gender-based paradigms and their replacement by a highly circumscribed formal apparatus', and to examine 'the emergence of a discrete definite article ... [and that of] the semantic specialisation of *that* ...' (11). In order to do this he includes a 'discussion of the typological transition in English from a largely synthetic structural pattern to one which is largely analytic in nature' (*ibid.*), and at the same time argues that 'the intense Scandinavian influence over the dialects of the North of England in the late Anglo Saxon period ... was responsible for the direct transfer of semantic and formal structures for the description of definition and deixis from Norse to English' (*ibid.*). It is possible that an acute awareness of the interconnected and multifaceted nature of language variation and change, combined with an attempt to show not only what happened but also how and why it happened have spread the writer's efforts in too many directions at the same time. This has important methodological consequences, not all of which are advantageous.

In terms of methodology, three things are going on in this book; there is a structuralist overall vision of language change which has been combined with a variationist and quantitative technique of language analysis and a sociolinguistic approach to the analysis of the causes and catalysts of change.

The model of language change behind this text, then, is structuralist and can be represented as system 1 – transition – system 2. Language is presented as a self-sustaining system and change is caused by some sort of malfunction in the system, as the title of the book clearly indicates. System 1 is identified as the 'classical' West Saxon Old English demonstrative paradigm(s) (27), to which Millar admits only a limited possibility of variation, saying that it can be 'a very close representation of the paradigm given in Chapter 1' (75). Variation in the transition period is assumed to be less limited. The implication that system 2 (Modern English determiners) is stable and invariant, that a unilateral change is complete, stands (29). Modern English, we are told, has a definite article and a distal pronoun that have distinct semantic and formal existences, apparently existing in different spheres; a 'fissure' has taken place (292), so that in place of one there are now two stable, self-sustaining systems within this postulated stable and standard Modern English usage. But sociolinguists and dialectologists may disagree, and since the corpus included texts from different parts of the country it would have been useful to see the modern reflexes and uses of these forms in those geographical areas too.

Secondly, the analysis of data is based upon a corpus, which of course means that the analysis has to account for more – and more accurately specified – variation than is usually included in conventional structuralist studies of language; it also implies that any qualitative analyses will be supported or modified by numerical frequencies, correlations and significances. Such interpretations are the main strength and sometimes the *raison d'être* of corpus-based analyses.

Full use of these analytic tools has not been made here. Complete word counts are not given: that is, nowhere are we given the universes from which the selected forms are the population under investigation. Neither are frequency counts of the selected words per text given. Where there seem to be very few or very many instances of a particular usage, it would be useful to know how that compares in weighted terms (e.g. percent or per thousand) with the frequencies of that usage in other texts; percentages of one form within the sub-universe of the totality of selected forms are given, but in texts of different lengths this is only of limited use. Statistical significance is altogether missing. Tellingly, there are no works on statistics for language analysis in the bibliography. A result of these omissions is that much of the data analysis reads more like the compilation of separate textual analyses than an integrated longitudinal study.

No mention is made of the Toronto-based *Complete corpus of Old English*, which was available at the time of Millar's data collection. He convincingly argues that excerpts do not provide the best material for analyses such as his, and that inclusion of material transcribed from manuscript is an advantage. The *Complete corpus of Old English* contains full texts, however. Furthermore, most of Millar's materials (at least six of the texts) have been taken from printed editions. It is, in fact, not made entirely clear which of his texts he transcribed from manuscript; one may assume that those that have not been openly acknowledged as taken from an edition were manuscript transcriptions, but this may not be the case.

The third methodological approach comes from a need to account theoretically for variation within and between texts and for change from one historical period to another, and here a certain amount of terminology from sociolinguistics is used (variable rule, koinéization, language contact). But a fully sociolinguistic approach involving a detailed analysis of the social history and structure of the periods and regions from which the texts were produced is not attempted. The central concerns and methods of sociolinguistics are not called upon, nor are the

data used appropriate for such an approach. This is, of course, largely a matter of the shortage of Old English materials amenable to social and linguistic analysis.

There are a number of formatting glitches in the book which make it less easy to use than should be the case: the tables and graphs are unnumbered, which is particularly unfortunate because no complete tabular breakdown of the data is given and finding one's way back and forth among the given data is irksome. There are very many typographical errors in which a word mid-line is split into two hyphenated parts. Other typographical and italicization errors are occasionally found, and there is a garbled sentence (146).

In general, there is much interesting and useful information in this book, and care has been taken to indicate where this study stands in relation to previous publications concerning the selected grammatical points. The provision of data in both text-by-text lists and less detailed longitudinal tables is to be commended, and the conclusions, in which the interactions between language internal and language external factors of change are sensitively demonstrated, are on the whole convincing.

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Carol Neidle, Judy Kegl, Dawn MacLaughlin, Benjamin Bahan & Robert G. Lee. *The syntax of American Sign Language: functional categories and hierarchical structure.* Cambridge, MA: MIT Press, 2000. Pp. x + 229.

Reviewed by ROLAND PFAU, University of Amsterdam

Extensive research since the early 1960s has convincingly proved that signed languages are in fact natural languages with complex grammatical structures. Interestingly, these structures are not only encoded manually; rather, important grammatical information may also be transmitted nonmanually, that is, by facial expressions as well as by head and body movements. Assuming that grammars of natural languages are constrained by the principles of Universal Grammar (UG), then clearly UG 'must provide a space in which the options instantiated by signed languages may be accommodated' (147).

