REVIEWS

Robert D. Borsley & Ian Roberts (eds.), *The syntax of the Celtic languages: a comparative perspective*. Cambridge: Cambridge University Press, 1996. Pp. viii + 368.

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This volume comprises a collection of ten papers on the syntax of Celtic, together with an introduction to the subject by the volume's editors. The papers grew out of a conference that was held in Bangor in 1992. All of the papers are cast within the Principles and Parameters or Minimalist frameworks, and together they provide a good overview of current issues in Celtic syntax as well as those that have been of interest to Celticists working within generative theory since the late seventies. Four of the papers are on Irish, one is on Scottish Gaelic, two are on Welsh, one on Breton, and two focus on the syntax of more than one Celtic language.

The introductory chapter by Robert Borsley & Ian Roberts provides an overview of the syntax of Celtic, and attempts to introduce the reader to principles and parameters theory, and to the main issues that are covered in the other papers in the volume. This chapter is interesting in that it is the first attempt I have seen to provide an overview of current issues in Celtic syntax, and as such is a valuable contribution in itself. It could provide a useful introduction to issues in Celtic syntax, for a reader new to the field. A reader unfamiliar with principles and parameters theory, however, would not find sufficient material in this introduction to allow him/her to follow the rest of the papers in the volume.

Turning first to the four papers on Irish, Jim McCloskey's paper 'Subjects and subject positions in Irish' takes on the structure of unaccusatives. He makes a convincing case for saying that in Irish, the internal argument of unaccusative and impersonal passive verbs remains in its D-structure position. There is no need for the argument to raise, as it is Case-licensed in-situ by a preposition. Thus, the Extended Projection Principle does not hold in Irish. Rather the position occupied by subjects of transitive and unergative verbs is only generated when a nominative argument is present.

The issue of the VSO word order of Celtic is probably the most discussed issue in generative approaches to Celtic syntax. This problem is dealt with in Jonathan Bobaljik & Andrew Carnie's paper 'A minimalist approach to some problems of Irish word order'. They argue that both the subject and the object raise out of VP in the overt syntax. At spell-out, the subject occupies Spec of TP, the Verb occupies AgrS while the object occupies Spec of Agr O.

Since this analysis was first proposed in manuscript form, several complications have been noted with respect to dialect differences in the position of the object in infinitival clauses (Carnie 1995, Noonan 1994). These are not dealt with here, but the paper is of interest as it is the first attempt to apply the minimalist theory to VSO order, and the earlier manuscript version has been frequently cited.

Nigel Duffield's paper 'On structural invariance and lexical diversity in VSO languages' takes up the issue of word order from the perspective of the internal structure of DPs. Drawing on data from Irish, Hebrew and Maltese, Duffield claims that all three languages form their construct-state nominals through N^0-movement, but the landing site of the moved head differs. The head of N raises to D^0 in Hebrew and Maltese, but moves to a lower functional projection (Num^0) in Irish. He uses this as a basis to argue that the underlying structure of construct state nominals is invariant cross-linguistically, and that the different word orders arise through variation in the landing site of the moved element.

Paolo Aquaviva's contribution 'Negation in Irish and the representation of monotone decreasing quantifiers' deals with the status of monotone decreasing quantifiers (e.g. *few*, *only*) in Irish. Through a complex series of arguments, he shows that Irish provides supporting evidence for the proposal that monotone decreasing quantifiers can be analysed as the negation of an indefinite, and an abstract NegP is present in these clauses.

The first of the three papers on Welsh is concerned with the issue of the syntax of fronting in two non-VSO structures. In 'Fronting constructions in Welsh' Maggie Tallerman proposes that there are two types of fronting constructions. The first is a cleft structure which is formed through *wh*-movement, and shows the usual characteristics of such structures (absence of agreement with the moved DP, restriction to a single fronted element, etc.). The second kind of fronted construction is a base-generated topicalization structure. Tallerman suggests that that the topicalized structure cannot appear in embedded contexts because it arises through left-adjunction to CP.

The other two papers on Welsh deal with the syntax of copular constructions. In '*Bod* in the present and other tenses' Alain Rouveret presents a unified analysis of *bod* 'be' and in so doing, presents a discussion of predicate, existential and identificational copular constructions. Assuming a VP-shell structure (Larson 1988, Travis 1992) he argues that *bod* heads a lower VP, and that the head of the upper VP may be occupied by a locative clitic. In stage level predicates, this clitic binds the spatio-temporal variable, and *bod* surfaces as *mae*. This augmented form of *bod* does not appear in negative, interrogative, or past forms, because in these forms an operator can bind the spatio-temporal variable. His account also discusses individual level predicates where *bod* can appear without the upper projection.

In 'Some syntactic effects of suppletion in the Celtic copulas' Randall Hendrick adopts a very different analysis of *bod*. Under his view the variation

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in the form of *bod* is due to the morphosyntactic properties of the complementizer that appears in the construction. He argues that the Welsh substantive copula exhibits suppletion that has syntactic effects, while in Breton, the functional copula varies morphophonemically, and is syntactically inert. His approach is based on optimality theory and explicitly denies that the variation in the copula is determined by semantic/syntactic factors, but rather is due to morphosyntactic factors.

'Long head movement in Breton' by Robert Borsley, Maria-Luisa Rivero & Janig Stephens deals with a type of long-distance movement in the language. Long head movement involves the movement of a non-finite V over one or more L-related heads to a non-L-related position. It is found in a number of unrelated languages (e.g. Slavic and Romance), and unlike remnant movement in Germanic, it is clause bound, and cannot cross negation. The authors adopt a Minimalist approach, and suggest that the phenomenon arises in certain languages as the result of the need to license Tense. The exact mechanism for doing so, and the domain within which this occurs do not involve the usual checking system we find within the Minimalist framework, and the lack of clarity on this point detracts from the authors' claims.

David Adger's paper 'Aspect, agreement and measure phrases in Scottish Gaelic' presents an explanation for the fact that Scottish Gaelic measure phrases, unlike other DPs, do not participate in object shift. The author claims that this is because measure phrases do not need to move overtly to a Case position, because they can be Case licensed in-situ by becoming part of a Tense chain as described by Guéron & Hoekstra (1988). The author defends a generalised version of Visibility, and suggests that Tense chains are formed from smaller aspectual chains via a mechanism of selection indices.

The paper entitled 'Pronominal enclisis in VSO languages' by Ian Roberts & Ur Shlonsky suggests that the absence of weak pronouns and clitics in Celtic and Semitic languages is linked to the absence of strong N features in Agr in these languages. They link this to the VSO word order of these languages. Following Kayne (1994), they suggest that affixal heads only appear on the right of the stem, while clitics are always and only on the left of their hosts. It then follows that a head-initial language will have affixes rather than clitics. They argue that enclisis in these languages, should be treated as affixation rather than as adjunction of pronominal clitics.

Together with its introductory chapter, this collection of papers provides a varied overview of the state of Celtic syntax. As in all collections, the quality of the individual papers is variable, but overall, the volume is a valuable contribution to the existing literature on Celtic syntax.

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(Received 20 September 1999)

Miriam Butt & Wilhelm Geuder (eds.), *The projection of arguments: lexical and compositional factors* (CSLI Lecture Notes **83**). Stanford: CSLI Publications, 1998. Pp. viii + 363.

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Miriam Butt & Wilhelm Geuder's edited volume is a re-examination of classical approaches to theories of ARGUMENT STRUCTURE and ARGUMENT PROJECTION. An important assumption shared by the major formal syntactic frameworks developed within the last decades is that information relating to a verb's argument structure is lexically encoded and 'opaque' to compositional syntax. In its simplest form, this approach predicts that the lexical semantics of a verb are associated with a set of thematic roles, which are linked to syntactic projections via the verb's argument structure. The distribution of arguments in the syntax is constrained by bi-uniqueness conditions such as the Theta Criterion, which require a one-to-one mapping between semantic roles and syntactically overt arguments.

As Butt & Geuder point out in their introduction, some well-known facts remain hard to account for under these assumptions. For example, syntactic valency-changing processes such as causatives and applicatives are difficult to analyse as purely lexical phenomena. Also, the number and classification of thematic roles remains controversial, and the LINKING of particular roles with particular phrasal constituents in the syntax remains a problematic issue. The familiar *spray/load* alternation, where either the Theme or the Location may be mapped onto direct object, illustrates the problem:

(1) (a) Miriam sprayed the ceiling with champagne.
    (b) Miriam sprayed champagne on the ceiling.

Mismatches also occur between semantic roles and syntactic arguments, for

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example in 'doubled' noun incorporation structures where two arguments appear to be linked to a single semantic role (discussed by Veerle Van Geenhoven in the current volume).

Many of these facts have been thoroughly examined in recent literature, for example in Baker (1988) and Dowty (1991), but Butt & Geuder claim that finer-grained, more empirically robust analyses are possible if the projection of arguments is examined in light of EVENT STRUCTURE. For example, the alternants in (1) differ with respect to their aspectual status; the argument *the ceiling* is interpreted as more affected by the spraying when it is mapped onto direct object, but the core semantics of *spray* remain the same in both examples. This suggests that the projection of arguments may be constrained by a separate level of representation associated with aspect. Several of the papers in this volume attempt to characterise an extra level of structure which mediates between event semantics and argument structure. Others address more tangential themes on the syntax-semantics interface related to the projection of arguments. Butt & Geuder take a rigorous and thorough approach in their introduction to the volume, reviewing the relevant literature and providing a summary of each of the papers. The first set of papers focuses on arguments and events in the lexicon; the second set examines the syntactic and discourse factors that constrain the projection of arguments. A running theme in many of the papers is that properties of argument projection cannot be derived from lexical information alone and must be to some extent COMPOSITIONALLY DETERMINED.

Bill Croft looks at a wide range of data that highlight problems associated with argument linking, including valency-changing processes, locative alternations, passives and psychological predicates. He presents an analysis within the framework of Langacker's Cognitive Grammar (to appear in a forthcoming book) which dissociates core event structure, typically associated with root forms, from a distinct level of event PROFILING closely associated with a given speaker's construal of the event in question. A profile denotes a particular (sub)section of the core event, and is delimited by the participants involved. It is this level that gets mapped onto the syntax according to a set of universal linking rules, mediated by a universal tendency for speakers to conceptualise events according to a FORCE-DYNAMIC template. In other words, Croft argues that the alternations mentioned above all show semantic or pragmatic contrasts such as degrees of affectedness and levels of causality. The strength of Croft's analysis lies in its impressive breadth of scope, though his emphasis on semantic contrast means that structures that seem to display genuine optionality, like passives, are problematic for his approach.

Gillian Ramchand focuses on alternations in Scottish Gaelic that appear to yield biuniqueness violations: a lexeme such as the verb *iarraidh* may assign two distinct sets of roles to its arguments, yielding something like English 'x wants y' versus 'x has wanted/requested y'. These alternations are

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conditioned syntactically by the presence of certain aspectual particles which also trigger changes in word order. Ramchand adopts a theory of aspectual roles and assumes LEXICAL CONCEPTUAL STRUCTURE (LCS) to be a distinct level of representation from argument structure. In English, verb lexemes map participants encoded at LCS directly onto argument mapping, but in Scottish Gaelic, it is the lexical entries for the aspectual particles, not the verbs, which mediate argument mapping and (indirectly) assign aspectual roles. Ramchand draws the radical conclusion that if only LCS holds within the lexicon then no systematic characterisation of the syntactic properties of the lexicon is possible. The lexicon, she argues, cannot exist as an autonomous level of representation with its own universal rules and processes.

