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REVIEWS

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Artemis Alexiadou, *Adverb placement: a case study in antisymmetric syntax* (Linguistik Aktuell/Linguistics Today **18**). Amsterdam & Philadelphia: John Benjamins Publishing Company, 1997. Pp. x + 256.

Reviewed by ANNA ROUSSOU, University of Cyprus

This book (a revised version of the author's doctoral thesis) discusses the properties and the position of adverbs in clause structure. The main claim is given in the form of the Adverbial Licensing Principle (17), according to which adverbs are licensed as specifiers. Alexiadou distinguishes between specifier and complement (temporal, manner and locative) adverbs. The former are directly merged in the specifier position of an appropriate functional head, while the latter are merged as V-complements and then raise to a specifier position, so that they can be formally licensed. Complement adverbs (with the exception of temporals) can also be licensed via incorporation (a head-head relation). The complement vs. specifier distinction is more broadly formulated in terms of A- vs. A′-elements respectively. It is then expected that complement adverbs interact with A-movement (e.g. object shift) and specifier adverbs with A′-movement (e.g. *wh*-extraction). The distinction between two classes of adverbs is empirically supported by comparative evidence. The main data comes from Greek, although there are a considerable amount of examples from other languages as well, primarily English and German. The theoretical framework is that of Kayne (1994) and the aim of the book is to consider the implications of antisymmetry for the licensing of adverbs. In particular, the restrictions of antisymmetry on phrase structure, i.e. no right adjunction, a single left adjunct/spec per head, give rise to a proliferation of functional categories, the postulation of which is partly supported by the licensing of adverbs. Alexiadou also follows minimalist assumptions, thus attempting to make antisymmetry compatible with Bare Phrase Structure (Chomsky 1995). The main assumption is that the rightmost non-branching element in the structure has to raise (a PF-linearisation requirement). On this basis it is argued that non-complex complement adverbs have to raise to an appropriate specifier. Consequently, in-situ complement adverbs have a complex internal structure (and are necessarily focused).

The discussion spreads over eight chapters. Chapter 1 introduces the data, the problems with the classification of adverbs, and a summary of the

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proposal. Chapter 2 presents the theoretical framework with the appropriate modifications for adverbial licensing via a spec-head or head-head relation. Chapter 3 discusses some basic facts concerning word order in Greek and provides the basic functional structure (page 56 for the IP layer and page 76 for the CP). Chapter 4 is restricted to temporal (complement) and aspectual (specifier) adverbs, which are licensed in Spec,TP and Spec,AspP, respectively. Assuming that Spec,TP is parametrised with respect to the occurrence of a subject, the next claim is that in those languages (e.g. Celtic, Icelandic) where the subject occupies Spec,TP, the temporal adverb is licensed in another position. Chapter 5 discusses manner (complement) and negative adverbs in both simple and periphrastic tenses. It is argued that manner adverbs in their non-complex form move to Spec,VoiceP; when they remain in situ they exhibit a more complex structure. Negative adverbs are licensed in the spec of a lower NegP. With respect to periphrastic tenses it is argued that these constructions involve a biclausal structure, where the complement clause is a MoodP (i.e. it lacks the CP layer). Finally, the distribution of sentential adverbs is also discussed, yielding a slightly revised clause structure, schematically represented in (120), page 171. Chapter 6 considers adverb incorporation, which is restricted to manner (and locative) adverbs, i.e. complement adverbs (temporal adverbs are taken to have more complex structure on the basis that they are referential, thus they cannot incorporate). Chapter 7 discusses the distribution of adjectives within the DP, and their similarities with adverbs, showing that the functional structure of the DP is less articulated compared to that of the CP. Chapter 8 concludes the overall discussion and considers some of its implications for Universal Grammar with respect to the hierarchy of functional categories.

