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REVIEWS

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Farrell Ackerman & John Moore, *Proto-properties and grammatical encoding: a correspondence theory of argument selection* (Stanford Monographs in Linguistics). Stanford, CA: CSLI Publications, 2001. Pp. ix + 197.

Reviewed by Malka Rappaport Hovav, The Hebrew University of Jerusalem

Dowty's (1991) theory of argument selection has proved remarkably influential, though it is rather modest in scope and was never intended as a fully-fledged theory of mapping between lexical semantics and morphosyntax. Ackerman & Moore (A&M) do not present a comprehensive theory of mapping either, but rather attempt to show that the basic apparatus introduced by Dowty can be harnessed in extending the theory's empirical coverage, thereby supporting the general approach. Since the phenomena Dowty studies in depth in the application of his theory are drawn from English, many questions remain concerning argument realization phenomena in languages with richer options for morphosyntactic expression. A&M introduce some extensions to the proto-role approach and apply them to aspects of argument realization in a range of languages with richer morphosyntax than English, such as Spanish, Finnish and Estonian, Russian, Polish and Marathi. The phenomena all involve alternations in the morphosyntactic expression of the arguments.

Chapters 1–3 lay out the familiar challenges of theories of mapping: the systematic correspondences between morphosyntactic expression and semantic argument type, as well as systematic alternations in argument expression for predicates of particular types. Chapter 2 reviews the well-known problems associated with theories based on atomic semantic roles, problems which motivated the development of the proto-role approach. Chapter 3 is a review and discussion of Dowty's theory of proto-roles and argument selection. A&M adopt most of the basic elements of Dowty's theory: the semantic determinants of argument expression are derived from lexical entailments imposed by predicates on their arguments; the entailments fall into two broad classes, an agent-like class and a patient-like class; no single entailment is necessary for subject or object selection; subject and object selection are based on a counting procedure and there is a direct mapping between the number of proto-role entailments and the expression of an argument as subject or object.

Chapter 4 presents the first major extension to Dowty's theory, and is in many ways the heart of the book. A&M describe Dowty's argument selection principle as syntagmatic argument selection, since it compares the proto-agent and proto-patient entailments for co-occurring arguments. They introduce what they call the paradigmatic argument selection principle to deal with cases in which there is an alternation in the encoding of a specific argument, accompanied by a change in interpretation. Each such alternation is analyzed as involving two semantically related predicates, and the principle is called paradigmatic since instead of comparing co-arguments, it compares corresponding arguments of related predicates. As with syntagmatic selection, it is based on comparing the number of proto-role entailments of two arguments. The paradigmatic principle states that when there is an alternation in the encoding of an argument, there will be fewer proto-patient entailments associated with the argument that gets the more oblique expression; degree of obliqueness is conversely correlated with degree of patienthood. For example, the experiencer argument in certain psych-predicates in Spanish can show an alternation in expression between direct (DO) and indirect object (IO). This is seen in the contrast between *Los perros* ***lo*** *molestan siempre que llega ebrio* 'The dogs harass him (DO) every time he comes home drunk', and *Los perros* ***le*** *molestan* (**siempre que llega ebrio*) 'Dogs bother him (IO) (*every time he comes home drunk)' (page 65). The change in grammatical function correlates with a subtle semantic contrast, as indicated by the difference of acceptability in the adverbial. A&M assume that there are two predicates *molestar*, related by a morphosemantic rule, and the difference in interpretation is attributed to the existence of a change-of-state entailment associated with the direct object experiencer argument, which is lacking in the indirect object experiencer argument. Most of the book deals with alternations of this kind. It is important for A&M's theory that the same alternation may be triggered by the manipulation of different entailments, since the selection principle is based on counting of entailments and not on individual entailments. The alternation between genitive and partitive objects in Estonian (see below) can be determined either by a telicity contrast or by a change-of-state contrast. The principle is eventually generalized in chapter 6 to cover subject–oblique alternations so that any deviation from the prototypical agent or patient will correspond to a more oblique encoding. There is certainly something natural about this principle, capturing the same insight embodied in theories which align height on a thematic hierarchy with height on various grammatical obliqueness hierarchies.

Chapter 5 introduces two further extensions. Much recent work on argument expression has focused on alternations accompanied by a change in telicity. The challenge for a theory like Dowty's is that the semantic determinants of argument expression are said to come from entailment on arguments, but telicity is a property of predicates, not arguments. The

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proto-role entailment in Dowty (1991) which comes closest to being relevant is the incremental theme entailment. However, A&M point out, along with other recent studies, that certain predicates may alternate between being telic and atelic, with an incremental theme on both variants. This would be the case for verbs like *drink,* whose direct object is an incremental theme both in *John drank a cup of water* and *John drank water*. If such predicates show an accompanying change in argument expression, the change cannot be attributed to the incremental theme entailment. A&M provide a careful and detailed analysis of accusative/partitive alternations in Estonian and Finnish (though in Estonian the accusative is morphologically realized as genitive), showing that the telic variants of verbs like *drink* have accusative objects, while the atelic variants have partitive objects. They also discuss the converse situation: predicates which alternate between telic and atelic variants, and an accompanying change in case, without an incremental theme on either variant. A&M introduce a new proto-patient entailment, BOUNDING ENTITY, to account for these alternations. It is unfortunately unclear from their discussion whether they take the bounding entity entailment to replace the incremental theme entailment, or to supplement it.

The Finnish and Estonian examples raise a further issue. Dowty presents his theory as one of selection of subject and object. He does not distinguish between the grammatical function borne by an argument and the morphological realization of this grammatical function, since there is no reason to make this distinction in English. However, as A&M point out, the alternation between partitive and accusative in Finnish and Estonian is not an alternation in grammatical function. They follow Kiparsky (1998) in adducing clear evidence that the partitive marked objects are true objects. They motivate an obliqueness ranking of morphological case, with partitive being more oblique than accusative. The alternation then naturally falls under their general extension of Dowty's theory, where attenuated patienthood correlates with greater obliqueness.

Chapter 6 is a review of the literature on alternations in languages such as Polish and Russian, and Hindi and Marathi, involving an argument variably expressed as a nominative/ergative subject and a dative marked NP. Here, too, the alternation may be one of grammatical function, where the dative marked NP is analysed as an indirect object, or one of case only. They show that these alternations fall naturally under their theory since the dative marked NP, whether a subject or an indirect object, is always associated with attenuated agentivity, usually reduced volitionality.

A&M analyse all the alternations they deal with as involving predicates related by some morphosemantic rule, as in the Spanish case above. On the face of it, one would expect the same principles of linking to deal with simple predicates and derived predicates, the input and output of morphosemantic rules, obviating the need for a paradigmatic principle. Indeed, Dowty deals

with the locative alternation in English using just his syntagmatic principle. Why, then, is the paradigmatic strategy necessary?

As it stands, Dowty's syntagmatic procedure cannot deal with the *molestar* examples and any others involving an alternation between a direct object and an oblique, such as the conative alternation in English (*shoot the bear*, *shoot at the bear*). This is because Dowty's theory constrains the expression of arguments of predicates already marked as transitive, a point made forcefully in Davis (2001) and Davis & Koenig (2000). Therefore, the variants of *molestar* and *shoot* with oblique complements fall outside of Dowty's theory, and A&M are correct that, to the extent that these alternations reflect general principles of lexical organization, an extension of Dowty's theory is needed. However, A&M do not explore the possibility that amending the syntagmatic principle of argument selection so that it can deal with two-argument intransitives and not only with two-argument transitives might obviate the need to introduce the paradigmatic principle. They point out that their paradigmatic principle is a departure from many standard theories of mapping, in that the encoding of an argument is not locally determinable on the basis of a single predicate, but depends on the existence of an alternation. While it is likely that the paradigmatic strategy plays a role in natural languages, it seems worthwhile to explore the possibility of refining the syntagmatic strategy so it can deal with many of the encoding alternations.

The paradigmatic selection principle is also motivated on the basis of causee encodings in causative constructions across languages. A&M argue that when the encoding of the causee is determined solely on the basis of transitivity, this is a reflection of the syntagmatic strategy, while when it is determined semantically on the basis of directness of causation, it is a reflection of the paradigmatic strategy. They explain the range of causee encodings available in Spanish by assuming that Spanish uses the syntagmatic strategy as a default, and invokes the paradigmatic strategy only when it is needed for disambiguation. When the syntagmatic strategy yields the same linking for direct and indirect causation, resulting in ambiguity, the paradigmatic strategy is invoked, violating the syntagmatic strategy, but distinguishing the direct and indirect causation readings. The discussion of the encodings of causees is based on Ackerman & Moore (1999), where they argue that a range of differences in the way causees are encoded across languages can be attributed to differences in the ways languages rank the paradigmatic principle and the syntagmatic principle.

Since the encoding alternations studied in the book are governed in A&M's theory by the paradigmatic argument selection principle, which compares proto-role entailments of arguments of related predicates, there is massive proliferation of predicates. In Spanish, for example, A&M postulate three causative verbs *hacer* to account for three degrees of directness of causation; to account for the genitive/partitive alternation in Estonian, they

postulate two semantically related predicates for each verb showing the alternation. A verb like *drink* will have two related predicates, one selecting an object with the bounding entity entailment and expressed in the genitive, and one with an object lacking this entailment and marked partitive. A&M argue that the predicate-based approach is justified in Estonian, since the telic version of the verb is sometimes expressed by a compound verb. But they still maintain a predicate-based analysis for Finnish, even though it lacks these complex predicates, just because the alternation in telicity correlates with an argument encoding difference. But this seems to put the horse before the cart: there is no reason a priori to assume that all argument alternations have to be traced to lexical entailments on arguments. It is certainly worth exploring the possibility that some encoding alternations may come from properties of the arguments themselves. Consider the well-known encoding alternation for objects in Spanish based on animacy. This phenomenon should be covered by their theory, since animate objects show a lower degree of patienthood and a correlated rise in obliqueness. It is difficult to believe that the appropriate analysis there attributes the difference in animacy to an ambiguity in the verb. But if we allow for an alternation not based on predicate entailments for object marking in Spanish, then the fact that there is an argument encoding alternation in Finnish does not force us to analyse it as involving semantically related predicates. In the case of *molestar* above, the difference in the acceptability of the adverbial points to an individual/stage level distinction, so it is not clear that a lexical entailment on an argument is relevant here. Returning to causatives, McCawley (1978) long ago showed that the indirect causation reading for periphrastic causatives in English is best analysed as arising from an implicature. Ackerman & Moore (1999) acknowledge that French data similar to the Spanish data are analysed by others as involving implicatures and not lexical entailments, but they do not present any evidence that their analysis based on entailments and multiply ambiguous verbs is superior.

In sum, A&M show ways to extend the empirical coverage of Dowty's theory, and in the process raise important and interesting questions concerning alternations in argument encoding which will continue to challenge researchers in the field.

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Alexandra Y. Aikhenvald & R. M. W. Dixon (eds.), *Areal diffusion and genetic inheritance: problems in comparative linguistics*. Oxford: Oxford University Press, 2001. Pp. xvi + 453.

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Areal diffusion and genetic inheritance is a collection of articles intended first and foremost to investigate the relationship between inherited and borrowed features in languages. Apart from the introduction, the volume contains two papers by the editors, and 12 by other – predominantly Anglo-Saxon – contributors. Most of the material was originally presented at the Research Centre for Linguistic Typology in Melbourne, Australia, which is the home base of the two editors.

An explicit additional goal of the volume is to discuss the 'punctuated equilibrium' model promoted by Dixon in his 1997 book *The rise and fall of languages*. For this reason, a few words about that book may not be amiss. His controversial claim, as I understand it, is that the classical *Stammbaum* model of genetic relationships between languages is the exception rather than the rule. If the proliferation-through-split process yielding the neat linguistic family trees we are all familiar with works pretty well so far as Indo-European languages are concerned, it is considerably less successful when it comes to Australian languages. Instead, Dixon argues, the Pama-Nyungan languages (grouping most of the native languages of Australia) may or may not be related, but whatever the case, the similarities observable today do not constitute proof of a genetic relationship. The claim is that on a continent where no or few truly radical perturbations upset the geolinguistic situation, features diffuse gradually until they are shared by a large number of not necessarily related languages, as any given language is constantly 'sprachbunding' with all of its neighbours.

When I originally read *The rise and fall*, I was less than convinced, and I was somewhat bothered by noting that *Areal diffusion and genetic inheritance* was intended to build on (and presumably promote) that idea, expecting

it to be a lengthy *hommage à Dixon*. Fortunately, this is not the case, and many of the contributions even make only cursory mention of the punctuated equilibrium model. A more thorough exposition of it, of course, is found in Dixon's own chapter (and the introduction co-authored with Aikhenvald). For some reason, I am more positive about the idea here than I first was. Yet, even if the model accurately captures the situation in Australia (which most Australianists doubt), it remains to be proven, of course, that it also does so in other parts of the world. I find two of Dixon's claims most interesting. First, he states, with regard to Australia, that 'there is absolutely no bunching of isoglosses, which would be needed for high-level subgrouping within a fully articulated family tree' (87). This is disputed by virtually all Australianists, although it is difficult for a non-expert to evaluate the data. Secondly (and this should be more easily acceptable to a general linguist, although the actual figures could be questioned), he points out (7) that a single proto-Indo-European has produced just over 100 surviving daughters in 7,000 years. If this family were representative, and if mankind has possessed speech for 100,000 years, we would expect there to be $10^{2*(100,000/7,000)}$, or in other words about 10^{28} languages in the world today. Clearly, this is not the case, and hence the case of Indo-European cannot possibly be representative. Whether one is convinced or not, the perspective certainly is interesting.

On the other hand, Dixon is aware that the Australian languages on which he builds his case are exceptional in some respects. There is, for instance (if we are to take his word for it), no difference in replacement rate between core and non-core lexemes (83–84), although nouns are replaced more rapidly than verbs (and grammatical morphemes are more stable than lexical ones).