Malka Rappaport Hovav & Beth Levin reiterate what they describe as a PROJECTIONIST analysis from earlier work on polysemous verbs in English such as *sweep* and *run*. These verbs raise interesting questions about the lexical-syntactic interface because they appear to contain multiple argument structures within a single lexical entry: *Eugene swept* may be augmented to *Eugene swept the floor*, *Eugene swept the floor clean*, or *Eugene swept the dust into a corner*. Like much previous work in this area they distinguish between idiosyncratic and structural components of a verb's meaning, but in contrast to the 'constructionist' approaches of Ramchand and Ritter & Rosen they identify systematic regularities in polysemous verbs which point to a lexical (rather than syntactic) mechanism for these alternations. The core of their analysis is the process of TEMPLATE AUGMENTATION, where event structure templates combine with each other to form complex events incorporating subevents, which in turn project in the syntax. In this way a simple event like *sweep* may be augmented in the lexicon to yield multiple lexical entries for related polysemous verbs. Although it does not present much in the way of new data, Rappaport Hovav & Levin's paper articulates important arguments in favour of a two-tier lexicon that interacts with argument structure in systematic ways.

Elizabeth Ritter & Sarah Thomas Rosen look at similar phenomena to Rappaport Hovav & Levin, but take the opposite view that event interpretation is compositionally rather than lexically determined. Following Borer (1994), they assume that event structure is determined in the syntax by functional projections that assign event roles. Ritter & Rosen's contribution to this approach is the notion of D(elimited)-events: only those events which are delimited by an endpoint can license a functional projection which assigns an event role. States and activities lack an event structure since they also lack delimiting expressions. Aspectually underspecified verbs such as manner of motion *walk* may combine with a delimiting expression such as *to work*, yielding a syntactically derived endpoint (rather than an additional lexical entry for the verb as in Rappaport Hovav & Levin's model). Another event role, that of initiator, may be licensed by a higher functional projection. An interesting corollary of their proposal is that (in English at least) only

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delimited predicates with event structure also license an initiator role associated with causation; events as such cannot be licensed by an initiator alone.

K. P. Mohanan & Tara Mohanan address the problems raised by reciprocal and reflexive expressions cross-linguistically, presenting a wide range of data from Kannada, Hebrew, Malay and English. The data display interesting variations in linking arguments to syntax: participants encoded in a given verb's argument structure may be mapped onto full phrasal arguments, or reflexive and reciprocal pronouns, or reflexive morphology, or suppressed (syntactically nonovert). Given these facts, any principle that posits a one-to-one correlation between semantic content, semantic structure and the syntax (such as the Projection Principle) must be too strong. Mohanan & Mohanan argue for a separate level of argument structure within the lexicon that allows multiple mappings from semantic structure (as distinct from semantic content) to surface syntax, elegantly accounting for the data at hand. They also argue against the notion of ENCAPSULATION, providing evidence that syntactic processes do have access to lexical semantic structure.

Along with Ramchand, Eloise Jelinek argues for argument structure as a distinct component in the grammar, but the data she presents are very different. She shows that in some languages, including the Uto-Aztecan language Yaqui, transitivity and voice are systematically encoded in the morphosyntax, combining relatively freely with both verbal and nominal roots. Various processes are examined, including (in-)transitivitising, passive, impersonal, applicative, causative and bahuvrihi constructions; these are minimally constrained by core lexical semantics of the root but are otherwise highly productive. Jelinek suggests a convincing compositional analysis in which the observed alternations are associated with the projection of functional heads encoding transitivity and voice, which also license the projection of arguments and check case. Jelinek's new data from Yaqui is clearly presented and provides some of the most conclusive evidence in this volume that in some languages at least, valency-changing processes are best analysed as syntactically derived.

Veerle Van Geenhoven provides a condensed account of noun incorporation structures in West Greenlandic based on her recent PhD thesis. The data presented in this paper pose another challenge to theories of linking: noun incorporating languages allow structures in which arguments appear to be doubled, with one copy attached to the verbal host and the other realised as a constituent noun or modifier which appears freely in the syntax. Van Geenhoven argues that previous accounts of noun incorporation, both lexicalist and syntactic, fail in these cases because they assume some version of the Theta Criterion. She presents an alternate account related to a proposal by Sadock (1991) in which noun incorporated structures are syntactically base generated and SEMANTICALLY INCORPOR-

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ATED. She uses a predicate calculus to show that verbs in West Greenlandic incorporate not nouns but predicates, a process which allows argument doubling. Discarding the idea that there is necessarily a one-to-one mapping of arguments to predicates as suggested by the Theta Criterion, she suggests that incorporating verbs may simultaneously license an internal argument in the syntax and existentially bind a variable in the semantics which also corresponds to the incorporated argument.

Paul Kiparsky is another author in the volume who argues that event interpretation may be compositionally determined. Kiparsky focuses on case in Finnish, but also draws on data from Russian, Mordvinian, Australian and Polynesian languages to support his analysis. Finnish is an interesting language for theories of case because the distribution of the two objective cases, accusative and partitive, encodes both aspectual and NP-related features. Elaborating on Krifka's (1992) semantic framework, Kiparsky shows that depending on the verb class, partitive objects signal that the NP itself is quantitatively indeterminate or UNBOUNDED, or that the event is aspectually unbounded, or both. In contrast, accusative objects have a narrower distribution and can only appear when both the event and the NP are bounded. Kiparsky's characterisation of the distribution of objective case in Finnish is the most successful yet proposed. The data are also highly relevant to the main themes addressed in the current volume, since the event-related property of boundedness is shown to be determined at the phrasal rather than the lexical level.

Ad Neeleman & Tanja Reinhart look at the familiar phenomenon of scrambling in Germanic OV languages. They note that the Minimalist merger operation allows base-generation of both scrambled and non-scrambled DPs. Taking Dutch as an example, they attempt to capture the typological correlation between scrambling and OV word order by looking at scrambling from a prosodic-pragmatic point of view. They argue that case checking is done preferentially within a prosodic domain (VO languages) or, if this is ruled out because the verb and its object do not form a prosodic unit, within a syntactic domain (OV languages), thus explaining adjacency effects in languages which do not permit scrambling. They then present an economy-based model of focus that links stress with both syntax and discourse representations. Neeleman & Reinhart's contribution is perhaps the least relevant to the central issues of the volume, but it contains a novel analysis which raises some interesting and testable hypotheses.

The book is nicely formatted in Latex with user-friendly footnotes and references at the end of each article. In a handful of places, typographical or editorial errors made the arguments difficult to follow: on (141) in Ritter & Rosen's article the English example *Sue swam the baby* is marked as grammatical; Kiparsky (284) introduces the term CUMULATIVITY at a crucial stage in his analysis but never defines or discusses the term; and Neeleman & Reinhart's paper is plagued by paragraph formatting problems. As

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pointed out in reviews of individual papers, not all present new data or analyses. However, the generally high quality of the work and the typologically diverse language data presented make this a thought-provoking book that achieves significant progress in solving some perennially sticky problems.

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Michael Hammond, *The phonology of English: a prosodic optimality-theoretic approach* (The Phonology of the World's Languages). Oxford: Oxford University Press, 1999. Pp xvi+368.

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Now that Optimality Theory (OT) has reached the status of textbook orthodoxy in phonology (see, amongst others, Kager 1999), it is not surprising that Michael Hammond's *the Phonology of English* (henceforth PE), the sixth volume in OUP's series The Phonology of the World's Languages, should be subtitled '*a prosodic optimality-theoretic approach*'. Or, at least, the '*optimality-theoretic*' part is not surprising. The '*prosodic*' part is worthy of comment, and we shall return to this point below.

OT has provided a new perspective on several aspects of phonology and is a fertile area for debate, both as to whether the theory is on the right lines at all and, if so, what kind of theoretical devices it needs. Given that such foundational OT texts as Prince & Smolensky (1993) are still not officially published at the time of writing, works such as PE (which to my knowledge is the first volume which confronts OT with a range of data from a single language) are, in principle, welcome.

The book is wide in scope. Hammond writes of the volume: 'the perspective taken is introductory…I assume no prior knowledge of English

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phonology or of OT' (vii) but, also, that PE 'makes a number of novel theoretical proposals within Optimality Theory' (viii). It is intended for both students new to phonology and 'technical OT phonologists' (viii). This gives the book an intriguing multiple aim – to be both monograph and textbook, for English, OT and phonology. Hammond justifies this with the observation that it thus provides 'a unique pedagogical opportunity for students to understand Optimality Theory (since the data are familiar) and a unique opportunity to test this theory (since the data are so complex)' (vii). This is laudable, but it means that PE is in danger of falling between several stools from the outset.

Various details give PE the flavour of an introductory textbook. In places, Hammond explicitly states that he has rejected his own recent analyses in favour of proposals which are 'the least controversial' (269). There are many 'summary' sections in the text and pointers to 'further reading' clearly aimed at beginners. The paucity of references at the book's end (not quite four full pages) further add to the introductory effect.

It is natural to ask how comprehensive PE's coverage of English phonology is. I have already made a point of noting the book's subtitle, and Hammond is frank from the outset that PE only deals with 'distributional regularities in monomorphemic English words' (vii) – although the occasional polymorphemic word slips in, e.g. *who'd* (10), *texts* (37) and *sheds* (63). PE deals with prosody, the 'allowable configurations of consonants, vowels, and phonetic prominence' (vii), that is, with phonotactics and stress assignment.

Given the above, one minor but obvious criticism is that PE does not clearly fit into the series in which it is published. The series foreword claims that each volume 'will offer an extensive treatment of the phonology of one language' (ii) and 'will provide comprehensive references to recent and more classical studies of the language' (ii). PE does not fit well with this description. Its at-times introductory nature and its empirical restriction mean that it is a very different kind of book to, for example, Wiese (1996), which provides an all-inclusive approach to the phonology of German. This criticism would not be serious for a monograph on English prosody and, despite the circumstances of its publication, if PE stands up to inspection on its own criteria, that would be justification enough for its approach. The omissions are sometimes glaring, however, and we shall return to them briefly below.

In what follows I first give a brief discussion of the contents of PE and then turn to other issues relevant to the book. One feature of PE is that ideas which are introduced in early chapters are frequently revised later, often in the light of new data or analyses. I thus endeavour to present the final form of an argument; this is not always easy, however, as there are a few occasions where analyses introduced in early chapters would be affected by points made later, but this is not noted by Hammond in the text.

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The contents of PE can be divided in two ways. Certain chapters are chiefly introductory (1, 2 and 5) while others are chiefly analytic (3, 4, 6, 7 and 8); on the other hand, certain chapters deal with phonotactics (3 and 4) and others deal with stress (6, 7 and 8). A final chapter briefly summarizes the key points made and addresses some open empirical and theoretical issues. The book finishes with references, a short subject index and a large word index.

Chapters 1 and 2 include a very brief discussion of the sounds of English and of distinctive features, an introduction to the main characteristics of OT, an introduction to phonotactics, a good overview of various evidence for the syllable and an introduction to moraic theory.

In chapters 3 and 4, Hammond presents copious and extensive tables to exemplify the distributional possibilities of English segments, and develops an OT-based account for these regularities. The approach to the data here is impressively thorough and a wide range of intricate generalisations are brought to light. A few of these are not quite true, however. For example, on pages 118 and 141, Hammond claims that [eps] is an impossible final sequence (where [e] is a tense/long vowel), but this misses *traipse* (presumably [treps] for Hammond) and the tables on page 79 indicate that [dw] and [nr] are non-occurring medial clusters, ignoring *Edward* and *Henry* respectively (PE makes great use of names as data).

