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REVIEWS

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Herman Cappelen & Ernie Lepore, *Insensitive semantics: a defense of semantic minimalism and speech act pluralism*. Oxford: Blackwell, 2005. Pp. xii + 219.

Reviewed by ROBERT J. STAINTON, University of Western Ontario,
& CATHERINE WEARING, Wellesley College

It is clear that speakers can utter the same sentence in diverse situations and thereby communicate a wide variety of things. What is less clear is how they do this. In *Insensitive semantics*, Herman Cappelen & Ernie Lepore (henceforth C&L) are concerned to defend 'semantic minimalism', that is, the view that linguistically context-sensitive words have a very limited effect on the semantic content of an utterance. Moreover, the only context-sensitive expressions are those that are perfectly uncontroversial, such as personal pronouns (*I*, *you*, *he*, *she*, *it*, etc.), demonstratives (*this*, *that*, etc.), time and place words (*here*, *now*, *then*, *today*, *yesterday*, etc.) and the adjectives *actual* and *present*. These days, this is a highly idiosyncratic view. It is now widely agreed that the range of expressions which, as a matter of their linguistic contribution, anticipate input from context to truth-conditions is simply vast. For example, it is commonly held that the genitive is linguistically marked as context-sensitive (for example, *Rob's book* depends upon the context to determine the relation between Rob and the book), that many verbs and adjectives (for example, *know*, *clean* and *tall*) are flagged in the lexicon as requiring a standard, and that verbs which allow null complements (as in *Mary followed*) have the 'missing' argument contributed by context. C&L think that this emerging orthodoxy, which they call 'contextualism', is severely mistaken.

Before saying more about the content of the debate, let us emphasize that this is a book of considerable importance, which deals with a topic currently at the center of research in the philosophy of language. As a result, *Insensitive semantics* has been and will continue to be widely discussed. The best parts, in our view, are C&L's specific replies to various contextualist arguments, and their introduction of 'speech act pluralism' (on which more below) as an alternative means of explaining away the data used in support of the contextualist position. While we have not been converted to 'semantic minimalism', C&L's ideas are important and genuinely original. Happily for those in linguistics, the presentation is clear, informal, and approachable.

The positions of C&L's opponents are sketched in enough detail to make the material accessible to a relatively wide audience, while at the same time, there are a number of useful arguments which will be of interest to the specialist. The only complaint one is likely to hear, in terms of style, is that C&L could have been clearer about how the various compelling pieces fit into the larger whole. In short, this book pushes the discussion of context-sensitivity forward in new and useful directions. Read it and learn from it.

C&L's discussion falls into two main parts. The first part takes aim at the wide variety of contextualists. For argumentative purposes, these divide into two broad positions: moderate and radical contextualism (but note that these labels are not in our view wholly appropriate for accurately describing any particular contextualist). Moderate contextualism (MC) is the view that there are SOME context-sensitive linguistic expressions beyond the standard set of indexicals, demonstratives, and the like. Contextually provided values must be supplied for these before sentences containing them have truth-conditions. In contrast, radical contextualism (RC) takes context-sensitivity to be pervasive – the semantic content of virtually EVERY expression in the language is sensitive to features of the utterance context in which the expression is used. Hence, no sentence has unrelativized truth-conditions. Note that this is not merely the widely accepted claim that practically every expression can be used in ways that exploit context; it is the claim that, just like *now* and *that*, nearly every expression is semantically incomplete, and its linguistic function, specified in the lexicon, is to exploit context.

C&L provide a veritable host of objections to both MC and RC. We will mention just a few. First, they note the striking similarity among the arguments which are employed by proponents of MC and RC for their respective positions. In light of this, they argue that MC is an inherently unstable position: the arguments that are advanced to show that some expressions are surprisingly context-sensitive can be deployed to show that all kinds of other expressions are context-sensitive. As a result, MC unwittingly collapses into RC. To give an example, the very considerations which lead MC to conclude that the sentence *Bill Clinton is tall* lacks truth-conditions (Is he tall for a President? Is he tall for a man?) can equally be used to argue that *Bill Clinton is tall for an American* lacks truth-conditions (Is he tall for a twentieth-century American? Is he tall for an American basketball player?). Thus, the moderate should either go radical or follow CL and go minimal. Second, C&L point out that various accounts within contextualism are empirically inadequate. For instance, they highlight the failings of positing hidden variables whenever one seems to have previously unnoticed context-sensitivity. Third, they contend that contextualism renders communication impossible. Insofar as MC and RC exhaust the range of opposing positions, this clears the ground for the presentation of C&L's positive account.

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Articulating and motivating their positive account, the combination of semantic minimalism and speech act pluralism, occupies the final third of the book. As stated above, semantic minimalism is the view that there are only a handful of semantically context-sensitive expressions; otherwise, the semantic content of a sentence *S* is what is shared by all utterances of *S*. As C&L are at pains to argue, this content is TRUTH-CONDITIONAL – it is not less than fully propositional, and it is the same proposition in every context (disregarding the handful of truly context-sensitive expressions). For instance, the sentence *Bill Clinton was better* expresses a proposition, although the sentence does not indicate compared to whom or what Clinton was better, nor in what respect. Crucially, the semantic content of *S* is not ALL that is communicated when a speaker utters *S*. Continuing with the example, given that the sentence *Bill Clinton was better* can be used to assert that Bill Clinton was better than George W. Bush *qua* statesman, C&L allow that an utterance can assert more than the proposition expressed by the sentence type. This is not just the Gricean move of introducing implicatures; the twist that C&L add is that the literal speech act content, that is, what is SAID, includes a wide range of further propositions, including many propositions that the speaker herself may be unaware of or may not herself endorse. THIS is the substance of speech act pluralism. Indefinitely many propositions are asserted, stated, or claimed, and the speaker and her context have no particular primacy in fixing which propositions those are. A corollary of speech act pluralism is that there can be no THEORY of the determinants of speech act content – the range of relevant factors is too diverse to be tractable.

Truth be told, we think it is harder than it first appears to locate the substantive disagreements between C&L's positive view and the position of certain contextualists. First, what C&L mean by 'having truth-conditions' is very thin. It amounts, in effect, to this: a sentence has truth-conditions if it can be substituted for *S* in Tarski's T-schema, '*S* is true iff *p*'. When contextualists say that certain sentences lack truth-conditions, even relative to the usual parameters, they mean something like the following: the sentence, relativized to the set of established parameters, fails to partition the set of worlds into those which satisfy it and those which don't. Moreover, many contextualists accept that sentence types have standing meanings and are thus in further agreement with C&L. Finally, contextualists manifestly accept that the literal content of a speech act goes beyond the content of the sentence type, once it is relativized to the familiar parameters. Indeed, that is their central plaint.

That said, what clearly sets C&L apart from ALL of their opponents arises in connection with what one might call 'the metaphysics of speech act content'. C&L hold a very original view of what constitutes speech act content, which is based on their rejection of what they label 'original-utterance centrism', that is, the view that the speaker's own context occupies

a privileged role in determining what she has asserted. C&L's thesis is that what is asserted by an utterance is partly determined by how it will be reported in later contexts. In rejecting original-utterance centrism, speech act pluralism is led to a very expansive characterization of speech act content. Virtually all of C&L's opponents would dispute this metaphysics of speech act content, endorsing instead characterizations that are tied more or less tightly to the words used and the speaker's own context of utterance. Note that, although C&L do not provide an argument for their rejection of original-utterance centrism, it is clear that they are motivated by a strong commitment to taking reports about speech act content seriously.

C&L's characterization of speech act content leads to the disputed question whether the study of speech act content is tractable, that is, amenable to scientific theorizing. C&L maintain that it is not. There will never be a concrete, scientific answer to the question with which we began; and our best hope for saving semantics from the intractable swamp is to become minimalists. In contrast, many of C&L's opponents are engaged in the very project of developing scientific theories of speech act content. Arguably, this constitutes the deepest divide of all.

Authors' addresses: (*Stainton*)
Department of Philosophy, University of Western Ontario,
1151 Richmond St., London, ON, N6A 3K7, Canada.
E-mail: rstainto@uwo.ca

(*Wearing*)
Department of Philosophy, Wellesley College,
106 Central Street, Wellesley, MA 02481, U.S.A.
E-mail: cwearing@wellesley.edu
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Andrew Carnie, Heidi Harley & Sheila Ann Dooley (eds.), *Verb first: on the syntax of verb-initial languages* (Linguistik Aktuell/Linguistics Today **73**). Amsterdam & Philadelphia: John Benjamins, 2005. Pp. xiv + 431.

Reviewed by M. Siobhán Cottell, University of Ulster

The papers in this volume originated in a workshop in 2003 at the University of Arizona and represent a considerable body of work on the syntax of verb-initial languages. This is timely; as Kenji Oda puts it, 'recent developments in the Minimalist program … provide an almost embarrassing wealth of possibilities for achieving V[erb]-initial order' (109), although it is to be

noted that not all the papers in this volume are Minimalist or even strictly generativist. Two interrelated questions motivate the collection:

(i) Are the apparent typological similarities between V-initial languages derivable from some underlying property?
(ii) Is a universal derivation of V-first order possible?

The contributions are divided into two sections: the first nine articles explore the derivation of V-initial word order, while the remaining seven consider categorical, prosodic and information structure issues.

Sandra Chung's 'What fronts? On the VP-raising account of verb-initial order' usefully sets the scene for what follows, presenting the issues that arise from a V(erb)P(hrase)-raising analysis of Verb Object Subject (VOS) word order. In particular, she shows, comparing Tongan, Tzotzil, Malagasy, Chamorro and Māori, that VOS languages do not pattern as a homogeneous group. She concludes that VP-raising is a possible, but not a necessary, solution to verb-initial order, and that much remains to be done.

Of course, one way of explaining predicate-initial order is to appeal to nonconfigurationality. In 'Coordination and constituency in St'át'imcets (Lillooet Salish)', Henry Davis addresses this possibility. St'át'imcets has two dialects: one with canonical VOS order with a VSO alternate; the other with canonical VSO with both VOS and SVO alternates. In an intricate and engrossing exposition, Davis first shows that, with the exception of coordination, all tests argue for a hierarchical clause structure. He then evaluates a number of possibilities for deriving the alternate orders. A major section of the paper is devoted to the intractability of coordination and to ways of overcoming it.

Chung's conclusion that there is no single path to VOS order is neatly paralleled in Yuko Otsuka's contribution, entitled 'Two derivations of VSO: a comparison of Niuean and Tongan', which argues that, in spite of close genetic relatedness and superficial similarities, predicate raising in Niuean and Tongan targets different positions. This accounts for cross-linguistic differences with respect to scrambling and clitic placement. The claim is made that this difference is derivable from the featural content of the functional category Tense (T). In Niuean, T bears a [Pred(icate)] feature, while Tongan T has a [D(eterminer)] feature which must attract the subject.

Felicia Lee's 'Force first: clause-fronting and clause-typing in San Lucas Quiaviní Zapotec' is the first of a number of papers which depend on the typological observation that verb-initial languages tend to have initial clause-typing particles. However, in San Lucas Quiaviní Zapotec, some of these particles surface in clause-final position. Lee uses this observation to motivate an analysis of VOS order in terms of remnant VP-movement to the specifier of TP, with subsequent movement of TP to ForceP. Even in the absence of an overt particle, string-vacuous remnant VP-movement will apply if the C(omplementiser)-related features in the language are strong.

Initial particles also play an important role in Kenji Oda's article, 'V1 and *wh*-questions: a typology', which depends on the clause-typing generalisation of Cheng (1997). Allowing that V-initial order can be derived by either V-movement or VP-movement, he distinguishes the two by parametrising the EPP [Pred] feature in the C-T system. In essence, Oda argues that in a VP-movement language, a phi-related element cannot move to a C-T specifier position since C-T bears the EPP [Pred] feature. An implication of this is that Irish, which disallows clause-typing by movement, must be a VP-movement language, contrary to what is assumed in most previous analyses. The obvious objection that Irish has clause-initial *wh*-phrases is finessed by an analysis of *wh*-questions as pseudo-clefts.

Dirk Bury's paper, 'Preverbal particles in verb-initial languages', interprets the correlation between order and clause-typing particles rather differently, using it to motivate a model of syntax which defines phrase structures as sets ('treelets') which state only dominance relations. Defined thus, clause structure is 'flexible' and can be extended without cost. V-movement is a process at the Phonetic Form interface level, involving pronunciation of the verb in a derived head position which must be preceded by a specifier. In V-initial languages, where there is no preceding specifier, the verb must occur in the position of an independent element, such as the familiar clause-initial particles.

A more familiar view of Irish syntax is defended in James McCloskey's 'A note on predicates and heads in Irish clausal syntax'. By characteristically careful examination of ellipsis and coordination, he demonstrates that predicate-initial and verb-initial constructions are not a single phenomenon, and, further, that not all fronted predicates behave alike, with some predicate-initial constructions showing evidence of additional head-movement. The paper concludes with a discussion of predicate–subject order and explores the implications both of a movement analysis and of base-generating the subject in a rightward specifier.

We return to particles in 'Seediq: antisymmetry and final particles in a Formosan VOS language', in which Arthur Holmer presents an antisymmetric analysis of a language which has VOS order and both initial and final particles, and compares the ordering of these particles to Atayal and Tongan. Holmer analyses Seediq as a predicate-raising language, which moves VP to TP. Subsequent movement pied-pipes a category dominating TP to a higher projection headed by a clausal particle, thereby deriving particle-initial order. In Tongan, however, the analogous particles surface as second-position clitics, which motivates a V-movement analysis, as in Otsuka's contribution.