In order to investigate this issue, Neidle et al. (henceforth NKMBL) focus on the organization and distribution of functional categories in American Sign Language (ASL) within the Minimalist Program framework. Their aim is two-fold: on the one hand, they attempt to demonstrate that formal universals (such as syntactic structures and operations) proposed for spoken languages are modality-independent; on the other hand, they want to show how data from signed languages may shed new light on current theoretical debates concerning some of these universals.

The book consists of eight chapters and an appendix detailing notational conventions. The introduction gives an overview of the organization of the book. In chapter 2, the authors make the effort to address some important methodological considerations concerning the socio-linguistic context as well as the collection and interpretation of data 'from a language used by a linguistically oppressed minority' (2).

In chapter 3, basic information about some aspects of ASL grammar is provided, such as linguistic use of space and use of nonmanual marking for expressing syntactic features like [+wh] or [+neg]. The information given is by no means extensive but certainly very helpful for the reader who is not familiar with grammatical properties of sign languages.

The issue of ASL word order is addressed in chapter 4. NKMBL show that the basic word order is SVO and that diverging word orders result from dislocated material – such as topics and tags – occurring in CP-external positions. The authors rightly point out that only after identifying such clause-external elements can one analyze CP-internal word order.

In chapters 5 to 7, specific aspects of ASL structure are considered in some detail: tense and agreement within the clause (chapter 5), the structure of DPs and agreement within DP (chapter

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6), and *wh*-questions (chapter 7). For all of these aspects (except for tense, which is shown to be realized by lexical tense markers), the presence and stretch of nonmanual markings is crucial. Amongst the important and thought-provoking claims NKMBL make in these chapters are: 1) contrary to what has been claimed in the literature, ASL has lexical tense markers; 2) clauses have a structure quite similar to that of DPs: within both domains, agreement may be expressed manually and nonmanually; 3) the hierarchical structure can be inferred from the onset and stretch of nonmanual markings: TnsP dominates Agr_sP and Agr_sP dominates Agr_oP; 4) *wh*-movement in ASL proceeds rightwards and targets a right specifier of CP.

The main findings are very clearly summarized in chapter 8. Moreover, the authors point out how far their findings are consistent with other theoretical results (based primarily on spoken languages) and how far they may be useful in resolving open questions about syntactic structure.

The central claim the authors make is that, in many cases, simultaneous nonmanual markings are a direct expression of abstract syntactic features residing in functional heads. Therefore, '[s]igned languages provide a unique ... type of evidence about functional projections of a kind not available in spoken languages' (1). For the sake of illustration, let us consider clausal agreement. Here, NKMBL challenge the traditional view that in ASL (at least) two types of verbs have to be distinguished: 'plain verbs', which do not overtly agree with any of their arguments, and 'agreeing verbs', which agree with their subject and/or object by means of spatial modification of the sign. In contrast to this, the authors claim that all ASL verbs agree with their subject and object, either manually and/or nonmanually. Consider the following example, which contains the verb LOVE (formerly known as a 'plain verb').

- | |
|------------------------|
| head tilt _i |
| eye gaze _j |
- (1) JOHN_i [+agr_i]_{Agr_s} [+agr_j]_{Agr_o} LOVE MARY_j
 'John loves Mary.'

In the example, the head tilts toward the spatial location associated with the subject while eye gaze targets the location of the object. This use of nonmanual marking is systematic. Whenever a functional head hosts no lexical material for a nonmanual to be articulated with (as in (1)), the nonmanual is forced to spread over its entire c-command domain. From the fact that head tilt begins immediately prior to eye gaze (and both begin before the verb is signed), it is inferred that Agr_s c-commands Agr_o. These facts imply that ASL verbs do not raise overtly. We must therefore assume that feature checking takes place at LF. Unfortunately, this information is banished (66) into a footnote (174f., fn. 5) and very little is said about how feature checking proceeds.

Obviously, close scrutiny is required in transcribing the videotaped utterances, because we are dealing with extremely subtle distinctions here. It is worth pointing out that, since glossed representations of signed utterances necessarily omit important details, the authors make available digitized videos of the key examples on the Internet (<http://www.bu.edu/asllrp/>) and on CD-ROM, this definitely being a very welcome innovation.

Not surprisingly, the most extensive chapter of the book is the one devoted to *wh*-questions, since it is *wh*-questions which have received the most controversial discussion in the recent literature. NKMBL claim that (optional) *wh*-movement in ASL targets a right specifier of CP, as in example (2) below, which contains a sentence-final subject *wh*-phrase. In contrast to this, proponents of a leftward analysis (Petronio & Lillo-Martin 1997) claim that SpecCP must precede TP universally. Therefore, for the same construction, they suggest that an empty [+wh] focus operator is raised to SpecCP, while the right *wh*-element, a base-generated 'double', resides in a right C position and is therefore necessarily a head (cf. (3) below).

- | |
|----|
| wh |
|----|
- (2) [[t_i HATE JOHN]_{TP} [WHO_i]_{SpecCP}]_{CP}
 'Who hates John?'
- | |
|----|
| wh |
|----|
- (3) [[e_i]_{SpecCP} [[t_i HATE JOHN]_{TP} [WHO_{j=}]_{cl_C}]_{CP}
 'Who hates John?'

This chapter is particularly exciting, since NKMBL investigate the data with amazing meticulousness. They show that the leftward movement analysis is confronted with a number of serious problems, for instance, the fact that possessive *wh*-phrases (like WHO POSS CAR 'whose car') may appear sentence-finally. The analysis provided is elegant and, once again, it is supported by the facts of nonmanual marking. Still, I experienced the style of the chapter as somewhat irritating. At times, NKMBL's discussion of Petronio & Lillo-Martin's analysis deals out criticism on a personal basis and has an almost aggressive flavour to it. I find this unfortunate, since an open and critical discussion of this topic will certainly turn out to be very fruitful.