Peter Bellwood's contribution is also interesting, as he is primarily an archaeologist rather than a linguist. Not surprisingly, given the earlier collaborations between him and Colin Renfrew, Bellwood's line of reasoning is closely aligned to Renfrew's. In other words, he takes the spread of agriculture to be a major cause of language spread. Bellwood also cautions (no doubt to Dixon's liking) that most of humankind's linguistic history belongs to a period of relatively egalitarian hunter-gatherer communities (30–31), for which reason we should be careful in applying experiences from modern language situations to older times. All in all, Bellwood's chapter is a good read which whets the appetite. I should point out, though, that as a linguist, I cannot vouch for the quality of his archaeological data (but see Campbell (in press) for a critical assessment of the Renfrew/Bellwood approach).

As in many of his earlier papers, Malcolm Ross discusses metatypy, a concept closely related to (and near-synonymous with) 'calquing' or 'syntactic borrowing' or 'regrammaticisation'. Frankly, little in this article goes beyond what common sense would suggest, but Ross does provide a useful list of languages he considers metatyped (146). He should also be

commended for setting up a typology of the phenomenon. He furthermore makes a few claims on how metatypy proceeds, starting with semantic reorganisation before going on to morphosyntax (146, 149). Ross also points out that the process is normally unilateral, so that Sprachbund phenomena in essence consist of reiterated metatyping over a larger area (153).

As a former creolist, I cannot help being puzzled and somewhat troubled by one aspect of Ross's terminology. On the one hand, he distinguishes (correctly in my view) between 'imperfect shift' and 'creolization' (160). Yet, he explicitly says that a creole is derived from an 'imperfectly learned version' of a target language (158).

Aikhenvald discusses the Amazonian region, drawing on the same fieldwork which produced her book with Dixon on Amazonian languages (Dixon & Aikhenvald 1999) a couple of years ago. Her main focus here is the impact of Bora-Witoto languages on the unrelated (Arawakan) Tatiana, which has been 'restructured beyond recognition'. Borrowed items include several personal pronouns, as well as classifiers.

Geoffrey Haig's contribution deals with a region less often thought of as a convergence area, namely eastern Turkey. He demonstrates several features which have come to be shared among the languages of the region (which include Laz, Kurdish, Zazaki and Turkish), but admits to being uncertain as to whether the area should be treated as a Sprachbund (209). The most spectacular item on display in this chapter is without doubt the Arderen dialect of Laz, which has become almost completely isomorphic with Turkish (216). Unlike many other contributors, Haig also ventures a theoretical claim: convergence proceeds from larger to smaller discourse units, since tighter grammaticalisation restricts the speaker's freedom (219). The author finds some supporting evidence in studies on code switching, where switches are more frequent between higher level constituents than at lower levels. Such a claim predicts that, for instance, major constituent order would be more easily affected by language contact than would, e.g., the placement of adpositions (219–220). This is most certainly true, but if it were not, Haig admits his hypothesis would be falsified.

James Matisoff discusses the problem regarding prestige relationships between languages. In south-east Asia, the direction of influence used to be from Mon to Burmese, and from Khmer to Thai, but in more recent times, both relations have been inverted, and this of course makes it more difficult to establish which parts of a language are original and which have been acquired through contact. Matisoff (302–303) usefully quantifies the interlinguistic influences on the 'borrowing scale' introduced by Thomason & Kaufman (1988). Most south-east Asian languages are only moderately affected by contact, and would score 1–2 on this scale. Four languages, however, display a higher degree of external influence (3–4). These are Burmese (influenced by Mon), Newari (of Nepal, influenced by its Indo-Aryan neighbours), Kelatan Chinese (influenced by Malay) and the mutual

influence between Thai and Khmer. Matisoff's article also includes several discussions which may perhaps not contain much new material, but which are most useful to those who are not specialised in south-east Asian linguistics. He gives a rather extensive list (301) of features making up the south-east Asian Sprachbund (including also pragmatic features, such as greeting formulas) and a list of laryngeal features relevant to tonogenesis (305). He also provides a most interesting discussion of what he calls the 'compounding/prefixation cycle'. The basis for this discussion is the observation that there is a correlation in Sino-Tibetan between monosyllabicity and 'toneproneness'. This then leads Matisoff to set up the following development cycle (305):

> Complex monosyllables (tones less important) → Simple monosyllables (tones very important) → Compounds (tones somewhat less important) → Sesquisyllables (prefixization of first constituent in compounds) (tones somewhat more important) → Complex monosyllables (tones less important)

Plain old erosion phenomena would shorten the lexical items under consideration, leaving only suprasegmental features behind. Presumably, then, a decrease in redundancy (despite the growing importance of tonal traits) would lead to circumlocutions which, when lexicalised, may re-enter the erosion phase. The setting up of a model for this development is clearly highly relevant in relation to discussions of concepts such as complexity, expressiveness and redundancy, which are close at least to my heart.

The chapter by Gerrit J. Dimmendaal discusses a moderately far-reaching case of language contact, involving the Bantu language Khoti of Mozambique, which has borrowed about 30% of its basic lexicon from neighbouring Makhuwa. He also treats a case of similar magnitude from the border between Ethiopia and Sudan. Some to my mind interesting comments are made more or less in passing. First, Dimmendaal proposes (371) that vowel harmony is an easy come, easy go feature, which can be introduced and lost quite easily, something that is highly compatible with my own experience. More puzzling, though, is the fact that another Bantu language, Bila, is mentioned (370) as being in the process of acquiring vowel harmony in the verb system alone. Secondly, Dimmendaal suggests (365) that a small group is more exposed to the effects of language contact than a bigger one is (also proposed by Dixon in this volume). This could have been the basis of an interesting discussion, although one gets the impression that the statement is founded on impressionistic data rather empirical evidence. Finally, the author mentions (382) Luo (Nilo-Saharan), which appears to be acquiring noun classes as a result of contact with its Bantu neighbours. This case is something I would have appreciated learning more about, as noun classes are usually thought of as highly genetically stable (demonstrated in e.g. Nichols 1992).

The book finishes off with a chapter by Jowan Curnow, which is basically a summary of the other contributions with a certain focus on the problems encountered. Worth citing is his discussion of the borrowability of inflexional morphemes as opposed to derivational ones. The unwillingness of the world's languages to borrow inflexion has sometimes been seen as a result of derivation being semantically 'heavier' than inflexion (though this hardly applies, the author points out, to some semantically concrete inflexional categories such as number). Curnow instead suggests (416) that the paradigmatic relations are what counts – in other words, derivation is easier to borrow because the derivational morpheme is happy to participate only in a syntagmatic relationship with its head, as opposed to the 'tout-se-tienting' (paraphrasing Saussure) inflexions.

Space constraints prevent me from discussing all the chapters in this volume, but for the record, it also includes contributions from the following: Calvert Watkins (Ancient Anatolia), Alan Dench (north-western Australia), Randy LaPolla, Nick Enfield and Hilary Chappell (each with a chapter on south-east Asia) and Bernd Heine & Tania Kuteva (southern and north-east Africa).

Areal diffusion and genetic inheritance is certainly a book worth acquiring and reading, but I must admit that it didn't rock my world, possibly because I had expectations difficult to live up to. Most of the material is interesting in that it provides data from several diverse areas, but at the end of the day, I experienced few aha-experiences, and didn't really burst into incessant eurekas. To some extent the volume displays each author's own favourite data, leaving the reader somewhat uncertain as to why these particular data were presented, and what to make of them. So far as generalisations are concerned, many of the contributors seem rather content to establish that strict adherence to the family-tree model is untenable, which is not exactly ground-breaking news. I, for one, would have liked to see more of a discussion of the pros and cons of various models. If the traditional *Stammbaumtheorie* fails to account for the facts, then should we just toss it overboard, or could it profitably be modified by incorporation of features from other models? Also, it would be most interesting to include more of a discussion on which particular features are sensitive to areal pressure, and why this is so. Why is it that gender systems and umlaut are genetically rather stable, while, for instance, word order is less so?

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Artemis Alexiadou (ed.), *Theoretical approaches to universals*. Amsterdam: John Benjamins, 2002. Pp. xviii + 316.

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This book is about the universal properties of the language faculty within the principles and parameters paradigm. Some papers deal with principles and others with parameters. Consisting of nine contributions, the volume covers topics such as the architecture of the grammar, the role of features in determining cross-linguistic variation, primitives of phrase structure, the mechanisms and motivation behind what can and cannot undergo phonological deletion, and differences in the morphological and semantic features of certain lexical items both within a language and across languages. The editor provides a comprehensive introduction, in which a historical view of research on universals is given and current issues in universals are provided. The collection of articles stems from the 1999 Berlin GLOW conference on Universals, organized by the Research Center for General Linguistics (ZAS, Berlin), the Linguistics Department of the University of Potsdam and the Dutch Graduate School in Linguistics (LOT). Two papers were not presented at the conference, but have been included in the volume, namely the articles by Boeckx and Fanselow & Ćavar.

The opening contribution is by Maya Arad. In 'Universal features and language-particular morphemes', she attempts to give a precise content to the minimalist claim that language variation is restricted to lexical items. According to her, there are three sources for language variation: the inventory of roots a language has, the features it has selected out of a universal set of features, and the way these features are bundled together. The paper concentrates on feature bundling and argues that the now well-known functional category *v* ('little v') proposed by Chomsky (1995) is not a primitive. Instead, languages bundle features in different ways, leading to the situation where there is not one *v*, but several (or at least two). Her proposal is in line with recent suggestions that functional categories are not primitives, but stand for bundle of features, cf. Marantz (1997), Chomsky (2000).

The second paper is by Cedric Boeckx. Entitled 'Agree or Attract? A Relativized Minimality solution to a proper binding condition puzzle', his paper examines a paradigm first discussed by Kroch & Joshi (1985) which Lasnik (2002) takes as an argument for feature-movement:

(1) (a) John is likely to win.
(b) There is likely to be a riot.

(2) (a) How likely to win is John?
(b) *How likely to be a riot is there?

Lasnik uses the paradigm above in support of the Attract-F hypothesis. However, Boeckx shows that Lasnik's solution to the puzzle is problematic, since it relies on a model of the grammar where there is a separated cycle, that of LF. Boeckx argues that it is both theoretically and empirically more advantageous to postulate a grammar with a unique cycle and without a separate LF component.

'Distributed deletion', by Gisbert Fanselow & Damic Ćavar, deals with discontinuous DPs and PPs in German and Slavic languages. Split-DP or split-PP constructions have the following characteristics: they either involve raising of a bare operator and the stranding of a nominal or, alternatively, raising of a nominal and stranding of an operator. The German data in (3) are from Fanselow & Ćavar (89).

(3) (a) Bücher weiss ich nicht wieviel er gelesen hat.
books know I not how-many he read has
'As for books, I do not know how many of them he has read.'
(b) Wieviel denkst du dass er täglich Bücher liest?
how-many think you that he daily books reads
'How many books do you think that he reads every day?'

The authors argue that movement analyses of such splitting face serious problems. They also claim that base-generation accounts of such constructions (both parts being base-generated *in situ*) do not fare any better. They propose instead the copy and deletion approach to movement. According to this theory, deletion after the copying operation is possible for both copies.

In 'Roots, constituents, and c-command', Robert Frank, Paul Hagstrom & K. Vijay-Shanker make the interesting proposal that syntactic structures should be characterized directly in terms of a primitive c-command relation, as opposed to a primitive dominance relation. Traditional concepts such as roots and constituents are described simply in terms of c-command without referring to dominance at all. The c-command based view of roots distinguishes between the categorical root and the site of cyclic attachment.

In 'A four-way classification of monadic verbs', Murat Kural argues that the two-way classification of monadic verbs as unaccusative or unergative, as proposed by Perlmutter (1978) and Burzio (1986), should in fact be viewed

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as a four-way classification. The four classes are as follows: verbs of being (verbs that indicate that their subject comes to exist in some fashion, e.g. *appear*, *arise*, *emerge*, *ensue*, *exist*), change of state verbs (verbs that indicate that their subject has undergone a change of state, e.g. *break*, *burn*, *change*, *fold*, *grow*), change of location verbs (verbs that indicate the motion of the subject, e.g. *fall*, *jump*, *march*, *roll*, *run*), and finally verbs of creation (verbs that indicate that the subject has produced an often abstract, though sometimes concrete but intangible product, e.g. *cough*, *dance*, *dream*, *laugh*, *sing*, *sleep*). Each class is associated with a distinct structure.

Luis Lopez considers the operations Agree and Move. The title of his paper is 'On agreement: locality and feature valuation'. Here Lopez argues that Agree is strictly local. Secondly, he argues that the operation Move is not Attract/Pied-Pipe, but is triggered by the instability created in the system by unvalued features. Finally, he proposes the concept of 'Co-valued features' in order to make explicit the relation between an expletive and its associate on the one hand and the relation between the links of a chain on the other. The combination of locality and co-evaluation allows him to analyse and account for subtle cross-linguistic variation.

In 'A minimalist account of conflation processes: parametric variation at the lexicon–syntax interface', Jaume Mateu & Gemma Rigau show that the 'conflation processes' involved in so-called 'lexicalization patterns' receive an adequate account when translated into syntactic terms. They demonstrate that the distinction between satellite-framed and verb-framed constructions correlates with the (un)availability of the relevant empty heads (*cause/go/be*), whose licensing involves Merge so that crashing at PF is avoided. They conclude that parametrized variation is not confined to the inflectional system, as is traditionally believed, but that it involves the non-inflectional system as well.

The next paper is by Juan Romero. In 'Morphological constraints on syntactic derivations', the author argues that there is no universal catalogue of formal features from which to choose. Instead, each language determines independently its own formal features from the universal set of features, F, made available by the language faculty. In order to support his hypothesis, Romero shows how certain restrictions, e.g. the Person Case Constraint, only show up if there are agreement features involved. Furthermore, he links this property to the capacity found in some languages, such as Japanese, to delete arguments without leaving any phonetic trace, agreement or pronoun. Other issues are discussed: for instance, the interpretability of the EPP feature and object shift in Scandinavian languages.