As one can see from the brief outline above, the book covers lots of material. The analysis is quite original, although similar in spirit to that argued independently by Cinque (published as Cinque 1999). In theoretical terms, Alexiadou's goal seems more focused on the properties of adverbs and their position in the structure. The feature content and the hierarchical position of functional categories in the sentence follow from this discussion. Cinque's work has the reverse goal: it is more focused on establishing a universal functional hierarchy (thus a more elaborated functional structure), using the position of adverbs as evidence. Both pieces of work are certainly complementary in nature. Alexiadou's work, apart from its more general theoretical aim, also has another angle as it makes specific claims regarding Greek syntax. In this respect, the book is also an important contribution to the study of Greek.

Despite its positive aspects, there are a number of problematic points in Alexiadou's analysis and presentation, which I would like to address in this review. The first problem has to do with some of the data, such as the analysis of the VOS order in Greek (chapter 3), as well as the claim that complex manner adverbs cannot precede the object (chapter 5). The second problem

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concerns adverb incorporation as a syntactic phenomenon in Greek (chapter 6). The third problem concerns the presentation, which is quite loose in parts or can give a misleading impression of some data.

Let us start with the first point. Alexiadou argues that the VOS order is derived via movement of the object over the subject to the specifier of a functional head (to be identified as Spec,VoiceP in chapter 5). As a result of this movement, the subject becomes the most embedded constituent and is necessarily focused ([+F]). The relevant example is given in (1) below (Alexiadou's (43), page 66):

(1) Efage to gliko **o Janis**.
ate the sweet the-John-NOM
'John ate the sweet.'

The pattern in (1) is accounted for as follows:

> Turning now to VOS orders, note that the subject is the element that is considered to be 'new' information and is also the constituent that receives the main stress. […] Clearly, the property that forces DPs to leave the VP, when they do not have to for reasons of formal feature checking, must have to do with their semantic and structural incompatibility with this VP domain. Objects cannot be interpreted as focal; hence they must move out. (67)

Object shift of this type is motivated by a PF property (linearisation): '[…] movement must occur in order for the non-complex item to be linearized at PF (see chapter 2) […]' (67).

There are a number of problems with this claim. First, it is not true that only the subject but not the object can be focused in the VOS structure in (1). It is indeed possible to have *efage* TO GLIKO *o Janis*, where the object DP *to gliko* is [+F]. This on its own may not be so problematic, as one could argue that the object in this case has moved to the spec of a lower FocusP (i.e. somewhere above VP). Even if we accept this alternative, the problem remains with the clarity of Alexiadou's analysis, which can give rise to circularity: does the object raise because the subject is [+F], or is it that the subject becomes [+F] as the result of object movement (a defocusing operation of the object)? Finally, the claim regarding object raising is that movement for PF purposes affects non-complex (i.e. non-branching) elements. However, as far as I can tell from the example in (1), the object cannot be regarded as non-complex, as it has a D head lexicalised by the article *to* (see also the discussion of DPs in chapter 7). To be more precise, the object has a D-N(P) structure (at least), and is therefore branching. It can also have a more complex structure in case it is modified by an adjective (e.g. *to sokolatenio gliko* 'the chocolate sweet') and still appear in a VOS structure. Thus the PF-linearisation requirement on non-complex elements cannot be the trigger for object shift in (1), as the object is internally complex.

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If this is true, then object shift will have to be accounted for independently of the complexity of the object.

A similar problem arises with respect to the position of manner adverbs in relation to objects in chapter 5. According to Alexiadou the V > Adv > Complement order is derived via movement of the (complement) manner adverb to a higher position (Spec,VoiceP). Given that object shift also targets Spec,VoiceP, we cannot find both the object and the adverb in this position. In VOS, then, the adverb will have to remain in its merged position. The claim is that

> […] we are dealing with two instances of the same item: an internally complex one and a non-complex one. The properties the former exemplifies are the visible evidence for the presence of internal structural complexity. The non-complex one is originally merged in final position, but moves obligatorily to the higher position (139–140).