Hammond's account of phonotactics develops the approach adopted by Prince & Smolensky (1993) for languages with simple onsets. PE extends this basic approach to account for the more complex clusters of English using a range of constraints. These either forbid configurations of segments in certain syllabic positions (e.g. *ONSET/ŋ forbids [ŋ] in onsets), or in linear sequence (e.g. *[sr], with other constraints, forces underlying /sr/ to surface as [šr]).

The constraints conspire to force a quite intricate pattern of syllable structure. The analysis is especially complex for intervocalic consonants, involving ambisyllabicity for single consonants after lax vowels and various patterns of affiliation to the left or right for intervocalic clusters, partly forced by MAX-CODA 'affiliate as many consonants to the left as possible when there is more than one' (134). This interacts with a family of constraints which require a specific number of moras to be assigned to various types of segments, a general constraint on the number of moras allowed in a syllable ('Trimoraic maximum (3μ) – syllables can contain no more than three moras' (136)) and a large family of constraints which instantiate the sonority hierarchy; together they capture a wide range of generalisations as to what is a possible word in English. There is also a special stipulation to account for sonority-violating sC clusters: 'Meta-constraint for [s] – constraints involving [s] are not subject to derived ranking' (98), where 'derived ranking' is a device introduced as part of the formalization of the sonority hierarchy to predict possible clusters (and the ranking of the constraints which allow them) from the sonority of the clusters' constituent parts.

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The intricacy of the account can be seen in the following: Hammond remarks that only coronal consonants can follow [aw] and [ɔy], absolutely when word final and with certain caveats word-medially. He accounts for this with a constraint which requires three moras to be assigned to the two diphthongs, one constraint which assigns one mora to coronal coda consonants and another constraint which assigns one mora to non-coronal consonants. These latter two constraints are ranked differently, so that the coronal constraint can be violated. Together with MAX-CODA and the well-known ONSET, these would force any non-occurring sequence to contain more than three moras, which is ruled out by 3μ. The precise ranking of M-PARSE 'words are pronounced' (51) makes it better not to allow any overt output for certain inputs than an output which violates higher-ranked constraints (such as 3μ).

The constraints formulated in these chapters generally seem successful in generating all and only the possible words of English (although some non-occurring sequences are claimed to be absent due to the statistical infrequency of certain segments). There are problems, however. In chapter 6, partly to account for the lack of aspiration of stops in what looks like the onset of stressless syllables (e.g. for the [t] in *vanity*), Hammond proposes the constraint NOONSET 'a stressless syllable has no onset' (226) which causes *vanity* to syllabify as [vænət.i]. However, this also causes words like *coypu* to syllabify as [kʰɔyp.u] (transcription without aspiration of [p] from page 121) which would violate 3μ and thus should not be pronounceable. This criticism could doubtless be neutralized by the invention of a new constraint or the re-ranking of old ones, but this is one reason why Hammond should have provided a final summary of all constraints used in the book and their ranking. The lack of any such summary is a distinct deficiency.

Chapter 5 introduces the notions of stress and the foot, again with a range of psycholinguistic and other evidence. Chapters 6, 7 and 8 provide a great deal of detail regarding the possible stress patterns for English mono-morphemic words, and address the difference between the stress patterns found in nouns and those found in verbs and adjectives, the distribution of schwa ('all schwas are derived and nonmoraic' (206)), the distribution of full vowels in stressless syllables, and these chapters provide PE's third and final analysis of aspiration. They also illustrate the necessity of ambisyllabicity for Hammond's analysis and the claim that ambisyllabicity is in fact covert gemination as 'phonological gemination need not be mirrored with phonetic length' (218). It is unclear how this fits in with the derived gemination in words such as *unnecessary* (with [n:]); of course, Hammond does not discuss this because PE only deals with monomorphemic words.

For the analysis presented in PE to work, it is also necessary for Hammond to make the following assumptions: verbs such as *scavenge*, *balance* and *harvest* are 'examples of true nouns' (252) and adjectives such as *frequent*, *brilliant* and *honest* are morphologically complex (with the

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suffixes [-ənt] and [-əst] as bound lexical morphemes of the 'cranberry' sort – this is because of the consonant clusters in their unstressed final syllable). To account for the difference in patterning of nouns compared to verbs and adjectives, Hammond assumes the mechanism of 'catalexis', i.e. a final 'invisible or catalectic syllable' (277) which is suffixed to all otherwise monomorphemic verbs and adjectives. Hammond provides some interesting evidence for this analysis and shows how it fits with the constraint ranking developed for nouns, but he does not discuss how such 'invisible' elements can be implemented in surface-oriented OT or how faithfulness to this emptiness is enforced.

Hammond is at pains, in places, to claim that the 'phonotactic' and 'stress' parts of PE are intertwined, asserting that 'stress is clearly a partial function of syllable structure' and that 'syllable structure is also a function of stress' (332), all of which seems like a paradox for rule-based analyses. However, it is unfortunate that Hammond does not clearly spell out exactly how the two parts fit together in his OT-based analysis.

One way in which the two interact is through the family of 'WSP' constraints which formalize the 'weight-to-stress' principle. When ranked high enough, these determine that a syllable which has more than one mora must be stressed. Hammond writes that 'the WSP must be cast in terms of input vowel quantities…[and]…must thus be conceived as a correspondence-theoretic constraint' (270). This is necessary to ensure that words like *minnow* do not have final stress: they are stored underlyingly with a final lax (monomoraic) vowel, but are forced to surface with final tense (polymoraic) vowels because of high-ranking BIMORAICITY 'all syllables are at least bimoraic' (207). However, this analysis seems unavoidably to conflict with a proposal developed in chapter 4 which assigns different numbers of moras to different types of segment. As we saw above, this is enforced by ranked violable constraints (a 'mora assignment schema', which is 'a constraint family assigning moras to peaks and codas' (206)).

This seems to result in a derivational paradox. Moras have to be underlying to determine stress correctly (through the 'WSP') but they have to be assigned by GEN during the input-output mapping to account for phonotactics (through the 'mora assignment schema'). Hammond does not show how this can be reconciled with his monostratal OT.

The above discussion will have shown that PE is tightly focused on certain aspects of English phonology and of OT. It is worth briefly considering what it thus does not discuss. These omissions fall into two categories: English-specific and OT-specific. I deal with these in turn below.

Any book with the title '*the phonology of English*' cannot entirely avoid the long shadow of Chomsky & Halle (1968). It is noticeable that Hammond does not provide analyses for such well-known phenomena as Vowel Shift or Velar Softening, and his frank explanation for this is interesting. There has long been debate as to whether such alternations should be treated as part of

synchronic phonology at all and Hammond writes that such things are 'not so readily or so obviously best treated in terms of' OT (vii). It is not quite clear how we should understand this, however: if such aspects of morphophonology are not readily treatable in OT, then is Hammond claiming (i) that OT can only be used to explain certain aspects of phonology, or (ii) that morphophonological generalizations are not part of phonology at all? Hammond does not engage with this question.

With very few exceptions, PE only deals with Hammond's own accent, basically General American. It is a shame that no attempt is made to deal with anything else, especially given that English has several well described 'standard' accents. This exclusively American bias will limit PE's usefulness as an introductory text outside of America, and, coupled with the fact that PE hardly touches on segmental phonology, it means that the book misses out on a lot of important empirical and theoretical issues. If it were not so restricted, a volume on the phonology of English could have included discussion of, for example, flapping and glottaling, various types of assimilation, the allophony of clear and dark 'l', the 'Scottish Vowel-Length Rule', or æ-tensing. Some important recent discussion has emerged from the consideration of these aspects of English phonology (see, for example, Giegerich 1992 and Harris 1994), and it is difficult not to feel that an opportunity has been missed here for further discussion.

PE's restrictions cause it to miss some important debate in OT. Because there is no mention of final 'r' deletion, intrusion and linking (which is common in many accents of English, including Eastern Massachusetts and many British accents) Hammond cannot address the considerable discussion that this has provoked in the OT literature, thanks largely to the treatment in McCarthy (1993). Also, PE hardly touches on such interesting and contentious notions as the 'richness of the base' and 'lexicon optimisation', 'the emergence of the unmarked' or 'prosodic morphology'. A book like PE cannot be expected to include all of these, but, given that one of PE's stated aims is to introduce OT to students, we might reasonably query whether it will prepare students to read other OT literature. These ideas are all well discussed in Kager (1999), as are the notions of constraint conjunction and output-output constraints, which Hammond fleetingly introduces in chapter 8.

PE explicitly rejects the common OT notion that constraints are an innate and universal part of UG. This seems justifiable for many reasons, given evolutionary plausibility, the non-general nature of many of the constraints used in PE and most of the OT literature, as well as the 'phonetic grounding' approach to constraint justification (which is widely adopted in the literature and which defends individual constraints on the grounds that they reflect physiological or acoustic universality – but if these can be abstracted from physics, they do not require a source in a mentalistic and modular Universal Grammar). Hammond proposes that general constraint schemata might

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replace UG-innateness as a restriction on the theory, but it does not seem obvious that all the constraints used in PE can be interpreted along these lines.

There are not many misprints or formal infelicities which would distract the reader, although at times technical notions are used as if they should be understood but are only later explained in an introductory way. One further niggling point is that Hammond ignores other accounts for phenomena, some of which are well known and much discussed. For example, non-initial restrictions on the occurrence of [ŋ] are accounted for in PE by stipulating that [ŋ] receives two moras, ignoring the proposal that this and various other observations could be accounted for if surface [ŋ] derives from underlying /ng/. The problem is not that this alternative is necessarily the right analysis, but rather that we might expect Hammond to discuss it (various such alternatives, for example concerning sC clusters and aspiration, are aired in Wiese (1996) and Honeybone (to appear)).

In conclusion, PE is a handy source of reference for the phonotactics and stress patterns of General American. It will be of interest to phonologists who work with such data, particularly those who deal with OT. However, the empirical and theoretical problems noted above are not all trivial and may well restrict the volume's impact. PE could only be used as a text for a course on English phonology or OT if distinctly supplemented by other texts.

It would probably be impossible to cover every aspect of English phonology to the satisfaction of all. Probably quite reasonably, Hammond hasn't tried.

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(Received 1 September 1999)

James R. Hurford, Michael Studdert-Kennedy & Chris Knight (eds.),
Approaches to the evolution of language. Cambridge: Cambridge University
Press, 1998. Pp. ix+442.

Reviewed by KATHLEEN GIBSON, University of Texas Houston

In 1996, a major Edinburgh conference explored continuities and discon-
tinuities in the evolution of human language from the behaviors of non-
human primates. This volume, which is an outgrowth of that conference,
greatly advances our understanding of this issue.

Somewhat paradoxically, the volume (as the conference that preceded it)
opens with a paper by Jean Aitchison pleading for discontinuing continuity-
discontinuity debates. Most volume contributors ignore her plea. Col-
lectively, papers by Ulbaek, Donald, Dunbar, Warden, and Batali present
strong arguments that cognitive precursors of linguistic functions are present
in non-human primate thought processes, a view that has been expressed
previously (see, for example, Gibson 1991). Dunbar, Worden, and Batali
expand this theme by suggesting that the mental processes needed for
language are precisely those processes exhibited by non-human primate
social behaviors that require some understanding of the mental processes of
others such as deception and reciprocal altruism. Donald also delineates the
linguistic importance of an additional component of social intelligence –
namely, imitative skills.