Throughout the book, the line of argumentation is clear and the main findings are conveniently summarized at many points. The pleasure of reading, however, is somewhat reduced by the 227 endnotes (35 pages), which require the reader to constantly jump back and forth. I personally felt that some of the endnotes are superfluous while others contain information so important for the argumentation that they would have been better integrated into the text.

In sum, this book is definitely an essential and stimulating read for everyone involved in sign language linguistics and, more generally, for people interested in clause structure and functional categories. Not only do the authors succeed in showing that syntactic structures and operations are modality-independent, they also expound what theoretical significance their findings have for evaluating proposals – such as Kayne's (1994) antisymmetry theory – that restrict phrase structure as well as directionality of movement. Still, linguists familiar with the research activities of the group, as documented in a number of articles, will find little new information in this publication. The book should therefore be seen as a comprehensive and handy collection of the results of approximately ten years of extensive research.

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D. Kimbrough Oller, *The emergence of the speech capacity*. Mahwah, NJ: Lawrence Erlbaum Associates, 2000. Pp. xvii + 428.

Reviewed by BARBARA L. DAVIS, The University of Texas at Austin

There are currently scholars from a number of intellectual vantage points seeking to resolve questions regarding the nature of the emergence of speech capacities in infants, as well as to evaluate the assertion that 'ontogeny recapitulates phylogeny' in evolution. Oller wishes to elucidate these complex issues via his methodological perspective on linguistically based competence and performance. Based on his long and productive career studying early infant vocal development, Oller proposes a methodology to allow the comparative evaluation of speech acquisition in typically developing infants and infants who may be at risk for developmental delay (e.g. hearing impaired infants). He proposes to extend this metric to comparative ethology with non-human primate vocalization as well as to proposals for the nature of the emergence of the speech capacity in ancestral speakers. In Oller's view, a route to understanding the emergence of the vocal capacity for human communication is comparative ethological studies of infant and non-human primate vocalizations. Oller proposes that an infrastructural hierarchy

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allows comparisons of vocal capacities in their most basic state, and he produces a logical analogue for considering phylogenetic origins for communicative vocal capacity. *The emergence of the speech capacity* describes this hierarchy via properties and principles underlying the signals employed (i.e. infraphonology) and communicative use (i.e. infrasemiotics).

Oller's infrastructural hierarchy focuses first on acoustic transmission units, the signals of language (19), described as infraphonology. Infraphonology refers to 'properties' and 'principles', underlying structures that specify familiar vocal signals or 'operational' units. Properties are the fundamental realms in which a communication system may evolve (i.e. syllabification and recombining). These design features, originally proposed by Hockett (1960a, 1960b), are reconfigured by Oller to reflect multidimensionality, hierarchical organization, and cognitive underpinnings more fully (225). Principles are ways in which the fundamental properties that are present in a communication system may be implemented (i.e. full resonance and normal phonation are principles implementing syllabification). Operational acoustic categories in vocal production reflect these 'hidden units' that specify design systems of language and principles for implementation. Crucially, Oller suggests that infraphonological categories are more appropriate than classic linguistic features (Chomsky 1968, 1993) for understanding acquisition as well as for comparative ethological studies. 'Shoehorning' of vocalization types into categories appropriate to describe linguistic competence or performance in modern adult speakers is proposed as distorting a valid analysis of the nature of infant acquisition. In Oller's view, 'shoehorning' also clouds examination of the origin of the speech capacity by creating artificial conceptual separations in comparative ethological study.

Oller's stated goal in proposing a hierarchical system of properties based on his revision of Hockett's (1960a, 1960b) design features is to 'offer a common ground for comparison among species. The success of the model hinges in part on the claim that the posited properties are constant across deep time and that they represent hidden units and relations underlying all possible vocal communication systems' (25). Properties, in Oller's view, represent a fixed storehouse from which communication systems are able to choose as they evolve, although implementation principles may vary from system to system across species and across deep time. Oller aligns his infraphonological properties with the abstractions of Universal Grammar (e.g. Chomsky 1968, 1993). He characterizes infraphonology as a 'sub-heading within Universal Grammar' (107). Alignment with UG is consistent with linguistic science in assigning underlying causality to a proposed system that has 'psychological reality', in contrast to biological and evolutionarily based inquiries in which the functional origins of the units are crucial (e.g. MacNeilage 1998). Oller concludes generally that ontogeny does not recapitulate phylogeny but that both ontogeny and phylogeny are governed by a common set of infrastructural possibilities. His proposals can be viewed as rich in allowing study of HOW vocal signals may compare across species as well as reflecting possible candidates for the simple forms of signals produced by early speakers as they might compare to contemporary infant and non-human primate signals. However, his system does not specify WHY speakers might have chosen the vocalization system they did or how such choices might have been driven by adaptive advantages within the environment. In chapter 13, comparing possible stages of vocal evolution in the human family, Oller raises a number of issues present in other treatments of evolution of vocal capacity, such as neotony and brain size effects. He gives a speculative prehistory of hominid vocal development employing infrastructural descriptors. However, his general perspective in this treatment is that 'both modern infants and ancient hominids may have been subject to similar constraints of a natural, logical infrastructural sort' (317).