The final paper is by Joachim Sabel. Entitled 'Intermediate traces, reconstruction and locality effects', it specifically argues against intermediate adjunction. The idea is that the only existing intermediate traces of a moved element are traces in specifier positions. The basic tests that are used to strengthen his hypothesis involve: *wh*-movement, empty-operator

movement, A-movement, extraposition, quantifier raising, scrambling and head movement. Instead of relying on intermediate adjunction, the analysis of scrambling in German and Japanese rests on the assumption that Japanese allows for multiple A-specifiers whereas German does not.

The editor of this volume has done a marvellous job of putting together all these articles. The introduction is very helpful, not only in setting out what the volume is about, but also in reviewing the issues involved in universals and the history of the topic in linguistics so far. The book is superbly edited. My only regret is that the papers are very different in nature. Although there is a common theme, that of universals, and a common theoretical thread, i.e. the Minimalist Program, it is often not so clear what unites all these contributions. The proposals are often very complex and no link between them is necessarily apparent. Perhaps, instead of appearing in alphabetical order, it would have been more judicious to group the articles together under common themes, e.g. papers dealing with the lexical aspects of universals, papers on phrase-structure, papers concentrating on the nature and properties of movement relations, etc. For example, the contributions by Arad, Kural and Mateu & Rigau belong together while the papers by Boeckx and Lopez are natural candidates to appear side by side. Another complaint is that the index is very minimal and does not do justice to the richness of the concepts and authors cited in the volume.

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Zeljko Bošković, *On the nature of the syntax-phonology interface: cliticization and related phenomena* (North-Holland Linguistic Series: Linguistic Variations Volume **60**). Amsterdam: Elsevier, 2001. Pp. ix + 328.

Reviewed by Andrew Caink, University of Westminster

Generative research on South Slavic clitics during the 1990s has been a prolific and rapidly developing field, resulting in many substantive developments both in our knowledge of the data and our theoretical approaches to them. This work, by one of the major contributors to this debate, makes the case for variable spell-out in the phonology, focusing principally but not solely on South Slavic languages. It includes some meticulous critical discussion of competing analyses, an often highly impressive account of data, and a magnanimous acknowledgement of the many other authors and unpublished researchers who have contributed to a number of the ideas.

Bošković's principal agenda is to carefully dismantle arguments from South Slavic that have been gathered by various authors in favour of Prosodic Inversion (Halpern 1995), whereby an unsupported enclitic generated by the syntax in sentence-initial position switches places with the next phonological word in the string. He argues that the phonology is able to modify the syntactic output not by movement, but via the selective pronunciation of copies in a movement chain.

With an eye on the wider field, the author asserts that South Slavic clitics represent 'a perfect ''laboratory'' for investigating cliticization' (2) on account of the variety and idiosyncrasies that they display. The empirical domain includes brief discussions of Germanic verb-second, object shift in Scandinavian, cliticisation and *wh*-movement in Romanian, Romance negation, and cliticisation in Polish and Czech in support of the main theory.

In addition to a succinct introductory first chapter and a brief summary conclusion in chapter 5, there are three chapters. Chapter 2 critically reviews much of the literature on 'second position' clitics in Serbian/Croatian (henceforth SC). The literature is divided for this purpose into 'weak' and 'strong' syntactic and phonological approaches (with an acknowledgement that there exist alternative accounts not covered by this typology). 'Weak' syntactic approaches are those analyses mentioned above that modify the syntactic output via Prosodic Inversion. Bošković is thorough in his deconstruction of the arguments for Prosodic Inversion, demonstrating that it is too powerful in many instances. This is a fine case of superficial observation of the data being proved untrue by careful scientific debate and analysis. The equally numerous 'strong' syntactic accounts are typified by movement of syntactic material in front of the SC clitics which are situated high in the clause, often in C. Bošković draws together an extensive list of arguments

to show that the SC clitics in fact can appear in a variety of positions in the syntax, but not as high as C. Next, Bošković rejects the idea of phonological movement utilized in Radanović-Kocić (1988), a 'strong' phonological approach, but concludes this chapter by adopting her descriptive generalisation that clitics must appear second in their intonation phrase. Following Klavans (1985), he makes this a lexical specification on SC clitics, along with the fact that they are marked 'suffix' (83). It is these lexical specifications that rule out the appearance of SC clitics in anything other than a second position. Previously published in Beukema & den Dikken (2000: 71–119), this chapter appears here with a number of additional arguments against the strong and weak syntactic accounts.

In chapter 3, Bošković adopts the claim that the phonology may pronounce a lower copy rather than the head of a movement chain. Informally, the pronominal clitic moves up in the syntax leaving multiple copies along the way, and PF spells out the highest copy that satisfies the prosodic requirements of the clitic. Hence, in cases where the syntax generates clitics in an unsupported position (i.e. first in the intonation phrase), we do not need recourse to phonological movement; the output of the syntax can be appropriately modified via deletion of the head and pronunciation of a lower copy.

(1) (a) *Ga voli.
him.ACC loves
(b) Voli ga.
loves him.ACC
(c) Ona ga voli.
'She loves him.'

In (1), the SC clitic and verb have independently raised for checking purposes to AgrO, leaving the clitic exposed in (1a) in sentence-initial position. This violates the clitic's lexical requirements at PF. However, if the head is deleted at PF and the copy following the verb is pronounced instead, we have (1b). Example (1c) demonstrates that the head is pronounced if prosodic support is available (135). Equally effective analyses of Slovene and Polish clitic placement follow the SC analysis, along with a brief but elegant reanalysis of Northern Norwegian verb-second data.

The account provides impressive coverage of the SC data, including a reanalysis of what have previously been analysed (problematically) as optional movements (132–141). The marginality of some crucial data, particularly data involving 'split' clitic clusters (51–61), remains curious, as it would appear to be predicted to be fully grammatical in Bošković's 'Pronounce a copy' (henceforth PAC) account, but this is a problem for all competing accounts too.

Chapter 4 focuses on Bulgarian and Macedonian clitic placement. The pronominal and auxiliary clitics lack the lexical specification of 'second

position' found in SC, and the cluster results from adjunction in the syntax. Bošković details several alternative ways in which the movement chains may be generated (depending on what theoretical limitations one adopts concerning left/right adjunction). He is not unduly concerned about the syntactic position to which the verb and clitics raise in order to provide the necessary copies for the analysis to work, however (185). There is a particularly effective account of the question (and focus) particle *li* that includes a vigorous dismissal of the Prosodic Inversion accounts (197–249). In an appendix, there is some discussion of Macedonian clitics, with, as is the case in competing accounts, variable success. A final appendix presents three arguments in favour of multiple spell-out combined with the PAC theory from Bulgarian, Scandinavian object shift and Romance negation.

Regarding the PAC mechanism, Bošković follows Franks (1998) in assuming that if the head of a chain cannot be pronounced owing to a PF requirement, then the next highest head that satisfies the constraint is pronounced (100, 124). He suggests that this is an attempt by the phonology to be 'faithful' to the syntax as far as possible (125). Intuitively, this makes sense to us on account of the generative history of syntactic movement and traces, but it remains a stipulation within the system. Bošković's 'weak' phonology approach has simplified the syntax (in comparison to the 'strong' syntax proposals), and elegantly ruled out the need for phonological movement, but this is made possible only by placing a great deal of sophistication in this mechanism (see the derivation of (2) below). It is unfortunate that discussion of how the PAC mechanism works in terms of scanning and deleting copies is largely restricted to footnotes 12 and 30 (193 and 210, respectively) in chapter 4.

The analysis of the SC 'clitic cluster' is persuasive and manages to avoid several of the theoretical pitfalls found in purely syntactic or phonological approaches ('look ahead', stipulation of the clitic position in C, phonological movement). A particularly interesting result of the analysis of SC is that the 'clitic cluster' itself is merely an epiphenomenon of the lexical requirements of individual clitics combined with Bošković's rule of 'PF merging' (84). This is an adaptation of Marantz's (1988) Morphological Merger, in which Bošković proposes that when X and Y merge at PF, the derived element takes on the requirements of both X and Y and is unable to affect linear order (i.e. Prosodic Inversion is ruled out). Hence, only copies in second position in the intonation phrase may be pronounced, and material intervening between the clitics (e.g. adverbs) is excluded. In contrast, Polish clitics lack the second position requirement and may appear in various positions in the clause (171). With the qualification that the PAC mechanism may have the power to spell out any copy in the chain optionally, the PAC theory predicts the Polish data fairly straightforwardly. Implying that the SC and Polish clitic systems are a minimal pair, Bošković

writes: 'apparently, removing the second position requirement results in the relaxation of the clitic clustering requirement, as expected under the current analysis' (171).

However, in languages that might be seen as better candidates for 'minimal pair' status with SC, such as Bulgarian and Macedonian, the lack of the second position requirement does not result in a relaxation of clitic clustering. The similarities between the clitic clusters in these languages is merely superficial for Bošković; the Bulgarian/Macedonian clitic cluster is derived in the syntax via multiple adjunctions as verb and clitics move up the hierarchy of functional projections (see (2) below for one example).

One of the similarities between the clitic clusters is the internal ordering, particularly the appearance of the 3rd person singular auxiliary clitic that follows the dative and accusative pronominal clitics. Bošković demonstrates that despite this ordering at PF, the SC clitic auxiliary *je* 'is' appears higher in the syntax than the pronominal clitics (125–127). The PAC theory copes with such an apparent paradox with attractive ease; syntactically, *je* appears above the pronominal clitics but a 'lower' copy is pronounced at PF. The all-important question is what the motivation for this alternative spell-out might be. Bošković cites the fact that *je* is losing its clitic status, and suggests that this leads to either (i) a 'low level constraint' which forces the auxiliary to be either first or last in the cluster, and the final position is chosen 'arbitrarily', or (ii) loss of clitic status means '*je* does not allow cliticization across it but is not strong enough to serve as a clitic host itself', hence it is pronounced at the end of the clitic cluster (130). The discussion here becomes somewhat opaque in comparison to the laudably lucid style that characterises this book, and consequently it remains unclear what effect 'losing its clitic status' has in this synchronic analysis. Either (i) this adds a hybrid category to the typology of 'bound' and 'unbound' morphemes, or (ii) sometimes *je* behaves like a clitic, and sometimes like an unbound morpheme. In any case, a PF requirement forcing the clitic to appear at the right edge of the cluster, ignoring all other copies, does not obviously follow. This is disappointing, given that a template approach to the clitic cluster has been partly rejected in SC on the grounds that it is evidently stipulative (62).

Bošković assumes that the Bulgarian clitic auxiliary *e* 'is' is also subject to a requirement that it appear at the right edge of the clitic cluster (192), though it is not stated whether this also derives from a partial loss of clitic status or is a further coincidence. Either way, its membership of a 'clitic cluster' is essential in determining which copy of the verb is spelled out in (2a).

(2) (a) Toj go e vidjal.
he him.ACC is seen-participle
'He has seen him.'
(b) Toj ~~e~~ go ~~vidjal~~ e ~~go~~ vidjal ~~go vidjal go~~. (193)

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For Bošković, the syntax generates (2b), and the items crossed out are those subsequently deleted in the phonology. The pronominal clitic *go* 'it/him' cliticises in front of the verb, they both then move up to the auxiliary, and *e* + *go* + *vidjal* moves up as a whole to AgrO. The auxiliary cannot be pronounced in the first position because of the stipulation that it appear at the right edge of the cluster, so it is deleted and the pronominal clitic *go* is spelled out. The existence of the clitic cluster as an entity that can be referred to in the PAC mechanism then becomes of significance because pronunciation of the main verb in the highest position 'leads to a PF violation, namely it prevents the accusative and the auxiliary clitic from being parsed into the same clitic group' (193). As a result, the head of the verb chain is deleted and the next element to be pronounced is the copy of *e*, followed by the next copy of the verb, as indicated in (2b). Therefore, not only is the position of the auxiliary in the clitic cluster stipulated, but the Bulgarian clitic cluster is, in some way, a primitive of the system, and not simply derived via syntactic adjunction.

Notwithstanding these points, this work is for the most part written in a clear, well-organised way. It presents a startlingly elegant analysis of much previously recalcitrant data in South Slavic and brings to light a great deal of fresh data. It is an important contribution to the field as a whole, and within Slavic linguistics it has already set a new benchmark for any discussion of cliticisation.

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Chris Knight, Michael Studdert-Kennedy & James R. Hurford (eds.), *The evolutionary emergence of language: social functions and the origins of linguistic form*. Cambridge: Cambridge University Press, 2000. Pp. xi + 426.

Reviewed by PHILIP LIEBERMAN, Brown University

Over the course of decades spanning a century, linguists avoided addressing the question of how human linguistic ability might have evolved. The apparent problem was the tendency towards unsupported speculation in the heady period in which evolution first became part of the conceptual framework of human thought. This remained the case until the last decade, even through the Chomskian 'revolution', although the centerpiece of the theories promulgated by Noam Chomsky is a hypothetical innate 'Universal Grammar' that determines the syntax of all human languages. And syntax was taken to be the defining quality of human linguistic ability. An innate organ of the mind governing syntax must be instantiated in the brain through the expression of genes. Although this amounts to a strong biological claim, the evolution of the putative Universal Grammar was, like most aspects of biology, ignored by virtually all linguists and consigned to fringe groups outside the mainstream of linguistic inquiry.

In recent years that situation has changed and a series of conferences on the evolution of language has been organized by linguists. The volume in question is a collection of some of the contributions to the Second International Conference on the Evolution of Language, which took place in 1998. Not surprisingly, the focus of the conference was on syntax, which since Chomsky's early publications has generally been taken by linguists to be the factor that differentiates human language from the communications of other species. However, though the conference papers were delivered by many intelligent scholars, for the most part the studies reported in this volume exemplify the hermetic nature of linguistic research. The findings and procedures that have exemplified evolutionary biology since the time of Charles Darwin, and which one might expect to be applied to the study of the evolution of human language, are conspicuous by their absence. Unfortunately, this absence results in claims that have little or no grounding in data, often contravened by facts that are biological certainties.