Even more than in McCloskey's paper, VP-ellipsis is central to Lisa deMena Travis's argument in 'VP-internal structure in a VOS language'. Data involving ellipsis is used to compare a number of previous analyses of Malagasy, and Travis concludes that only an intraposition analysis, which

allows iterated predicate fronting, accounts for the interaction of ellipsis and adverbials. Crucially, the analysis takes Malagasy adverbs to be inflectional heads which license ellipsis, similarly to I(nflection) in English. However, while English licenses ellipsis of a complement, Malagasy elides specifiers. Thus, this paper not only addresses constituent order, but also furthers cross-linguistic understanding of ellipsis processes.

Niuean, the focus of Diane Massam's 'Lexical categories, lack of inflection, and predicate fronting in Niuean', lacks inflectional morphology. Massam analyses 'verbal' elements as nominal/participial in nature (a claim that is not new), which are thus freed from a need to check Tense or Agreement features. The category headed by such elements is available for predicate fronting, on the (universal) assumption that all clauses must involve displacement of some constituent from within *v*P to a higher position. This view of predicate fronting has, of course, considerable typological and featural consequences for V-initial systems in general.

In David Gil's 'Word order without syntactic categories: how Riau Indonesian does it', the notion of underspecification is much more pervasive. Analysing Riau Indonesian as lacking any lexical, head/phrase or lexical/functional distinctions, he asks how the language comes to present many typological correlates of V-first order. The solution proposed adopts a single linearisation principle (heads precede modifiers) and principles of Iconicity (for conceptual reasons) and Information Flow (for functional reasons). Gil argues that, in combination, these principles result in superficial verb-initial order.

Another view of feature distribution is presented in Mélanie Jouitteau's 'Nominal properties of *v*Ps in Breton: a hypothesis for the typology of VSO languages'. Jouitteau reduces three familiar Celtic phenomena (anti-agreement, construct states and genitive assignment to objects) to a single parameter, namely the interpretability of a [D] feature on *v*. Parallels between *v*Ps and DPs then follow, with V-to-*v* raising as the counterpart of N-to-D raising. Complementary agreement effects are accounted for if, in addition to [D], *v*P bears phi-features, in an analysis which draws on previous treatments of both incorporation and government.

Parallels between nominals and CPs are also addressed in Hilda Koopman's 'On the parallelism of DPs and clauses: evidence from Kisongo Maasai'. The claim is defended that, in Maasai, DPs that are headed by common nouns are always relative clauses, which entails that clauses and nominals have an identical substructure. In an intricate analysis of Maasai DPs, the author shows that a remnant copular constituent raises to CP. For clauses, Koopman argues for a similar movement of a predicate remnant to ForceP. In this way, clauses and nominals are shown to be maximally similar in their derivation.

In 'Ordering clitics and postverbal R-expressions in Tagalog: a unified analysis?', Loren Billings investigates whether Tagalog has VSO or VOS

order, together with the issue of whether, outside semantics, the notion 'subject' has any role in the language. The central claim is that Tagalog allows proper names to appear in clitic positions. Once this is accepted, the language has unmarked subject-final order, with the effect that subjecthood is indeed relevant. The paper leaves open, however, whether or not subject-final order is derived or base-generated.

In 'The syntax of Chalcatongo Mixtec: preverbal and postverbal', Monica Macaulay discusses two types of preverbal constituents in an otherwise VSO language. Having compared the data to the standard implicational universals (Greenberg 1966), which yields interesting and not entirely predictable results, she analyses the preverbal constituents in terms of Topic and Focus. VSO order is argued to involve movement of V to a position above IP, with postverbal subjects in the specifier of IP.

Movement to C in the derivation of predicate-initial order is also proposed in 'Accounting for verb-initial order in an Australian language' by Mary Laughren, Rob Pensalfini & Tom Mylne. Once again, Focus is involved. For the Australian language Wanyi, the authors claim that predicate-movement to the specifier of CP checks the Focus feature and, importantly, that predicates are lexical predicative phrases. Verbal and nominal predicates are analysed as complements of functional V and N, respectively. Evidence for a Focus analysis comes from the complementary distribution of these predicates with *wh*-phrases, focussed XPs and the phrasal marker expressing clausal negation.

As this brief summary shows, the answers to questions (i) and (ii) above appear to be negative, and the editors admit as much in the introduction. What emerges instead is that V-first order is a most intricate matter. This conclusion has important implications for syntactic theory more generally, since if V-first order can be derived in many different ways, verb-medial and verb-final orders may be just as diverse. Thus, this book will be of interest not only to specialists in the area, but to typologists and syntacticians more widely, and will offer both support and challenge.

Most of the papers here rely on either head-movement or XP-movement of the predicate. Neither is entirely innocent. The first depends on a process which some have argued not to be part of syntax at all (cf. Bury's article in this collection and Chomsky 2001). The second would appear in many cases to require evacuation of all phrasal constituents, including adjuncts, from *v*/VP prior to raising – a matter which is touched on by McCloskey and Massam, but not by all contributors. It remains unclear which positions these constituents eventually come to occupy, although, to be fair, V-movement analyses are also not immune from an apparent requirement of argument positions external to *v*P (cf. McCloskey 1996). However, much Minimalist work of the last decade has been devoted to eliminating such positions, although this is, ultimately, an empirical issue.

Finally, there is one central and poignant fact that pervades this book. Linguistic analysis of this calibre relies crucially on the intuitions of native speakers, and written records – if they even exist – are always woefully inadequate for such finely-grained work. With so many of the languages examined here in danger of extinction, a collection such as this may be simply impossible in a couple of decades' time.

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Author's address: School of Communication, University of Ulster at Jordanstown, Shore Road, Newtownabbey, Co. Antrim BT37 0QB, Northern Ireland, U.K.
E-mail: ms.cottell@ulster.ac.uk
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Laura J. Downing, T. Alan Hall & Renate Raffelsiefen (eds.), *Paradigms in phonological theory* (Oxford Studies in Theoretical Linguistics **8**). Oxford: Oxford University Press, 2004. Pp. x + 349.

Reviewed by JEROEN VAN DE WEIJER, Leiden University

This book, which resulted from a conference held at the Zentrum für Allgemeine Sprachwissenschaft in Berlin in March 2001, is a collection of eleven chapters, of which the first is an introduction by the editors. In this chapter, entitled 'Introduction: the role of paradigms in phonological theory', the editors first present a brief history of the role of paradigms in pre-generative work, focusing on the Neogrammarian doctrine of exceptionless sound laws. Exceptions to these laws were analysed by having recourse to analogical influence. In the period of phonology dominated by Chomsky & Halle's (1968) *The sound pattern of English*, there was no role for paradigm uniformity in any formal sense (although its effects were of course observed, as Luigi Burzio points out in his contribution). The advent of Optimality Theory brought new possibilities of incorporating paradigm uniformity into the grammar, most specifically by recognising Output–Output correspondence relations between paradigmatically related words. This line of

investigation was fuelled especially by Laura Benua's (1997) dissertation, in which she formulated her Transderivational Correspondence Theory (TCT). Two competing ideas can be distinguished in theories such as the TCT – an issue which plays a key role in many of the other chapters of the book: either the idea is adopted that one form in the paradigm plays a central role, so that derived forms mimic the base form ('base priority' models); or it is held that all forms of the paradigm enjoy the same status, so that forms tend to reflect the shape of the majority of the members of the paradigm ('symmetrical' models). In his chapter, 'Optimal paradigms', John McCarthy suggests that base priority models are best suited for derivationally related forms, while symmetrical models are better suited for inflection. The editors' introduction outlines the differences between the two models in detail, arguing that the difference lies in implementation rather than, for instance, the constraints that are used. They convincingly show that the kinds of effects that are accounted for cannot be derived in earlier approaches, for example, by using cyclicity.

In the introduction, more attention might have been devoted to important, general questions such as the definition of 'paradigm', which is not trivial at all. Authors who do address this point to some degree are Péter Rebrus & Miklós Törkenczy (see below). Related to this, of course, is the question of what it actually means for a paradigm to be uniform, and how this dimension should be incorporated in linguistic categories. For instance, does paradigm uniformity refer to identity at the segmental level, or could identity also be defined in terms of distinctive features, or even in terms of smaller, phonemically irrelevant but phonetically salient properties? These topics play a role in a number of the later chapters.

In the second chapter in the volume, entitled 'The morphological basis of paradigm leveling', Adam Albright deals with the well-known case of analogical paradigm levelling in Latin, which eliminated the *-s* in the nominative singular form in words like *honoːs* 'honour', on the basis of oblique forms such as *honoːris* 'honour.GEN-SG'. Special discussion is devoted to the question of how a base can be established, that is, how we know whether it is form X that influenced form Y, rather than the other way around. The case of Latin is interesting because an oblique form influenced the nominative singular, rather than the nominative singular influencing the oblique form as might be expected. We may therefore assume that the oblique form served as the base and that other forms were derived from this base.

The next chapter, 'Competing principles of paradigm uniformity: evidence from the Hebrew imperative paradigm', by Outi Bat-El, starts by examining the notion of paradigm, which is no easy task in the context of partly non-concatenative and partly concatenative morphology as in Hebrew. The interesting observation here is that the imperative form of verbs is phonologically based on the future form. Bat-El shows that different dialects of Hebrew make use of different rankings of the same constraint set.

Luigi Burzio's 'Sources of paradigm uniformity' tackles, among other things, the issue of which form is the source of Output–Output faithfulness, thus raising the question whether there is a base for paradigm uniformity and examining the implications of this for our view of the relation between phonology and morphology. First, as in much of Burzio's earlier work, he argues for the representation of the lexicon as a kind of neural network, in which uniformity can be regarded as the activation of a particular neighbourhood in the network. Second, he shows that there is no need to allow for cyclicity under his approach.

In '"Capitalistic" vs "militaristic": the paradigm uniformity effect reconsidered', Stuart Davis addresses a matter relating to the phonetics–phonology interface, viz. the question whether phonetic (i.e. non-contrastive) properties can also be subject to paradigm uniformity effects. As argued by Steriade in previous work (see, for example, Steriade 2000), a positive answer would undermine the need to recognise a distinction between the two fields. The bone of contention is the pronunciation of [t] in *capitalistic*, which is flapped (according to Steriade) because of a paradigm uniformity effect involving *capital*, which also has a flapped [t]. This is contrasted with unflapped [t] in *militaristic* (cf. *military*, also unflapped). On the basis of a wide array of data, Davis argues in favour of an approach in which (uniformity of) foot structure takes pride of place, thereby invalidating Steriade's argument for recognising phonetic properties in phonological constraint systems.

In 'Jita causative doubling provides optimal paradigms', Laura Downing shows that traditional approaches which embrace cyclicity cannot deal with the pattern of causative doubling in the Bantu language Jita (spoken in Tanzania). Her discussion bears on the question whether the similarity of PARTS of words can also play a role in the computation of paradigmatic uniformity – which Downing answers in the affirmative – and, once again, on the question of the appropriate definition of a paradigm, which in this case must involve ONLY the causative and no other forms.

Michael Kenstowicz's 'Paradigmatic uniformity and contrast' is a lucid paper dealing with a number of cases involving various languages (Spanish, Russian, Bulgarian, Chi-Mwi:ni, Arabic). In this contribution, paradigmatic CONTRAST rather than, or in addition to, paradigm uniformity plays a role – that is, the tendency to keep forms with different morphological functions distinct, although, if regular phonological rules were allowed to apply, these forms would be pronounced similarly. While this is an expected effect, its exact place needs to be determined by further study.

John J. McCarthy's influential chapter, 'Optimal paradigms', was widely circulated before this publication. It lays out in great detail the theory of Optimal Paradigms (OP) and applies this model to Arabic paradigms. In this extension of Optimality Theory, candidates consist of entire (inflectional) paradigms, and correspondence relations hold from every paradigm member

to every other paradigm member. As a result, it is predicted that phonological characteristics that hold for a majority of forms in the paradigm may be extended to other forms which lack such characteristics. A second clear prediction is that underapplication effects are not to be expected in paradigms because less marked forms that are similar will always be preferred to similar forms that score worse for some markedness constraint. Both these predictions need to be scrutinised in further work, as well as the extent to which inflectional paradigms and derivationally related forms can act differently.

In 'Paradigm uniformity effects versus boundary effects', Renate Raffelsiefen (like Davis above) discusses the extent to which phonetic properties engage in constraints related to paradigm uniformity. She puts forward the hypothesis that phonetic properties may not always be directly involved in uniformity constraints, but are rather the effect of differences in morphological structure, that is, they involve domain effects. These two approaches need to be carefully distinguished, and Raffelsiefen presents a number of criteria for doing so.

Péter Rebrus & Miklós Törkenczy's chapter, 'Uniformity and contrast in the Hungarian verbal paradigm', offers a useful introduction to the notions of paradigm and paradigm uniformity, giving at least a working definition of these concepts. Their specific focus is a set of complicated facts in the Hungarian verbal paradigm, which shows, among other things, that contrast in one morphological dimension (as, for example, person–number) may be more important than contrast in another dimension (as, for example, definiteness). It will be interesting to compare this situation to other languages.

The final paper, 'A note on paradigm uniformity and priority of the root', by Suzanne Urbanczyk, once again harks back to the central discussion of 'base priority' versus 'symmetrical' models. For the case at hand, which involves schwa insertion in the Central Salish language Halkomelem (spoken in southwestern British Columbia), Urbanczyk shows that the adoption of a base priority model does not lead to a simpler analysis, thus offering support for McCarthy's Optimal Paradigms model.