Infrasemiotics reflects Oller's second major requirement for specifying infrastructural relationships in communication systems. He explicates 'recognition of the emergence of infrastructural properties of vocal action that underlie communicative function ... usage of potential signals' (151). Reviewing stages of vocal development in the first year of life, his main focus is on fixed signals occurring before use of 'protophones' (i.e. the vocal precursors to canonical babbling). He sees these 'fixed signals' as most productive for comparative ethology with non-humans as well as being candidates for early vocal signal types in early hominid communication.

Oller reviews a large body of work in the area of early infant vocal development. He also covers his own seminal work on infants at risk for vocal delay. He proposes that the infrastructural hierarchy is a potentially fruitful way to predict which infants may be at risk for vocal delay (i.e. hearing impaired infants) and which may not (i.e. low SES infants). In this regard, his infrastructural methodology provides an explicit method for comparing across

infants without recourse to linguistic categories that may not be appropriate to describe the nature of early vocalizations or their range of use.

The emergence of the speech capacity attempts a broad synthesis across highly varied domains to evaluate a proposed methodology. Description of typical development, prediction of persistent vocal delay, comparative ethology, and the evolution of the vocal capacity are described from the infrastructural perspective. Oller succeeds most straightforwardly in proposing a methodology to facilitate these diverse types of comparisons (i.e. how vocal systems are alike and different). In the case of typical early development, he proposes a valid method for representing the nature and development of vocalization capacity when the infant may be pre-intentional or pre-symbolic, which does not rely on linguistic categories. Infrastructural modeling can also be fruitful in comparative ethology. Potentially, his proposal of a method for understanding how communication systems at their most simple compare may provide a tool leading to the more crucial issue of why such systems evolved.

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Ho-Min Sohn, *The Korean language* (Cambridge Language Surveys). Cambridge: Cambridge University Press, 1999. Pp. xiv + 445.

Reviewed by ONGMI KANG, Chosun University

When I was a graduate student, *The structure of the Japanese language* (Kuno 1973) was frequently referred to in linguistics classes. However, it was hard to find introductory Korean linguistics books written in English. For this reason, this book is what we have been awaiting for a long time.

Sohn has dedicated his whole life to introducing Korean linguistics into international linguistic circles. This is an extended version of his book *Korean*, published in 1994. This is a comprehensive introductory book on Korean linguistics for beginners who are interested in Korean as well as undergraduate and graduate students. It covers Korean grammar descriptively so that theoretical issues are avoided in discussion. All areas of Korean linguistics such as historical linguistics, dialects, phonology, morphology, the writing system and syntax are discussed. Most notably, Sohn's synchronic and diachronic knowledge of Korean is well reflected in discussions of the genetic affiliation of Korean, dialects, lexicon and the writing system.

Chapter 2 reviews several scholars' assumptions concerning the genetic affiliation of Korean. Although the hypothesis that Korean belongs to the Altaic languages is disputable, it is still a dominant one. The book extensively reviews the Altaic hypothesis.

Even though the book is general, some parts are more detailed than expected. Chapter 5 summarizes three components of the Korean lexicon, native Korean, Sino-Korean and loan words, and their function and historical background in word formation. This chapter introduces lots of data in which each component is put together to derive words. Phonological,

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morphological and semantic aspects of each component receive a thorough treatment. The pronunciation of foreign words should be elaborated (116). Korean tends to reflect the actual sounds of the source language. Foreign voiced lax consonants (b, d, g, ʝ) are pronounced as voiceless consonants (p, t, k, ç) in Korean (e.g. desk → tesikhi, band → panti) while foreign voiceless lax consonants (p, t, k, ç) are pronounced as aspirated consonants (ph, th, kh, çh) in general (e.g. trophy → thirophi, post → phosithi). Syllabification of foreign words also needs to be specified. Syllable-initial or coda clusters are fully syllabified by the insertion of the unmarked vowel (trophy → thirophi, mask → masikhi) and after palato-alveolar consonants the front vowel i is inserted (brush → pirəʝi, bench → pençhi). Chapter 6 introduces the Korean writing systems from Itwu (an earlier script of Chinese characters which were adopted to represent Korean during the period of the Silla Kingdom and during the Kolye and Chosun dynasties, between the 7th and the 19th centuries) through to Hangul, the Korean alphabet.

This book missed the inclusion of the new romanization of Korean proposed by the Korean Ministry of Culture and Tourism, and the National Academy of the Korean Language in July 2000. The big difference between the new system and the McCune-Reischauer system lies in the romanization of lax consonants. In the new system the lax consonants are spelled as <g, d, b, ç> before vowels but as <k, t, p, ʝ> before consonants and word-finally. Table 1 shows the differences between the two systems

	Lax stops	Affricates	
	_____V	Lax	Aspirated
McCune-Reischauer	Kumi, Paegam	Choson	Ch'ong
New system	Gumi, Baegam	Joseon	Chong

Table 1

The differences in Korean romanization systems

Chapter 7 provides a descriptive analysis of automatic and non-automatic sound alternations in Korean phonology, with the data being mostly confined to word-level words rather than phrase-level words. Unfortunately, the book does not explain how and why each alternation has to apply. Since it does not consider theory-internal issues, the relationship between the trigger and the target in sound alternations is not discussed. For example, a discussion of the syllable structure of Korean and the relative strength between the coda and the onset consonant would be useful to enable readers to understand assimilation processes such as nasalization and decoronation (place of assimilation), and consonant cluster simplification.