This point is illustrated in (2) ((34), page 138), while (3) shows the restricted distribution of a complex adverbial ((42), page 140; see also chapter 2, page 44).

(2) Efage **gorga** ti soupa **gorga**.
ate-3SG fast the-soup-ACC.FEM fast
'He ate the soup fast.'

(3) (a) Katalave ta themata **pragmatika para poli kala**
understood-3SG the-issues-ACC.PL really very very well
'He understood the issues really very well.'
(b) *Katalave **pragmatika para poli kala** ta themata.

Regarding (2) the claim is that *gorga* in clause final position has more internal structure compared to *gorga* in pre-object position. The question is what the complex vs. non-complex distinction amounts to in structural terms. In other words, if in both cases we are dealing with an AdvP, what else apart from an Adverb head do we find in the complex counterpart? Is there some sort of a null complement, and if so, what are its properties? Apart from this more technical point, there is a problem with the ungrammatical status assigned to (3b), which for me and other native speakers clearly counts as grammatical. In general, the non-complex requirement that Alexiadou invokes does not seem to be empirically supported, as it is perfectly possible to have complex adverbials and PPs between the verb and the object. The same holds for examples (35b), page 138; and (43b), (44b), page 140.

Apart from questioning the data, the next issue is what features manner adverbs and Voice share so that the latter would license an adverb (or a shifted object instead) to its specifier. The answer is given on page 135:

> A potential answer is that the possibility of a verb to license a manner adverb is related somehow to its voice features. Chomsky (1965: 101–105)

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relates the possibility of passivization to the presence of a manner adverb […]. Note that this proposal might account for the obligatory presence of a manner adverb in the middle construction (cf. (25)).

The example provided to support this comes from Greek ((25) in the text, page 135):

(4) To pukamiso plenete *(efkola).
the-shirt-NOM wash-PASS.3SG easily
'The shirt washes easily.'

The argument quoted above regarding the presence of the adverb in Spec,VoiceP has a loophole. The reference to Chomsky (1965) concerns passives, while the example in Greek is a middle construction, which also exhibits passive morphology. This should be clarified for those readers who are not Greek speakers (English uses the active morphology in this context). It also presupposes an analysis of middle constructions as passives, which is not mentioned in the text (but see Tsimpli 1989). Apart from the morphological differences regarding Greek and English middles (passive vs. active morphology, respectively), there is a further difference, which could perhaps be attributed to the morphology, namely that the manner adverb is not obligatory in Greek, as shown in (5).

(5) To pukamiso plenete (dhen xriazete katharistirio)
the-shirt-NOM wash-PASS.3SG not need-PASS.3SG dry-cleaning
'The shirt can be washed (it doesn't need dry cleaning).'

How exactly one accounts for the non-obligatoriness of the manner adverb in Greek middles is a separate issue. The point that I would like to raise with respect to Alexiadou's claim is simply that the adverb is not obligatory in this context, i.e. there is a parametric difference between Greek and English middles. Thus a middle construction in Greek is perhaps not the best example to illustrate the relation between adverbs and Voice.

In connection with this, it is worth pointing out another problem that arises regarding the discussion of periphrastic tenses (*eho* ('have') + participle) in this chapter. Alexiadou argues that the active participle occurs in Asp, while the passive one is in Voice, i.e. lower in the clause. This is supported by the fact that manner adverbials follow the active participle, as Voice is lower than Asp, while they precede the passive participle, implying that the passive participle is in Voice, which is below Asp. However, it is possible for the passive participle to precede the manner adverb when, according to Alexiadou, a 'specific point of reference appears in the sentences' (155). The relevant examples are given below ((76) and (77), page 155):

(6) (a) I mihani ixe **ligo** **kinithi**.
the-machine-NOM had little moved-PASS
'The machine had been operated a little.'