The volume is beautifully designed and impeccably typeset; there are only a handful of typos, and the only (slight) inconsistencies between authors are in the Optimality Theory tableaux. The book also has a very good index with only minor errors (see, for instance, the reference to Tibetan).

To conclude, this is a very welcome addition to the literature on the role of paradigm uniformity in contemporary phonological theory. While the editors of this volume have done a superb job of bringing these stimulating papers together, many debates in this area are still wide open for discussion. The next few years will doubtless bring further advances in this area, in which phonology, morphology, phonetics and other aspects of linguistics crucially interact.

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Author's address: Leiden University Centre for Linguistics (LUCL), P.O. Box 9515, 2300 RA Leiden, The Netherlands.
E-mail: j.m.van.de.weijer@let.leidenuniv.nl
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Robert Frank, *Phrase structure composition and syntactic dependencies* (Current Studies in Linguistics **38**). Cambridge, MA & London: MIT Press, 2002. Pp. xiv + 324.

Reviewed by CHUNG-HYE HAN, Simon Fraser University

This book presents a theory of grammar which incorporates the structure composition operations of Tree Adjoining Grammar (TAG) with a number of ideas from the Minimalist program. TAG is a mathematically well-defined formal grammar, first outlined in Joshi, Levy & Takahashi (1975). It is a tree-rewriting system which is more powerful than context-free grammars, but not as powerful as context-sensitive grammars. Work on linguistic applications of TAG began in the early 1980s, for example by Kroch & Joshi (1985).

The fundamental thesis in TAG-based linguistic work is that grammatical well-formedness conditions and syntactic dependencies are localized within the domain of TAG's primitive elements, that is, within a set of predefined pieces of tree structure, called ELEMENTARY TREES. Elementary trees are also the source of any cross-linguistic variation, as languages may differ in what they take to be well-formed elementary trees. Elementary trees are combined with one another by the two universal derivational operations of SUBSTITUTION and ADJOINING. In essence, TAG-based linguistic theories can be seen as theories of the well-formedness of elementary trees.

In this book, Robert Frank builds a comprehensive theory of TAG-based grammar, which is founded on some of the early work in this area, including his own (Frank 1992). He provides a thorough review of the linguistic work in TAG and proposes several principles for elementary tree formation, consolidating these principles into a coherent overall structure. Throughout the book, Frank demonstrates that Subjacency, Relativized Minimality and

Shortest Move, which need to be explicitly stipulated as locality constraints on movement in transformational grammar, can be derived from the nature of TAG's derivational machinery when coupled with independently motivated assumptions concerning the nature of elementary trees. This is a truly ground-breaking idea, as it provides an explanation as to why these constraints exist in natural language. Frank shows that this new model captures in a simple and elegant fashion many of the valuable insights gained in several decades of research on generative grammar. An additional advantage of Frank's model is that it has a more restricted computational power than its Minimalist alternative.

In chapter 1, 'Setting the stage', Frank places TAG-based grammar in the historical context of transformation-based generative grammar, clarifying his assumptions and describing the basics of TAG. TAG's main structures and operations are similar to Chomsky's (1957, 1975) kernel sentences and generalized transformations, and it may be noted that some of the ideas in Chomsky's earlier work have independently found new currency in the Minimalist program (Chomsky 1995, 2000, 2001). Frank postulates that TAG's elementary trees are constructed by Move and Merge. Once elementary trees have been built, they combine with each other by substitution and adjoining to form recursively bigger structures. Frank emphasizes the fundamental TAG hypothesis that 'every syntactic dependency is expressed locally within a single elementary tree' (22), and shows that apparent non-local dependencies are created by adjoining. For example, a sentence with an unbounded *wh*-dependency, as in the complement clause in (1), is derived from an elementary tree representing the embedded clause [*which book*$_i$ *his friends should read* t$_i$], in which the *wh*-phrase *which book* has already been fronted.

(1) I wonder [which book$_i$ Gabriel had thought his friends should read t$_i$].

As a second step, an auxiliary tree representing [*Gabriel had thought*] adjoins to the C′ node, and thereby stretches the dependency between the *wh*-phrase and its trace. Frank also explains the notion of TAG derivation structure, which represents the derivational history of a derived phrase structure tree.

Chapter 2, 'The nature of elementary trees', discusses in detail well-formedness conditions on elementary trees. Frank argues that an elementary tree consists of a thematic domain. A thematic domain includes the structural context in which a single lexical predicate takes its arguments and the predicate's extended projections (in the sense of Grimshaw 1991), such as T(ense)P(hrase) and C(omplementizer)P. Frank recasts these well-formedness conditions as the three statements in (2)–(4), which together guarantee that an elementary tree must be an (extended) projection of a single lexical head with all and only its argument slots appearing as frontier nonterminals.

(2) *Condition on Elementary Tree Minimality* (*CETM*) (54)
The syntactic heads in an elementary tree and their projections must form an extended projection of a single lexical head.

(3) *θ-Criterion* (*TAG version*) (*part 1*) (55)
If H is the lexical head of elementary tree T, H assigns all of its θ-roles within T.

(4) *θ-Criterion* (*TAG version*) (*part 2*) (55)
If A is a frontier nonterminal node of elementary tree T, A must be assigned a θ-role in T.

Argument content is inserted into the frontier nonterminals by substitution.

In order to force the V(erb)P-internal subject D(eterminer)P to move to the specifier of TP ([Spec,TP]), Frank adopts the Extended Projection Principle, as stated in (5).

(5) Extended Projection Principle (EPP) (57)
All TP projections within an elementary tree must have specifiers.

Frank notes that modifiers like prepositional phrases, adverbial phrases, and relative clauses are introduced into the structure by the adjoining of auxiliary trees with recursive root and foot nodes. He argues that these auxiliary trees are well-formed elementary trees according to the CETM and the θ-Criterion, assuming that θ-identification (in the sense of Higginbotham 1985) is subsumed by θ-role assignment.

In chapter 3, 'A case study: raising', Frank presents a TAG-based analysis of subject-to-subject raising, extending the work by Kroch & Joshi (1985) and Frank (1992). The goal of this chapter is to demonstrate how the TAG derivational machinery described in chapter 1 and Frank's theory of elementary trees developed in chapter 2 can derive apparent non-local dependencies created by raising. To derive a raising sentence such as *Eleanor seemed to know the answer*, there must be an elementary tree representing the infinitival clause *Eleanor to know the answer*, in which the subject has moved from [Spec,VP] to [Spec,TP]. Secondly, a T′-rooted auxiliary tree representing *seemed* must adjoin into the elementary tree at T′, expanding the infinitival clause. Frank shows that in a TAG-based analysis of raising, the impossibility of super-raising as in **Eleanor*$_i$ *seemed* [*it was certain* [t_i *to know the answer*]] follows directly from the principle of elementary tree well-formedness and the way in which adjoining composes elementary trees. In contrast, past analyses required explicit stipulations, such as Shortest Move, the Extension Condition, or an antecedent government condition on traces, to rule out instances of super-raising.

Frank then addresses the absence of intermediate traces in TAG-based analyses, and argues that an array of phenomena which have traditionally been attributed to the presence of intermediate traces – for example, reconstruction – can all be given alternative accounts. This chapter also

includes a discussion of (non-)raising in nominals, as in **Leon's appearance to have left surprised me*, and explains the ungrammaticality as due to the fact that elementary trees for nominals are not able to provide the recursive structure which permits adjoining. Frank concludes this chapter with a detailed analysis of copular sentences, as in *The assassination of the king was the cause of the war*, arguing that they are different from raising sentences, contra Moro's (1997) analysis. In Frank's analysis, copula *be* cannot head its own elementary tree because it is not a lexical predicate given that it does not assign any θ-roles. Instead, an elementary tree for copular sentences must include the copula as well as the structure for the small clause complement of the copula. Frank goes on to consider how this analysis may account for those syntactic properties of copular sentences which remained unresolved under Moro's raising analysis.

Chapter 4, 'Local constraints and local economy', presents economy conditions and grammatical constraints which apply to elementary tree derivation. Frank recasts conditions on elementary trees, such as the EPP, in terms of a feature-checking mechanism along the lines of Chomsky (1995, 2000). The EPP results from the presence of an uninterpretable selectional feature on every T head which must be checked by an interpretable feature D in [Spec,TP] in the course of the syntactic derivation.

Frank further assumes that elementary tree derivation starts with a numeration which has the following two properties (124): (i) 'the numeration may include only as much structure as can produce a single elementary tree' (124); and (ii) numerations 'may include nonprojected nonterminals along with lexical and functional heads' (124). Frank's conception of numeration as the set which produces elementary trees is an important departure from the Minimalist notion of numeration, which allows for an unbounded set of lexical items but bans the presence of nonprojected nonterminals.

Based on his assumptions regarding numerations in elementary tree derivations, Frank formulates three economy principles, stated below.

(6) *Maximal Checking Principle* (*MCP*) (126)
The output of an elementary tree derivation from numeration N must contain as few uninterpretable features as possible.

(7) *All-or-Nothing Checking Regimen* (*ANCR*) (136)
In an elementary tree, if some of the uninterpretable features of a lexical item are checked, they must all be checked.

(8) *Greed* (158)
Adjoining may apply at some node of an elementary tree T only if it results in the elimination of uninterpretable features in T.

The MCP ensures that if there is a DP which can move to [Spec,TP], then it must do so. At the same time, it allows EPP-violating elementary trees, such as T′-headed auxiliary trees in raising structures, as these trees do not include a potential DP which can check the EPP of T. The ANCR rules out

elementary trees which contain expletive *there* without an associate. While the EPP-feature of T is checked within such an elementary tree (through the merger of *there*), T′s φ-features remain unchecked (as there is no associate to check them). Finally, Greed rules out examples like **Daniel seems adores his brother*, where the T′-headed auxiliary tree representing *seems* has adjoined into the elementary tree representing the finite clause *Daniel adores his brother*. Here, the case feature of the DP *Daniel* has already been checked by finite T, and adjoining of the auxiliary tree into T′ will not eliminate any uninterpretable feature on the elementary tree.

Chapter 4 also contains analyses of Icelandic constructions involving long distance agreement between a higher verb and a nominative DP in a lower clause, and so-called quirky subjects, using the ANCR and MCP. Frank concludes the chapter with a detailed illustration of the mechanism involved in feature checking/feature identification across elementary trees during adjoining, extending Vijay-Shanker & Joshi's (1988) TAG-based feature-structure unification.

Chapter 5, 'A case study: *wh*-dependencies', provides a TAG-based analysis of *wh*-dependencies. As in the chapter on raising, Frank demonstrates how his proposed well-formedness conditions on elementary trees and the TAG derivational machinery can derive apparent non-local *wh*-dependencies. In a fashion parallel to his analysis of raising constructions, Frank postulates initial elementary-tree-internal *wh*-movement, followed by the adjoining of a C′-rooted auxiliary tree. After a detailed analysis of the feature checking mechanisms involved in *wh*-dependencies, Frank demonstrates how island effects can be derived in this TAG-based account of *wh*-movement. For example, extraction from *wh*-islands (e.g. **What*$_i$ *book did Mark ask* [*whom you gave* t$_i$]*?*) would necessitate an elementary tree with multiple specifiers of CP for multiple *wh*-fronting, given that all movement must be local to an elementary tree. But an elementary tree with multiple specifiers is not possible in English, as such a configuration is not permitted in monoclausal English questions (e.g. **I wonder* [*what book whom Mark gave*]). This analysis predicts that languages that allow multiple *wh*-fronting in monoclausal questions should allow elementary trees with multiple specifiers of CP and therefore allow extraction out of *wh*-islands. Frank, citing Kroch (1989), shows that this prediction is confirmed by data from Romanian and Bulgarian.

Other island effects, such as extraction from complex noun phrases, relative clauses, adjunct clauses and subject clauses, are ungrammatical either because there is no well-formed elementary tree which would allow such a movement, or because the TAG derivational machinery makes it impossible to combine the elementary trees involved. This chapter also discusses Multi-Component TAG (MC-TAG), an extension to the TAG system which can handle extraction out of weak islands (e.g. *Which car*$_i$ *does Sally wonder* [*how to fix* t$_i$]*?*) and nominals (e.g. *Which politician*$_i$ *did you take* [*a picture of* t$_i$]*?*).

In chapter 6, 'Looking onward', Frank concludes his book by discussing some linguistic phenomena which are known difficulties for a TAG-based theory, such as Romance clitic climbing and scrambling phenomena in German and Japanese. As Frank points out, some progress has been made in suggesting analyses for these phenomena (Kulick 2000). Frank also discusses interface issues and suggests, following Kallmeyer & Joshi (2003), that the TAG derivation structure, which encodes the sequence of derivational steps, may serve as the interface for semantic interpretation. A good candidate for an input to the phonological component may be the TAG derived phrase structure tree.

Frank's clear exposition throughout makes the contents of the book accessible to readers who are novices to the TAG framework. Nonetheless, his in-depth comparison between TAG-based and Minimalist analyses and thorough study of many empirical phenomena make the book essential reading for advanced readers interested in the issue of locality in syntax in general. The data used in the argumentation are not only from English but come from various other languages, such as Icelandic, Romanian, Bulgarian, Georgian, and German, to name but a few, illustrating how cross-linguistic variation can be handled in the model of grammar advocated in the book. References abound to works on TAG-based as well as transformation-based grammar, where more detailed discussion can be found on several topics touched upon in this book.