The syntactic structures assumed in chapter 9 need special attention, since they reflect a major issue in the current literature on Korean syntax: a particle suffixed to each phrase is the head of its own phrasal projection. Im (1997, 2000) argues that INFL in Korean takes XP as its complement so that any case markers and sentential suffixes form their own projections. For example, *-ka* 'nominative case marker' and *-il* 'accusative case marker' take NPs as their complements, as shown in (1b).¹

- (1) (a) Chelsu-ka pap-il mək nin-ta.
 Chelsu-NOM rice-ACC eat PRES-DEC
 'Chelsu eats a meal.'

[1] The abbreviations used in this review are as follows: ACC: Accusative Case Particle, DF: Declarative Final Ending, DEC: Declarative Sentence-type Suffix, DFP: Declarative Final Ending Phrase, NK: Nominative Case, NKP: Nominative Case (Kasus) Phrase, NOM: Nominative Case Particle, NP: Noun Phrase, NT: Present Tense, NTP: Present Tense Phrase, OK: Objective Case Particle, OKP: Objective Case Phrase, POL: Polite Speech Level Suffix, PRES: Prospective Modal Marker, S: Sentence, SF: Sign of Full Stop (Period), TC: Topic Contrast Particle, VB: Verb-Bar, V: Verb, Vowel, VP: Verb Phrase.

[2] NOM, ACC and PRES correspond to NK, OK and NT, respectively, in the syntactic structures that Im (1997, 2000) assumed.

- (b) s[_{DFP}[_{NTP}[_{VP}[_{NKP}[_{NP}[Chelsu]_{NK}[ka]]]_{VB}[_{OKP}[_{NP}[pap]_{OK}[il]]]_V[mæk]]]_{NT}[nin]]_{DP}[ta]]_{SF}]²

Along these lines, Sohn assumes that RC (relative clause) is composed of RL (relativizer suffix)+NP and GNP (genitive phrase) is composed of G (genitive case particle)+NP.

The semantic transitivity that is discussed on page 288 is disputable. Sohn defines verbs and adjectives as transitive when the subject in a sentence is an agent or experiencer and the object is a patient or a theme. This is a semantic approach to syntax. He considers the adjective shown in (2a) and the verb in (3a) to be transitive.

- (2) (a) Na-nun kay-ka musew-eyo.
I-TC dog-NOM afraid of-POL
'I am afraid of dogs.'
(b) Na-nun kay-lul musewe-ha-eyo.
I-TC dog-ACC afraid of-do-POL
'I am afraid of dogs.'
- (3) (a) Mia-ka cha-ka iss-eyo.
Mia-NOM car-NOM exist-POL
'Mia has a car.'
(b) Mia-ka cha-lul kaciko iss-eyo.
Mia-NOM car-ACC have exist-POL
'Mia has a car.'

Both (2a) and (3a) have corresponding transitive verbs with an accusative case marker, as shown in (2b) and (3b). I wonder what feature is responsible for semantic transitivity. In short, transitivity cannot be defined semantically but has to be defined syntactically.

In conclusion, despite several minor points mentioned above, Sohn's synchronic and diachronic knowledge of the Korean language is well integrated into this book. The book will be of great help to readers seeking information on Korean linguistics.

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Bruce Tesar & Paul Smolensky, *Learnability in Optimality Theory*. Cambridge, MA: MIT Press, 2000. Pp. vii + 140.

Reviewed by APRIL McMAHON, University of Sheffield

It is quite an event to be invited to review a book about Optimality Theory (OT) – a whole, actual monograph, that is, rather than a collection of papers or a textbook. Practitioners of OT have been somewhat coy about publishing in conventional paper form, and from the perspective

of a commentator on the model, having a detailed statement of this sort to evaluate is by any measure a step forward. This impression is strengthened by the content of the book itself: Tesar & Smolensky (henceforth T&S) have compressed an extraordinary amount of information and analysis into this brief treatment, and although I shall suggest below that some sections are underdeveloped, the general rigour of their argument does extend and develop learning theory under OT in a coherent and suggestive way.

The book consists of eight chapters, plus fairly minimal endnotes, bibliography and index. Chapter 1, 'Language learning', sets out exceptionally clearly the principal challenge of acquisition for OT, namely 'the ambiguity of the overt information that constitutes the actual data received by a learner, and the resulting interdependence of the core grammar and the structural analysis of overt linguistic forms: which grammar a learner chooses depends on how they interpret the forms they hear, and which analysis they choose for a form depends on what grammar they are using' (1). T&S also outline here their proposed solution, the fabulously-named RIP/CD, or Robust Interpretive Parsing with Constraint Demotion. Their algorithms operate in successive iterations, allowing the learner to begin from an initial grammar; then note difficulties and use these to reconfigure the grammar; and then proceed until the best fit between data and hypothesized grammar is achieved. T&S argue that this kind of gradual convergence on the correct grammar can be achieved most fully and speedily using algorithms of the sort they propose, in an overall theoretical envelope supplied by OT: these together can overcome the apparently insoluble paradox that '[t]he learner cannot deduce the hidden structure in learning data until she has learned the grammar, but she cannot learn the grammar until she has the hidden structure' (7–8).

Chapter 2 provides an extremely concise outline of OT, and leads on to the introduction of Constraint Demotion in chapter 3. Here, the key idea is that learners must have access to the input, to Gen, and crucially also to the competitor candidates, which provide 'implicit negative evidence' (33). The inevitable consequence, if the winning candidate is indeed to win, is that T&S need to guarantee that the marks against all the losing candidates will necessarily be greater than the marks against the winning one. 'Constraint Demotion solves this challenge, by demoting the constraints violated by the winner down in the hierarchy so that they are dominated by the constraints violated by the loser' (34–35). Again, this applies iteratively, but within a stratified constraint hierarchy: initially, all constraints are grouped into small sets, or strata, making the computational (and learning) problems more contained. Ultimately, the adult grammar will be a totally ranked one, in which formally each constraint forms its own mini-hierarchy.