Taken as a group, these papers convincingly argue that the emergence of
full human linguistic capacity demanded a prior or concomitant evolution of
social intelligence. That imitative skills and the ability to comprehend the
thoughts of others are necessary conditions for the emergence of all forms of
modern language seems self-evident. Yet, these points have often been
ignored by previous language origins theorists. The capacities to imitate and
to understand the thoughts of others are, of course, also essential for many
non-linguistic behaviors including the teaching and learning of crafts, tool-
making, song and dance. That language depends upon general intellectual
capacities, such as these, has often been overlooked or even denied by those
who would propose that language arose from a single genetic mutation or
that there are special, genetic modules dedicated only to language functions.
Hence, the emphasis on theory of mind and imitation is a welcome and major
contribution of this volume.

Ulbaek expands the theme of cognitive precursors of language to claim
that precursors of language will not be found in the communicative systems
of non-human primates, but only in their cognitive systems. Similarly,
Aitchison (like Gibson 1991) concludes that the greatest primate/human
discontinuities exist in the realm of vocalization. Major strengths of this
volume, however, are papers by Ujhelyi, Locke, Studdert-Kennedy,
MacNeilage, Kohler, and Lindblom that demolish widely-held views of

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major gaps between human phonological capacities and the capacities of other primates. Ujhelyi notes that many primate species have 'long calls' (alarm calls, contact calls, or territorial songs) built from small, distinguishable units that can be combined in different ways. Gibbons, in particular, produce complex songs from smaller vocal units. Lindblom models the emergence of human phonology as a natural outgrowth of the anatomy of the human vocal tract. Studdert-Kennedy, MacNeilage, Locke, and Kohler propose frameworks for the evolution and ontogenetic development of syllable production from respiratory, swallowing, sucking, chewing and communicative movements such as the alternating opening and closing movements of the mandible and lips that can be found in non-human primate feeding behaviors and lipsmacking gestures. Taken as a group, these papers provide the first comprehensive models of the evolution of human phonology.

Until recently, linguists have rarely entered debates about the origins of syntactic capacities – often following Chomsky's lead in assuming that syntactic capacity is unlikely to have evolved by natural selection. Several papers in this volume demonstrate a refreshing trend away from this nihilistic position (Newmeyer, Carstairs-McCarthy, Berwick, Bickerton, Kirby, Steels, Batali). Most editors and authors, with the exception of Bickerton, successfully avoid the pitfalls of assuming that all aspects of syntax are genetically predetermined or emerged suddenly in one holistic package. Indeed, several contributors provide very sophisticated mathematical models convincingly demonstrating that there is no need to postulate specific genes for syntax. For example, Carstairs-McCarthy demonstrates that syntax and semantics may be emergent phenomena that result from complex changes in vocal tract anatomy, while Kirby, Steels and Batali posit that syntactic rules may result from linguistic processes themselves. That is, they may be adaptations to the needs of languages to propagate themselves, as opposed to genetic adaptations.

My conclusion from the sophisticated discussions of phonology and syntax presented in this volume is that Aitchison is correct. It is time to discontinue continuity-discontinuity discussions. Papers in this volume clearly indicate that continuity theorists have already won the debate.

Another of the volume's impressive contributions is Berwick's chapter. He demonstrates that a single principle, 'merge' coupled with hierarchical concatenation can account for grammar and for some non-linguistic capacities. His findings are significant in that they appear to verify previous works positing that it is just this capacity, hierarchical construction, that accounts for enhanced human (as compared to ape) linguistic, tool using, and social skills (Gibson 1996; Greenfield 1991). Unfortunately, he fails to acknowledge these previous efforts.

Indeed, failure to acknowledge previous efforts is a shortcoming of the volume as a whole. Aitchison, for example, credits the beginnings of the

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modern scientific focus on language origins with a paper by Pinker & Bloom (1990). In actuality, numerous conferences on language origins were held in the 1970's and 1980's, articles on this subject were appearing in *Behavioral and Brain Sciences* as least as early as 1979, and the Language Origins Society was formed in Vancouver in 1983. This lack of awareness of earlier efforts may, however, be in some respects be a positive sign. Previous efforts primarily represented the works of anthropologists and other evolutionary biologists, while the current volume clearly demonstrates that linguists have now entered this field. It is perhaps understandable that they lack awareness of efforts by scholars in other disciplines. One would hope, however, in the future the efforts of the two groups would be merged.

Anthropologists are well aware that language is not the only major human communicative channel. All cultures have well-developed rituals and ritual means of communication. Surprisingly, however, the evolution of ritual has received short-shrift in the anthropological literature. Three contributors to this volume (Donald, Power, and Knight) are to be congratulated for opening dialogue on this critical issue, and I consider this to be another major contribution of the volume. I leave it to the reader, however, to decide whether or not to accept Power's and Knight's fanciful explanation that symbolic ritual first evolved in order to allow human females to jointly engage in 'menstrual strikes' – that is, for groups of females to jointly refuse sex to all males except those who were willing to bring home meat to all members of the female group.

To the extent that language is an evolved capacity as opposed to a mere social construct, it must have emerged via natural selection. One idiosyncrasy of the current volume is the extent to which several of the authors, including Ulbaek, Knight, Power and Dessalles, subscribe to selfish-gene models of language origins. These authors note that much language consists of voluntary exchanges of accurate, helpful information. They consider this to be a major paradox, because according to selfish gene theories, liars should possess genetic advantages over truth-tellers. This apparent paradox leads to the presentation of several creative 'just so stories' (including the menstrual strike hypothesis) to account for the 'unexpected' evolution of truth-telling.

These arguments assume that language evolved among groups of genetically-unrelated individuals. Missing from these discussions are the obvious points that language is learned in families and that, throughout most of human evolution, populations were small and would have consisted primarily of related individuals. For example, a genealogical study of two Canadian Indian hunting bands found that even if one assumed that the bands were formed 150 years ago by completely unrelated individuals, the current average degree of relatedness among members of the two bands members would be at the level of third cousins (Gibson, Thames & Molohon, 1991). Under such circumstances, exchanges of information helpful for survival or reproductive success would have been advantageous to the genes

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of the truth-teller as well as to those of the information-recipients. That, in fact, human groups may well have been composed primarily of genetically related individuals throughout much of human history is also indicated by current evidence that the human species exhibits less genetic variability than other primates. Hence, no 'just so' stories are needed to explain helpful exchanges of information. Truth-telling would have been advantageous to groups of genetically-related individuals throughout human evolution.

Papers in this volume provide an in depth focus on vocal languages. Very little is said about gestural or written languages. Some authors, however, conclude that since precursors to speech can be found in the communicative and ingestive behaviors of non-human primates, there exists no need to posit a role for gesture in the evolution of language. Modern speakers, however, use gesture to amplify and clarify speech, and human infants use gesture to aid in the mastery of speech. Consequently, in my view, a full account of human language origins will eventually require that the emergence of gestural capacities and of the interactions between gesture and speech also be explained.

Over the last twenty years or so, many language origins conferences have been held, and many just-so stories of language evolution have been proposed. Often I have doubted whether any real progress could ever be expected in this area. Despite a few short-comings, this volume has removed those doubts. Many of the volume's papers should be considered 'must reads' for all language origin theorists, and the volume as a whole represents a major advance.

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Claire Lefebvre, *Creole genesis and the acquisition of grammar* (Cambridge Studies in Linguistics **88**). Cambridge: Cambridge University Press, 1999. Pp. xviii + 461.

Reviewed by INGO PLAG, University of Hannover

This book deals with one of the central problems in the field of creole studies, the nature of the linguistic processes that are involved in creole genesis and that are ultimately reponsible for the grammatical properties of these languages.[1] Three main positions characterize this debate. The first, so-called superstratist, position attributes creole properties mainly to the lexifier language. The substratist view, on the other hand, maintains that creole structures are primarily the result of transfer from the native language(s) of the underprivileged groups involved in the contact, for example African languages in the case of the Caribbean creoles. A third camp of scholars holds universal tendencies of language development and acquisition (some call it UG) responsible for creole genesis. Lefebvre is a well-known proponent of the substratist position and the book under review further substantiates arguments and evidence for the important role of the substrate, without denying other influences. Her book will certainly provoke responses by people of all theoretical persuasions, who will feel challenged by the strength of Lefebvre's hypotheses and by some of the weaker points in her study (to which I will turn towards the end of this review).

Creole genesis and the acquisition of grammar is a substantial and welcome contribution to the above-mentioned debate in several respects. It summarizes twenty years of research by the author and her associates on French-based Haitian Creole, impressing the reader by both its breadth and its depth. Furthermore, it is the first study in the field that gives a comprehensive and systematic overview of major areas of grammar across the main languages involved in one specific contact situation, i.e. the creole itself, the superstrate French and one of its major substrate languages, Fongbe.

Lefebvre applies a very strict methodology, making the work an example of explicitness in terms of assumptions and procedures, and certainly setting standards for other studies of this kind. Due to its breadth of coverage it transcends the common practice of picking out an isolated area of grammar, showing a similarity between creole and substrate in this particular area and claiming to have found evidence for substratal transfer.

Because of the wealth of data and constructions presented, the book may also serve as a reference for a more general readership that is simply interested in the properties of Haitian grammar. The text is highly accessible owing to Lefebvre's clear style and because it is relatively free of unnecessary

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[1] I am grateful to Claire Lefebvre for comments and discussion.

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technical jargon. Some basic knowledge of Principles and Parameters theory is, however, required for a full understanding.

The central aim of *Creole genesis and the acquisition of grammar* is to test the so-called relexification hypothesis. Under this hypothesis, creole grammar and lexicon emerge – roughly – in the following way. Substrate speakers take a lexical entry of their mother tongue, copy it, and replace its phonological representation by the phonological representation of a semantically related superstrate item (a process called 'relabeling'). In this process, the superstrate phonological representations undergo considerable restructuring, the details of which are outside the scope of the book. The relexification hypothesis thus predicts that a given creole word should have the syntactic and semantic properties of its substrate equivalent but should be phonologically related to some superstrate lexical item. The process of relexification (or 'calquing') is also well-known from studies of second language acquisition, and Lefebvre's hypothesis entails the view that creole genesis is a matter of second language acquisition, challenging views by Bickerton and many others that creoles emerge through processes of first language acquisition. Besides relexification, Lefebvre acknowledges two other major linguistic processes participating in creole genesis, dialect leveling and reanalysis.

The book is effectively organized into fourteen chapters, three appendices and three indices. Chapter 1 (pp. 1–14) introduces the main aims and background of the study, chapter 2 outlines in more detail the three processes which, according to the author, are of major importance in creole genesis: relexification, dialect leveling and reanalysis. Chapter 3 then lays out the research methodology. In this chapter, the reader is informed about the socio-historical and demographic background, which is necessary for the understanding of the non-linguistic factors that must have played a role in the formative period of Haitian in the early days of the colony (1659–1740). This is followed by a typological characterization of the substrate languages pertinent in the formation period, and a determination of the kind of French the creators of Haitian were presumably exposed to.

The following eight chapters (4–11) deal with a wide range of grammatical phenomena (functional categories in NPs, tense, mood and aspect markers, pronouns, functional categories in clauses, clausal determiners, the syntax of verbs, derivational affixes and compounds). Each phenomenon is presented to the reader with data from Haitian, Fongbe and French, accompanied by glosses and translations. The three languages are systematically compared in order to detect similarities and differences, with the basic argument running as follows: the relexification hypothesis predicts that the lexical and grammatical properties of Haitian should parallel those of Fongbe and should differ from those of French. Thus, the hypothesis can be falsified by finding structures in which Haitian and Fongbe do not pattern alike. As it turns out, such cases are in the clear minority and where they occur Lefebvre can offer accounts along the lines of dialect leveling or reanalysis. Only a

small portion of phenomena are left more or less unexplained as independent innovations. The discussion of the data may sometimes appear to be a bit one-sided or ad hoc, but in general the argumentation is well-balanced and convincing. The results are summarized in chapter 13, which culminates in the conclusion that 'Haitian was created by adult native speakers in possession of mature lexicons and grammars' (394).