While there are some differences in the details, Chomsky's (2000, 2001) definition of PHASE overlaps and shares motivations with the TAG-based model of grammar. Similar to an elementary tree, a phase is a privileged derivational domain where many syntactic dependencies are localized. Chomsky's Phase Impenetrability Condition allows movement out of the edge of phases, obtaining similar results as adjoining. The two systems are thoroughly compared in Frank (to appear). Despite transformation-based syntactic theory seemingly developing into an approach which is comparable to the TAG-based theory of grammar, the body of work on linguistic applications of TAG has not received much attention from the wider community of generative syntacticians. Frank's comprehensive and detailed work properly situates the TAG-based theory of grammar within the larger context of the framework of generative grammar. It remains to be seen whether this book will encourage a re-evaluation of the existing TAG syntactic literature by a broader readership. In any case, it is sure to produce a positive impact on syntactic theorizing in the long term.

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Author's address: Department of Linguistics, Simon Fraser University, 8888 University Drive, Burnaby BC, V5A 1S6 Canada.
E-mail: chunghye@sfu.ca

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Yafei Li, *X^0: a theory of the morphology–syntax interface*. Cambridge, MA: MIT Press, 2005. Pp. x + 222.

Reviewed by PETER ACKEMA, University of Edinburgh

After Baker's (1988) elegant attempt to unify a number of 'grammatical function changing' morphological processes in terms of a theory of syntactic incorporation (head-to-head movement), the question has repeatedly been asked whether it is possible to deal with all word formation processes syntactically, or whether word formation requires a designated morphological module. In this monograph, Yafei Li argues that both positions must be regarded as being simultaneously correct. To be more

precise, Li claims that all words are formed in a designated morphological module – a position which he, following many others, labels the 'lexicalist hypothesis', which is perhaps an unfortunate term since the issue of what is contained in the lexicon is orthogonal to the issue of whether words and phrases are formed in the same grammatical module or not (cf. Di Sciullo & Williams 1987). According to Li's proposal, in some circumstances, each of the constituent parts of a morphologically complex word must in addition be associated with the head position of distinct syntactic phrases. Which circumstances these are is described by the central hypothesis of the work, the MORPHOLOGY-SYNTAX MAPPING HYPOTHESIS (MSMH):

(1) If morphological components X and Y are in a word W and there is a relation R between X and Y, then R is reflected in syntax if and only if
   a. R is thematic, and
   b. the representation of R in syntax obeys all syntactic principles. (4)

In the first three chapters, Li discusses the consequences of the MSMH for cases of verb incorporation (including verb-based causatives, applicatives and Chinese resultative compounds), adjective incorporation (adjective-based causatives) and noun incorporation. In the fourth, penultimate chapter, 'From X-bar theory to the lexicalist hypothesis', Li attempts to derive the impossibility of forming words by syntactic head movement, using a modified version of Kayne's (1994) Linear Correspondence Axiom. The last chapter functions as an epilogue, containing some 'Philosophical thoughts on linguistic research'.

In chapters 1, 'Verb incorporation', and 2, 'Adjective incorporation', Li argues that the need for a dual outlook on word formation, as embodied by combining the lexicalist hypothesis with the MSMH in (1), is demonstrated by the contrasting behaviour of causatives based on a verbal root and causatives based on an adjectival root. In particular, the binding properties of reflexives accompanying such causatives appear to indicate that deverbal causatives involve a biclausal syntactic structure (where both the verb root and the verbal causative morpheme head a distinct phrase), whereas de-adjectival causatives behave as one head projecting a single clause in syntax. This conclusion is based on contrasts such as that between the Arabic deverbal causative in (2) and the de-adjectival causative in (3).

(2)	ʔal-mudrris-uun$_i$	ʔajlas-uu	ṭ-ṭulaab$_j$-a	(13)
	the-teacher-PL.NOM	made.sit-AGR	the-students-ACC	
	biʒaanib-i	baʕḍihum	l-baʕḍ-i$_{*i/j}$	
	next-GEN	each	the-other-GEN	

'The teachers made the students sit next to each other.'

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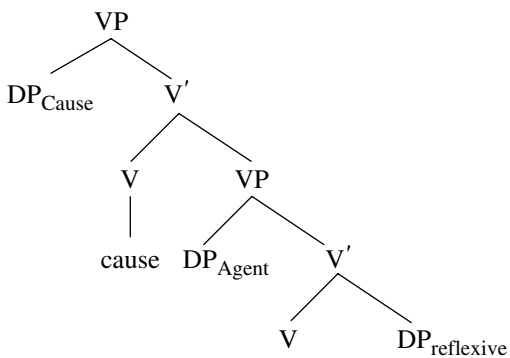
(3) ʔan-nisaaʔ-u$_i$ ʔaɣmam-na l-banaat-i (13)
the-women-NOM made.upset-AGR the-girls-ACC
min baʕḍihin l-baʕḍ-i$_i$
from each the-other-GEN
'The women made the girls upset about each other.'

In (2), the anaphor obligatorily takes the Agent argument of the verbal root ('the students') as its antecedent; it cannot have as its antecedent the Agent argument of the causative affix ('the teachers'). In (3), this is different: here, the anaphor CAN take the causer ('the women') as its antecedent. According to Government-and-Binding-style binding theory, an anaphor must be locally bound, that is, roughly speaking, inside its own clause. These data then seem to indicate that the verb root in (2), but not the adjective root in (3), projects its own clause, with its Agent argument as the subject of that clause. In (3), there must be a monoclausal structure, with the causer as the only subject and the originally external θ-role of the adjectival root demoted to an internal role.

That verb incorporation should involve a biclausal syntactic structure follows from the MSMH in (1). The thematic relation between the causative morpheme and the verb root is syntactically reflected by the former taking as its complement a projection of the latter, as shown in (4) (adapted from Li (18)).

(4)



The causative V(erb)-V(erb) compound, which is formed in the morphological module, is inserted in the lower V head and raises to the higher V head to check the abstract causative features of the higher V against the features of the causative affix. This is a standard case of licit head movement. Another case of syntactic verb incorporation is presented by applicatives, for which Li rejects an analysis in terms of syntactic preposition incorporation, which would at any rate not be allowed by his theory (more on this below).

Li then demonstrates that it follows from the MSMH that adjective incorporation is NOT reflected by syntactic structure (cf. (3) above).

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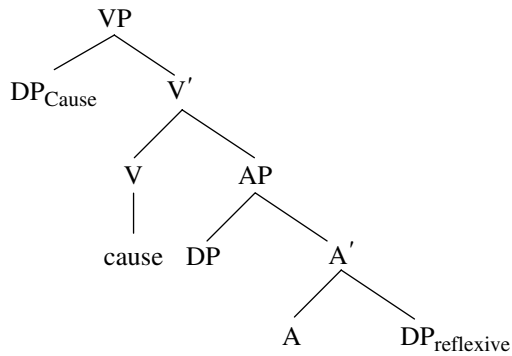
Deadjectival causatives are formed by combining two morphemes that belong to two different categories, namely A(djective) and V. Once again, there is a thematic relation between the two morphological components since the adjectival root functions as the internal argument of the causative morpheme. If this relation were syntactically reflected, the following structure would result:

(5)



The question now arises as to where the morphologically formed A-V compound is to be inserted in this structure. Since the compound is headed by the causative morpheme, it is of category V and cannot be inserted in the lower head position, which is of category A. Consequently, the compound must be inserted in the higher head position. But in that case, the features of the lower head cannot be checked since this would involve a lowering operation, which is not permissible in Universal Grammar. In accordance with the MSMH, which requires the syntactic representation of a thematic relation to obey all syntactic principles, the thematic relation between the morphological components in a de-adjectival causative cannot be reflected by a biclausal syntactic structure. Instead, the complex word simply projects a single Verb Phrase, which accounts for the fact that the Cause argument can act as a local antecedent for the anaphor in (3).

A similar reasoning applies to all cases in which the morphemes that make up a complex word are of different category. Where there is a thematic relation between two morphemes, the head of the word assigns a θ-role to the nonhead. If this relation is reflected syntactically, the head of the complex word will occupy the structurally higher head position, and the other morpheme will project the complement to this head. Given that the head determines category membership, a complex word whose nonhead is not of the same category as the head can be inserted only in the higher head position, which means that feature checking can be achieved only via lowering, thereby resulting in a violation of the MSMH.

On this account, it is similarly impossible for noun incorporation to be reflected in the syntax. In chapter 3, Li contends that the arguments for

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syntactic noun incorporation put forward by Baker (1988, 1996) are, at best, inconclusive, and in some instances require complications that do not arise with the purely morphological alternative. Li shows that all relevant data can be accounted for if, at least in polysynthetic languages, parts of words are visible for the interpretative module of grammar (Logical Form), and if thematic relations can be established within morphological structures in the same way as they are established in syntactic structures in nonpolysynthetic languages (cf. also Ackema 1999). Li's assumption that thematic relations cannot be established at all inside words in nonpolysynthetic languages is problematic, given the existence of synthetic compounds such as *truckdriver*, in which *truck* is somehow assigned the verb's internal θ-role (witness the impossibility of inheritance of this role, cf. **truckdriver of a Mercedes*). However, Li does show convincingly enough that the data that Baker puts forward as evidence for the presence of a syntactic object NP in cases of noun incorporation can equally well be accounted for in a morphological analysis of the construction.

In chapter 4, Li proposes the Modified Linear Correspondence Axiom (MLCA), from which he derives the lexicalist hypothesis. Space does not permit me to go into the technical details here, but the consequences of the modifications that Li proposes to Kayne's (1994) original LCA are as follows. First, the universal basic syntactic order is Object Verb (the opposite of what Kayne assumed). Second, head-initial structures can be derived by head-to-head movement only if the higher head is empty (which makes the MLCA nonapplicable), that is, head movement can never involve adjunction to a phonologically overt head and thus cannot result in word formation. Third, the same holds for head movement in (surface) Object Verb languages. In such languages, the higher head is allowed to contain an overt morpheme in the base, but since head movement to this morpheme would result in a violation of the MLCA, word formation is possible only if the two morphemes are already string-adjacent (word formation through adjacency at Phonetic Form). Apart from such cases, morphological merger is the only option to form complex words.

In a brief final chapter, Li argues that, in cases where two apparently conflicting theories have been proposed, as with respect to the question in which grammatical module complex words are formed, a synthesis of both theories sometimes gives the most fruitful results. However, considerations of parsimony would, on the contrary, suggest that the burden of proof is on a theory that involves two distinct grammatical modules to account for word formation rather than just one (whether this is the morphological or the syntactic module). The analyses put forward to defend the MSMH are certainly interesting and well worked-out, but the necessity of adopting a principle like this seems less certain. Why, for example, is it specifically thematic relations that must be expressed syntactically, rather than the more general head–complement relation, and why are head–modifier relations

not similarly expressed? And why is syntactic expression of thematic relations required at all, given that nothing goes wrong in the many instances where this is impossible for independent reasons, as, for example, with de-adjectival causatives or noun incorporation in polysynthetic languages?

In this connection, it is interesting to note that amongst all the constructions that Li discusses, there is in fact only one type for which he argues that the empirical evidence forces us to adopt a biclausal structure, namely verb incorporation. Given the problems that Li identifies with syntactic projection in all other cases (as well as the general problems for an all-syntactic approach which repeatedly have been pointed out in the morphological literature), it seems worthwhile to reconsider the arguments that he advances against a purely morphological account of verbal causatives and applicatives. For example, the different binding relations in the verbal and the adjectival causatives in (2) and (3) might not be the result of a different syntactic structure, but of the different type of external θ-role associated with verbs and adjectives. Various authors have argued that anaphoric binding is sensitive to thematic relationships (see, for example, Reinhart & Reuland 1993, Williams 1994, Neeleman & Van de Koot 2002). It is quite conceivable, then, that there are anaphors that want to be bound not just by a local noun phrase but by the thematically most prominent local argument. Suppose that not only the adjectival but also the verbal causative is monoclausal. There are two Agent arguments present in the verbal causative, and the anaphor will be bound by the most local one of these. An adjective, on the other hand, assigns something like a Theme role, rather than an Agent, to its argument, so in this case the Agent will take precedence over the adjective's argument if the antecedent for the anaphor is determined on thematic grounds. In periphrastic (nonmorphological) causatives, the adjective's argument must be the antecedent for an embedded anaphor because it is the only local antecedent available.

Similar considerations may apply to Li's argument against a morphological treatment of applicatives, which is based on different word order possibilities in applicatives with a Benefactive argument and applicatives with a Locative/Instrument argument, notably the fact that the latter allow for the Instrument/Locative argument to be merged in a position either higher or lower than the verb's Theme argument. It seems to me that Li's own account of this, which argues for an alternation between an argument structure with a 'bleached' Theme and a semantically contentful Theme, in combination with a thematic hierarchy Benefactive >> Theme >> Instrument/Locative >> 'bleached' Theme, can be directly translated into any account that assumes a monoclausal structure for applicatives.

On the whole, then, I think the case for a 'dual' approach to word formation is not conclusively proven. Nevertheless, the book is not to be missed by anyone remotely interested in the debate on morphological versus syntactic word formation. Li certainly sheds fresh light on the issue and

provides numerous pieces of interesting analysis. The book is also well-written, and well-edited: one of the very few typos is the name change that the same linguist undergoes between pages 41 and 42 – and if this arouses your curiosity, you should seek out the book.

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Author's address: Department of Linguistics and English Language, University of Edinburgh, 40 George Square, Edinburgh EH8 9LL, U.K.
E-mail: packema@ling.ed.ac.uk
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Bettelou Los, *The rise of the* to-*infinitive*. Oxford: Oxford University Press, 2005. Pp. xvi + 335.