Chapter 4, 'Overcoming ambiguity in overt forms', goes on to develop the analysis further in the domain of metrical stress. T&S argue that, during acquisition, the learner must use both production-directed parsing and interpretive, or comprehension-based, parsing, continually refining and re-evaluating decisions on the basis of coherence with observed data and with the hypothesized grammar. T&S outline cases where this iteration may fail, but show that in a series of simulations, convergence on a total ranking was nonetheless achieved in the overwhelming majority of cases; the number of steps involved was also generally rather low, even when the parser was allowed to exit an attempt altogether after a series of unsuccessful steps and start again. Chapter 5, 'Issues in language learning', is an extremely brief, nine-page outline of essentially OT-internal factors such as Richness of the Base and lexicon optimization, while chapter 6, 'Learnability and linguistic theory', summarizes the previous chapters and T&S's conclusions. Finally, chapters 7 and 8 are semi-detached from the rest of the book, being primarily concerned with formal definitions and proofs, and the formal working-through of the model.

In a brief review, it is impossible to discuss the content of this book or the questions it raises in any depth; I shall therefore focus on a small number of issues. The first is one of audience: it is not quite clear who this book is directed at. A general treatment of learning theory in OT terms might appeal to students of phonology and of acquisition, and to practitioners in both those areas; but this is not a very student-friendly book. Chapter 1 is impenetrable without some prior knowledge of OT, with completely unelucidated references to Gen, Con and constraint interaction, for instance; and when the outline of OT in chapter 2 does come, it is so concise and intensive that it does not offer much respite. I find it slightly puzzling that the authors should seek to be offputting quite so early, potentially robbing themselves of an audience for their own contribution discussed in later chapters. Similarly, colleagues in the field of language learning will be disappointed, I suspect, to find so little discussion of general problems of acquisition: those featured are relevant primarily for phonologists who have already decided to use OT (thus,

chapter 5, 'Issues in language learning', begins with Richness of the Base). Chapters 7 and 8, though clearly necessary in establishing that what T&S have claimed can actually be done, will inevitably be pretty hard going even for many working in OT already.

The second major issue, and a much more important one, concerns the nature of the input in OT. This is a notoriously under-investigated question, but it is disappointing that T&S, despite the absolute necessity of an assumed initial input to allow RIP/CD algorithmic iteration, do not really tackle it at all. There are times when it is not clear, to this reader at least, quite how the terms 'input' and 'underlying form' are being used; and even more worryingly, a certain relationship between input and output has to be assumed, such that 'the underlying form is contained within the overt form' (14), although this does not appear to be the case for all current versions of OT, with their proliferating types of correspondence constraints. This is not the only serious issue still unresolved in OT, where there remain, for instance, questions over the innateness and universality of constraints (here, T&S assume the latter, but not the former). One might reasonably challenge the robustness of any theory of learning produced at the moment, when OT itself is in such flux.

A third, related problem involves cases where terms and issues are not fully defined. For instance, when T&S discuss how learners access competing candidates, they say, '[s]uppose the learner receives a piece of explicit positive evidence like $p = \square.V.CV.<C>$ ' (33). In what sense is that 'a piece of explicit positive evidence'? It might mean evidence that can only be interpreted as indicating that a particular string must be syllabified in this way; but T&S do not say what constitutes such evidence – only that any alternative parse must be suboptimal, hence less harmonic, because p 'is given as well-formed learning data and is thus optimal' (33). Later on the same page, this parse is again referred to as 'a single positive example, a parse p of an input I ', and 'the positive datum p ' – but is this really data, or rather a hypothesis about the possible analysis of a datum? Similarly, there is a gap, and T&S admit as much, between the output of learning, which may not terminate in a totally ranked grammar, and the adult grammar, which must be totally ranked. T&S speculate that '[i]n human terms ... one could suppose that by adulthood, a learner has taken the learned stratified hierarchy and refined it to a fully ranked hierarchy' (49). Why 'in human terms'? And how does this fit with versions of OT which require various instantiations of non-total ranking, whether that means allowing ties between violation marks to generate surface variation, or floating constraints? This again relates to the problem that T&S treat constraints as if they were monolithic, whereas in fact there are many varieties of constraints, including constraint schemata, emerging in OT: if these are valid, surely they would cause additional complexity in learning?

It is natural that certain questions should remain unresolved given the current liveliness of the debates over the nature of OT itself and the phonological phenomena it can be expected to account for. In this last respect, T&S take the expected course in sticking to prosody. It is particularly to their credit, however, that they do at least introduce the issue of alternations in chapter 5, though this in itself raises the further unresolved question of how long constraint ranking remains plastic. For instance, data such as the Vowel Shift alternations in English, which admittedly would not have much relevance for the typical OT account of stress and syllabification, but which would be highly relevant in extending OT into the domain of alternations, are acquired rather late. Finally, T&S do in many cases carry out their simulation work on deliberately restricted data-sets and using simplifying assumptions. Thus, in considering tableaux showing how mark-data pairs can be worked out for each winner-loser pair, T&S note (36) that 'the cancellation operation ... is defined only on pairs of sets of marks', though if they are strongly committed to the idea of parallelism in OT, this would probably have to happen for the winner and all losers simultaneously. More generally, in assessing work of this kind, we cannot afford to lose sight of the fact that it is based on computer simulations, and that computers are essentially very powerful one-track minds. Parallel processing of infinite sets may not be such a big deal computationally, but in real-world acquisitional terms, we are not dealing with computers, but with children, who are at the same time trying to learn to run, climb stairs, use their own knife and fork, distinguish yellow from blue and take their own shoes off.