Overall, the book succeeds very well in showing the importance of substrate influence and, what is more important, in defining its role more precisely. There are simply too many phenomena of Haitian grammar whose close resemblance to Fongbe grammar can hardly be accidental. Both amount and quality of the accumulated evidence are devastating for superstratist accounts of the genesis of Haitian and present a serious challenge to universalists. I look forward to seeing alternative accounts of the many grammatical properties so thoroughly discussed in this book.

However, even for those who sympathize with the idea that creole genesis is primarily a second language acquisition phenomenon, the particular model espoused by Lefebvre, i.e. relexification, involves a number of problems that I feel are not sufficiently addressed in her book. These problems concern (1) the selection of the superstrate element for relabeling, (2) the relabeling of function words by zero, and (3) the role of word order in relexification. I will discuss each in turn.

In order to relabel a native lexical item with a phonological representation of the superstrate, a particular superstrate item has to be picked out. According to Lefebvre, this selection process is determined by three factors, the most important of which is semantic overlap between the two entries involved (this holds for content words). The other two factors come into play when substrate functional items are relabeled with phonological represent-ations taken from superstrate content words. Here, distributional properties and phonological similarity also play a role. While these criteria look rather straightforward, there are cases where the analyst runs into problems.

For example, Lefebvre argues that the Haitian irrealis marker *pou* is derived from French *pour*, which is a plausible analysis. The reason for choosing *pour* to relabel the Fongbe marker irrealis marker *ní* is that *pour* was 'the closest parallel that the relexifiers found' (119). Fair enough, but why didn't the relexifiers choose a zero form for relabeling? This possibility is explicitly allowed by Lefebvre in those cases where French 'did not present any appropriate form to relabel that lexical entry' (165, see also 37 f., 164). *Pour* as in the sentence presented as key evidence (*Jean est pour partir* 'John is about to go') doesn't strike me as particularly 'appropriate' and would therefore be a good candidate for relabeling with zero. Relabeling by zero is argued to have happened for instance with the Fongbe reflexive expression *ɖéè*, for which no appropriate French form could be found (163–167). The exact reasons for these particular choices remain however obscure. The reasons Lefebvre presents may appear convincing or not; my point is that

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one would like to know which kinds of reasons are good reasons, which kinds of reasons would be bad reasons. In other words, which principled properties of the respective items involved are responsible for the fact that *pour* is appropriate for relabeling an irrealis marker, but *même* is not appropriate for relabeling a reflexive form? When do creole creators find an item 'appropriate', or similar enough, for relabeling? This is not a trivial question because the selection of an item is an integral part of the relexification process, so the model should be more explicit about how this proceeds. Perhaps it is not possible at this stage of our knowledge to spell out these criteria in more detail, but if so, this should be more openly discussed.

The point just mentioned is closely connected to the second problem I want to briefly address. It is argued in chapter 2.5.2 that only those functional category items of the substrate that have some semantic content can be relabeled by major category items taken from the superstrate. 'Functional category lexical entries without semantic content are assigned a null form at relabelling' (37). Why should this be so? What exactly is meant by 'without semantic content'? Why do creole creators not simply abandon functional items, i.e. replace them by nothing instead of zero? Sometimes this is just what happened with some Fongbe items, for example, the negative marker *a* (213–217), or the postposition *wɛ* (199–123), which simply disappeared altogether. On the basis of which factors or criteria did the creators of Haitian make their choice? Again, Lefebvre's post factum generalizations are in order, but I don't see why these, and not other, generalizations should emerge. Perhaps this question is unanswerable yet, but I would have liked to see it spelled out more clearly in the book.

My final problem with the relexification model concerns the puzzling word order facts of Haitian. As shown by Lefebvre, Haitian word order is partly modeled on French (with regard to lexical heads), partly modeled on Fongbe (with regard to functional heads) and partly inexplicable (with regard to morphological heads). Lefebvre tries to explain superstrate word order in lexical phrases by stating that 'because [the relexifiers] identify major category lexical items of the superstrate language, they acquire the directionality properties of the superstratum major lexical category items' (39, see also 388). The creole creators 'keep the original directionality properties' (388) of the substrate functional items, because they 'do not have enough exposure to the superstratum language' (388). These explanations raise some problems, however.

First, why should the relexifiers preserve only the phonological and directionality properties of the superstrate lexical category item at relabeling but abandon all other syntactic and semantic information? I see no a priori reason for this kind of selectivity. Second, why do they preserve substrate word order only in functional phrases and not also in lexical ones? If we assume with Lefebvre that the creole creators had no access to certain types of information due to insufficient exposure to the superstratum language, the

question arises as to what kinds of information speakers have access and to what kinds of information they don't. Obviously, they must have discerned and made use of quite a lot of linguistic properties that are not salient on the surface. For example, all arguments for the selection of a certain functional item for relabeling imply that the creators of Haitian had access to surprisingly detailed distributional and semantic properties of the pertinent superstrate item. This makes me suspicious whether it is access or rather processing and selection that is at issue here.

I should add that the problems I have pointed out above may be hard or even impossible to solve at this stage of our knowledge. However, I would have liked to see these problems more openly discussed in the book. *Creole genesis and the acquisition of grammar* is nevertheless an outstanding achievement. Lefebvre's claims are spelled out in a way that allows falsification, provokes further discussion and opens up new and promising research perspectives. Thus, even those who remain unconvinced would be ill advised to ignore this book in their future work.

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Christopher Lyons, *Definiteness.* Cambridge: Cambridge University Press, 1999. Pp. xx + 380.

Reviewed by ANDREW SPENCER, University of Essex

The blurb describes Lyons' book as a textbook, but that is misleading.[1] Although it would make an excellent source text for an advanced undergraduate or postgraduate seminar course, it is an equally excellent introduction to and survey of the field for other researchers, as well as containing the most accessible account of Lyons' own very interesting ideas on the subject. Lyons is well known as a Romance scholar and Hispanist, but this book demonstrates a very wide typological scope as well as including some very interesting original thoughts on, amongst other things, the history of German and on the structure of possessive phrases in Semitic (an indication of the typologically informed breadth of the book is that there is a separate four-page languages index). Moreover, this is a wide-ranging work in its theoretical coverage, tackling a whole host of important issues in

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[1] I am grateful to Chris Lyons for comments on an earlier draft.

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morphosyntax, semantico-pragmatics and diachronic linguistics. Any student reading the book will find that it serves as useful revision for half a dozen other courses, and seasoned researchers will find conceptual links and thought-provoking ideas throughout.

The book consists of nine chapters, beginning with a general overview of the phenomena, which also gently introduces the most important theoretical concepts connected with definiteness. Chapters two and three offer typological surveys of simple (in)definites, expressed by some sort of article and complex (in)definites, expressed by other means. Chapter four discusses the semantic basis, covering questions such as whether definite nominals denote entities that can be identified by the speaker/hearer (identifiability) or whether they denote the totality of the entities possible (inclusiveness, in the limit, uniqueness). This chapter also includes a summary of Lyons' arguments for a three way contrast between count, mass and proper nouns. Chapter five examines the way definiteness interacts with case marking, agreement and so on, while chapter six surveys so called 'definiteness effects', for example in existential *there*-clauses.

These first six chapters mainly survey current research findings. In the last three chapters we see more of Lyons' own viewpoint emerging, as he presents us with a distillation of his research over the past twenty odd years. In chapter seven Lyons presents us with a summation in the form of an attempted definition of definiteness, emphasising the distinction between definiteness as a semantic/pragmatic concept (common to all languages) and definiteness as a language-particular grammatical feature. This chapter plays out in more detail the tension between accounts in terms of identifiability and inclusivity, and the discussion ranges across such topics as generalized quantifiers, Discourse Representation Theory and Relevance Theory. Chapter eight summarizes recent syntactic approaches to definiteness marking in the wake of the DP hypothesis. This chapter defends Lyons' provocative view that it is only definite nominals that project a D functional head, and that 'DP' therefore means 'Definiteness Phrase'. This is an interesting idea that has considerable repercussions for syntactic theory. Chapter nine concludes with a survey of the way definite articles develop, arguing for a 'definite article cycle' in which the articles develop, for example, from demonstratives and then weaken to mere noun class markers while new demonstratives weaken again to articles.

A popular problem area for discussion of definiteness has been the semantic interpretation of definite descriptions. Two basic positions can be isolated. In one account, definite descriptions denote entities which the hearer is expected to be able to identify in the context. Thus, in a room with three windows of which one is open a speaker can say 'Close the window' and expect the hearer to successfully identify the window referred to. This is the identifiability analysis. On the other hand, there are contexts in which identifiability seems to give the wrong answer and the definite description

seems to invite the conclusion that there is some unique object satisfying the definite description. This is found in so-called associative uses, as when a theatre nurse says 'I wonder who the anaesthetist is today'. The point here is that the speaker presupposes that there is a unique anaesthetist, whose uniqueness is derived from association with a highly specific context of utterance. This is the inclusiveness analysis: the denotation of the definite description includes all possible referents in that context. The problem is that both explanations seem to be required and yet they are incompatible.
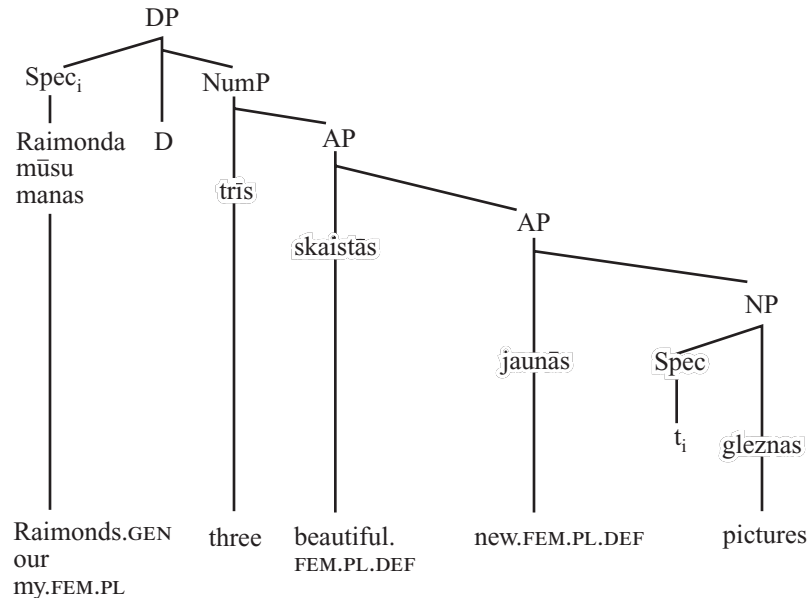
Lyons offers a way off the horns of this dilemma. He makes what ought to be an obvious point about features, namely, that we have to distinguish semantic features (or meanings) from purely grammatical features, which may be grounded in semantics but which generally have a rather complex relationship to semantics. Thus, definiteness is a grammatical feature found only in languages with definiteness markers, such as definite articles. On the other hand, all languages have semantically definite nominals which may or may not interact with other aspects of grammar (for instance, subject or topic nominals in many languages are obligatorily interpreted as definite, whether or not definiteness is expressed as a grammatical category). Lyons then argues that the original semantic grounding is identifiability, but that semantic shift then gives rise to an additional inclusiveness interpretation. Given that there is some leeway in how [+def] is interpreted it isn't surprising that languages can make different choices about definiteness marking with certain types of nominal. Thus, in some languages proper nouns have to be definite, in others they can't (normally) be, and in some languages generics are given definiteness marking and in others they are left bare. One consequence of taking definiteness as a grammatical feature is that demonstratives are not marked with the [+def] feature but rather bear the feature [+dem]. However, the [+dem] feature is interpreted as a kind of deictic identification, which triggers the identifiability, and hence the semantico-pragmatic effects of the [+def] feature, giving the illusion that demonstratives are definite.