Reviewed by TERESA FANEGO, University of Santiago de Compostela

This book, which describes the emergence and spread of the *to*-infinitive in Old and Middle English, is a welcome addition to previous studies dealing with the history of English complementation patterns. The theoretical framework adopted is Government and Binding Theory, with some excursions into the Minimalist Program. Bettelou Los claims (22) that the exposition does not assume an up-to-date knowledge of generative theory, as she has tried to keep the material presented accessible to a wider audience. However, this is true only of the data-oriented chapters in parts II and III, as opposed to the more theory-dependent chapters in parts IV and V, some of which may prove difficult for readers not familiar with generative grammar, especially as many of the Minimalist concepts are not explained at the outset.

The book is based on Los's doctoral dissertation and a series of articles published between 1998 and 2003. It consists of six parts, divided into eleven chapters. The introduction in chapter 1 (so titled) expounds the main changes concerning the infinitive in Old and Middle English, namely

(i) the massive increase in the frequency of the *to*-infinitive in Middle English (ME), with the bare infinitive restricted more and more to relatively few contexts, and (ii) the rise of new structural types in ME, such as passive *to*-infinitives or so-called Exceptional Case-Marking (ECM) constructions (as in *They believe John to be a liar*). The traditional view has tended to interpret (i) in terms of a gradual encroachment of the *to*-infinitive upon the domain of the bare infinitive, while the changes in (ii) have been linked by Lightfoot (1979) and later generative studies to a change in the categorial status of the OE infinitive from noun to verb. Los's aim is to check whether these assumptions can be confirmed or not in the light of the evidence retrieved from a large collection of Old and Middle English electronic corpora.

Chapters 2–6 investigate the distribution of bare and *to*-infinitives in OE. It has often been claimed that the *to*-infinitive first started to replace the bare infinitive as a purpose adjunct, but chapter 2, 'The expression of purpose in Old English', convincingly shows, *contra* Callaway (1913) and others, that by the time of the earliest OE records, the bare infinitive was no longer used to express purpose, except in slavish translations from Latin and in a few idiomatic expressions. Los further shows that in OE the *to*-infinitive occurs as an argument in basically the same constructions as in present-day English (PDE) and is in direct competition with the bare infinitive only after a subset of intention verbs, such as *fon* 'attempt' or *wenan* 'hope, expect', and a few verbs of commanding and permitting, such as *bebeodan* 'command' or *biddan* 'ask'. It thus follows that the increase in *to*-infinitives from late OE onwards may have taken place not just at the expense of the bare infinitive, as has often been claimed, but also of some other structure.

Los's detailed investigation reveals that the structure in question is the subjunctive *that*-clause. *That*-clauses matched the distribution of the *to*-infinitive, as both occur as purpose adjuncts and as arguments of (i) monotransitive subject control verbs with meanings like 'intend' or 'try'; (ii) ditransitive object control verbs of persuading, urging, commanding and permitting; and (iii) ditransitive subject control verbs with meanings like 'promise'. Competition between the two types of clause can be observed already in late OE. Comparing the numbers of *to*-infinitives and subjunctives in the same syntactic environments in several corpora of OE and early ME allows Los to show that the subjunctive clause in all its functions is replaced by the *to*-infinitive in the transition from OE to ME, contrary to the traditional view that the increase in *to*-infinitives occurred at the expense of the bare infinitive. However, Los grants that it seems likely that 'the massive increase in *to*-infinitives in e[arly]ME that resulted from the competition with the finite clause is bound to have affected the position of the bare infinitive' (298) in late ME, but as her investigation stops at 1350 this is an issue left to future research.

Chapter 7, 'The category of the *to*-infinitive', discusses the categorial status of the *to*-infinitive in OE. Los concurs with Mitchell (1985) and all OE specialists in the view that the OE infinitive is not a noun but a verb, since it lacks such typically nominal characteristics as the ability to govern a genitive object or be preceded by a preposition other than *to*. Lightfoot's (1979) assumption that the OE infinitive is still a noun is therefore incorrect. With regard to the status of the *to*-infinitive clause itself, Los argues that it is a Complementizer Phrase (CP), on the basis of evidence such as its obligatory clause-final position, which in OE is characteristic of full-blown CPs, or the fact, already mentioned above, that it had entered into competition with the finite subjunctive clause and was ousting it in a number of environments. From this Los concludes that the *to*-infinitive must have been reanalyzed by OE speakers as a non-finite subjunctive.

Chapter 8, 'The changing status of infinitival *to*', explores the implications of the analysis of the *to*-infinitive as a subjunctive equivalent. Following Pullum's (1982) account of PDE infinitival *to* as a non-finite modal, Los proposes to check infinitival *to*, the inflectional subjunctive and the modal verbs in OE and PDE in the same functional projection (namely, Tense (T)), as this captures the fact that they all express similar functional information. In other words, the position of infinitival *to* has not changed throughout the recorded history of English: 'it heads the projection that hosts the subjunctive ending in OE and the modal verbs in ME' (233).

Los further argues that in OE, *to* behaved largely as 'a clitic or even a bound morpheme' (230), since no material could intervene between *to* and the infinitive, and *to* could not be dropped from the second or subsequent conjuncts in a sequence of coordinated *to*-infinitives. From ME onward, second conjuncts appear freely without *to*, and split infinitives (for example, Wyclif, *Matthew* 5, 34: *Y say to 3ou, to nat swere* 'I say to you, do not swear') become possible, thus suggesting that *to* has degrammaticalized and is no longer a clitic or prefix but a free word. Degrammaticalization changes are generally considered to be 'unnatural' and hence very uncommon, but Los accounts for the degrammaticalization of infinitival *to* by linking its development from OE to ME to that of the inflectional subjunctive. The hypothesis is that the change in the morphological status of infinitival *to* from clitic to free morpheme

> was triggered by the behaviour of the finite counterpart of infinitival *to*: the finite subjunctive, which was increasingly coming to be expressed by a free form (a modal verb) raising to T^0 overtly, rather than by a bound form (a subjunctive ending), raising to T^0 covertly. The overt movement of *to*, then, would bring it in line with the rest of its paradigm: the modal verbs. (230)

Chapters 9 and 10, entitled 'The rise of *to*-infinitival ECM' and 'Innocent bystander: the loss of the indefinite pronoun *man*', respectively, examine

two apparently unrelated developments that took place in ME, namely the emergence in the fifteenth century of the *to*-infinitival ECM-construction after verbs of thinking and declaring, and the loss of the indefinite pronoun *man* 'one'. Concerning the ECM-construction, Los acknowledges the traditional view (Warner 1982) that verbs like *believe* came to allow *to*-infinitives under Latin influence, but suggests that the introduction of the new construction was also promoted by the loss of verb second (V2). OE is a V2 language, but V2 is gradually lost in the ME period, and this process had consequences for the organization of Theme/Rheme material (that is, of given versus new information). The ECM-construction after *believe*-verbs – which in both ME and PDE is almost invariably passive (as in *Many of these actors are assumed to be Americans*) and has the infinitival subject functioning as unmarked (given) Theme – became acceptable for this reason, that is, English needed new strategies to move noun phrases (NPs) containing given information into subject position. A similar explanation is proposed in chapter 10 for the decline of the ultra-indefinite pronoun *man* 'one', whose main role had been to provide a contentless subject functionally equivalent to a passive. With the changes in information structure resulting from the loss of V2 and the generalization of subject-verb order, subject NPs became an important device to maintain textual cohesion. *Man* was too contentless to play this role, and English preferred the use of 'impersonal' (agentless) passives, which increasingly took over the function of *man* in many clauses. The book ends with chapter 11, in which Los provides a 'Summary and conclusions'.

Los's book is rich in empirical facts and provides an in-depth analysis of important aspects of OE and ME syntax – two features that make it a valuable addition to the personal library of anyone interested in the history of English. Some objections could be raised, though. The first objection concerns Los's views on the way in which the infinitive, originally a nominalization of a verb, was recategorized from noun to verb in prehistoric times. She envisages (18f., 192ff., 299) a scenario where the nominalizing suffix on the *to*-infinitive (represented by *-enne* in recorded OE)

> competed so successfully with the other nominalizing suffixes that it eventually accepted any V-stem as input. From this point on, learners analysed it as inflectional rather than derivational morphology and hence no longer category-changing … The recategorization, then, may well have been abrupt rather than the long-drawn-out process it is usually thought to be. (18f.)

This is all highly implausible: verbal and nominal categories are distinguished on the basis of properties such as the type of object they each govern (in OE, accusative phrases in the case of verbs, genitive phrases in the case of nouns), so that speakers will not start reanalyzing deverbal nouns

as verbs simply because they happen to contain a very productive suffix. Changes in word class do not take place overnight (Haspelmath 1998: 327ff., 1999: 1045), and Los could have paid more attention to the evidence afforded by other well-known categorial changes, such as the development in relatively recent times of the English gerund from a noun of action into a part of the verb system.

With regard to the organization of the book, parts II, 'The *to*-infinitive as GOAL', and III, 'The *to*-infinitive as THEME', should have been conflated and given a more appropriate title, such as 'The distribution of the *to*-infinitive in OE'. This would have been the logical place for the sections dealing with the competition between *that*-clauses and *to*-infinitives, which now appear in part IV, 'Syntactic status'. Finally, there are quite a few typos and disconnected sentence fragments. For instance, in the very first paragraph of the conclusions, we not only find a misspelling of 'preposition', but there appear to be one or two lines missing altogether:

> [a]lthough the rise of the *to*-infinitive … is an instance of grammaticalization, … a close examination of extant OE texts clearly shows that this process was already completed at the earliest recorded stage. *To* must have [?] as is also strongly suggested by the homophony of infinitival *to* and the proposition *to* but it has already developed into an infinitival marker in OE. (297)

Moreover, the examples in (2a–c) in chapter 2, which all date back to the nineteenth century, cannot be said to illustrate usage in early Modern English (1500–1700). Table 2 in appendix 2 contains several errors and provides information that does not coincide with the information given earlier in the volume (75ff.) for the same set of verbs. Closer attention to bibliographical detail would also have been desirable: the reference to Jespersen (1927) (6) is not documented in the list of references, and the correct title of Jespersen (1940) is *A modern English grammar on historical principles*, not *A modern English grammar* (324). Finally, the volume would have benefited from a more detailed subject index than the two pages provided, and should have included a name index and an index of verbs and their occurrences.

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Author's address: Department of English, Facultad de Filología, University of Santiago de Compostela, 15782 Santiago de Compostela, Spain.
E-mail: iafanego@usc.es
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Paul M. Postal, *Skeptical linguistic essays*. Oxford & New York: Oxford University Press, 2004. Pp. 414.

Reviewed by CEDRIC BOECKX, Harvard University

Paul M. Postal's recent book, *Skeptical linguistic essays* (henceforth *SLE*), is divided into two parts. Both promote a skeptical stance. Skepticism has a long and distinguished tradition in natural philosophy (see Popkin 1960), and a healthy dose of skepticism has invariably proven extremely useful at all stages of scientific development. I was therefore favorably predisposed toward *SLE*, and the first part of the book, entitled 'Studies in linguistics', did not disappoint. There Postal applies his well-known skills to a host of complex phenomena in the syntax of English, such as locative inversion (chapter 1, 'A paradox in English syntax'), putative cases of subject-to-object-of-preposition raising (chapter 2, 'A putatively banned type of raising'), raising-to-object data more generally (chapter 3, 'A new raising mystery'), subtypes of nominals including semantically light indefinites (chapter 4, 'Chromaticity: an overlooked English grammatical category distinction'), and 'minimizing' elements like *squat* (chapter 5, 'The structure of one type of American English vulgar minimizer').

In my view, the first five chapters of part I constitute the most valuable part of *SLE*. It is here that Postal is at his best: carefully reviewing previous analyses, exposing their limitations, expanding the data base, unearthing new factual generalizations, and applying a battery of tests to justify factual claims. It is virtually impossible to do justice to the richness of that type of work in the confines of a review, and I will not even try, but I urge every linguist interested in syntactic issues and the nature of the syntax–semantics interface to study these chapters with great care. They are bound to prove an invaluable source of ideas and puzzles.

Chapter 6, 'The openness of natural languages', which concludes part I of the book, and the whole of part II are devoted to more general methodological and ontological issues in linguistics, although I should stress that

in all these chapters Postal never fails to analyze specific linguistic examples. In chapter 6, Postal looks at examples involving direct speech reports (for example, *The Martian said* '*XDEGDSDGHJCGDFG*' *to Bill*) to argue that set- and proof-theoretic approaches to the study of natural language (which Postal calls generative) are fundamentally flawed, and that instead, model-theoretic approaches ought to be pursued. The crux of Postal's argument is that natural language sentences can contain arbitrary symbols of potentially infinite size, as in *I didn't like that* '*mooooooooooooooooo …*' *sound*. Postal takes these data to falsify the claim that the lexicons of natural languages are fixed, finite sets – something he associates with the generative tradition on the basis of quotes like the following from Chomsky (174):

> I will consider a *language* to be a set (finite or infinite) of sentences, each finite in length and constructed out of a finite set of elements. All natural languages in their spoken or written form are languages in this sense, since each natural language has a finite number of phonemes (or letters in its alphabet) and each sentence is representable as a finite sequence of these phonemes (or letters). (Chomsky 1957: 13)

I see two major problems with Postal's criticism. First, it misses the point that the goal of the generative enterprise in linguistic theory is not to decide whether natural languages can be studied in terms of sets, proofs or models. The idea expressed in Chomsky (1957) that it is possible to bifurcate the set of sentences into the grammatical and ungrammatical and define theoretical adequacy on the basis of that distinction was quickly abandoned. Instead, as is made extremely clear in the first chapter of *Aspects of the theory of syntax* (Chomsky 1965), the goal of linguistic theory, once firmly placed in a cognitive, and ultimately biological, setting, is to give an account of how children are able to acquire their native languages. In such a setting, talk of models, proofs or sets is largely irrelevant.