In short, T&S perform a considerable service to those interested in the applicability of OT to learning problems, in spelling out a possible algorithmic solution, and pursuing in some detail its associated definitions and proofs. However, 'in short' is quite important here, in that the service would arguably have been greater if the book had been somewhat longer. Although their argument is formally rigorous, the terse and rather unforgiving style means T&S will inevitably have restricted their own audience, and that many intriguing questions are undeveloped. The

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assumptions T&S make about the nature and type of constraints may make their learning model work well for demonstration purposes, but they represent a radical simplification, not just in terms of the complexity of the real phonological world, but measured against the current state of OT itself, which is no longer either pure or simple. This makes the book an intriguing attempt to simulate a complex problem; but it is unlikely to tempt the unconverted into OT.

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Rosalind Thornton & Kenneth Wexler, *Principle B, VP ellipsis and interpretation in child grammar* (Current Studies in Linguistics 31). Cambridge, MA: MIT Press, 1999. Pp. x + 241.

Reviewed by JEFFREY LIDZ, Northwestern University

At least since Chomsky's review of Skinner's *Verbal behavior* (Chomsky 1959), syntactic theory has been investigated with explanatory adequacy, defined from the perspective of language acquisition, as the central goal. A syntactic theory must ultimately be in a form that sheds light on how language acquisition is possible given the indirect relation between sentences and the grammar that generates them. As we continue to uncover the complexity of natural language syntax, language acquisition becomes correspondingly mysterious. In fact, it is the richness of the theoretical apparatus required to explain the syntax of human languages that is often taken as the hallmark of an innate/universal grammar. Learning a language could not be driven by the input alone since the input is only a shadowy reflection of the grammar underlying it (Hornstein & Lightfoot 1981).

Psychologists and others working outside the generative framework often take issue with this characterization of language acquisition because it grows out of a logical argument about how language must be acquired rather than out of data concerning how languages are actually acquired by real children (cf. Seidenberg 1997; Tomasello 2000). It is an argument based on what must be, isolated from the time-course of language acquisition. Psychologists are interested in how language is acquired, which is neither instantaneously nor perfectly as the Chomskyan idealization would hold, rather than the mere fact THAT it is acquired. Such researchers point out that children differ from adults in their use of language and so it is incumbent on us to ask how and why.

While it is obvious that these two perspectives must ultimately come together, there have been very few attempts to bridge the theoretical and methodological divide. Work categorized under the heading 'psychology of language' rarely takes into account the subtleties of grammatical analysis, whereas work categorized under the heading 'generative perspectives on language acquisition' rarely uses sophisticated experimental methodology to uncover children's grammatical knowledge. Thornton & Wexler's (henceforth T&W) book represents the first book-length study illustrating how this gap can be bridged.

T&W take children's errors in understanding as a starting point for an experimentally driven investigation into whether the grammatical competence of 4-year-olds differs from that of adults. The methodological lesson of this book is that nonadult behavior does not always result from nonadult knowledge. The empirical contribution is that 4-year-olds do know Principle B of the Binding Theory (Chomsky 1981), despite apparent evidence to the contrary, but their lack of discourse competence interferes with our ability to detect this knowledge. In its details, there is much to like and much to object to, but to my mind the contribution of this book is to illustrate how one can simultaneously take grammar and children seriously.

The book centers around the observation that young children seem to violate Principle B in allowing a pronominal to have a clausemate antecedent in sentences like (1).

(1) Goldilocks washed her.

A large literature has emerged in the past fifteen years attempting to show that children do not in fact violate Principle B, claiming instead that this kind of local coreference reading is due to

experimental artifacts, to the child's underdeveloped processing system, or to children's failure to apply certain discourse principles (see chapter 2 for a review of this literature). T&W argue for the third perspective, providing arguments against the view that children's errors are due to experimental artifacts or to a broken parser. They show that children obey Principle B when the antecedent is a quantificational noun phrase, as in (2a), and in VP ellipsis constructions like (2b).

- (2) (a) Every bear washed him.
 (b) Goldilocks washed her and every bear did too.

Following Reinhart (1983), the leading idea is that pronouns can be linked to an antecedent by two mechanisms: variable binding or accidental coreference. On this view, the binding theory deals with variable binding only. The other kind of coreference is pragmatic in nature. For adults the accidental coreference reading is only allowed in a restricted range of discourse contexts. Crucially, because accidental coreference necessarily involves two referential NPs, this mode of linking a pronoun to its antecedent is not allowed when the antecedent is quantificational (QPs being nonreferential). So, if Principle B, which governs variable binding, is being violated by children, then they should be equally likely to violate it in cases like both (1) and (2). They are not. Children who allow local coreference in (1) behave like adults in rejecting it in (2).

The book has two major strengths. First, because it follows the same children through a range of experimental conditions, we are able to see the fine detail of the grammar of binding and ellipsis. Second, the book considers a large set of experimental conditions which differ minimally from each other, allowing us to pinpoint the extragrammatical source of children's errors and providing some limiting conditions on the theories of binding and ellipsis. By demonstrating which things do and do not break together in children's understanding of sentences involving pronominalization and ellipsis, T&W allow us to successfully identify the components of coreference that any theory of the adult grammar must link together.