Several of Lyons' proposals raise a variety of interesting questions which ought to stimulate further research effort. For instance, he provides a raft of arguments against treating indefiniteness as simply the negative value of a binary [def] feature. Instead, he offers various reasons from synchronic grammar and from typology for believing that indefiniteness markers are heads or specifiers of a special Cardinality projection. This raises the question of what kind of a feature [def] is: the obvious conclusion is that it is a unary property. This is important for the general architecture, in that it makes it even more difficult to distinguish between the notion of a syntactic head in Principles and Parameters and that of a feature on a head (always an area of unclarity in the theory). In effect, 'D' is identical to the unary feature [def]. This in turn can be viewed as strengthening Lyons' claim that definiteness is simply what you get when you project a DP, but it means that there is a

greater burden on the syntactician to provide evidence for a projection as such in language which express definiteness morphologically.

The obvious way to incorporate definite articles into a grammar is to take them to be functional heads, projecting their own DP. However, Lyons argues that this is only true of affixal articles. Free form articles occur in the specifier position of the DP. This allows him to analyse double determination in languages like Swedish. In a phrase such as *den långan resa-n* 'the long journey-DEF' the free article *den* is a specifier while the affix article -*n* is the D head into which the noun *resa* 'journey' moves. This analysis is linked ingeniously to the analysis of possessive constructions. Lyons proposes a distinction between 'DG' and 'AG' constructions (mnemonically 'determiner-genitive' vs. 'adjectival genitive'). In DG possessives the possessor 'ousts' the definite article and appears at the left edge of the phrase. Thus, we have 'my beautiful pictures' not '*the my beautiful pictures' or '*beautiful my pictures'. In AG possessives the possessor generally has the form and syntactic positioning of an adjective and can be preceded by an article (definite or indefinite) as in Italian *i miei bei quadri*, lit. 'the my.MASC.PL beautiful.MASC.PL. pictures'. Lyons assumes that all possessor phrases originate in [Spec,NP] position and then move leftward. The DG constructions involve 'full possessor movement' all the way to the left and this entails projection of the [Spec,DP] position as a landing site. This in turn entails that the whole phrase is definite, of course (non-definite phrases lack the D projection altogether). In AG constructions movement is partial and therefore doesn't necessitate projection of D.

There are interesting cases which are intermediate between Italian AG and English DG constructions. A case in point is that of Latvian. As Lyons explains (84) adjectives in Latvian have a non-definite and definite declension. In a nominal phrase consisting of just noun and adjective it is only the adjective that tells us we have a definite phrase. The natural assumption is that the adjective moves into a D position to check its [+def] feature. However, matters are complicated by the fact that where we have a string of adjectives all of them are marked for definiteness, so it looks as though definiteness is an agreement feature as well. Similarly, in the presence of a possessor or demonstrative as well as certain other definiteness contexts, all adjectives have to be marked [+def], (apparently agreeing with the possessor/demonstrative etc., though this is not how things would be viewed in Lyons' system, of course).

DP

Spec$_i$ NumP

Raimonda   D   AP
mūsu
manas   trīs   AP

skaistās

AP

NP

jaunās   Spec

t$_i$ gleznas

Raimonds.GEN   three   beautiful.   new.FEM.PL.DEF   pictures
our   FEM.PL.DEF
my.FEM.PL

'Raymond's/our/my three beautiful new pictures'

The version with *my* looks very much like an AG construction in the sense that 1sg., 2sg. and reflexive possessives take adjective-type agreement with the possessed. However, 3rd person and 1st, 2pl. pronominal possessors and all full nominal possessors take the genitive case form and thus resemble DG constructions. The adjectives bear definiteness inflection but they cannot be associated with the D head, it would seem. This suggests that they are simply agreement forms. But we know that a simple Adj + Noun phrase is rendered definite solely by the definiteness marking on the adjective. This suggests that there has to be an obligatorily empty definite article in the D position in Latvian. But this is a rather artificial solution and fails to link definiteness to word order variation, which is otherwise the only real justification for positing a projecting head rather than just a feature. Similar problems will arise in any language in which adjectives take definiteness markers in agreement with definite nouns, for instance, Semitic. It is not clear how such languages are to be treated in Lyons' system.

Lyons also turns his attention to pronouns and to the long-standing debate as to their relation to definite articles. He unequivocally equates these, so that DP is reinterpreted as 'person phrase'. The crucial data here are expressions of the form *we linguists*. Here the pronoun is a personal determiner which behaves like a definite article marked for 1st person plural. Elsewhere pronouns are analysed as definite articles which lack a noun complement. Free form unstressed ('weak') pronouns appear in [Spec,DP] position with

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an empty D head, while clitic/affix pronouns are D heads into which 'host' nouns incorporate. ('Strong' pronouns, often used in emphatic contexts, are person-marked demonstratives in [Spec,DP] position.)

Lyons adduces two sets of arguments for the crucial conflation of personal determiners and personal pronouns:

 (i) pronouns are incompatible with indefiniteness;
(ii) person and definiteness show complementary distribution: person is marked typically on verbs (as agreement), while definiteness is typically marked on nouns.

An intriguing consequence of this analysis is that only definite 3rd person nominal phrases are personal (since person and definiteness are the same thing).

Lyons points out that the second argument is somewhat weakened by the fact that many languages do express definiteness on verbs, as when Hungarian verbs agree with definite but not indefinite objects. He also discusses very interesting constructions in which phrases of the form 'we men' exhibit person marking on the noun, doubling the overt pronoun in non-third person forms, as in Nama (Nahuatl and Armenian are similar, 143; Chukchee and Koryak could also be added to the list). It would be interesting to see how widespread this phenomenon is in those languages which have predicative agreement morphology such as Samoyedic, as well as Salishan languages which have definite articles and in which nouns regularly inflect like verbs.

I have only touched on a handful of the issues which Lyons raises in his thought-provoking and informative book. This exhaustive but highly accessible survey will serve linguists of all theoretical persuasions, including typologists, field workers and descriptive grammarians, as an excellent introduction to the issues as well as a very useful reference work, and it is bound to stimulate much interesting discussion.

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Rebecca Posner, *Linguistic change in French.* Oxford: Clarendon Press, 1997. Pp. xxi + 509.

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More than just another history of the French language, the volume under review presents a survey of theoretical developments relevant to diachrony in

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general. The copious data found herein represent the depth and mastery of a scholar of long standing in Romance linguistics and are offered to illustrate a certain view of language history and language change. Thus, there are two parallel themes of presentation. The first is the wealth of progress of linguistic knowledge which has taken place, most of it within the lifetime of the author. From the overview she gives of this progress we quickly discern a steady determination to keep abreast of linguistics through the years. The second theme is to recast much of what we know of the history of French, but no small amount have I come to learn only for the first time, within the context of the first theme of theoretical development. For this reason, the presentation is both erudite and relevant.

There is a further theme, however, which is evident throughout the exposition and this involves a distinction between two issues already mentioned above, viz. language change and linguistic change, variously compared to Saussure's distinction between external and internal history or Malkiel's distinction between the history of a language and historical linguistics. The first member of each pair 'is concerned with social and political influences on the language – contact, conquest, control, etc.' (3–4). The second member of each pair, on the other hand, deals

> with the **natural** attrition of a language system, by virtue of its use in discourse, and the **adaptive** processes by which the language reforms its system, once disrupted. The former can look like any other sort of history – of events, of technology or of ideas – while the latter is the true domain of the linguist, requiring a specialist interest in what language is, and how it functions (4).

After a brief Introduction (1–6), the book is divided into two parts. Part I involves an essay on language change and is divided into two chapters. The first chapter is enlisted for the purpose of 'Defining the domain' (11–55). The second is a discussion of the 'Sociolinguistic history of French' (57–99) and includes among others such issues as sociolinguistic variation, causes of social change, popular French, age and regional variation, the spread of French both in France and abroad, dialect and creole. Part II, which constitutes the major part of the volume – hence the title – is devoted to 'Linguistic change' (103–416) and has individual chapters concerning 'Processes of linguistic change' (105–142), 'Lexical change' (143–183), 'Semantic change' (185–214), 'Phonological change' (216–292), 'Morphological change' (294–300) (further subdivided into 'Verb morphology' (300–324) and 'Nominal and pronominal morphology' (325–343)), and finally 'Syntactic change' (344–416). The body of the volume is followed up by some closing remarks 'In place of a conclusion' (419), a bibliography (425), a name index (489) and a subject index (499).

As mentioned above, the view of linguistic change is presented within the context of contemporary developments in linguistics. Since these de-

velopments have been so numerous and, in their turn, so productive, I can only assume that this explains the merely cursory application to the vast amount of data properly taken up in the volume. Some topics, of course, are given greater attention, such as Principles and Parameters (113–115) under which the Minimalist Programme is given scant mention to serve merely as an introduction to Henning Andersen's proposals regarding Abductive change (115–118).

Some matters are featured even more, such as the syntactic issue of pro-drop (377–80) and its relevance to changes involving the development of pronoun subjects, to relative pronouns (380–381), to the definite article (381–390) and to partitive determiners (390–394). Clitics understandably come in for fuller treatment (394–410) given the extensive attention they have attracted in recent years. Grammaticalization and morphologization are, as to be expected, taken up as closely related (207–208, 297–300) and reference is made to the functionalism they were devised to promote.

Other theoretical topics are given shorter shrift. Lexical diffusion is allotted perhaps briefer mention than it deserves (124, 222). Extrametricality is referred to several times, only in passing in a remark regarding the deletion of final consonants (257), but more at length in an interesting explanation for the dropping of final consonants in possessive adjectives (335); and as a casual explanation for the behavior of mute-*e* (266). Interrogation in general and *wh*-questions in particular are taken up in connection with pro-drop (366–368), but curiously enough no reference is made to Langacker's early generative treatments of possessives (1965) and interrogatives (1968) in French. The nature of rule-ordering is taken up briefly not so much in connection with its place within a generative grammar, very controversial in its time, but rather in the context of a discussion of the Neo-grammarian Hypothesis (223–225). This perhaps might explain why there is no reference in the bibliography to a fuller discussion of the phenomenon, viz. the 'KSN Hypothesis' (see Koutsoudas, Sanders & Noll 1974).

In connection with this issue, Posner chooses an often cited problem in the development of French and the relationship of Gallo-Romance with Western Romance in general. The following derivation will serve as an illustration:

		kuppa	sapa	ko:da
(1)	ú > ó	kóppa	——	——
(2)	voicing	——	saba	——
(3)	fricative	——	saßa	koða
(4)	fronting	——	seva	kφða
(5)	[o] > [u]	kuppa	——	——
(6)	CC > C	kupa	——	——
(7)	-a > -ë	kupë	sevë	kφë
		coupe 'cup'	*sève* 'sap'	*queue* 'tail'

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Posner rightly reports that both degemination (6) and voicing of intervocalic stops (2) are general characteristics of the Western Romance languages. She explains, however, that both processes cannot be posited for Proto-Western, as such prima facie evidence might suggest. This is clearly the case because the voicing of intervocalic stops (2) must be ordered before the simplification of geminate consonants (6) for two reasons. Firstly, if we assume the opposite ordering, degemination (6) would feed the rule for voicing (2) and we would derive such erroneous forms as *kóba > *kóßa > *køvë. As shown by these starred derivations, the opposite ordering would also lead us erroneously to posit an intermediate open syllable */kó-pa/, which would in its turn also feed the rule for the fronting of /ó/ > /ø/ in free syllables (4) (e.g. *coda > queue*). This is of course unsatisfactory, since the modern form *coupe* displays the vowel expected in closed syllables (/ó/ > /ú/ (5). Therefore, since we have /u/ in the modern form, we rightly infer that the vowel was still in a checked syllable (/kóp-pa/) at the time of the vowel change yielding the form /kúp-pa/ with subsequent degemination as shown in the illustration.