Virtually all aspects of the theory that Darwin proposed in 1859 are consistent with current data. One of the principles introduced by Darwin amounts to the claim that, unless there is compelling evidence to the contrary, one should not assume that events in the past followed different principles. Historical evidence shows that languages slowly metamorphose into different forms over time, that the connections can be traced, and that

changes do not derive from genetic distinctions. It is, for example, clear that the indigenous inhabitants of China are no more genetically predisposed to learn a Chinese language than English, Latin, Walpiri or Sanskrit. And within any given group of human beings who live in a particular region and speak a particular language a range of genetic diversity exists. As Darwin (1859) noted, natural selection acts upon the variation that is always present in the state of nature. We know that natural selection will act to enhance fitness in human populations. Tibetans, for example, generally are adapted to extract more oxygen from air at high altitudes than other human populations. Natural selection over the course of 40,000 years has resulted in the survival in Tibet of those individuals who had the genetic disposition for more efficient oxygen transfer. If the hypothetical genetically determined Universal Grammar actually existed, we should have found that natural selection for the particular parameters which yield Tibetan linguistic competence should also have occurred within the same time span, given the indisputable contributions of language to biological fitness. In light of the substantial differences between Tibetan and English we would expect some delays or deficiencies in the acquisition of English by children of Tibetan ancestry who are born and raised in the United States, if their 'language' genes had been optimized for Tibetan, but that is not the case.

It is possible that the common elements that one can see in seemingly unrelated languages may derive from cultural transmission and from the indisputable fact that all human beings have a common ancestor. Except for a few isolated scholars, evolutionary biologists generally accept the theory that modern human beings evolved in the last 200,000 to 150,000 years in Africa. The first human group must have had a particular language. Over the course of time the group or groups of people speaking the ancestral language dispersed and different languages gradually formed. The human groups who left Africa and populated most of the world took this original language or group of related languages with them. A concerted attempt to reconstruct the vocabularies of languages in prehistory has been underway for decades. Unfortunately the findings of this endeavor (e.g. Ruhlen 1996), namely that words rather than syntax appears to be conserved, are absent in this volume except for peripheral citations in two papers whose focus is syntax, namely those by Mark Pagel ('The history, rate and pattern of world linguistic evolution') and Frederick J. Newmeyer ('On the reconstruction of "Proto-World" word order').

Moreover, the claim that human syntactic ability is innate needs to take into account the fact that the genetic distinctions between humans and chimpanzees are slight. The common ancestor of humans and living apes lived no more than seven million years ago; the genetic distinction between human beings and chimpanzees is somewhere between 5 and 2 per cent, depending on how differences are tabulated. And advances in molecular biology confirm the continuity of evolution – much of the genetic endowment

of humans is present in fruit flies. The question then arises, how could anyone account for the evolution of a detailed genetic program for complex syntax, the 'Universal Grammar', in this short span of time? The improbability of a genetic code that specifies the syntax of all human languages is reinforced by evidence from neurophysiological and behavioral studies, which show that the details of most motor acts, including walking, are not genetically specified – they are learned (see Lieberman 2000).

Moreover, syntax is not the unique 'derived' feature of human linguistic ability. Evolutionary biology differentiates 'primitive' features that are present in a species and species ancestral to it and related species. The possession of five digits, for example, is a primitive feature present in frogs and humans. Horses are differentiated by not having five toes. Comparative studies of living species, one of the most powerful tools of evolutionary biology, show that speech production is arguably the most derived feature of human language. The ape language studies of the Gardners and Savage-Rumbaugh & Rumbaugh show that apes can attain a vocabulary of about 150 words and can produce and comprehend simple syntax – but they cannot talk. In this sense, human lexical and syntactic ability are 'primitive' qualities, most likely present in the species ancestral to living apes and humans, arguing against the presence of a 'protolanguage' that lacked syntax.

In contrast, speech production is a 'derived' feature that characterizes human beings. The particular properties of speech permit communication at rates that exceed the fusion frequency of the auditory system, allowing long complex sentences to be comprehended within the attention span of verbal working memory. This would suggest that research on the evolution of the physiology and neural bases of human speech should concern linguists.

One of the papers in this volume, that of Andrew Carstairs-McCarthy ('The distinction between sentences and noun phrases: an impediment to language evolution?'), touches on the proposal that the neural mechanisms involved in speech production may have been the starting point for syntax, a suggestion that has previously been noted and developed. In a meaningful sense, syntax codifies relationships that hold between various aspects of behavior, which in their totality achieve a particular purpose. Lashley (1951) realized that this is the case and suggested that the 'roots' of human syntactic ability most likely lie in the neural structures that regulate motor control. The relationship that may hold between the neural mechanisms regulating motor control and syntax is discussed in detail in Kimura (1979) and Lieberman (2000).

In the volume in question, three papers specifically address the evolution of human speech. First, Peter MacNeilage & Barbara Davis, in their paper 'Evolution of speech: the relation between ontogeny and phylogeny', propose that basic syllable structure derives from opening and closing one's jaws. They further propose that the ontogenetic development of speech in

infants may reflect the evolutionary pattern. Neither proposal is novel: for instance, Muller (1848) and Jakobson (1940) also pointed out the possible relationship between the sequence in which sound patterns are acquired and the frequency with which they occur in the various languages of the world. Nonetheless, the MacNeilage & Davis paper makes specific predictions that can be experimentally tested and it provides a refreshing starting point for further research.

Second, the paper contributed by Michael Studdert-Kennedy, 'Evolutionary implications of the particulate principle: imitation and the dissociation of phonetic form from semantic function', focuses on the 'particulate' nature of speech, citing phonetic transcriptions and alphabetic orthography, downplaying the fact that speech is encoded into syllable-sized units. Given Studdert-Kennedy's long association with Haskins Laboratories, where many of the seminal studies on speech encoding originated, this omission is difficult to understand. Studdert-Kennedy cites alphabetic systems of orthography as evidence for the psychological reality of particulate phonemes, but he disregards the fact that other successful orthographic systems code entire syllables and words, as is the case for Chinese. And there is much evidence that syllables are primary units in both the production and perception of speech. For example, it is extremely difficult for monolingual speakers of English to produce words that begin with the final [ŋ] of the English word *sing*. Studdert-Kennedy also claims that meaning plays no role in the process by which children imitate sounds – a most unlikely conclusion. Even dogs pay attention to the meaning of a word: every dog owner knows that dogs quickly learn to respond to words such as 'out' or 'biscuit'. In contrast, consider the third paper focusing on speech, by Marilyn Vihman & Rory A. Depaolis, 'The role of mimesis in infant language development: evidence for phylogeny'. These authors disagree with Studdert-Kennedy and discuss the role of imitation in the acquisition of speech by children.

Chris Knight, in an interesting contribution on 'The evolution of cooperative communication', points to the role of play in the acquisition of language; however, he neglects the body of evidence documented by Greenfield (1991) which argues for a gradual transition from gestures to vocal signals in the course of human language development. Knight instead favors a model that leads to an abrupt saltation from 'protolanguage' to the sudden appearance of full human linguistic ability.

A number of papers propose various aspects of human behavior in which language may have enhanced Darwinian biological fitness, thereby resulting in natural selection for linguistic ability: cooperation (Knight); politics (Jean-Louis Dessalles, 'Language and hominid politics'); 'secret female languages' (Camilla Power, 'Secret language use at female initiation: bounding gossiping communities'); social transmission (James R. Hurford, 'Social transmission favours linguistic generalisation') and other social forces are all noted. All of these factors may have contributed to

biological fitness – the survival of progeny and the ultimate evolution of human language through Darwinian natural selection. The synergy between linguistic ability and these activities may very well have enhanced fitness and led to natural selection for language. However, it is difficult to identify any aspect of human behavior (excepting suicide, protracted warfare, and tendencies in males towards misogyny) that would not profit from the exchange of information by means of language, thereby enhancing fitness.

In contrast, several papers take the position that natural selection played a minor part in the evolution of human linguistic ability. Although biologists – including Darwin (1859) – acknowledge the role of chance events in shaping the course of evolution, natural selection acting on variation clearly is the key element in evolution, as Mayr (1982) notes. The argument that the evolution of human language is somehow different would have to be supported by compelling evidence. However, various papers in this volume present computational models rather than data to support their claim. For example, Simon Kirby, in 'Syntax without natural selection: how compositionality emerges from vocabulary in a population of learners', appears to claim that syntax develops because people acquire many words, but his model starts with individuals who are preloaded with context-free grammars at the start of the evolutionary process which leads to complex grammars. In short, Kirby starts with a built-in grammar to prove that grammar develops spontaneously. Bart de Boer, in 'Emergence of sound systems through self-organisation', presents a different computational model, which claims that vowel distinctions 'emerge' without 'evolution-based explanations for the universal tendencies of vowel systems' (193). Studies that date back to Muller (1848), who noted that the vowels [i], [u] and [a] occur most frequently in human languages, refute this position. Paraphrasing George Orwell, some sounds are more equal than others. For example, Stevens' seminal paper (1972) shows that these vowels have physiological and perceptual properties that are better adapted for vocal communication. Although de Boer references Stevens (1972), he ignores Stevens' findings, as well as Lindblom's and Labov's studies of sound change. Olmsted's (1971) study, showing that children first acquire the speech sounds which are most perceptually salient and which also occur most often in different languages, is neither referenced nor noted.

A tendency to rely on computer model studies rather than data documenting the utility of syntax characterizes other contributions. Jason Noble, in 'Cooperation, competition and the evolution of prelinguistic communication', makes use of game theory simulation to attempt to account for a transition from 'prelinguistic' to linguistic communication, disputing results from other simulations. Observations of the actual communicative abilities of apes and other animals which might provide insights into 'prelinguistic' communication are absent. David Lightfoot, in 'The spandrels of the linguistic genotype', takes as a given an innate Universal Grammar. Lightfoot

claims that the putative 'linguistic genotype' is a 'spandrel' – derived fortuitously as the result of evolution directed towards a different end. Lightfoot's linguistic analysis invokes traces – presently relegated to the dust heap in Minimalist linguistic theory. Derek Bickerton, in his contribution 'How protolanguage became language', discusses the transition from a 'protolanguage' that lacked syntax to 'human' language. However, as noted above, cross-fostered chimpanzees who have been raised with signed or manually expressed human language comprehend sentences with simple syntax and produce simple 'utterances' (Savage-Rumbaugh & Rumbaugh 1993, Gardner & Gardner 1994). It is most unlikely that any archaic hominid communicated with a 'protolanguage' lacking syntax. Bickerton also claims that the brains of other species are incapable of controlling actions that must be 'maintained without external stimulation for long periods' (272), thereby rendering them incapable of comprehending or producing sentences. A reading of Jane Goodall's comprehensive account of chimpanzee behavior (1986) will inform the reader that this is not the case. Anticipatory tool preparation and chimpanzee warfare clearly involve long-term cognitive activity without external stimulation.

In short, the hermetic tradition of linguistic research pervades many of the papers in this volume. Computer simulations can be useful but they must take account of biological facts if they are to provide any insights on the course of evolution. And the broader framework of evolutionary biology must be considered. Studies that take account of the communicative and cognitive behavior of human beings and other species incorporating relevant anatomical, physiological, neurophysiological and genetic data as well as the archaeological record would better illuminate our understanding of the evolution of human language.

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Aditi Lahiri (ed.), *Analogy, levelling, markedness: principles of change in phonology and morphology* (Trends in Linguistics: Studies and Monographs **127**). Berlin: Mouton de Gruyter, 2000. Pp. viii + 385.

Reviewed by PATRICK HONEYBONE, Edge Hill College of Higher Education

This welcome volume illustrates once more how important historical data can be in linguistic argumentation and how exciting work on the diachronic innovation of linguistic phenomena can be. Several of the volume's twelve chapters address key issues in contemporary linguistic theory and the light that diachronic data can shed on them; others make original contributions to our understanding of the innovation and diachrony of specific linguistic phenomena. The volume is multi-authored and contains eleven original articles and an introduction, which I refer to here as chapters 1 to 12.

The introductory chapter 1, written by the editor, is an interesting piece, if something of a missed opportunity. It does not summarize the following chapters or explain how they connect with each other or in what way they engage with the notions of analogy and markedness. In fact, the volume is not completely coherent in terms of the theoretical issues or the types of data discussed, and several of the papers do not obviously deal with analogical change or even with notions of markedness. Given the nature of the introduction, the only real way to discover this is to read through all the articles. This is, indeed, no bad thing to do, but it is likely that different readers will be interested in different articles, as should become clear from the chapter descriptions below.

The introduction provides a brief historical overview of the notions of 'analogy' and 'markedness', tracing the former through usages in other

disciplines up to its seventeenth century usage in linguistics and its later famous exploitation by the neogrammarians in attempts to explain non-regular linguistic change. Putative universals of directionality in analogy are discussed, along with the work of Kiparsky (e.g. 1968), who first proposed a generative understanding of analogy as grammar simplification. Given that the chapters are predominantly generative, it is not surprising that Kiparsky's influence is visible in much of the book; this is also reflected in the volume's dedication to him.

The eleven main articles are loosely grouped by their sequencing. Chapter 2 is Paul Kiparsky's 'Analogy as optimization: "exceptions" to Sievers' Law in Gothic', chapter 3 is B. Elan Dresher's 'Analogical levelling of vowel length in West Germanic', 4 is Aditi Lahiri's 'Hierarchical restructuring in the creation of verbal morphology in Bengali and Germanic: evidence from phonology', 5 is Renate Raffelsiefen's 'Constraints on schwa apocope in Middle High German', 6 is Frans Plank's 'Morphological re-activation and phonological alternations: evidence for voiceless restructuring in German', 7 is Wolfgang Ullrich Wurzel's 'Inflectional systems and markedness', 8 is Carlos Gussenhoven's 'On the origin and development of the Central Franconian tone contrast', 9 is Tomas Riad's 'The origin of Danish *stød*', 10 is Paula Fikkert's 'Prosodic variation in "Lutgart"', 11 is Haike Jacobs' 'The revenge of the uneven trochee: Latin main stress, metrical constituency, stress-related phenomena and OT' and 12 is Richard M. Hogg's 'On the (non-)existence of High Vowel Deletion'.