This latter point is seen clearly in the examination of the parallelism constraint on ellipsis. It is well known that a pronoun in an elided VP must receive an interpretation that is parallel to the interpretation of the corresponding pronoun in the antecedent VP. Thus, in (3a), the pronoun in the elided clause must refer to the same entity as the overt pronoun. Similarly in (3b), if the pronoun is interpreted as a variable locally A-bar bound by *every student*, the pronoun in the elided clause must also be a locally A-bar bound variable. Consequently the second conjunct cannot mean that every professor saw every student's mother (ellipsis indicated by strike-through).

- (3) (a) Sally saw him and Hilda did [_{VP} see him] too.
 (b) Every student saw his mother and every professor did [_{VP} see his mother] too.

Following Fox (1998), T&W refer to these two cases of parallelism as referential parallelism (3a) and structural parallelism (3b). In principle, these two parallelism facts could result from a single constraint or from different constraints. What T&W show is that children who sometimes allow a local coreference reading of pronouns do not violate structural parallelism but do sometimes violate referential parallelism. This suggests that the distinction between referential and structural parallelism should be reflected in the grammar, since the two kinds of parallelism develop independently. It also suggests that whatever leads children to allow local coreference also allows them to violate referential parallelism, suggesting that the appropriate theory of pronoun resolution in the adult grammar will link accidental coreference readings with referential parallelism, a result which may not have been achieved using standard linguistic methodology.

The book also has two major weaknesses. First, the organization of the experimental section of the book is frustrating. T&W lay out ten predictions in chapter 3 and then report on the experiments testing those predictions in chapter 4. Because the predictions are separated from the results by as much as 50 pages, it is often difficult to remember the motivations for any given manipulation. Second, the rhetorical stance of the book is somewhat offputting. The first chapter lays out a set of assumptions in so cursory a way as to make their motivations seem extremely shallow.

Chapter 1 reads more like a manifesto than a reasoned, well-supported introduction to T&W's perspective on language acquisition and grammatical theory. I found this chapter difficult to take (and I share most of the authors' assumptions; I can only imagine how a hostile audience would react), but fortunately for the reader, the objectionable parts of this chapter are mostly

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irrelevant to the overall conclusions of the book. For example, findings of this book concerning children's knowledge of certain grammatical principles (e.g. Principle B, structural parallelism) are important in ways independent of whether these principles are innate. By framing the discussion in terms of innateness, the authors run the risk of alienating those psycholinguists who stand to benefit most from the lessons that what children have to learn is complex and that what they know about grammar, even when they appear to be behaving differently from adults, is exceptionally rich.

A related problem arises in the authors' discussion of the processing system. They take it as the null hypothesis that children and adults share the same parser. This seems reasonable given the learnability problems which could arise if the child had to develop both a grammar and a parser. It should be stressed, however, that this assumption derives from a hunch about a problem (as opposed to facts) and is made in order to take one potential source of variance out of the problem. Unfortunately, this simplifying assumption is presented as a logical necessity. It is entirely an empirical question whether children and adults have the same parser. Moreover, it is possible that children have the same parser as adults but that it doesn't work as efficiently as it does for adults, since children have had less practice using it. Thus, there are lots of available explanations about children's nonadult behavior that T&W dismiss out of hand. Finally, there is even some evidence showing that children do not parse certain ambiguous sentences in the same way as adults (Trueswell et al. 1999), suggesting that T&W's simplifying assumption is unfounded in fact and so needs to be abandoned.

The most egregious misstep comes in T&W's assertion that 'blaming the parser for children's nonadult responses is a "nonexplanation"' (3), since such a position is untestable in the absence of a theory of the parser. While it is true that this hypothesis would need further elaboration in order to be tested, the same is true of the hypothesis developed in this book: that children's grammars work just like adults' grammars, but that children and adults differ in their application of real world knowledge and in their knowledge of the pragmatics of guise creation that restricts accidental coreference readings. Since T&W do not give an explicit theory of guise creation, an explicit description of the real world knowledge that children lack, or an account of how a child could progress from their nonadult state to an adult state, T&W's own conclusions are subject to the same objection that they lay against parsing accounts. In both cases an explicit theory of how adults and children differ is lacking.

Now, there is some evidence in the book that the pragmatic approach is more likely to account for the differences between children and adults in this domain than a processing approach. For example, Grodzinsky & Reinhart (1993) propose that children's failures in Principle B contexts arise from the child's processor being unable to consider two representations (the bound variable representation and the accidental coreference representation) at once, such a comparison being required to determine whether the accidental coreference representation differs sufficiently from the bound variable representation to warrant using it. The fact that some children in T&W's experiments were shown to explicitly consider a range of possible meanings for the sentences in their interactions with the experimenter suggests that the children were able to hold multiple structural descriptions in their head at once, arguing against this kind of processing account. This is a real argument against a processing account. Simply stating that you couldn't imagine how a child with a nonadult processor could turn into an adult is not an argument, especially considering the fact that T&W offer no account of how a child with a nonadult pragmatic system could turn into an adult either.

In sum, T&W's book is an important milestone in research on linguistic development. It mixes careful grammatical analysis with careful psychological experimentation, highlighting the importance of both and illustrating that the examination of children's grammars can provide insight into the organization of adult linguistic systems. While there are some serious rhetorical and logical flaws, these flaws are generally overshadowed by the quality and depth of the research.

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