Second, and more directly related to Postal's concerns, I think that data like direct reports and quotes do not really have the effect that Postal would like them to have. Much of the force of Postal's argument depends on the nature of lexical items and lexical insertion. If a late insertion approach, such as that of Halle & Marantz (1993), is adopted, as many generative studies have done in recent years, direct reports are mere appendices to a fixed, finite set of syntactic primes. In other words, it becomes possible to maintain the view that sentences are constructed out of a fixed set of elements, even in light of the data presented by Postal.

Part II of *SLE* is entitled 'Studies of junk linguistics'. Postal regards much recent work in syntactic theory, especially work within the Principles and Parameters framework, and even more specifically, Chomsky's work in that tradition, as 'junk linguistics' – that is, bad science. Postal wants to denounce junk linguistics to prevent its proliferation. A priori, it is

hard to disagree with Postal: bad science is bad indeed, and it should be avoided. But I strongly disagree with Postal when it comes to what he regards as junk linguistics. Much of what he characterizes as such, I regard as normal rather than bad science, for reasons that I will try to make clear in the remainder of this review.

Take, for example, the first two chapters of part II, entitled 'Junk syntax 1: a supposed account of strong crossover effects' (chapter 7) and 'Junk syntax 2: "there remain a few as yet unexplained exceptions"' (chapter 8). In chapter 7, Postal objects to what has become the standard generative treatment of strong crossover (**Who*$_i$ *does he*$_i$ *like* t$_i$*?*), namely as a violation of Condition C of the binding theory; and in chapter 8, he attacks the standard analysis of passivization as A-movement. Postal takes issue with these accounts on largely empirical grounds, offering sometimes long lists of exceptions. On such grounds Postal, like any scientist, is entitled to be skeptical of the standard treatments in question. But, at times, Postal appears to be skeptical of the very spirit of these analyses, labelling them 'junk syntax'. It is here that Postal and I part company. At the risk of engaging in what Postal calls 'junk ethics' or 'junk reasoning', I fail to see why exceptions that remain unexplained (even if for a long time) have the power to invalidate an approach like Principles and Parameters.

Let me expand. I am among those who are persuaded that in the past fifty years, linguistics has progressively established itself as a genuinely scientific discipline (for some discussion, see Boeckx & Piattelli-Palmarini 2005). As physics and biology have taught us, there is a price to pay for this. Many aspects of language that capture our attention and stimulate our curiosity as laypersons have been left out of the scientific picture, for instance, literary style, the social differentiation of accents and nuances, and the growth of specialized jargons in different walks of life. By and large, with few notable exceptions, all that spans over and above the single sentence has been left out of the research program. This self-imposed restriction has been very productive, as the history of the discipline amply shows.

Even aspects of language that capture the attention of linguists must sometimes be left out of the scientific picture. Consider, for example, the old observation that *promise* (and a handful of verbs patterning like it) does not behave like other transitive verbs, in that it requires subject rather than object control, as shown in (1).

(1) John promised Mary [to <John>/*<Mary> go to England].
(2) John persuaded Mary [to *<John>/<Mary> go to England].

Rosenbaum (1970) argued for object control (cf. (2)) to follow from the Minimal Distance Principle (MDP), which requires the controller to be the closest noun phrase. Recent movement approaches to control, like Hornstein (1999), have deduced the MDP from a more general principle like Shortest

Move. That (1) does not pattern like (2) suggests to a variety of linguists (see, for example, Culicover & Jackendoff 2001) that it is a mistake to implicate the Minimal Distance Principle in (1), let alone to deduce it from a more fundamental principle like Shortest Move.

It is likely that Postal would regard this alleged mistake as an instance of junk syntax. However, as Boeckx & Hornstein (2003) point out, the alleged mistake is actually a virtue because it may shed some light on why children acquiring control constructions are puzzled by *promise* and master the pattern in (1) extremely late in the course of linguistic development. The standard logic in generative grammar, whose goal is to account for how linguistic knowledge is acquired, is to relegate (1) to the periphery of the grammar, that is, *promise* really must be treated as exceptional. In some cases, one can try to make sense of the exception by further analyzing and decomposing it; sometimes, the exception is merely to be listed. There is nothing 'junk'-like about this approach; such practice is inherent to any scientific enterprise that follows what has come to be known as the Galilean method of science, with its heavy reliance on idealization. As stated by the biologist D'Arcy Thompson (1942: 1029), one needs 'a principle of negligibility' and must 'learn from the mathematician to eliminate and discard; to keep the type in mind and leave the single case, with all its accidents, alone'. Needless to say, nothing ensures that the idealization made will be the right one, but judging from the course of scientific progress, there is little doubt that SOME idealization is required and desirable.

I would not be surprised if Postal were to characterize the point just made as an 'irresponsible passage' – a term he applies to notions such as 'virtual conceptual necessity' (chapter 13, passim) – or as a 'lust for error' (289), which he sees illustrated in the quotation below.

> Suppose that counterevidence is discovered – as we should expect and as we should in fact hope, since precisely this eventuality will offer the possibility of a deeper understanding of the real principles involved. (Chomsky 1982: 76)

Throughout *SLE*, Postal claims that such statements are irresponsible and made to shield the author from criticism. Consider the following passage, taken almost at random:

> The thinly disguised contempt for getting the facts right … emerges particularly clearly when one is told that 'failures are also interesting' and 'point the way to better solutions'. [footnote omitted] Such claims can only be seen as self-serving defensive attempts to make descriptive failure seem acceptable. (5)

But why should we interpret the 'irresponsible passages' in the way that Postal does? Results in science are temporary, and the best theory at any point in time is bound to be replaced in the future. This is no sufficient reason

to reject the entire approach or to forbid any feeling of optimism, in linguistics or elsewhere.

Underlying much of *SLE* is the tension between descriptive and explanatory adequacy, and the introduction to *SLE* makes it abundantly clear which side Postal favors. This is not to deny the importance of and 'the need for truly intensive factual study' (6). But one should not lose sight of the aim of explanatory adequacy, which characterizes the linguistic works criticized by Postal. It is true that this goal is

> a very long way indeed from concern with realizing the earlier goal (Chomsky 1957: 13) of a grammar that would describe all the well-formed sequences and none of the ill-formed ones. (5)

But as I pointed out above, that early goal was abandoned as soon as it was formulated, for reasons that are well-documented (see, for example, Chomsky 1975).

As Chomsky (1999: 33f.) notes, '[i]nternalist biolinguistic inquiry does not, of course, question the legitimacy of other approaches to language' (a quote which Postal dubs 'the most irresponsible passage'). Postal may question the idea that fundamentally there is only one human language when he points out the lack of proof for such a claim (10). He may also disagree with the claim, often made in the Minimalist literature, that the Principles and Parameters approach has pointed to a way of solving the logical problem of language acquisition. But his skepticism means that one is left with absolutely no idea whatsoever as to how to begin to approach biolinguistic questions. Surely, no factual study, no matter how extensive, will do.

Let me conclude. Much of what I have said in this review concerns truisms of the scientific method. Idealization and attempts to reach explanatory adequacy will always produce some of the results with which Postal takes issue (Richard P. Feynman once quipped that the reason physicists are so successful is because they focus on the simplest case of hydrogen). Postal's *SLE* serves as a useful reminder that some of the optimism found in certain corners of linguistics may be premature (as Chomsky never fails to mention in the context of the Minimalist program), but I have found no reason in *SLE* to doubt that the big picture underlying the various efforts that Postal attacks is correct. As it turns out, junk linguistics may not be as bad to one's (mental) health as junk food; it may even be as rich in insights as junk DNA.

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Author's address: Department of Linguistics, Harvard University, Boylston Hall 313, Cambridge, MA 02138, U.S.A.
E-mail: cboeckx@fas.harvard.edu

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Ian G. Roberts, *Principles and parameters in a VSO language: a case study in Welsh* (Oxford Studies in Comparative Syntax). Oxford: Oxford University Press, 2005. Pp. viii + 207.

Reviewed by RANDALL HENDRICK, University of North Carolina at Chapel Hill

It is sometimes said that Government and Binding theory was popular largely because it was not very challenging. It allowed linguists to continue old descriptive habits focused on individual languages. Such a view is a lament for practices that reflect the true aim of linguistics. It usually foreshadows an attempt to rally linguists to a research agenda providing a restrictive and learnable explanation for language variation. Ian Roberts has taken an important step to dampen this lament. Few works make a stronger attempt to integrate a wide range of cross-linguistic facts in their evaluation of principles and parameters. Roberts argues that morphological variation has a central place in setting parameters, even in Minimalist theory based on lexicalist assumptions.

The book is composed of five chapters. In chapter 1, 'The analysis of VSO languages', Roberts argues that the Welsh verb raises out of the verb phrase (VP), but not into the Complementizer position. The second and third

chapters, entitled 'Case agreement and mutation' and 'Genitive case, word order in DP and object of non-finite verbs', respectively, are concerned with the structural conditioning of agreement and initial consonant mutation in direct objects. Chapter 4, 'The C system and the Extended Projection Principle', offers an analysis of Welsh complementizers in an attempt to unify their distribution with familiar verb-second phenomena from the Germanic languages. Finally, chapter 5, 'Head movement and EPP features', defends the conceptual and empirical utility of head movement within Minimalist syntax.

As Roberts is quick to point out, the book contains little that is new empirically; instead, it offers a novel way of viewing what we know about Welsh syntax. Because of its theoretical focus the chapters are necessarily rather technical, and Roberts takes numerous detours through the theoretical literature. What makes the book easy to read and digest is the clarity of the summaries that Roberts provides at major junctures throughout the book. The chapter summaries in particular are sharply crafted and helpful to the reader. Each chapter touches on a set of empirical issues that carry special theoretical freight. The novel analyses themselves are invariably provocative, and Roberts casts his theoretical net so widely that most syntacticians will find something that bears on their special interests. Specialists in Welsh, for example, will appreciate the novel analysis of the traditional problem of consonant mutation, whereas students of the related Celtic languages, especially Irish and Breton, will appreciate the careful comparisons made between these languages. Readers less interested in the morphological intricacy of Welsh will be sure to find Roberts' perspective on the organization of functional projections above the verb phrase fascinating, and his attempt to reconcile head movement with the general outlines of the Minimalist program to be well worth study. Since they are likely to be of general interest, I will focus on these two aspects of the book for further comment.

Consider first the issue of where the Welsh verb is positioned. Roberts argues that it raises out of VP but not as high as the Complementizer system. Since he claims that the subject raises out of VP, there must be at least two functional projections between VP and CP. On the basis of evidence involving the position of the verb, its agreement pattern and the relative position of subjects, Roberts argues that there are in fact three head positions between VP and the C system, which are ordered as follows: [PERSON [NUMBER [TENSE VP]]]. The verb is forced to raise to PERSON because PERSON houses an affix that requires morphological support. The subject occupies the specifier of NUMBER, which explains why the Welsh verb fails to agree in number with overt, non-pronominal subjects.

An exception is presented by the verb *bod* 'be', which is held to raise further into the C system. The reason for this claim is that *bod* syncretizes with preverbal particles in the C system and is also implicated in the licensing

of clause-initial predicate nominals within CP. One might wonder why these properties of *bod* could not be accounted for by lowering the preverbal particle out of the C system and onto the verb, akin to McCloskey's (1996) analysis of the Irish negative preverbal particle *ní*. McCloskey argues that *ní* must be generated in C because it is able to c-command and license clause-initial negative polarity items (NPIs), such as *neach ar bith dínn* 'not one of us' in (1), but that it is subsequently lowered to cliticize to the verb at Phonetic Form (PF), given that sentential adverbs cannot intervene between the preverbal particle and the verb.

(1) Neach ar bith dínn ní bheidh beo.
being any of-us NEG will-be alive
'Not one of us will be alive.'

Roberts rejects this kind of non-syntactic PF movement and instead inverts McCloskey's lowering analysis. In Roberts' view, the C system comprises FORCE, FOC and FIN phrases, which host complementizers, *wh*-phrases and pre-verbal particles, respectively, and which occur in the following hierarchical order: [FORCE [FOC [FIN PERSONP]]]. While the Irish negative particle is generated in FIN, the NPI occupies the specifier of FOC, which is also the locus of sentential adverbs. At Logical Form (LF), the negative particle raises to the FORCE position, where it c-commands and licenses the NPI.

Although Roberts is thus skeptical of PF-movements like McCloskey's, his system allows PF an important role in triggering syntactic movement by recognizing a class of PF features that require phonological realization. These features drive the Welsh verb to raise to PERSON. Moreover, they drive the exceptional Welsh verb *bod*, as well as Breton and German verbs, to raise to FIN, which produces the familiar verb second phenomena. It is worth noting that on this account, FIN is not only the surface position of the Welsh preverbal particle but also the locus of the verb second phenomenon.

Traditionally, Germanic verb second has been derived by requiring that C have a filled specifier and that the finite verb move into C. FIN plays a crucial role in Roberts' attempt to make the general outlines of this classic analysis a consequence of Universal Grammar. Roberts hypothesizes that FIN must be phonologically realized. In embedded contexts, FIN becomes phonologically realized by fusing with a selected complementizer in the FORCE position. In root clauses, the phonological requirement of FIN is satisfied by verb raising, as in the familiar verb second pattern of German, or, alternatively, by insertion of a preverbal particle. In Roberts' analysis, the fact that German has the verb-second pattern, but that Welsh apparently does not, is linked to the fact that German uses a movement operation to satisfy the requirement of FIN. Welsh does not use

movement, choosing instead to insert a particle. Breton exhibits a verb second pattern like German, and Roberts offers as evidence for his analysis the fact that Breton lacks the affirmative preverbal particles that Welsh uses in root clauses. Instead, Breton topicalizes a phrasal constituent (like German) or moves a non-tensed verb to FIN by head movement, as shown in (2).