In chapter 2, Kiparsky discusses analogical levelling in Gothic nominal and verbal inflectional morphology, taking an essentially OT perspective for his elegant analysis. However, crucial use is made of uniquely specified underlying representations ('inputs'), arguing against the standard OT notion of 'Richness of the Base'. Claiming that all of the discussed cases of levelling can be accounted for by changes to better satisfy one constraint (STEM-FORM, which militates against stems ending in a short vowel), he argues that (i) in line with his previous analyses of analogy as grammar change, all changes involve the simplification of the grammar, and not simple generalization from surface forms, and that (ii) this illustrates the potential of his model to predict the direction of change in that 'each reinforcement of STEM-FORM has a "snowball" effect which adds to the structural pressure for subsequent innovations' (40).

Dresher's chapter 3 takes an anti-OT rule-based approach to the much-debated case of Open Syllable Lengthening (OSL) in Middle English (MEOSL), also addressing cases of OSL in Middle Dutch and Middle High German. Dresher argues that the new orthodoxy on MEOSL (following Minkova 1982, who proposed that there was in fact no OSL, but that it was rather a case of compensatory lengthening) is mistaken. He argues that the kinds of word counts used by Minkova are severely flawed as they do not take into account the paradigmatic alternations that the words belonged to.

Instead, he argues that analogy should be reinstated as a crucial part of the understanding of MEOSL and that the type of analogy which occurred in this case is predicted precisely to be as random in terms of directionality as he shows it to be (the length of the vowel levels unpredictably to that of the singular or plural).

The editor's own contribution, chapter 4, is an important discussion of patterns in grammaticalization, focusing on two cases where auxiliary verbs have diachronically fused to lexical verbs to create new affixes. The case-studies involve the creation of the progressive and perfect in Bengali and of the weak-verb dental preterite in Germanic; their importance lies in the fact that Lahiri shows them to be cases where the ex-auxiliary is not simply fused to the verb as one morpheme, but is analysed as two separate morphemes, with the original root of the auxiliary reanalysed as a class marker (MORPHEME$_1$) and its inflections as a separate inflectional suffix (MORPHEME$_2$). Intriguingly, Lahiri shows through the careful consideration of phonological processes (understood as ordered rules), which are known to occur within specific phonological domains, that the actual domain bracketing in these cases is [[ROOT + MORPHEME$_1$] + MORPHEME$_2$].

Raffelsiefen's chapter 5 argues that a constraint-based analysis of final schwa deletion in Middle High German is more insightful than previous rule-based analyses because it avoids the problems caused by the restrictions on the process due to lexical, semantic and inflectional-class membership criteria. One important aspect of her analysis is that the focus is changed from (i) the rule-based PROMOTION of an active process in certain environments to (ii) the INHIBITION of an entirely general process (formalized as *SCHWA) which is imposed by a set of constraints. Ranked in OT-style, Raffelsiefen has to formulate some of the constraints very precisely; one crucial constraint is LEVEL, which requires that '[a]ll stem consonants must occupy the same syllable position in each member of a paradigm' (137), where 'paradigm' is further restricted to the forms of one tense of a verb; the constraint will presumably need to be formulated more generally to do work in other cases of analogy. LEVEL can essentially be seen as an OT-ification of the neogrammarian position on analogy, much as LAZY (e.g. Kirchner 2000) names the age-old idea that change occurs when ease of articulation wins out over the preservation of inherited or underlying forms.

In an informal chapter 6, Plank discusses cases of the detachment of one member from an inflectional paradigm, such as German *weg* 'away', which separated off from the noun *Weg* 'way'. This case is quite well-known in generative historical phonology as its behaviour is instructive in the case of the loss of Final Obstruent Devoicing (FOD) in Germanic dialects. This is because, where FOD has been lost, no voicing surfaces finally in *weg*, as it has been detached from *Weg* (where voice does indeed surface in the final segment, as it was involved in morphonological alternations which are best analysed, in rule-based approaches, as deriving from an underlyingly voiced

obstruent). Plank presents some important novel evidence weighing on these cases from instances where words such as *weg* are semi-consciously re-inflected.

Wurzel's chapter 7 investigates synchronic German nominal inflection in detail. He presents an analysis of the patterns of inflection based on lexically specified marks and 'regular' Paradigm Structure Conditions, which dictate the type of inflection according to phonological, syntactic or semantic features. The discussion involves the notion of markedness, and Wurzel makes it clear that he considers this to be crucially linked to diachronic predictions in patterns of change; he further shows how his model predicts some of the types of changes which have occurred in the recent past in German nominal class affiliation.

In an important chapter 8, Gussenhoven investigates the remarkable rise of a lexical tone contrast and connected changes in the intonational systems of the Central Franconian area of the Dutch–German dialect continuum. He ties in the innovations, which he separates out into four separate neogrammarian-type post-lexical changes, with an articulated model of (either ergonomic or social) motivations for phonological change. The case of tonogenesis discussed involves the innovation of a highly marked feature, but Gussenhoven ingeniously argues that this can be understood as a compromise attempt to sound like a neighbouring speech community while at the same time maintaining a contrast in inflectional paradigms. The only thing lacking is evidence for the social motivation for the change – the unacknowledged problem being that it is difficult to reconstruct the social pressures which might have caused the speakers who innovated the tone contrast to want to sound like their neighbours.

Riad's chapter 9 deals with the intriguing development of *stød* in Standard Danish. He adopts the standard position that *stød* is a development of the forms of tonal accent which still exist in dialects of Swedish and Norwegian. He claims that *stød* is a basically predictable realization of a high followed by a low tone within the same syllable and traces this development and some related tonal phenomena in other Scandinavian dialects, arguing that the high-low sequences of tone, now realized as *stød*, derive from a set of reanalyses of the patterns of highs and lows which previously existed in the tone accent systems. He addresses issues of the chronology, sociology and geographical direction of some of the innovations that he discusses, showing how, in certain cases, the marked tonal accent was generalized at the expense of the unmarked.

In chapter 10, Fikkert argues for a particular description of the word stress system and certain other phonological features of Middle Dutch, using metrical and rhyme evidence from the lengthy poem *Sente Lutgart*, which Zonnefeld (e.g. 1998) has shown to be written in quite classical iambic tetrameter. Fikkert argues against Zonnefeld's own analysis to claim that *Lutgart* provides evidence that Middle Dutch word stress was considerably

different from that of Modern Dutch, and much closer to that of earlier stages of other West Germanic languages. Part of the evidence derives from a reconsideration of the dating of Open Syllable Lengthening in Dutch. Fikkert shows that certain cases of variability in the manuscript's metrical patterns are in fact predicted if typical Germanic foot structure (as described by, for example, Dresher & Lahiri 1991) is assumed and if OSL was not completed at the time that it was written.

Jacobs' chapter 11, a compact OT paper, shows how several problems which the author identifies in previous work, such as the analysis of Latin word stress in Prince & Smolensky (1993), can be straightforwardly solved if a small number of novel assumptions are made: these involve the reranking of the constraints used, so as to allow a type of foot previously disallowed, and a simplification in the formulation of a constraint. The key problems are that the previous analyses predict impossible stress systems and fail to account for all occurrences of related phonological processes.

In the final chapter, Hogg thoughtfully considers some generally overlooked aspects of High Vowel Deletion in Old English, something which has frequently been discussed in anglicist and theoretical literature. He shows that the process as it is generally conceived not only involves a clear example of opacity, but that the opacity is actually created by the very operation of the process. Hogg also shows how certain previous analyses have, problematically, conflated data from several dialects and periods in the history of English. While ultimately non-committal as to the best analysis of the data, Hogg sketches aspects of an OT analysis, claiming that this may resolve some of the problems of rule-based analyses. He claims that the new analysis is superior as it involves cases where two candidates tie in terms of constraint violations on the OT tableaux which he presents, predicting that certain types of variation should exist in the forms found in manuscripts, and this variation is, indeed, attested. He does not consider the problems that have been recognized for such accounts of variation in OT, however, namely that it is almost inconceivable that two candidates could ever really tie if they are considered against all the (universal?) constraints in the hierarchy.

Any such multi-authored volume as this could be evaluated in its totality according to several criteria: whether it is coherent in terms of (i) the linguistic subdisciplines involved, (ii) the theoretical and data issues discussed and the theoretical viewpoints and frameworks represented, (iii) the languages covered; and as to whether it includes persuasive (iv) records of new data and/or (v) reinterpretations of old data. The volume holds up well against most of these criteria, if not quite consistently all.

In terms of (i), the mixture of articles on phonology, morphology and philology is neither disturbingly diverse, nor is it closely focused, especially given that a range of types of phonology are covered: segmental, low-level suprasegmental (e.g. stress) and high-level suprasegmental (e.g. intonation).

In connection with (ii), a range of issues crop up, including tonogenesis and tone-death, grammaticalization, the types of evidence available for diachronic linguistics and the correct ways to interpret them, the use of constraints versus the use of rules, markedness and the possible paths in the direction of analogy. Certain processes are addressed in more than one chapter, including High Vowel Deletion, Open Syllable Lengthening and Final Obstruent Devoicing. One key theoretical theme which emerges (very clearly in chapters 2, 3, 6 but also elsewhere) is a reinforcement of the importance of 'non-surface' (or 'non-output') levels in phonology. This focus on underlying representations/inputs and on lexical levels of the types used in Lexical Phonology runs counter to the mainstream of much current phonology, but finds reinforcement in other recent historical work, both within OT (e.g. Bermúdez-Otero 1999) and in non-OT work (e.g. McMahon 2000), all of which argues that some recognition of non-surface levels is necessary in order to make sense of phonology. In terms of (iii), the volume is in line with a great tradition of historical linguistics in having its focus distinctly on the Germanic languages. Indeed, the only real exceptions to this are the discussion of Bengali and Latin in chapters 4 and 11. The volume performs well on criteria (iv) and (v), with novel data (for example, in chapters 6 and 8) and credible novel analyses of well-known data in most of the other chapters.

Despite the coherence that some of these points provide, this is likely to be the kind of book that readers will consult if they know that a particular article is in it, rather than the kind that will be read from cover to cover. This impression is strengthened by the fact that, although there are several useful and extensive indexes, there is little cross-referencing between articles, even when they address similar points. A book like this will ultimately stand or fall on the value of its individual contributions, and several of those included here are compelling and important. Their intrinsic merit makes the book an important purchase for any research library, and several individual chapters should be required reading for linguists with interests in the points listed under (ii), in both historical and synchronic linguistics.

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Pieter Muysken, *Bilingual speech: a typology of code-mixing*. Cambridge: Cambridge University Press, 2000. Pp. xvi + 306.

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In the contentious world of code-mixing (CM)[1] research, where most contributions are little more than attempts to discredit earlier work, and each successive model proclaims universal applicability to all existing and future bilingual data, Pieter Muysken's has always been the voice of reason. In contrast to the prevailing emphasis on the uniqueness of code-mixing theories, Muysken's efforts have been directed to understanding how they resemble each other, and where (and why) their predictions overlap. *Bilingual speech* is the culmination of over twenty years of such efforts to make sense of the diverse and often contradictory CM literature, viewed through the lens of a tripartite division of CM that Muysken views as his 'main contribution' (32): INSERTION of material from one language into structure from the other language, ALTERNATION between the structures of the two languages and CONGRUENT LEXICALIZATION (CL) of material from different lexicons into a shared grammatical structure.[2] The goal is modest (perhaps necessarily so, given the state of the field): to 'tie together a set of intermediary results rather than giving a conclusive account' (2).

In the first chapter, 'The study of code-mixing', Muysken provides an overview of research on language mixture. He argues that the various

[1] Muysken uses the term 'code-mixing' to refer to 'all cases where lexical items and grammatical features from two languages appear in one sentence' (1), restricting the term 'code-switching' to a subset of CM. For the purposes of this review, we follow Muysken's terminology.

[2] One of the dominant traditions in CM research distinguishes insertion from alternation, in contrast to unitary theories that attempt to provide a single analysis for all CM. The three-way division (and the phenomena to be included under each) is original to Muysken.

grammatical constraints on CM that have been proposed (e.g. Poplack's (1980) Free Morpheme and Equivalence Constraints; di Sciullo, Muysken & Singh's (1986) Government Constraint; Myers-Scotton's (1993) Matrix Language Framework) can be characterized in terms of each of the three CM processes. Noting that the field has moved from constraints specific to particular language pairs to more universal principles, he argues further that all constraints can be reduced to a number of 'primitives' involving issues of equivalence (categorial or syntactic), clausal peripherality and the role of function words. Muysken makes the bold claim that CM is 'impossible in principle' (30) and views the three CM strategies as 'escape hatches' within a unified theory of bilingual speech.

The second chapter, 'Differences and similarities between languages', provides theoretical perspectives on language differences and on the division of labor between grammar and the lexicon. If all such differences are ultimately lexical, in many cases reflecting the requirements of function words, potential violations of CM processes can be attributed to the (in)compatibility of function words in the two languages. Reviewing the mismatches between lexical and grammatical structures as dimensions in typological classification, Muysken arrives at his working hypothesis that differences between languages result from differences in the interaction of different autonomous modules: specifically, whether information is encoded lexically or grammatically (51).

In the next three chapters, Muysken discusses each of the three CM strategies individually, providing a wealth of examples culled from a broad range of studies. Chapter 3, 'Insertion', examines the grammatical dimensions of INSERTIONAL CM, uniting lexical borrowing, nonce borrowing (Sankoff, Poplack & Vanniarajan 1990) and constituent insertion (Naït-M'barek & Sankoff 1988). Insertions, which are morphologically integrated lexical (rather than functional) elements, form a single syntactic constituent, usually an object or complement (rather than an adjunct) which exhibits a nested structure ($L_A [_A L_B]$). Insertion implies the existence of a base or matrix language (ML) and considerations of syntactic dependency, but Muysken concludes that problems in determining the ML are empirical rather than theoretical (68). He invokes the notion of government (di Sciullo et al. 1986) to account for observed selectional restrictions. Although insertional CM bears obvious similarities to lexical borrowing, Muysken insists that the former is 'supralexical', while the latter is 'sublexical' and 'listed' (i.e. 'part of a memorized list which has gained acceptance within a particular speech community' (71)).