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(b) Kthes, i mihani ixe **(*ligo) kinithi ligo**.
yesterday the-machine-NOM had little moved-PASS little
'Yesterday, the machine had been operated a little.'

It is also possible to have *ligo* following *kinithi* in (6a), which is compatible with Alexiadou's analysis, as we are dealing with a (non-complex) manner adverb which can remain in situ. Thus, the Participle > Adverb order is not problematic in this case. The problem remains with respect to (6b) though. To be more precise, I think that the order Adverb > Participle is possible, given the right intonation. If this is true, then it's not so obvious that the presence of a temporal adverb can affect the position of the manner adverb in relation to the participle.

Be that as it may, let us consider the analysis provided for the contrast between (6a) and (6b). Alexiadou assumes that

> the passive perfect has a static meaning (cf. Joseph & Philippaki-Warburton 1987) and refers to the result of an action. I would like to suggest that the reason for the asymmetry observed in the data above is related to the special interpretation associated with the passive participle. Specifically, [(6)] where the participle is below the manner adverb, i.e. in Voice^0, has a generic reading. Recall that the head of Voice Phrase is marked [±Active], where [−Active] covers a wide range of what is [sic] traditionally called the *diatheses* of the verb, i.e. it covers the middle, the reciprocal, the reflexive and the passive diatheses alike. […] In Modern Gr, Tsimpli (1989) argues that middles are inherently generic and non-specific and that they admit *by*-phrases with arbitrary reference. (156)

First, the static meaning is derived in a combinatory fashion from the semantics of *have* and the participle, and not from the passive participle exclusively. In that respect the same holds for the active perfect as well. Second, it is very hard to interpret (6a) generically, although it could be achieved given the right context, for example, if we take the DP *i mihani* in (6a) to refer to the engine as a kind, something like 'the engine, as a kind, had been operated little in the 19th century' (see also Cinque (1999: 102–103)). However, the generic interpretation depends on the properties of the subject DP, which may be compatible with a generic interpretation. In other words, the generic interpretation is independent of the [−Active] specification in Voice (for one thing generic readings arise with [+Active] as well). In this respect, the reference to Tsimpli (1989) is not that relevant, as she talks about middles and the discussion here involves passives. The morphology might be the same in Greek, but their properties are crucially different. Moreover, Tsimpli (1989) attributes the generic reading of middles to the imperfective specification of the (morphologically passive) verb and not to the passive affix as such. Thus the reference is not simply inaccurate but also misleading.

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Let us now consider the second problem raised earlier, which has to do with adverb incorporation as a syntactic phenomenon in Greek. Alexiadou essentially follows Rivero's (1992) analysis in this respect and uses adverb incorporation as evidence for the complement status of manner (and locative) adverbs. I think that these are two separate issues. More precisely, the relation between complement adverbs and incorporation should not be seen as an 'if and only if' relation. Even if there is evidence that what looks like incorporation is actually a compound, this by no means undermines the claim that manner adverbs are indeed complements. After all, not all complements incorporate, and incorporation as such is a parametrised property. The arguments against an adverb incorporation analysis have been presented in a number of papers in Greek. Alexiadou cites Drachman & Malikouti-Drachman (1992). Other references include Kakouriotis, Papastathi & Tsangalidis (1997), Smirniotopoulos & Joseph (1997, 1998) and partly Xydopoulos (1996). The strongest argument that these authors offer against an incorporation analysis has to do with the limited productivity of the phenomenon. For example, while we can have the adverb *kala* ('well') combining with the verb *ferome* ('behave') to give us *kaloferome* ('behave well'), other manner adverbs such as *sosta* ('properly'), *sklira* ('cruelly'), *psixra* ('coldly') cannot combine with it, i.e. **sostoferome*, **skliroferome*, **psixroferome* (cf. Kakouriotis et al. 1997: 78). As the same authors point out (page 81), Rivero's claim that adverb incorporation is restricted to non-statives, endorsed by Alexiadou as well, is also problematic as it is indeed possible to find the adverb *poli* ('much') combining with a stative like *thelo* ('want') or *ksero* ('know'), as in *polithelo*, *poliksero*. Similar arguments extend to what looks like noun incorporation, but could be best analysed as compounding (see the above references). It thus seems that Alexiadou simply adopts Rivero's analysis without further inquiry or elaboration.