(2) Kavet$_i$ [am eus [bet [[e]$_i$ al lever]]].
found 1SG have had the book
'I have found the book.'

In standard accounts, head movement is assumed to be strictly local, involving only a head and the head of its sister constituent. The Breton construction in (2) has special interest because it appears to be a case of so-called long head movement, which violates strict locality.

Ultimately, Roberts argues that the Extended Projection Principle (EPP) can also be reduced to a requirement that a syntactic constituent be phonologically realized, much as he requires FIN to be, and he attempts a theoretical unification. The EPP is a parametrized principle that requires sentences to have overt subjects. Roberts argues that a head can require that it itself or its specifier be phonologically realized. Consider the traditional tense-agreement projection I(nflection) in this context. We derive the EPP if the specifier option is selected; V-to-I raising, on the other hand, results if the head option is employed. The same choices appear in the C system. German requires both the head and specifier of FIN to be phonologically realized, which produces the verb second effect. Breton FIN shares with German its EPP requirement, which causes a constituent to raise into FINP, either by topicalization or by long head movement. Welsh differs from Breton and German in that it satisfies the phonological requirement of FIN by insertion of a root affirmative particle.

There are several empirical problems with Roberts' account. The first problem concerns the importance that Roberts places on the asymmetry between root and subordinate clauses with respect to German verb second and Welsh preverbal particles. This asymmetry is built into Roberts' analysis by allowing a selected complementizer in FORCE to satisfy the PF requirements of FIN so that only in root clauses verb movement will be needed. This leaves unexplained Yiddish, which is a close relative of German that shows the verb second pattern in both root and subordinate clauses. The second problem concerns the link that Roberts forges between verb second and movement. Breton has the verb second pattern, but as Press (1986) notes, it can be satisfied by the dummy verb stem *bez* 'be'. This presents a problem for Roberts' analysis because *bez* has apparently not been moved from another clausal position but is merged *in situ*.

The appeal to long head movement in Breton also has its weaknesses. In general, head movement does not fit well with Minimalist syntax. The

problem is that it does not clearly obey the Extension Condition, which requires every syntactic operation to target the root of a structure under construction and yield a new root node. Head movement also seems to be out of step with the lexicalist assumptions that are reflected in postulating feature-checking to be the trigger for syntactic movement. Roberts defends the importance of head movement in Minimalist syntax by claiming that movement can be motivated by the need to phonologically realize certain categories. Because his treatment of Breton relies on the existence of long head movement in the language to satisfy an EPP requirement on FIN, it is crucial for Roberts' argument that this instance of head movement be as central to the core of syntax as the verb second pattern of German. Yet there is an important sense in which the verb raising operations in the two languages are at polar extremes. The raising of the German verb conforms to the strongly local limitation on head movement. Presumably this constraint is a sub-case of the well-known superiority effect, which can be derived from the Minimal Distance Condition and which is observable in *wh*-movement and other syntactic phenomena. However, the long head movement of the Breton verb in (2) is insensitive to the Minimal Distance Condition, exhibiting a type of anti-superiority effect. The problem is that Roberts does not explain why Breton long head movement fails to show the standard minimal distance effect that other movements exhibit.

The incongruity of Breton long head movement with other syntactic movement processes recalls McCloskey's use of a post-syntactic head lowering operation to explain the distribution of the Irish preverbal particles that Roberts assigns to FIN. For these Irish structures, Roberts suggests LF head movement of FIN to a higher head position so as to avoid McCloskey's post-syntactic operation. Usually, movement at LF is employed to define a level of representation where distinct surface patterns are treated as equivalent, as in the classic argument that Chinese *wh*-phrases, which are superficially *in situ*, move to the specifier of CP at LF. At LF, English and Chinese *wh*-phrases occupy the same structural position, allowing for an elegant statement of selection properties and perhaps other shared semantic properties. Roberts uses head movement at LF to encode language variation. Irish allows its negative preverbal particle in FIN to raise at LF to a higher head (FORCE), from which it licenses an NPI in subject position. English does not allow its negatives to undergo head movement at LF and does not license NPIs in subject position, but it is unclear why. The choice is whether LF is a vehicle to represent language variation or whether language variation is better accounted for at the PF interface level, which is McCloskey's approach.

To conclude, this is an ambitious book with strong claims that have implications for much of what is currently on syntacticians' research agenda. It deserves, and will repay, close attention.

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Author's address: 312 Dey Hall, Department of Linguistics, Campus Box #3155, University of North Carolina at Chapel Hill, Chapel Hill, NC 27599-3155, U.S.A.
E-mail: hendrick@email.unc.edu
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Pieter A. M. Seuren, *Chomsky's Minimalism*. Oxford: Oxford University Press, 2004. Pp. viii + 244.

Reviewed by PIUS TEN HACKEN, University of Wales Swansea

The book under review constitutes an attack on the Minimalist Program (MP), which Chomsky proposed in the late 1980s in part as a reaction to the lack of a restrictive theory of parameters within Government and Binding theory (GB). As so often in Chomsky's career, the MP was first outlined in Chomsky's MIT lectures and seminars, before the material was informally circulated. Finally, Chomsky published revised versions of the most important papers to make them more easily accessible, resulting in Chomsky (1995), which describes the development from late GB to early MP. But of course development did not stop there. Many linguists working on a variety of languages were inspired by and have since built on Chomsky's ideas.

It is perhaps an understatement to describe Seuren's book as an attack on the MP, since it is intended to refute the MP in its entirety and to do so with an impressive overkill. One quickly gets tired of Seuren's style, which is filled with sarcasm, scorn and indignation. Although the book is organized in eight chapters, it is basically a long list of all the arguments that the author could rustle up against the MP; and many arguments are based on misunderstandings or particularly uncooperative interpretations of Chomsky (1995). Given the rather unstructured, list-like approach of the book, I will not attempt to give a chapter-by-chapter summary, but rather discuss some of the recurring themes.

First, it is not always clear what Seuren intends to be the referent of the label 'MP'. The following three interpretations are possible:

(i) MP refers to Chomsky (1995);
(ii) MP refers to Chomsky's work in linguistics from the late 1980s;

(iii) MP refers to work from the late 1980s onwards which elaborates the framework proposed by Chomsky and applies it to data from various languages.

The evidence as to which interpretation Seuren adopts is ambiguous. The book starts with the following programmatic statement:

> This book is a sustained argument purporting to show that Noam Chomsky's latest version of his linguistic theory, recently published as *The Minimalist Program* (MP) (Chomsky 1995), though presented as the crowning achievement of the Chomskyan version of generative grammar, is in fact the clearest possible demonstration that that version is fundamentally flawed. (3)

This suggests that Seuren uses 'MP' in sense (i). Although rather uncommon, it would be academically sound to have a book-length review of Chomsky (1995), which has some references to later developments of the theory. However, a proper book review should take into account the book's purpose. Chomsky (1995) is neither an overview of achievements in the MP, nor a systematic presentation of a particular state of the theory, but a collection of papers meant to suggest analyses and to inspire other linguists to pursue them. It is no wonder then that analyses outlined in different chapters are not always consistent, and that the data are invoked merely to illustrate the theory. Seuren's repeated criticism that Chomsky (1995) contains inconsistencies between chapters (for example, regarding the treatment of X-bar theory) and does not analyse data properly is, in my view, irrelevant.

Perhaps Seuren intends 'MP' to have the interpretation in (ii), given that he explicitly refers to some of Chomsky's later work. However, I fail to see how Seuren's line of criticism can apply to these later papers, as all of them are non-technical and philosophical in orientation. In fact, Seuren's complaint that there is a lack of serious attention to linguistic data and his conclusion that the MP suffers from a 'fatal scarcity of data' (228) are meaningless unless he intends 'MP' to be interpreted as in (iii). In that case, he would need to discuss actual work by linguists working within the MP – something he fails to do in this book. In short, Seuren does not provide a proper argument, and his attack does not convince.

A second recurring theme is the architecture of grammar. Chomsky has always considered syntax to be an autonomous module, which, while interacting with other modules, has its own set of operating principles. The theory of the internal structure of the syntax module has developed considerably. In the MP, items from the lexicon are assembled into syntactic structures until the derivation reaches the conceptual–intentional interface level called Logical Form (LF). While there are no intermediate representations, at some point in the derivation, prior to LF, the derived structures feed to the articulatory–perceptual interface level called Phonetic Form (PF). The point at which the derivation bifurcates into PF and LF is called Spell-Out.

Seuren calls this model of autonomous syntax a random-generator grammar. In contrast, he advocates a Generative-Semantics-style view of grammar as a mediational device, which directly mediates between meaning and sound. He repeatedly states that the random-generator model is absurd. His main argument is that in an autonomous theory, the grammar generates sentences at random, whereas we can readily observe that human beings do not do so. Seuren thus claims that Chomsky cannot account for communication. However, as I have argued elsewhere (ten Hacken 2002), communication can be integrated in a Chomskyan framework if performance is the result of interacting modules, one of which is competence. Competence occupies a privileged place in that its output can be studied in isolation, whereas this is much more difficult for the other modules involved. The only sense in which Seuren's objection to Chomsky's architecture can then be interpreted is as a rejection of the legitimacy of the distinction between competence and performance.

Unrelenting opponents of Chomsky's views may enjoy reading that the architecture of the MP causes it to be 'trapped between absurdity and perversity' (161), but others will not be impressed by his arguments. Seuren sees the MP as 'absurd' because it is a 'random generator', while the alleged perversity refers to the existence of a 'perfectly reasonable alternative model' (161), viz. Seuren's own. This model, represented as a complex graph with various styles of boxes, lines, arrows and text (162), is not explained in any detail.

The final theme to be addressed in this review concerns the historical context. In Seuren's unorthodox view (3, 151–158), Chomsky is not a major figure in the recent history of linguistics. The main line of development, as presented in this book, proceeds from Bloomfield, via Harris and Katz & Postal (1964), to Generative Semantics. Seuren further claims that Chomsky initially supported Generative Semantics but changed his mind in the late 1960s and managed to attract his following through a combination of rhetoric and other non-academic tricks.

Seuren sees the opposition between Chomsky and himself as one between paradigms. Thus, he states that the MP is

> the undoing of the development started by Chomsky in the late 1960s, when he decided to dissociate himself from what was then the natural continuation of the new paradigm of transformational-generative grammar. (3)

Whatever the historical events, Seuren's claim that he and Chomsky operate in different paradigms has two immediate consequences. First, Chomsky's paradigm cannot be falsified by data. Following Kuhn (1970), we must assume that anomalies, that is, empirical counterexamples, are normal and do not automatically falsify the entire theory. Falsification of a paradigm is inherently impossible. A paradigm may enter a crisis and be phased out in favour of another if its adherents no longer believe in its prospects, but it

cannot be falsified. A second consequence is that we have to be very careful in comparing theories belonging to different paradigms. As Kuhn shows, paradigms impose conflicting evaluation criteria on theories, making them incommensurable. While statements can be translated between paradigms, there is no common ground for evaluating theories.

The fact that Seuren does not even mention, let alone consider, these consequences renders futile much of his argument, in particular his reference to falsifiability (4) and the comparison of analyses for particular data (99–106, 143–147, most of the last chapter). When Chomsky (1995) appeals to conceptual necessity and naturalness, these notions are relative to Chomsky's paradigm. However, when Seuren invokes these concepts, he uses notions belonging to Seuren's paradigm. If the data analyses prove anything, it is at best that Seuren's theory is better adapted to Seuren's paradigm than Chomsky's theory. In most cases, however, not even this level of comparison is achieved because Seuren's evaluation of how Chomsky accounts for data exhausts itself in the statement that it is non-existent or absurd.

There is certainly more to say about this book, but the points above may suffice to indicate that much of Seuren's argument is misconceived and unconvincing. The book may be enjoyed by linguists who share with Seuren a sense of sarcasm, scorn and indignation against Chomsky.

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Author's address: School of Arts, University of Wales Swansea, Singleton Park, Swansea SA2 8PP, U.K.
E-mail: p.ten-hacken@swan.ac.uk

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Jeroen van de Weijer, Vincent J. van Heuven & Harry van der Hulst (eds.), *The phonological spectrum*, vol. I: *Segmental structure* & vol. II: *Suprasegmental structure* (Current Issues in Linguistic Theory **233** & **234**). Amsterdam & Philadelphia: John Benjamins, 2003. Pp. x + 306 (vol. I); pp. x + 262 (vol. II).

Reviewed by Christian Uffmann, University of Marburg & University of Tromsø

In a recent article, Newmeyer (2002) admires the state of phonology as a field: in contrast to syntax, which splits into more and more theoretical models and which is characterised by a general incompatibility of models, phonologists (according to Newmeyer) always tend to rally around the latest available model, currently Optimality Theory (OT), and theories are generally compatible with one another. If anything was ever needed to refute Newmeyer's claims about the homogeneity of phonological theory, it is the two volumes under review. While many papers are written in the OT framework (seven out of twenty), the majority are not. Instead, as the title promises, the volumes show the entire 'phonological spectrum'. They do so in two ways: first by collecting papers on all subfields of phonology, from segmental issues to prosody; and second, by presenting the range of models and theories in the field today, including Feature Geometry, Government Phonology, Laboratory Phonology and also some fundamentally different approaches within the framework of OT. Hence, this collection shows how greatly scholars' assumptions about the scope of phonology can differ and what different views may be held regarding the division of labour between phonetics and phonology. I will return to these points in the discussion of individual articles.