Chapter 4, 'Alternation', discusses the properties of ALTERNATIONAL CM. Unlike insertion, alternation involves the switch of longer, more complex elements, typically multiword constituents in a non-nested sequence ($L_A \ldots L_B$). Alternation is a process characterized by the absence of selectional restrictions (other than equivalence) in which clausally peripheral

elements such as adverbials and discourse particles often figure. Reviewing CM models formulated in terms of phrase structure, Muysken concludes that linear equivalence is better conceptualized as a subset of categorial equivalence and that equivalence constraints should be oriented to surface structures rather than deep structures, in contrast with models that adhere to Chomskyan syntactic theories.

The consequences of CM for grammatical convergence and linguistic variation are outlined in chapter 5, 'Congruent lexicalization'. Unlike insertion and alternation, which impose strict grammatical requirements on CM, 'anything goes' in CL (128). Constituents may be single or multiple (or not even constituents at all) and may belong to any category (lexical or functional). CL is bidirectional, characterized by back-and-forth switching and the existence of 'diamorphs' (words that are homophonous in the two languages).

Muysken examines in more detail the importance of function words in CM (the 'functional element effect') in chapter 6, 'Function words', and argues that their restricted participation in CM is a result of the non-equivalence of function words across languages rather than considerations of language production (contra Myers-Scotton 1993). Reviewing the different definitions of function words, Muysken concludes that the functional-lexical distinction is gradient and proposes a distinction couched in terms of cross-linguistic equivalence. According to Muysken, nouns and verbs are universal, but the featural complexes associated with each are language specific. For example, the insertion of an L_A verb triggers the L_A tense system.

Chapter 7, 'Bilingual verbs', considers the phenomenon of verbal compounds combining elements from two languages, manifested in different ways: unmarked verbs, verbs marked with native affixes and verbs adapted (morphologically or phonologically) before being morphosyntactically integrated. Muysken argues (215) that we cannot adopt a unitary analysis of bilingual verbs but rather must classify them into three types, corresponding to each of his CM strategies: nominalized verbs (insertion), adjoined verbs (alternation) or combinations of auxiliary and infinitive (CL).

In chapter 8, 'Variation in mixing patterns', Muysken relates the different CM strategies to psycholinguistic and social factors: language dominance, duration of contact, bilingual proficiency, speaker type, age-group or generation and language attitudes. He associates insertion with shorter-duration contact situations and more isolated groups (such as recent immigrant communities); alternation with communities with strong norms, competition between language groups and typological distance between languages; and CL with looser norms, balanced bilingualism and structurally parallel languages. Nonetheless, after attempting to characterize a number of bilingual communities according to CM strategies, Muysken correctly concludes that differences between them are not absolute, since various social factors can interact to yield mixed CM strategies within each community.

A final chapter, 'Code-mixing, bilingual speech and language change', relates CM strategies to bilingual production and to language change. Muysken argues that bilingual speech data constitute evidence for a 'simultaneous access' model of production, with different modules from each language activated. He cites Thomason & Kaufman (1988) in considering the relevance of CM strategies to processes of language contact, such as relexification, convergence, pidginization, lexical borrowing, second language acquisition and substratum effects. These processes, he suggests (268), can be viewed as the gradual importation of more and more structure from one language into another. Muysken concludes with a discussion of future avenues of research, such as the difference between content words and function words, the role of adjunction as a 'fallback' strategy ('potentially available … at moments when the grammar fails' (277)) and considerations of language separation and economy in interference.

To what extent will the model proposed in this ambitious volume succeed in providing a framework for the study of CM, or even a 'taxonomic phase' (2) in the study of CM data? This remains to be seen. While Muysken provides detailed theoretical justification and exemplification for his proposal to distinguish three different CM strategies, they have yet to be applied SYSTEMATICALLY to corpora of actual bilingual production. Insertion and alternation will be familiar to most readers, whether or not they subscribe to the distinction between borrowing and code-switching. The concept of 'congruent lexicalization' is more elusive, though likened by Muysken to monolingual (e.g. stylistic) variation. Muysken presents an eloquent plea to conflate CM and linguistic variation, 'to deal with variation and its quantitative coordinates in terms of variation in lexical insertion' (126). How can such an analysis be implemented? No heuristic is provided. Indeed, readers searching for empirical criteria to distinguish among the three CM types will be disappointed, since the same criteria are often offered for more than one type. For example, morphological integration is diagnostic of both insertion (64) and CL (134); CL may occur in borrowing, the main type of insertion (123); and phonological integration is variable in borrowing as well as in other types of CM (70). Muysken is not unaware of this dilemma, observing that 'it is impossible to *prove*, for every case, that it is alternation, insertion or CL. The best we can do is study patterning at the level of the whole corpus' (231). Identification of patterns requires quantitative analysis, raising the issue of method.

Muysken's methods are as eclectic as his ideas. He states at the outset that he will combine structural analysis with 'quantitative analysis as in the work of Labov and Sankoff' (2). Correctly rejecting the rule-and-exception paradigm so characteristic of CM theories, he endorses instead 'probabilistic statements, linked to different language pairs and contact settings' (28). A good deal of the discussion does in fact refer to percentages, many reproduced or adapted from the quantitative work of others. His examination

(chapter 8) of patterns of co-occurrence of CM types in different bilingual communities (or more accurately, data sets) relies particularly heavily on quantitative trends. He rightly concludes (247) that the variation in mixing patterns is not explicable by a single factor, a situation which lends itself perfectly to multivariate analysis. Muysken's assertion that a probabilistic model is 'only possible in the abstract at present' (249) is puzzling, given the large body of empirical work that has successfully implemented this model, e.g. in predicting syntactic sites likely to host a switch, in a preference for different CM types in different bilingual communities, and in distinguishing different language contact phenomena. By the volume's end, he has dismissed this approach: 'we have reached the limits of what can be learned about [CM] using the Labovian techniques introduced in the 1960s' (250). Yet most of the limitations he enumerates, such as lack of diachronic perspective and the primacy of syntax (250), result from the prevailing research climate rather than the Labovian/quantitative paradigm, which is amply equipped to handle them. (Indeed, one could argue that the reason CL remains the weakest link in Muysken's tripartite division is precisely because its nature (and even its existence) have not been subjected to the rigors of the variationist method.) An exclusive 'reliance on corpora of spontaneous bilingual speech' (250) is endemic to variationism, a fact which Muysken laments – curiously, since the whole of chapter 8 (and much of the rest of the volume) would have been impossible without such corpora. His call for greater reliance on experimental data (249) contradicts the problems he cites (28f.) as inherent to them, and places unwarranted faith in the ability of elicitation and experimentation to resolve the outstanding problems in his analysis.

In short, while there is much to quibble with here, this should not obscure the remarkable achievements of this original and exciting book. Its scope, cogent argumentation and vast range of examples are all testaments to the breadth of Muysken's interests. He has undertaken the daunting task of confronting the formidable literature on CM and has attempted to ground its findings in research from diverse fields. Perhaps the greatest contribution of this volume is its success in conveying 'the excitement of working in a field that is moving quite rapidly and is located at the crossroads of structural analysis, sociolinguistics and psycholinguistics' (278).

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Henk van Riemsdijk (ed.), *Clitics in the languages of Europe* (Empirical Approaches to Language Typology). Berlin: Mouton de Gruyter, 1999. Pp. xxii + 1026.

Reviewed by CATHERINE RUDIN, Wayne State College

The first striking fact about this volume is its sheer bulk: at well over 1,000 pages, surely it must contain everything you ever wanted to know about clitics? And indeed, it does provide considerable depth and breadth of information, along with a sense of the ongoing theoretical controversies surrounding clitics. The book caps five years of collaboration by one of the working groups of the 'Typology of Languages in Europe' (EUROTYP) project. As such, it brings together the work of numerous experts in an unusually coherent format, with interlinking among chapters and a rare degree of give and take among the authors, providing an excellent overview of the state of clitic studies in the late 1990s.

After the editor's introduction, the volume is organized into two unequal parts, plus a 118-page appendix . Part I, 'Area studies', contains three overview articles totaling 110 pages. Part II, 'Theory', is 742 pages long. It consists of two 'Feature articles', each with peer comments by several scholars and a reply by the author(s), and five 'Topics' sections: clusters of one to five articles on related issues. The volume also contains lists of abbreviations and of contributors' addresses, two prefaces, and three indexes (languages, names and subjects).

Van Riemsdijk's introductory article, 'Clitics: a state-of-the-art report', effectively sets the background for the volume, summarizing major viewpoints on the properties and analysis of clitics from Kayne (1975) and Zwicky (1977) through much more recent work.

The 'area' (actually family/subfamily) overview articles in Part I are by Anna Cardinaletti on Germanic and Romance, Mila Dimitrova-Vulchanova

on Slavic, and Lars Hellan & Christer Platzack on Scandinavian languages. Somewhat surprisingly, there is no overview of the Balkan area, a well-studied geographical group of languages with fairly unified clitic behavior, nor any mention of Celtic or any of the non-Indo-European languages of Europe. All three articles focus on clitic pronouns, with little attention to other clitics. Cardinaletti argues convincingly for a three-way classification of pronouns as strong, weak or clitic, and shows that this classification applies in various Germanic languages as well as in Romance. Hellan & Platzack extend Cardinaletti's tripartite classification to spoken varieties of Scandinavian languages, which have clitics as well as the weak pronouns of literary Germanic. Their analysis, in terms of adjunction to V on different cycles, is explicitly tentative, but their data are complex and fascinating. Dimitrova-Vulchanova's article departs from the others in this section, concentrating on clitic placement rather than clitic vs. non-clitic forms. Unfortunately, her central claim, that clitics in Slavic languages are enclitic to a vague entity called 'FRONT' (essentially, anything that precedes the clitics), is highly problematic; the many stipulations and exceptions required by this analysis are all unnecessary under a more standard view of Bulgarian clause structure in which clitics are syntactically proclitic to V (see e.g. Tomić 1996, Rudin et al. 1999). This article deals mostly with Bulgarian, though Macedonian, Serbo-Croatian and Czech are also briefly considered.

The feature articles and peer commentaries in Part IIA are the heart of the volume: both articles are deep and thought-provoking, and the dialogue between authors and commentators is particularly valuable. The first feature article, Anna Cardinaletti & Michal Starke's 'The typology of structural deficiency: a case study of the three classes of pronouns', details universal semantic, syntactic, morphological and phonological differences between the strong, weak and clitic pronoun types introduced in Cardinaletti's earlier paper, and proposes as the underlying source of all these differences that clitic pronouns instantiate fewer morphemes (functional heads) than weak ones, which in turn instantiate fewer than strong ones. Strong as opposed to weak or clitic pronouns must have [+human] referents, can be modified, coordinated, dislocated, clefted or occur in base θ positions. Clitics, unlike weak or strong pronouns, are X^0 rather than XP, can occur in doubling constructions, form clusters with phonological idiosyncrasies, and so on. Some of the article's claims strike me as too strong; for instance, English *it*, a non-human and therefore weak pronoun, should never be coordinated, but (1) seems fine:

(1) Is this cup new? Yes, I got it and that blue one at the auction.

However, the article is admirably clear in its claims and assumptions, and presents a great deal of insightful data. The details of the explanation in terms of numbers of functional heads in the pronoun's syntactic structure are

quite theory-dependent, but the idea that deficient pronouns lack certain functional features could translate into different frameworks.

The article is followed by commentaries by Josef Bayer, Molly Diesing, Carmen Dobrovie-Sorin, Liliane Haegeman, Anders Holmberg and Juan Uriagereka. By and large, these commentators all accept Cardinaletti & Starke's tripartition of pronoun types, though several raise doubts about whether the tripartite division is as clear, absolute and universal as claimed. Bayer argues that the facts concerning differences among the pronoun types in humanness, case, modifiability, and so on are not entirely watertight; Diesing and Dobrovie-Sorin make similar points; while Holmberg raises the issue of how demonstratives fit into the classification. On the other hand, nearly all the commentators have significant objections to Cardinaletti & Starke's explanation in terms of degrees of morphological deficiency (lack of functional categories/heads), on both semantic and syntactic grounds. Dobrovie-Sorin proposes an alternative explanation, that the distinction between strong and weak/clitic pronouns is syntactic (strong pronouns are DPs while the others are not), but the distinction between weak and clitic pronouns (as well as agreement affixes) is not syntactic but morphophonological; these types differ essentially in their degree of incorporation to a host. While this explanation is perhaps less unified and neat-looking than Cardinaletti & Starke's, it strikes me as more likely to be correct. Uriagereka and Diesing both point out problems relating to the semantics of pronoun reference, as well as various, largely theory-internal syntactic issues. Haegeman is the only commentator who raises no objections at all; in fact, she argues that some West Flemish pronouns which at first sight seem problematic for the tripartite division are actually demonstratives and thus not counterexamples. On the whole, regardless of the eventual fate of their analysis, Cardinaletti & Starke are to be commended for setting forth a bold, strong proposal which stimulated thoughtful replies and whose descriptive core is solidly perceptive.

The second feature article is by Joseph Emonds, 'How clitics license null phrases: a theory of the lexical interface'. Using mostly French data, Emonds argues that clitic constructions, especially including 'clitic climbing', involve no movement. Clitics have only grammatical, not lexical, features, and thus can be 'Alternatively Realized' as X^0 grammatical morphemes under an X^0 sister of the position a full XP corresponding to the clitic would occupy. The 'Invisible Category Principle' allows the XP to be empty if all its features are realized elsewhere (by the clitic). For all clitic constructions to have the required tree-structural relation, Emonds must show that apparent clitic climbing constructions are actually monoclausal.