Posner proceeds to draw a more general conclusion from the above exposition. She maintains that

> this leaves us in a dilemma: the comparative 'evidence' [sic] suggests that the simplification of geminates…was an innovation shared by Western Romance languages, but the French evidence tells us that it must have occurred in French independently of the same change in, for instance, Spanish. One way out of the dilemma is to place more confidence in the rigorous methodology that establishes internal ordering, than in the postulation of the real-life existence of unified **proto-languages** like Western Romance' (224).

Conspicuous because of the lack of any reference to his work is the main proponent of the reconstruction of proto-languages in Romance. In his well known classic article, Hall (1950) addressed the very methodological issue raised by Posner. For reasons more immediately obvious than the ones presented above by Posner, he pointed out that Proto-Gallo-Romance must have continued to have geminate consonants – of necessity inherited from Proto-Western Romance – in order to account for such forms as Modern French *battre* 'to beat' and Old French *chape* 'cape', which otherwise would have resulted as something like *bettre* and *chèpe*, (cf. *mare* > *mer* 'sea', where we find the characteristic fronting of North French /a/ > /e/ in free syllables (4)).

More importantly, however, Hall insisted that geminate consonants must be posited for Proto-Western also for comparative reasons. Quite contrary to there being a lack of comparative evidence, as asserted by Posner, Hall made a point of incorporating the data from Aragonese and Bearnese

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collected by Elcock, the eminent British Romanist (1938). This evidence clearly indicated a single intervocalic voiceless stop series in contrast to what could only be a geminate series. Thus, *cuppa* in contrast to *sapa* (cf. Bearnese *sapo*) to give *coupe* 'cup' and *sève* 'sap' respectively in Modern French. Proto-Western Romance, therefore, includes not only French, Spanish, Provençal and Catalan, but all of their dialects as well. There may well be instances in which internal evidence is present where comparative evidence is of no help, but this is certainly not one of them.

Since the issue of proto-languages was taken up in the volume, it is clear in the light of the foregoing discussion that some more reflexion on the work of a well known Romanist such as Hall might well have been in order. An inclusion of his 1950 article in the bibliography would certainly have been more relevant than merely one minor proposal of his on the status of Middle French. It might also have not been unexpected to see a number of other Romanists cited in this regard. I will pause to mention only one other scholar here, however. In a review for another journal, Pulgram (1999), well known for his work in Latin and Romance linguistics, has noted the lack of any reference to his own major contribution of viewing proto-languages as essentially diasystems. Posner's rather thin treatment of proto-languages should not be taken, therefore, as being the last word on the topic.

Space restrictions make it difficult to delve into further matters in any significant way. However, there is one further remark that should be made about the volume. In one of my opening quotations, Posner professes to focus on 'the **natural** attrition of a language system, by virtue of its use in discourse…' (4). This is more a statement of conviction rather than the unfolding of an agenda. Readers thus should not be led by this to anticipate explicit links between discourse structure and linguistic change – especially of the sort found in Marchello-Nizia (1995).

Nevertheless, this is an ambitious and valuable volume. The organization of a vast body of data is in itself no minor accomplishment; and its correlation with the rapid progress of contemporary linguistic theory recommends it highly as a class manual for a course on the history of the French language or a reference work for a course on historical linguistics. I am sure to have numerous occasions when I will welcome the opportunity to consult it in my own work.

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(Received 2 August 1999)

Sue Savage-Rumbaugh, Stuart G. Shanker & Talbot J. Taylor, *Apes, language and the human mind*. Oxford: Oxford University Press, 1998. Pp. vii + 244.

Reviewed by SIMON KIRBY, University of Edinburgh

Perhaps the most serious problem for those interested in evolutionary linguistics is that, as Berwick (1998) has pointed out, human language appears to be an AUTAPOMORPHY. That is, language is a biological trait unique to humans. This makes understanding its origins (and indeed developing a truly explanatory theory of language) extremely difficult. Essentially, we lack a comparative method for Universal Grammar.

Ape language research (henceforth, ALR) promises so much for linguistics since it holds out the tantalising possibility that many of the 'interesting' aspects of human language may not be unique after all. For this reason all linguists should be excited by a book entitled 'Apes, language and the human mind'. Unfortunately for those interested in the structure of language, at least, this book will fail to live up to the interest it is sure to generate. This is not because the work that Sue Savage-Rumbaugh and her colleagues have carried out is not impressive – it is – but because the focus of the book is misdirected onto rather sterile debates about 'understanding' in apes, and often reads like an embattled attack on ALR critics (and indeed the last 200 years of Western psychology). A calmer assessment of the similarities and differences between the language behaviour of human and non-human primates would surely have won more devotees to their cause. (My words here are chosen carefully; ALR to the authors is a CAUSE. An outsider reading this book is likely to find the fervour, frustration and proselytizing apparent on both sides of the ALR debate breathtaking.)

The book is divided into four chapters primarily authored by Sue Savage-Rumbaugh, Shanker, and Taylor respectively, with the authorship returning to Savage-Rumbaugh for the concluding chapter. The rest of this review will treat each chapter in turn.

The first chapter reads like a personal introduction to Kanzi, the most

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famous bonobo that lives in Georgia State University Language Research Center in Atlanta, and perhaps the most famous non-human primate in history. What makes Kanzi special is the way he was brought up. He was brought to Atlanta when he was six months old with his adopted mother, Matata. It was Matata, rather than Kanzi, who was the focus of Savage-Rumbaugh's initial research into ape language; Kanzi was considered to be too young to undergo training to use the LEXIGRAMS that had been used to allow common chimpanzees in the research center to communicate. These lexigrams are arbitrary symbols arranged on a board that apes can point to that stand in for words such as *banana*, *look*, *goodbye* and so on. Whilst Matata's performance was disappointing, the research team were surprised to discover that after being separated from his mother at age 2 and a half, Kanzi seemed to be adept at using the lexigrams WITHOUT HAVING BEEN EXPLICITLY TAUGHT. Furthermore, and more importantly, Kanzi seems to be able to understand an impressively broad subset of spoken English.

Savage-Rumbaugh's explanation for the unique abilities of Kanzi relates to the way in which his exposure to language use was both early (presumably before some hypothetical critical period for acquisition) and, perhaps more importantly, the fact that his language learning was socially embedded. It seems fair to say that, with Kanzi, we have the first case in which an ape has been reared in a linguistic environment in a similar way to a human child. The importance of the naturalness of this rearing to ALR should not be understated, and is conveyed very effectively in this chapter through many anecdotes about living with Kanzi. Although she is likely to be criticised for the style of this part of the book, this narrative approach is important, I think, for conveying the way these apes are treated.

On the other hand, this chapter will frustrate many linguistically oriented readers for its extremely shallow treatment of the language that Kanzi understands. For example, the sentences:

(1) (a) If you don't want the juice put it back in the backpack.
    (b) Get some water, put it in your mouth.
    (c) Get the hot dogs and put them in the hot water.

are treated as evidence for understanding of the use of anaphoric pronouns, because, for example, Kanzi didn't put anything other than hot dogs in the hot water on hearing (1c). In fact, from these and other examples, a skeptical conclusion might be that only contentive elements plus basic word order are required to achieve the behaviour noted. Sentences like:

(2) Get the ball that's in the cereal.

are taken as evidence that Kanzi understands embedded sentences, but if he

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doesn't have access to functional categories, then this would reduce to the same (non recursive) structure as:

(3) Get the ball from the cereal.

Some of the explanations of the cases where Kanzi failed also seem a bit dubious. For example, Kanzi has difficulty with conjoined NPs, such as:

(4) Give me the milk and the doggie.

Savage-Rumbaugh interprets this as a memory failure, rather than a grammatical problem, since she claims that the grammatical structure of these is simpler in some way than those like (2). However, it is more plausible, that if functional elements are being ignored, these problems may be due to difficulties assigning thematic roles to the NPs in the sentence. It would have been interesting to see Kanzi's performance on sentences that involve dative alternations, for example, to understand what role if any that functional elements play in his language comprehension.

In chapter 2, the style and direction of the discussion shifts radically to a densely argued philosophical attack on Cartesianism. Here, the tone of the book changes to become overly defensive, arguing that the vocal criticism of Savage-Rumbaugh's work arises not from peculiarities of her research methodology, but from the Cartesian perspectives of western psychology. The chapter starts with a history of philosophical approaches to animal language and cognition from the 17th century forward. There is some interesting discussion of the ambiguity of Descartes' original (negative) responses to animal mentalism revolving around whether they were logical or empirical. In other words, was Descartes suggesting that we should be skeptical of animal thought as a matter of scientific prudence, or because it is correct to be? For modern bifurcationists (including nativist linguists) Shanker claims it is more like a logical claim that is being made that it is simply not possible for Kanzi to acquire the ability to be a linguistic individual. Here Shanker is reacting to critics of ALR who suggest Kanzi's communication is EFFECTIVE rather than MEANINGFUL. Unfortunately, I feel that the discussion here will be of little interest to the majority of linguists, who do not wish to be told that Kanzi should be 'the spark that is needed to ignite the paradigm revolution that will lead us beyond Cartesianism' (138), but would rather learn about the structure of Kanzi's behaviour.

Many of Shanker's criticisms of the typical ALR-skeptic's response to Kanzi are well made (especially those that use terms like 'scientific rigour'), but the book as a whole misses the mark. Language can be analysed as a system in its own right, and this neither necessarily relies on nor is exclusive of, an analysis of language users. A similar two-leveled approach would have been informative in the case of Kanzi, and would have allowed us to see past so many of the problems (like worrying about criteria for 'understanding') that Shanker highlights.

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Chapter 3 continues in a similar vein. It starts with suggesting (wrongly, I believe) that the question everyone wants to know the answer to is 'does he *really* understand what we say?' (139). Just as the previous chapter is a plea to kill off Cartesianism, this chapter is a plea to eradicate an epistemological conception of this question. That is, Taylor argues that much of the rhetorical structure of the ALR debate is founded on the assumption that the questions such as the one quoted above are 'matters of knowledge about hypothetical states of affairs' (154). Although, as an analysis of rhetoric, this chapter raises many interesting points, it is again likely to leave anyone who is actually interested in Kanzi's linguistic abilities deeply frustrated. This is unlikely to be helped by the mistaken connection that Taylor makes between parsing, principles & parameters theory, and understanding on page 170. (Essentially, he suggests that the generative view holds: to understand a sentence it must be parsed; to parse a sentence, one needs a principles & parameters-type grammar; therefore understanding relies on principles & parameters.)