The articles in these two volumes are a selection of papers presented at the fourth Holland Institute of Linguistics Phonology Conference in Leiden in 1999. In general, the editors have done a very good job to prevent the volumes from being a random and haphazard collection of articles, which is always a danger for proceedings based on conferences that have a general topic. Each volume is divided into three sections, each focusing on a specific issue and containing three or four articles. In some instances, this is exceptionally well done, while other sections cannot escape a certain arbitrariness, as I will show in the discussion below.

The first volume, entitled *Segmental structure*, consists of the following sections: 'Nasality', 'Voice' and 'Time, tone and other things'. 'Nasality' is perhaps the most intriguing section in this volume since it offers competing analyses of the phenomenon of nasal harmony, showing the diversity of approaches within the field of phonology. More specifically, the papers are concerned with a unified analysis of the two types of harmony systems noted in the literature: (i) systems in which nasality spreads until being blocked by an incompatible segment (type A), and (ii) systems in which incompatible segments are transparent to spreading (type B) (Piggott 1992).

Paul Boersma gives an account of these two types in 'Nasal harmony in functional phonology', using a model which closely ties together phonetics and phonology and which distinguishes between a production grammar and a perception grammar. For Boersma, nasal harmony systems are evidence for such a distinction. He argues that type A languages show articulatory spreading while type B languages show perceptual spreading. In

his functionalist model of OT, articulatory spreading results from the interaction of constraints which seek to minimise the number of articulatory gestures and faithfulness. Transparency, however, is a perceptual effect. Intervening plosives in type B languages are transparent: while they are articulatorily realised as non-nasal, perceptually, nasality is carried across them. Boersma represents this by interpreting the prohibition against crossing association lines as violable if the resulting sequence is still perceived as a unit.

Rachel Walker's paper, 'Reinterpreting transparency in nasal harmony', seeks an explanation which is phonological in a more traditional sense. She derives the ability of a segment to become nasalised from the sonority hierarchy, which she translates into an OT markedness hierarchy, thus explaining why only segments up to a certain sonority threshold may nasalise in a given language. With respect to type B languages, she observes a curious asymmetry. While any segment may act as a blocker, only stops – that is, the least sonorous segments – may be transparent. She thus concludes that type B languages are really like type A languages, except for the pro-spreading constraint SPREAD outranking all markedness constraints. Hence, there are no blockers in type B languages. The transparency of plosives is grounded in the fact that they are physically not nasalisable, and Walker invokes Sympathy Theory (McCarthy 1999) to account for their transparency (the sympathetic candidate being the one which nasalises all segments).

The two approaches invite comparison and discussion since they come from two different angles. Space permits only a brief discussion of two selected points. First, Walker's paper is in the original spirit of OT, trying to derive observed cross-linguistic variation from the re-ranking of a single set of constraints. In contrast, Boersma also appeals to phonetic (articulatory and perceptual) grounding in order to account for the data, as, for example, when he argues for perceptual as opposed to articulatory spreading. The incorporation of phonetic reasoning has additional consequences. Boersma does not consider candidates which he deems articulatorily impossible (for example, nasalised stops). Walker, however, includes articulatorily impossible candidates and invokes Sympathy as a repair mechanism that yields licit output segments. This point touches upon the important but unresolved question of which candidates are actually generated by GEN, the generator function of OT, and suggests an interesting condition on the applicability of Sympathy Theory.

The final paper in this section, 'Can "phonological" nasality be derived from phonetic nasality?', by Stefan Ploch, raises the thought-provoking question of how the phonetics and the phonology of nasality interface. For Boersma, there seems to be a clear one-to-one correspondence: articulatory nasality corresponds to velum lowering, and perceptual nasality has clear acoustic correlates. This view is strongly contested by Ploch, who

proposes Element Theory as an alternative to traditional feature theory. Unfortunately, much of the force of his argument is lost by the venomous (and often repetitive) manner in which Ploch attacks mainstream phonology, by some rather dubious statements (for example, the claim that phonetic rules are never language-dependent) and by what many phonologists under attack here would probably consider misrepresentations of their claims.

A shortcoming of the two volumes which becomes apparent in this section is that, although papers are often on similar topics, there are few direct connections between them. With the exception of Boersma quoting Walker, the authors do not discuss their colleagues' ideas or analyses, although the papers could surely have been made available to all the authors. It would, for instance, be interesting to read what Ploch would have to say about Boersma's functionalist model of phonology (and vice versa). Alternatively, the editors themselves could have tried to paint a larger picture by including a few opening paragraphs, but they decided to provide only a brief summary of each paper at the beginning of the volume. It is therefore left to the reader to uncover the connections between the papers and to find out how and where authors' concepts of phonology diverge.

A similar reservation holds for the second section of the first volume, 'Voice', which contains three papers on laryngeality. Here, the issue of the phonetics–phonology interface figures prominently. In 'The role of phonology and phonetics in Dutch voice assimilation', Mirjam Ernestus gives a new and original account of voicing neutralisation and assimilation in Dutch. She argues that voice assimilation and final devoicing in Dutch should be viewed as purely phonetic processes. Coda segments are unspecified for [voice] and receive the most natural phonetic implementation, depending on their environment. This view reduces the role of phonology considerably (in this case, it is reduced to feature specification) and allows all that is considered 'natural' to be part of phonetic implementation. Caroline Féry's paper, 'Final devoicing and the stratification of the lexicon in German', reappraises the role of phonology. Féry assumes final devoicing to be phonological (as in the mainstream view) and discusses the role of ambisyllabic consonants, which become devoiced in words that are part of the core native vocabulary of German, but stay voiced when occurring in loanwords (many of them of Low German or Dutch origin and thus hardly identifiable as foreign). Féry finds that, in addition to final devoicing, a rather large set of phonotactic restrictions is sensitive to this stratification of the lexicon, in that they apply less strictly in the loan vocabulary, and proposes an OT analysis which uses faithfulness constraints that are indexed for different lexical strata. Thus, the same faithfulness constraint can be ranked differently in the constraint hierarchy, depending on which lexical stratum the input form comes from. What some researchers might view as lexicalised alternations or restrictions, Féry thus treats in a single synchronic

grammar, thereby widening the scope of phonology. In this respect, the papers by Ernestus and Féry complement one another in a stimulating way, raising the question of what place phonology occupies between phonetics and the lexicon. Eon-Suk Ko's paper, 'The laryngeal effect in Korean: phonology or phonetics?', which is an experimental study of the interaction of high tone and aspirated/tense consonants in the Seoul and Chonnam dialects of Korean, returns to this question. It argues that this interaction of tone and laryngeal features is not phonologised in pitch accent assignment but rather phonetic in nature. Like Ernestus, the author argues that certain phenomena are best understood and described when viewed as purely phonetic processes.

The final section of the first volume, 'Time, tone and other things', is, as its title suggests, a bit of a hotchpotch, but nonetheless it contains interesting articles. Three of the four papers argue for specific feature-geometric representations to account for the phenomena they discuss, showing that despite the sweeping success of OT in general, research into representations is still alive and kicking. Markus Hiller discusses 'The diphthong dynamics distinction in Swabian', and argues for a feature-geometric distinction between two classes of Swabian diphthongs, using a modified version of Clements & Hume's (1995) Unified Feature Theory model. In his contribution, 'Depression in Zulu', Philipp Strazny proposes a feature-geometric analysis of the interaction of high tones and depressor consonants, which he analyses as having antagonistic laryngeal specifications. Unfortunately, Strazny does not consider a similar analysis in Bradshaw (1999), a comparison to which would have been stimulating. Onno Crasborn & Els van der Kooij also invoke feature geometry in their article, 'Base joint configuration in Sign Language of the Netherlands: phonetic variation and phonological specification', which is a most welcome paper. There are some interesting parallels across the two modalities, and Crasborn & van der Kooij show that the phonetics–phonology interface, as well as the distinction between these two levels of linguistic organisation, is similarly significant in signed and spoken language. The fourth paper in this section is entitled 'Weakening processes in the Optimality framework'. Here, K. G. Vijayakrishnan proposes a novel OT analysis of lenition and neutralisation, which is based on alignment constraints, as opposed to standard positional faithfulness or positional markedness accounts. Although the analyses proper tend to be a bit sketchy, this is a stimulating proposal.

For reasons of space, *Suprasegmental structure*, the second volume of the work under review, will be discussed more briefly, although it also contains a number of interesting papers and proposals. Overall, the papers connect less well than the papers in the first volume, which is perhaps not surprising given that the three sections, 'Syllabic structure', 'Metrical structure' and 'Prosodic structure', cover more general themes. Still, the papers collected in this volume live up to the promise of showing the

'phonological spectrum', in that they illustrate a host of different theories and approaches to suprasegmental structure.

The section on syllable structure contains four papers which differ in both their theoretical assumptions and their methodology and thus provide the reader with a wide range of different approaches to this topic. Heather Goad & Kathleen Brannen discuss the 'Phonetic evidence for phonological structure in syllabification'. Using evidence from early child language, they argue first that in many languages, word-final consonants are onsets, and, secondly, that there is a distinction between languages which project a Laryngeal node and languages which project only a Spontaneous Voicing node. On this set of assumptions, it becomes possible to account for the variation found in children's forms. In his paper, 'The phonology–phonetics interface and Syllabic Theory', Shosuke Haraguchi presents a different, set-theoretic view of the syllable. He argues that, often, linearisation need not be underlyingly present since it is predictable: for example, the phonemes /h, ɪ, d/ in *hid* can only be ordered in one way to yield a licit English word (cf. **dih*, **hdi*, etc.). Haraguchi thus advocates a view of syllable structure which is considerably more abstract than many other models, leaving all that is predictable to implementation. A rather abstract model of syllable structure is also found in Krisztina Polgárdi's paper, which argues for an analysis of 'Hungarian as a strict CV language'. Polgárdi shows how the facts of Hungarian phonotactics – including long *á* and *é*, which behave exceptionally in Hungarian phonology – can be accounted for in a strict CV-model of Government Phonology, where CV is the only licit syllable shape and surface clusters are underlyingly separated by empty positions. Finally, in 'Syllable structure at different levels in the speech production process', Dirk-Bart den Ouden & Roelien Bastiaanse discuss syllable structure and syllable markedness using evidence from aphasia. They find evidence for the mental reality of abstract syllable structure, arguing that articulatory factors alone cannot sufficiently explain the deletion patterns observed in aphasic speech. In sum, these four articles offer fresh perspectives on the topic of syllable structure, introducing and defending different theories of syllable structure, but the search for the one and only correct theory continues.

The section on 'Metrical structure' contains three papers that deal with issues of stress. In the first paper, 'Quantity-sensitivity of syllabic trochees revisited', Heli Harrikari contests the claim that syllabic trochees are always quantity-insensitive, adducing data from Finnish dialects where a post-tonic onset consonant before a long vowel geminates, thus generating an even trochee. Jay Rifkin proposes a new analysis of ternary stress systems, suggesting that 'Ternarity is prosodic word binarity'. He argues that prosodic words are optimally three syllables long and that ternary stress systems assign one stress per prosodic word. His analysis of Cayuvava presents evidence that prosodic words are indeed optimally trisyllabic

in this language. Finally, Ellen van Zanten, Rob Goedemans & Jos Pacilly experimentally probe into 'The status of word stress in Indonesian'. They challenge traditional analyses of Indonesian as having a bidirectional stress system with main stress on the penult. Data from Jakarta Indonesian suggest that this language does not have any word stress at all; only data from Toba Batak indicate penultimate stress. This paper shows the importance of thorough data investigation before theoretical claims of any sort can be made.

The final section on 'Prosodic structure' is more uniform than the previous section, if only because all papers are situated within an experimental Laboratory Phonology framework and concerned with the same language, namely Dutch. In their paper, 'Perceived prominence and the metrical-prosodic structure of Dutch sentences', Karijn Helsloot & Barbertje M. Streefkerk propose a new model of Dutch prosody, which does not take into account complex syntactic structure but assigns prominence only on the basis of word class membership, supplemented by some surface fix-up rules. In 'Phonetic variation or phonological difference? The case of the early versus the late accent-lending fall in Dutch', Johanneke Caspers investigates whether an early versus late pitch fall in Dutch declarative sentences has any phonological function. Although her evidence is somewhat inconclusive, it nevertheless shows that speakers are aware of this difference and assign different functions to the different loci of pitch fall. Finally, Bert Remijsen & Vincent J. van Heuven present data from perception experiments to shed some light 'On the categorical nature of intonational contrasts'. While classification tasks show the S-shaped curve that is so familiar from perceptual experiments on phoneme categories, the picture is less clear in discrimination tasks, which do not show one clear peak. Remijsen & van Heuven propose additional methods to overcome this problem.

To conclude, the two volumes collect a range of interesting papers which are generally of high quality and which raise a variety of issues that are central in phonological theory today, from the division of labour between phonetics and phonology or representational issues to questions of constraint interaction. The selection and organisation of the papers is good, as is the editing, which (with minor exceptions) also emphasises expositional uniformity. The books are thus a welcome addition to any linguistic library and provide plenty of food for thought.

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Author's address: *Center for Advanced Study in Theoretical Linguistics* (*CASTL*),
University of Tromsø, N-9037 Tromsø, Norway.
E-mail: christian.uffmann@hum.uil.no

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