Peer comments on this article are by Mark C. Baker, Marcel den Dikken, Frank Drijkoningen, Hubert Haider, Marco Haverkort and Tarald Taraldsen. Nearly all the commentators praise Emonds' analysis for its explicitness and elegance, and Baker provides additional support for the

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analysis by contrasting the behavior of clitics with that of incorporated nouns, which do have lexical content. However, they also raise numerous problematic issues: semantic features of restructuring verbs (den Dikken); the behavior of adverbial clitics like French *y* and *en* (Drijkonigen, Haverkort, Taraldsen); extension of the analysis to Germanic (Haider); and obligatoriness of certain clitics (Taraldsen), among others. Several of the commentators argue that a movement analysis is superior to Emonds' Lexical Insertion analysis, or at least equally able to account for the facts.

The next section of the book, Part IIB, with several thematic groups of articles, is more like the usual collection of papers: each article is a self-contained, relatively independent unit, dealing in most cases with a single language, though there are a number of common themes and points of contact among them. By far the majority are concerned with syntax (even the few articles on phonology or semantics also discuss syntax), and issues of movement vs. non-movement of clitics, what triggers movement or licenses clitic positions, the relation of clitics to DPs in doubling or non-doubling constructions, and the relation of clitics to the functional categories at the left edge of the clause arise repeatedly.

The first topic, 'Clitic clusters and the Wackernagel position', includes articles by Damir Čavar & Chris Wilder on 'clitic third' constructions in Croatian, Mila Dimitrova-Vulchanova & Lars Hellan on Bulgarian clausal clitics, and Carmen Dobrovie-Sorin on adjunction to I vs. IP in Romanian. Rather oddly, given the section title, two of the three articles are about languages (Bulgarian and Romanian) in which clitics are not in Wackernagel's (second) position, but verb-adjacent.

The next group, 'Functional categories and the position of clitics', continues the theme of clitic positioning within the clause, with articles by Adriana Belletti on Italian and other Romance languages, Cecilia Poletto on Northern Italian, Ian Roberts on Welsh and Romance, Alain Rouveret on European Portuguese, and Dominique Sportiche on French. Of these, I found Poletto's and Roberts' papers the most interesting, for their close attention to less commonly discussed data. Poletto examines subject clitics and subject agreement in 100(!) Northern Italian dialects, concluding that syntactic behavior parallels morphological type. Roberts' is the only paper in the volume to focus on Celtic data (though, as he points out, 'the Celtic languages all have very rich clitic systems' (622)). The main thrust of the article is that while French clitics move to Agr, Welsh ones are generated there; Breton and Irish data are also considered. Unfortunately, some examples are unglossed or otherwise hard to interpret, and the argumentation is sometimes less than totally clear.

The third topic, 'Clitics and scrambling,' contains articles by Kathrin Cooper on Zurich German and Gunlög Josefsson on Scandinavian. Both authors return to the theme, prominent throughout the volume, of what distinguishes clitics from other types of pronouns or DPs, particularly

including syntactic position. Cooper's paper is another one which presents fascinating and little-known data.

The fourth topical group, 'Semantic features', consists of articles by Elena Anagnostopoulou on clitic doubling as a reflex of semantic referentiality in Greek and universally, and Norbert Corver & Denis Delfitto on pronoun movement. Using Cardinaletti & Starke's idea that full pronouns must be [+human], Corver & Delfitto make a surprisingly convincing case for lack of [+human] specification as a movement trigger.

Finally, the section entitled 'Phonological aspects' contains just one article, Marina Nespor's 'Phonology of clitic groups', which identifies the clitic group as a phonological entity distinct from both prosodic word and prosodic phrase. Nespor also offers a clear discussion of syntactic vs. phonological cliticization and argues that the two are fully independent. Several articles in the syntactic sections, particularly Cooper's and Dobrovie-Sorin's, also consider syntactic vs. phonological cliticization.

A unique feature of the book is the very long appendix by Riet Vos & Ludmila Veselovská – almost a book in itself – of raw clitic questionnaire data. A series of yes-no questions (e.g. 'Can a weak pronoun appear after negation?') are presented along with charts summarizing answers to each question in 29 languages (mostly European, but also including Berber and Hebrew) and examples from each language. Although this material is obviously somewhat superficial, compressed and incomplete as a description of the clitic system of any given language, it nonetheless represents a wealth of information, potentially extremely useful as a starting point for investigation of the patterns revealed, or as a check on overly glib universality claims.

The volume is well edited on the whole, with relatively few errors and none which cause serious confusion. However, a more standardized presentation of examples would have been welcome; some authors gloss much more fully than others, and there are inconsistencies in details such as whether Greek examples are given in Greek alphabet or transcribed and whether interlinear glosses are lined up under the words they translate.

In summary, this is a very valuable book, though perhaps few will read it from cover to cover. The feature articles and replies are excellent examples of useful dialogue and polite disagreement among experts; they would be particularly useful to assign for courses in linguistic argumentation. Virtually all the individual articles, even those I have not had space to comment on specifically, contain valuable insights into data, theory or both. Van Riemsdijk and the entire EUROTYP working group are to be congratulated on a monumental achievement.

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Geoffrey Sampson, *Empirical linguistics*. London & New York: Continuum, 2001. Pp. viii + 226.

Reviewed by SHRAVAN VASISHTH, Saarland University

Empirical linguistics (EL) is a slightly revised and updated collection of ten previously published articles. The main theme of the book is the importance (and near-absence) of empirical methods in linguistic research. In this review, a summary of the chapters is presented first, and this is followed by a discussion of some of the conclusions of the various chapters.

Chapter 1, 'Introduction', summarizes the contents by chapter, and provides some background to the book.

Chapter 2, 'From central embedding to empirical linguistics', is an entertaining discussion regarding the common misconception in linguistics that centre (or central) embeddings occur only rarely in language. Sampson presents one example after another of multiple centre embedding constructions occurring in different registers and contexts. The moral of the story is that any claim about a natural language phenomenon must rest on empirical evidence.

Chapter 3, 'Many Englishes or one English?', examines the question of whether there is a single grammar or many distinct English grammars that determine(s) genre differences. The Lancaster-Oslo/Bergen corpus is used to answer this question; the corpus suggests that there is in fact only one grammar underlying different genres: 'What feel like large overall differences in the "shapes" of sentences from different genres arise as the cumulative effect of tiny statistical differences in the patterns of choice among grammatical alternatives that are available in all genres' (7).

Chapter 4, 'Depth in English grammar', uses a 'treebank' of English (SUSANNE) to investigate Yngve's 'depth' hypothesis, which asserts that in English right branching occurs freely but left branching is restricted. Yngve speculated that the depth limit is seven, and suggested that this may be due to the fact that left branchings overload a resource-limited working memory. To illustrate, consider the sentence *He is as good a young man for the job as you will ever find*. Here, as soon as the first *as* is uttered, the speaker

is committed to uttering a segment beginning with a second *as*. This commitment has to be retained in memory while the intervening elements are uttered. In order to reduce such long-distance dependencies, a limit is imposed on the number of left branchings.

Sampson finds it odd to define a non-local constraint on grammaticality/acceptability, i.e., one that operates in terms of the overall shape of a syntactic tree. He reasons that there must be some other factor that restricts left branching. The question then becomes: is there a way to quantify Yngve's restriction as a local constraint, which is furthermore invariant over different sentence lengths (since Yngve's restriction should be independent of sentence length), and which predicts that the number of words at a given depth in Yngve's sense will decline as depth increases? Sampson finds that the measure least sensitive to sentence length is the mean, over all the non-terminal nodes, of the fraction of each node's non-terminal daughters which are left branching. The observed depth restriction falls out from this metric, and instead of a sharp cut-off of depth seven (as Yngve proposed), there is a continuum of increasing depth over which the probability of occurrence of left branching decreases.

Chapter 5, 'Demographic correlates of complexity in British speech', examines an electronic corpus of spontaneous spoken English, and makes two main points: (i) there is no evidence for the claim (originally due to Basil Bernstein) that middle and working classes employ different versions of English; and (ii) the corpus provides evidence against the view that language acquisition occurs primarily during a critical period, as opposed to steadily over the lifetime of an individual. The first conclusion, regarding Bernstein's claims, is in fact a null result, which Sampson acknowledges does not decide the matter. The second result, regarding the critical period versus incremental lifetime learning, is based on Sampson's quantifying the speech complexity of each individual in terms of an embedding index; this is defined as the average degree of embedding of the various words uttered by a speaker, where degree of embedding is treated as a property of words. Sampson's main finding is that there is a positive linear correlation between speaker age and the average embedding index for that age; he regards this correlation as evidence against the critical period hypothesis, since (it is claimed) the latter would predict no age versus embedding index correlation beyond some predefined critical period.

Chapter 6, 'The role of taxonomy', argues that a large-scale taxonomic effort is required in order to create a systematic database of real-life data.

Chapter 7, 'Good-Turing frequency estimation without tears', explains the mathematical underpinnings of a method for reliable frequency estimation, and provides a step-by-step method for carrying out the computation. Accurate frequency estimation becomes important if a random sample ends up not including an (infrequently occurring) item that is part of the true population. This has the consequence that the item's frequency, if

based on the random sample, is underestimated to be zero. It is the latter part (the 'recipe') that the phrase 'without tears' in the title probably refers to. Linguists are the intended audience (9), and the presentation is certainly clear enough for linguists with the equivalent of high school algebra and statistics, and a knowledge of statistical methods for hypothesis testing. Sampson's home page (www.grsampson.net) also provides C code that carries out the calculations with minimal user involvement.

Chapter 8, 'Objective evidence is all we need', discusses the absence of an empirical basis for Chomskyan (generative) linguistics. Sampson points out that the standard linguistic 'method' of introspection cannot provide objective evidence for developing theories about language. Sampson also argues that 'negative evidence' has no place in linguistic research. This is the sort of evidence linguists use when they place an asterisk (*) or the like before a sentence. According to Sampson, many linguists argue as follows: since linguistics crucially relies on both positive and negative evidence and since observational data (such as corpora) provide only positive evidence, such objective evidence is insufficient and only introspection can provide the relevant data. However, Sampson notes that sciences like physics do just fine by relying on positive evidence alone, and yet are able to make positive and negative (if unobservable) predictions. Since there is nothing special about language, the logic used in physics should apply to linguistics as well.

Chapter 9, 'What was transformational grammar?', is a deconstructive look at Chomky's *The logical structure of linguistic theory* (LSLT; published as Chomsky 1975), the work that gave birth to Chomskyan generative linguistics. Sampson argues that the actual content of LSLT falls far short of what has been claimed of it. Apparently (examples are provided), the formalism is clumsy, sometimes wrong, and occasionally just 'perverse' (154). LSLT was made available in print long after it had become famous, and Sampson speculates that had this curious reversal in chronological order not occurred, linguistics would have a very different face today.

As the title suggests, chapter 10, 'Evidence against the grammatical/ungrammatical distinction', argues with the help of corpus evidence that the widely assumed boolean distinction between grammatical and ungrammatical sentences is illusory; there is no firm boundary separating possible sentences of a language (see also Keller 2000).

Chapter 11, 'Meaning and the limits of science', argues that the empirical scientific method (which entails Popperian falsifiability) cannot be applied to the study of the meaning of words: 'human lexical behaviour is such that analysis of word meaning cannot be part of empirical science' (181). Sampson argues that componential analysis of word meaning (where a word like *bachelor* is defined in terms of a feature structure with boolean values like +male, −married, etc.) is redundant, since one needs meaning postulates anyway to define certain entailment relationships (e.g. *A bought*

B from C entails *C sold B to A*), and meaning postulates can capture any generalization that componential analysis can. Sampson then asserts that nothing, not even meaning postulates (with or without a probabilistic component), can characterize word meaning. The word *cup*, for example, cannot be characterized exhaustively: one cannot specify an exhaustive set of boolean-valued or even probabilistically-valued features, nor can one define a set of meaning postulates that allow us to draw inferences like *X has a handle* from *X is a cup*.

Although overall this is an admirable collection, there are several problems with the views presented in EL. Regarding Yngve's depth hypothesis (chapter 4), Sampson believes that reformulating Yngve's allegedly non-local constraint as a local one is important for 'computational language-processing models' because, due to recursion in language, any depth level should be generatable, and there would have to be an undesirable extra mechanism that would look at the overall shape of the tree to decide whether the tree is acceptable (or grammatical, producible, or processable). It is not clear, however, why Yngve's depth constraint is non-local: assuming incremental parsing (a somewhat controversial assumption in sentence processing research, but one with compelling supporting evidence), a local processing overload could occur as soon as a certain depth is exceeded, due (for example) to decay of items held in memory. In order to rule out such possible (and, of course, possibly wrong) explanations, a discussion of existing psycholinguistic research on the matter (e.g. Frazier 1985: 152–157) would have been useful, since Yngve's hypothesis is, after all, a claim about human sentence processing. In this context, see also the lively exchange of letters between Yngve and Sampson in the journal *Computational Linguistics* (1998 and 1999).

Furthermore, Sampson's evidence against the critical period hypothesis (chapter 5) strikes this reviewer (who, by the way, has no opinion on the matter) as extremely weak. Sampson uses production data to draw conclusions about language acquisition; specifically, he assumes that the embedding index provides a measure of the degree of language acquisition that has occurred. But there appears to be no empirical justification for making such an assumption, and the argument falls apart from there. If measures like the embedding index are used, independent (empirical) evidence must first be provided for choosing such a measure. The steadily increasing mean embedding index by age group is surely an interesting result in itself, but there are alternative explanations (e.g. increasing sophistication of usage with increasing age, with no increase in knowledge) and these alternative explanations must be ruled out with further experiments (not necessarily involving corpora). This is fairly standard experimental methodology. Until alternative explanations are ruled out in this manner, nothing much can be concluded about language acquisition from the increasing mean embedding index *per se*.