Another problem that arises is why temporal adverbs, although complements, do not incorporate. A possible answer is outlined on page 191, according to which it is the features of T, and not those of the verb, that are relevant for the licensing of temporal adverbs. Moreover, given that the latter are referential DPs, they cannot incorporate, as this operation is restricted to bare NPs. This is a rather problematic explanation for the reason that it is indeed possible to find incorporation with referential DPs, or with quantificational DPs, whereby the NP part only incorporates to the verb (by stranding the determiner or the quantifier). There are a number of examples provided by Baker (1988) (cf. examples (51), page 94, (55)–(57), page 96). As Baker (1988) further shows, the incorporated NP can be interpreted as specific or non-specific (cf. example (5), page 77). Thus, following these assumptions, the unavailability of incorporation with temporal adverbs cannot be attributed to their DP/referential properties.

Before turning to the presentational problems, there is a more theoretical issue that I would like to raise. As Alexiadou argues, complement adverbs are

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merged inside VP and may or may not raise to Spec,VoiceP, depending on how internally complex they are. Suppose that we take this to hold (despite the problems raised above). Recall that with respect to (2), the claim is that *gorga* ('fast') in clause final position is internally complex and remains inside the VP. The question then is the following: if adverbs need to relate to a functional head for licensing purposes, how is this achieved when the adverb remains in situ? Does covert movement take place (e.g. in the form of feature raising), or is there some alternative chain formation mechanism (e.g. in Brody's (1995) sense) that achieves this licensing? The answer as such may not be technically difficult to provide, but it is rather surprising that this issue, i.e. the licensing of in-situ adverbs, is not mentioned. The question becomes more complicated if we consider the VOS-Adverb order, whereby the object is in Spec,VoiceP. Can Voice license both the object in its specifier as well as the in-situ adverbial? In the case of temporal adverbs, which are merged as complements but then move to Spec,TP, it is explicitly stated that this can be done overtly or covertly via feature movement (115). Presumably the same holds for manner adverbs, although it is not mentioned in the text. Considering next temporal adverbs in clause initial position (e.g. *Yesterday*, *John bought the car*) the claim is that the adverb is 'base generated in sentence initial position which is non-related to a final position via movement' (113). The question then is, is there any kind of relation between the sentence initial position and the V-complement one? If this is the case, how is this relation established? Suppose that there isn't any such relation. The problem still remains whether or not the sentence initial adverb relates in some way to the T head (its licensing head), given its temporal properties. Once again, how is this relation achieved? If it is not related to T either, then what are the features that license temporal adverbs in this position? Finally, one wonders how locative adverbs are licensed; the only references are found on page 7 and pages 124–125, footnote 23.

Next, I would like to raise some problems regarding the presentation, limiting my discussion to few indicative cases. For example, in chapter 1 Alexiadou discusses the order of functional projections in Greek and mentions the problems with the positioning of NegP with respect to MoodP (55). On page 56 she argues that 'NegP is situated higher than AgrSP', and provides the following IP structure ((20), page 56):

(7) $[_{\text{MoodP}}$ na/tha $[_{\text{AgrSP}}$ V_i $[_{\text{TP}}$ t_i $[_{\text{AspP}}$ t_i $[_{\text{VoiceP}}$ $t_i[_{\text{VP}}$ t_i]]]]]]

Given that there is no NegP present in this schema, the reader is left wondering about the position of (sentential) Negation, i.e. whether it is above or below MoodP (which, in any case, is above AgrSP). This is a non-trivial matter as the negator *min* follows *na*, while the negator *dhen* precedes *tha*, both in Mood. Furthermore, it is not clear how the CP structure provided in (77), page 79, relates to Rizzi's (1997) articulated CP, which is

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mentioned in the immediately preceding paragraph (albeit with FinP missing).