Taylor points out that skepticism of animal understanding is virtually a requirement for a scientifically sound stance in ALR. However, if we were to hold the same view about other humans' understanding we would be deemed ludicrously over-skeptical. He claims that this asymmetry arises from our need to uphold our stance on the ethics of animal exploitation. To put it very crudely, we would find it hard to eat animals if we believed them capable of understanding. Rather than change the way we exploit animals we therefore find it easier to maintain skepticism about their mental states. This seems highly unlikely – there exists a parallel skepticism of Artificial Intelligence, for example. It seems implausible that I believe my computer to be incapable of understanding because I need to be able to turn it off at the end of the day without ethical qualms. Rather, my LACK of skepticism about the mental processes of other humans arises from my belief that they are like me (they look like me, act like me, we have a shared history, and so on). As we learn more about the commonalities between species of primates, we may learn to reevaluate our skeptical responses, but surely we do not NEED to become vegetarian in order to do so.[1]

Authorship returns to Savage-Rumbaugh for the final chapter of the book. Here, many interesting points are raised; in particular there is some (rather brief) discussion of the evolutionary implications of research into Kanzi's abilities. For example, Savage-Rumbaugh highlights the remarkable flexibility of the primate brain and suggests that all major anatomical modifications in the primate line must have been preceded by behavioural

[1] Of course, I am not making an argument here about any particular stance on animal rights, just about the likelihood that any such stance informs our skeptical approach to ALR. For example, as we understand more about these remarkable primates, it clearly throws into stark focus the plight that their small number are currently facing in the wild.

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changes. Here she is essentially invoking genetic assimilation (also known as the Baldwin Effect) as an evolutionary mechanism, but ironically does not seem to be aware that the same suggestions have been made by Pinker in his integration of generative linguistics and evolutionary biology (Pinker & Bloom 1990). That said, Savage-Rumbaugh is right here to point out that the behaviour of stone age man really is surprisingly similar to that of other primates. She suggests that we see language as a hugely important adaptation only because for us it has lead to culture and with this, technology. However, this is a relatively recent and not necessarily inevitable outcome of having language. Here she makes the thought-provoking comparison between the methodology of an anthropologist studying some forgotten stone-age tribe, and a primatologist studying a group of apes. How much of the way we think about these groups is due to these wildly different approaches?

However, once again, this chapter is let down by some uninformed discussion about modern linguistic theory. Savage-Rumbaugh, in an attack on the poverty of linguistic inquiry, basically sets up a 'straw linguist' which few engaged in serious study of language would recognize. This 'linguist' believes that:

1. language can be studied as a system in its own right (OK so far);
2. syntax is therefore NECESSARILY autonomous (this term is not used in the chapter, but I take it that this is what is meant);
3. an autonomous approach to language NECESSARILY places no importance on language use;
4. the autonomous representational mechanisms MUST be inherent in the human brain;
5. self-reflexive thought uses these same mechanisms;
6. these mechanisms are species specific.

We are left with the conclusion that the study of language in its own terms leads inevitably to the belief that animals are not capable of self-reflexive thought. Later in the chapter Savage-Rumbaugh continues in a similar vein, stating that 'linguists have almost managed to do away with the issue of intentionality' (195). She gives a simple example of conversational implicature to show what modern linguistics cannot handle. This kind of caricature of an extreme nativist, formalist, autonomous position really will not help the cause of ALR. The authors' are either not aware of functionalism, pragmatic theory, discourse analysis, or the recent attempts to integrate syntactic autonomy with language use (Newmeyer 1998, Kirby 1999), or do not tar these fields with the term linguistics.

Ape language research is very important to linguistics, and the abilities of Kanzi (and more recently, his sister, Panbanisha) far surpass what our theoretical perspective might have lead us to expect. A book like this is the response to a sustained skeptical attack from ALR critics. However, it is the worst response possible, as it is more likely to turn away those who could

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have the most to gain from a reasoned analysis of the language of non-human primates.

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Esther Torrego, *The dependencies of objects*. Cambridge, MA: MIT Press, 1998. Pp. xii + 197.

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The dependencies of objects is an attempt to provide a theory of those accusative objects that receive extra morphological marking in many languages in the form of a prepositional element. The core data are cases like (1) and (2), from Spanish:

(1) Ana levantó a un niño.
     Ana lifted   to a child
     'Ana lifted a child.'

(2)   Conocieron un linguista.
     pro met   a linguist
     'They met a linguist.'

In (1) the prepositional element *a* surfaces to further mark the accusative object, but this preposition does not always occur, as can be seen from (2). Torrego refers to the object in (1) as a 'marked accusative'.

Torrego focuses mainly on various varieties of Spanish, but also adduces evidence from Hindi, Greek and Albanian. Part of the theoretical interest of this endeavour is that Torrego's theory involves a deeper articulation of the properties of the lower functional structure of the clause (especially 'little' v), and how its syntax interacts with lexical and utterance-level semantics.

The book is structured into five chapters, but the main discussion stretches from chapter 2 to 4. After outlining the aims, concerns and direction of the book in chapter 1, Torrego uses chapter 2 to tease out the various factors that influence the appearance of the marked accusative. She discusses correlations between the appearance of the marked accusative and the telicity of the main predicate, the animacy of the object, the definiteness/specificity of the object, the affectedness of the object and the agentivity of the subject. She then proposes that much of the complexity of the situation can be resolved by assuming that marked objects raise to [Spec, v]. This movement process only takes place when v itself is specified for a D feature and the object is placed in a position where it can affect the aspectual interpretation of the verb. Torrego shows how statives with marked accusatives are interpreted as activities, and how activities are interpreted as accomplishments. It is also plausible that the correlation of marked accusatives with agentivity of the subject derives from the idea that v's thematic function is to introduce agents. Furthermore, overt movement of the object to a position outside of VP is well-known to give rise to a specific interpretation for that object (Diesing 1992, among others). A final outcome of this idea is that if clitics are treated as overt realisations of the D feature in little v, then a link can be established between obligatory clitic doubling and the appearance of marked accusatives, a correlation which is found in many Spanish dialects. The proposal, then, seems to have a great deal of motivation.

In addition to this, Torrego provides analyses of extraction patterns, the manifestation of case in nominalisations and in restricted ECM constructions, which show the utility of some of her assumptions.

However, on reading the chapter carefully, it becomes clear that the apparently impressive unification of all these factors rests on rather loose foundations. This chapter, in particular, suffers from a lack of explicitness about mechanisms: it is not made clear how the shifting of states to activities or activities to accomplishments takes place, nor is it clear exactly why raising of the object entails agentivity of the subject. The chapter also proposes that marked accusatives come in two flavours, inherent and structural, with the former related to the affectedness of the object. But again, no mechanism is suggested that explicitly ties these together. The animacy and definiteness restrictions are tentatively assumed to have their featural locus in D, but no suggestion is made as to what these morphosyntactic features are and how they relate to their clear interpretative effect. Nor is an explicit system adopted so as to explain the specificity effect, so there is no way of knowing how that system will interact with any of the other assumptions or corollaries of the theory, especially since Diesing's notion of VP-external does not immediately translate into a clause structure with v-V articulation.

I agree that there seems to be a link between all these factors at an intuitive level, and that the idea of raising the marked accusative to [Spec, v] is an

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interesting hypothesis to explore, in terms of how the semantic content of the light verb might interact with the syntactic dependency that is built between it and the object. However, in the absence of concrete proposals it is difficult to evaluate the empirical strength of this hypothesis. Linked to this is the unfortunate fact that the chapter offers no empirical evidence that the object has actually raised overtly and the theory internal considerations proposed are too vague to suffice. In addition, there are several technical problems with the proposals. For example, Torrego suggests that the preposition that appears in these structures may host a D feature, so that the whole marked accusative constituent can be attracted to [Spec, v]. If this is the case, though, then this D feature must be an interpretable categorial feature since it is attracted, and this in turn makes the preposition a determiner, an idea that is falsified on simple morphological and distributional grounds.

The discussion in chapters 3 and 4 attempts to expand on some of the ideas laid out in the second chapter. These chapters are more explicit, and some of the ideas developed and the empirical claims made are very interesting. Chapter 3 concentrates on causatives, and establishes a link between the apparently dative marked causees in examples like (3), and the construction in (1):

(3)   Hizo a su hermano vender la   casa.
    pro made to her brother   sell   the house
    'She made her brother sell the house.'

The core idea is that the case on the causee is checked by the (light verb associated with the) causative verb itself, a conclusion which seems to be backed up by the evidence, and to relate in the expected way to the contention that marked accusativity correlates with agentivity. Torrego supposes that the argument structures of the causative verb and the head of its complement are somehow syntactically 'fused', and claims that this is not a control structure but rather involves a little *pro*. Once again, however, the actual nature of this fusing is left vague.

This chapter does however introduce an interesting new set of data, the Emphatic Pronoun Causative (EPC), where a nominative emphatic pronoun doubles the dative object of the causative verb:

(4) Me   hizo pedir yo.
    to-me made ask   I
    'She made me ask.'

Torrego argues that the overt pronoun doubles a nominative *pro* subject of the lower predicate and that the case of both is checked by matrix Infl. The object, to be case checked, must incorporate into the verb as a clitic. Since

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the features of the emphatic pronoun must raise at LF, an intervening DP will give rise to a Minimal Link Condition effect, ruling out cases like (5).

(5) *Me   hizo abrir la ventana yo.
     to-me made open the window I
     'She made me open the window'

The constraint that those verbs that allow EPC must take non-affected agents as subjects is extremely interesting. Torrego argues that *pro* and the emphatic pronoun bear different theta-roles (*pro* is a patient, while the pronoun is the agent) and supposes that there are two possible ways of accommodating this: either the pronouns are in an A-chain, or in something which she refers to as a 'non-A-chain'. In the former case, she suggests that the agent role can be subsumed as secondary, and in the latter that the causative verb and the lexical verb jointly license an agent role. Once more, the lack of clarity of both of these proposals makes them difficult to evaluate: what are the constraints on subsuming theta-roles into A-chains as secondary, and how exactly does joint licensing work? Both of these suggestions are contrary to the intuition behind the Theta Criterion, and both of these analyses require more solid theoretical foundations if they are to be convincing.

The fourth chapter of the book is dedicated to the implications of the preceding discussion for ditransitives. Torrego shows that some verbs allow a marked accusative to occur along with a true dative, while others do not. She argues that the verbs which allow this are precisely those that trigger object raising of the marked accusative, and she draws comparisons between these predicates and those that allow double object constructions in languages like English. This is a novel and interesting observation. The chapter is less successful, however, in its discussion of intervention effects arising from the appearance of dative arguments of raising verbs. As is well known, indirect objects in English do not block subject-to-subject raising:

(6)   The talk seemed to the linguists to be very good.

This is perhaps unexpected, since one might imagine that the Case features in matrix T might preferentially attract the features of the closer indirect object, rather than the embedded subject. In French and Italian, such indirect objects do block raising, unless they are clitics, while in Spanish, even clitics block raising. Torrego argues that in English, the indirect object is merged in the inner specifier of T, which means that the embedded subject is the closest possible attractee. In French, however, the dative merges lower down, unless it is a clitic, while in Spanish, the dative is always low, and in fact, must be licensed by a clitic. This analysis certainly captures the facts, but suffers from two problems: firstly it predicts the wrong surface order for English, since the finite verb precedes rather than follows the indirect object; secondly, it really simply states the parametric variation, rather than explaining it.

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The book also houses a number of minor irritants. Often examples and their glosses are given in the actual text (presumably to save space) and without translations, rather than set out as is standard. This decreases legibility and means that somewhat dense discussions are rendered presentationally even more opaque. The glosses given are often not helpful, and sometimes inconsistent. There are also a number of infelicities in the English and the argumentation is sometimes far too tentative in its rhetoric, which tends to disillusion the reader.

Although I have criticized *The dependencies of objects* on the grounds that the proposed mechanisms are often (but not always) not explicit, I think that the main intuition that is defended here is a good one and that the book is replete with interesting ideas for further work. Torrego has also done a great service in presenting and disentangling the tortuous web of empirical factors that influence this phenomenon.

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