Turning to the issue of positive versus negative evidence (chapter 8), Sampson's argument in favour of relying on positive evidence alone can in principle be challenged, since there is no reason why physics and linguistics must have objects of inquiry that possess similar properties. Just because one cannot (in normal circumstances) observe an unhindered apple not falling to the ground does not ENTAIL that one cannot observe that native speakers of a given language consistently reject a certain construction as part of the language.

But the most surprising views in this book (for this reviewer) relate to the acceptability-judgement based methodology. Sampson believes that linguists' intuitions about language are unreliable, but lay persons' intuitions may be even less so (129). As an example, he mentions (140, footnote 1) the case of one lay native speaker of English who had misunderstood/mislearnt the noun-determiner relationship for the *a*/*an* alternation: he thought that one should say *an good egg* and not *a good egg*.

I believe Sampson is mistaken in ruling out as a scientifically valid technique the elicitation of judgements under controlled conditions. Cowart (1997) lays out, in great detail, standard procedures for designing experiments involving syntactic research, and demonstrates their relevance for linguistics. Recent linguistic research (e.g. Keller 2000) has employed such techniques to further develop linguistic theories previously based only (or largely) on introspection. Of course, the existence of such research does not in itself show that Sampson is wrong in writing off judgements as valid sources of inference. But one would need to explain why one can obtain reliable grammaticality judgements from random samples of lay native speaker subjects. Such judgements show statistically significant differences among minimal pairs of interest, and the results can be (and are) replicated. It is likely that one needs to use more sophisticated dependent measures than the 1–5 or 1–7 Likert scale in the case of more subtle differences; for example, techniques such as magnitude estimation (Bard et al. 1996) have been successfully used in linguistic research. But if Sampson believes that such approaches are worthless (it is not completely clear whether he does think so, but this conclusion seems to be implicit in this book), he should at least address the question, using hard empirical evidence rather than the anecdotal *a*/*an* story.

In fact, Sampson's *a*/*an* example simply underlines the validity of such experiment-based research: it is likely that a random sample of native speakers would consider phrases like *an good egg* to be non-English compared to *a good egg*. It is possible that a handful of participants would provide judgements diverging from this overall (hypothetical) result. The important point is not to rely on a small set of individuals or just one individual (as Sampson does in the *a*/*an* example), but to use a sufficiently large sample of the population to elicit judgements for a controlled set of constructions. The phrases 'sufficiently large sample' and 'controlled set

of contructions' have well-defined meanings in this context, and texts like Cowart's explain this in great detail (also see Ray 2000).

The validity of such experimental methods also suggests that negative evidence can be found scientifically (as opposed to relying on intuition alone), and can be utilized in linguistic theorizing just as it has been in the past. Negative evidence in this context would simply be (statistically significantly) less acceptable sentence type(s), compared to some control sentence type(s). The gradability of acceptability also has a place in such a methodology (Keller 2000).

Sampson's views on the unreliability of judgements tend towards the bizarre. It is probably true (subject, of course, to empirical verification) that the string *book the* would overwhelmingly be rejected by native English speakers as being part of their language. Sampson's fantastic response is as follows:

> But to suggest that the construction is not just very unusual but actually impossible in English is merely a challenge to think of a plausible context for it … There would be nothing even slightly strange, in a discussion of foreign languages, in saying *Norwegians put the article after the noun, in their language they say things like bread the is on table the* … Talking about foreign languages is one valid use of the English language, among countless others. (177)

One might ask whether Sampson would have equally chosen to have his imaginary speakers invert the string sequence in ALL the noun phrases rather than just the ones mentioned but not used. Also, if mentioning a construction is the same as using it, why not apply the same approach to unknown foreign words? For example, one could include Japanese words as part of English too (words hitherto unknown to the vast majority of English speakers). By this convenient collapsing of the use-mention distinction, I can claim that the Japanese compound word *fukujoushi*, literally 'dying on stomach' ('dying during the course of sexual intercourse'), is part of English because I can imagine a discussion about foreign languages where someone says, *Apparently, Japanese has a rather colourful word for* '*dying during the course of sexual intercourse*': '*fukujoushi*'. By Sampson's reasoning, as soon as I have uttered such a sentence, *fukujoushi* becomes an attested English word (it is a different matter if this word becomes a loanword in English, like *samurai*, but that only happens over time).

Regarding the claim that it is impossible to precisely define the meaning of words, the way Sampson sets up the problem certainly appears to lead to this conclusion. But, as he notes Kilgarriff to believe, 'in general it makes no sense to ask how many meanings a word has in the abstract; one can only ask what meanings it might be convenient to distinguish for the purposes of some specific task' (200). Surely this is quite an achievement: it allows task-specific meaning to be studied scientifically. Just because we cannot perfectly

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characterize word meaning in the abstract does not mean that we cannot study meaning using imperfect but falsifiable domain-limited models.

During the course of Sampson's extended critique of 'mainstream' linguistics, the periodic attacks on Chomsky become a bit tiresome. Acknowledgement of Chomsky's achievements is grudging and rare ('There is no question that Chomsky produced some good work in mathematical linguistics – notably the theorems in "On certain formal properties of grammars" (1959)' (153)). Many of Sampson's views are reasonable and he justifies his arguments very thoroughly indeed, but a more balanced presentation of the achievements and failures of the Chomskyan enterprise would have guaranteed a far wider audience. In fact, his hostile approach to Chomsky will probably alienate the very people who need to read this book most.

For all its eccentric argumentation and vigorous Chomsky-bashing, EL is a very interesting and illuminating read. Sampson's main point, that empirical (read: corpus) methods must become the driving force behind linguistic theorizing, is important and needs to be understood by all linguists. The imbalance and extremeness of some of his views will alienate many readers who might otherwise benefit from his work, and will probably result in this book being taken less seriously than it should be. Even so, EL ought to be required reading for all students and practitioners of linguistics.

Finally, the title is misleading – it really should have been *Corpus linguistics*. The empirical method as applied to linguistics is not limited to corpus research – if it is, this needs to be demonstrated empirically, as Sampson would undoubtedly agree.

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Alison Wray (ed.), *The transition to language*. Oxford: Oxford University Press, 2002. Pp. xii + 410.

Reviewed by DANIEL NETTLE, Open University

The study of the origin of language has become an intellectually reputable, indeed fashionable, pursuit. One testament to this fact is that international conferences on the topic have become biennial fixtures. From each of the first two of these conferences there emerged a stimulating and high-quality collection of papers (Hurford, Studdert-Kennedy & Knight 1998, Knight, Studdert-Kennedy & Hurford 2000; see review of the latter else where in this issue). This volume is the equivalent offering for the third conference, held in Paris in 2000.

The intellectual agenda for the renaissance of interest in language origins was set by the coupling of evolutionary psychology's concern with adaptation to Chomskyan nativism (Pinker & Bloom 1990). A subsequent strand became concerned in a more detailed way with the uses of language, and in particular the role of language in social interaction (Dunbar 1993). This third volume sees language origins research becoming more eclectic and in many ways more iconoclastic. There is a prevailing orthodoxy in linguistics that consists of several elements, including notably

1. a strong nativism, with a commitment to innate structures specifically concerned with language, not general learning or intelligence;
2. an assumption of abrupt discontinuity between human language and animal communication systems;
3. an assumption that all human languages spoken now and in the past differ only trivially from each other.

When put together and applied to the evolution of language, these statements can add up to a view on which the appearance of language is an abrupt saltation – even a miracle, as Herb Terrace points out in his chapter 'Serial expertise and the evolution of language'. If language is subserved by structures that have no other function, no precursor in animal communication, and which are fully evolved as soon as the first one appears, then one is left groping around for implausible macromutations. Many of the papers in this volume end up undermining one or other aspect of the saltationist view. What emerges instead is a view of gradual rather than saltationist evolution, with multiple intermediate stages between the absence of language and its modern form, in which abilities and structures present in other species are exapted to do the work of language processing, and in which cultural rather than genetic evolutionary processes have a significant role to play.

Rather than examine all seventeen chapters plus introduction here, I will focus on four interesting themes that emerge, with a brief description of the relevant chapters. The first theme concerns cross-species comparison. Tecumseh Fitch's chapter 'Comparative vocal production and the evolution of speech: reinterpreting the descent of the larynx' lays to rest the old shibboleth that *Homo sapiens* has a low larynx and so can produce a wide range of formant patterns, whilst extinct hominins and other apes have a high larynx and are unable to do this. Fitch shows through cineradiography that the mammalian larynx is in fact more mobile than earlier anatomical investigation suggested; many species can lower it temporarily during phonation, even if its resting position is high. What is more, several mammal species including the taciturn koala have it permanently lowered. Thus the low resting larynx is not a distinctively human characteristic, and any inference from a high resting position of the larynx to an inability to produce a wide range of sounds seems likely to be invalid.

Kazuo Okanoya, in his chapter 'Sexual display as a syntactical vehicle: the evolution of syntax in birdsong and human language through sexual selection', argues intriguingly that the song of the Bengalese finch, which consists of strings of repeated units, can be modelled by grammatical rules, albeit of the finite state variety. This is syntax without semantics, involving constraints on well-formed sequences that exist without the atomic units having any reference. Like vocal production in many species, the finch's abilities are lateralised to the left hemisphere (which rather undermines Tim Crow's argument in his chapter that the development of cerebral lateralisation is a human-specific characteristic that facilitates the evolution of language). Okanoya raises the interesting possibility that syntax and compositionality evolved separately from or perhaps before the use of words as referents. The function of this melodious vocalisation would have been, as for the Bengalese finch, to advertise oneself to prospective mates. Such a scenario would see a rapid escalation in the complexity of syntactic structures before their mapping onto conceptual representations ensued.

The second theme is the critique of the assumption that no intermediate stage can exist between fully modern syntactic language and its absence. Alison Wray, in her chapter 'Dual processing in protolanguage: performance without competence', points out that even modern language contains large domains where strings are essentially unanalysed and decoded holistically (proverbs, idioms, fixed constructions and so on). She turns conventional wisdom on its head and suggests that protolanguage might not have consisted of single words, with compositionality and complex meanings following later, but equally plausibly of strings whose meaning was decoded holistically, with decomposition following subsequently. This echoes Okanoya's very different contribution in envisaging a language where complex strings gradually evolve to have internal units of meaning, rather than single word utterances gradually being joined up.

Robbins Burling, in his chapter 'The slow growth of language in children', turns to ontogenetic evidence. He argues that there is no evidence for a 'syntactic explosion' in language acquisition, but rather that syntactic competence develops incrementally. His point is not to claim that ontogeny recapitulates phylogeny, but rather to show that grammars of intermediate complexity between simple one-word systems and full syntax are logically and empirically possible (and communicatively functional). This is another blow against those who have sought saltationist explanations by arguing that such intermediate states could not exist.

Bernd Heine & Tania Kuteva come at the 'intermediate forms' issue from a very different methodological background. Their chapter, 'On the evolution of grammatical forms', draws on the cross-linguistic study of grammaticalisation. They argue that since grammatical forms develop historically along a unidirectional pathway from lexical forms, a stage can be envisaged in which there were no grammatical forms at all. There would have been bare noun- and verb-like words, with no morphology or function words, and a reliance on word order and pragmatics to express grammatical relations. It is not clear if they envisage the transition from this stage to modern language as a purely cultural development, or dependent on ongoing changes in the brain.

The third theme I wish to discuss is the assumption of linguistic uniformitarianism; that is, the notion that language and language change have been the same for as long as human language has existed. This kind of uniformitarianism only makes sense when coupled with a relatively abrupt view of language origins; if the birth of language was incremental, then by definition there were non-uniformities (though a more circumscribed uniformitarianism applying to the last few tens of thousands of years could still be defended).

Heine & Kuteva's chapter, already discussed, questions the uniformitarian assumption, as does Frederick Newmeyer in his chapter, 'Uniformitarian assumptions and language evolution research', which opens up the possibility that the dynamics of language change and the functional pressures on grammars could have been very different at different stages of our history. He can produce no decisive argument in favour of this view, but it should at least be considered as a possibility in reconstruction. In this kind of argumentation there is no clear separation between the point at which LANGUAGE evolved by genetic change, and the point at which LANGUAGES began to evolve by cultural evolution. Rather, the implied scenario is one of communication systems gradually developing, in the context of ongoing brain and behavioural changes of their users.

This leads to the fourth theme – the challenge to some aspects of linguistic innatism coming from studies of cultural evolution. Papers pursuing this theme stem from computer and robotic simulations of the emergence of communication systems. Luc Steels' robotic experiments are well known, and there is an engaging account of them here ('Crucial factors in the origins of

word-meaning', coauthored with F. Kaplan, A. McIntyre & J. Van Looveren). This work demonstrates that a conventional referential vocabulary can emerge amongst robots as long as the social and pragmatic environment is appropriate, even in the absence of shared hard-wired communicative or conceptual structures. This, of course, is far short of language.

Other studies look at the evolution of syntax itself. Simon Kirby's simulation work has been influential here (Kirby 1998), and although Kirby himself is absent from the volume, several chapters discuss and replicate it. Bradley Tonkes & Janet Wiles, in their somewhat misnamed chapter 'Methodological issues in simulating the evolution of language', replicate Kirby's finding that a structured system can emerge culturally in a population of agents without innate dedicated mechanisms, simply as a by-product of learnability differentials between different grammars. Morton Christiansen & Michelle Ellefson, in their chapter 'Linguistic adaptation without linguistic constraints: the role of sequential learning in language evolution', use simulations to suggest that observed patterns of head ordering in natural language will evolve culturally through learnability differentials without the need for an innate dedicated module. This type of work is certainly suggestive. It does not address head on the argument from the poverty of the stimulus, which was always the primary ground for the positing of innate parameters. The status of that argument now that grammatical theory has become so much more minimalist, and the learning capabilities of connectionist networks better understood, seems to stand as a central unresolved issue in psycholinguistic theory.

This is, then, a thought-provoking volume, with implications not just for language evolution but for how we conceptualise language acquisition, language structure and language change. Alison Wray's choice of title, *The transition to language*, stands in a pleasingly oxymoronic relation to most of the contents. Perhaps there was no single transition; perhaps, like the Greeks, language was always in the process of becoming.

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