The discussion on pages 69–70 of the same chapter regarding the distinction between Clitic Left Dislocation (CLLD) and Hanging Topic Left Dislocation (HTLD) is not illuminating either, as although the position of CLLD is identified with Spec,TopicP that of HTLD is not identified (somewhere above TopicP) until chapter 5, page 164, where it is mentioned that domain adverbs, which occur in Spec,DomainP (the highest head in the CP domain), have properties similar to HTLD. This allows us to deduce that HTLD is also in Spec,DomainP. A cross-reference to chapter 5 would have helped.

Another problematic point involves the misquotation of Horrocks (1997) in chapter 5 again, where the perfect tense is discussed. Alexiadou writes:

> As noted in Horrocks (1997, 228–234), in the later period of *Koene* [sic], that is in the language of the New Testament, the perfect tenses begun [sic] to be expressed periphrastically by using the verbs *eho* 'have' + aorist infinitive or *thelo* 'want' + aorist infinitive as their complements. (151)

A reader familiar with the history of the perfect and future constructions in Greek, but not with Horrocks' book, would have thought that Horrocks has simply got it all wrong. In fact it is well known that the constructions with *eho* and *thelo* + infinitive were initially used for the expression of the future and not of the perfect (cf. the *habeo* + infinitive structure in post-Classical Latin). This is precisely what one finds, when the relevant pages from Horrocks (1997) are checked, so it seems that the above quotation is a misrepresentation on Alexiadou's part.

In this context I would also like to comment on the treatment of the participle in Greek as being synchronically identical to the 3rd person perfective subjunctive (151). This allows Alexiadou to conclude that the participle presumably bears mood features. The problem is that Modern Greek has no morphological subjunctive; instead the subjunctive is expressed periphrastically by means of the particle *na* and the verb, which is specified for tense (±past) and aspect (±perfective) (cf. Veloudis & Philippaki-Warburton 1983). The [−past], [+perfective] form of the verb is a 'dependent' form in the sense that it has to be supported by a modal particle or some appropriate conjunction. It is the 3rd person of this form that is homophonous with the perfect participle. This formal identity, however, does not seem to provide evidence for the presence of mood features on the participle; thus it is by no means sufficient to justify Alexiadou's claim that the *-i* ending (e.g. *dhiavas-i* = read-perfective) can be treated as a mood marker. Moreover, what sort of mood marking would that be, as it is found in indicative forms as well? If anything, this ending in non-participial forms corresponds to an agreement suffix (see also the discussion in chapter 3, page 52); recall also that participles do not show agreement. A better estimate would have been to treat *-i* synchronically as the default agreement (3rd

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person), thus arguing that participles in Modern Greek carry the default agreement. All in all then, there is no synchronic evidence for analysing the participle as having mood features (as there is no subjunctive inflection in Modern Greek), nor a diachronic one (the perfect participle is a residue of the Classical Greek infinitive).

A few other points may deserve discussion (e.g. how abstract is the nominal feature of adverbs argued for on page 203, chapter 7?), but there is lack of space to do so. Concluding this review, I would like to point out that the essence of the analysis regarding adverbial licensing is on the right track and indeed original, although its implementation could have been a bit more refined and carefully presented. This would be desirable as it wouldn't give the (native Greek) reader the impression that the data may be sometimes tailored to fit the theory or in general let the reader fill in the gaps that are left open in the argumentation. At the same time, adverbial licensing is a rather difficult topic, so some loose ends in the presentation can be expected. Despite its shortcomings, I do think that this book is a desirable contribution to the field as it reflects the more recent attempts to define the set of functional categories and their hierarchical occurrence in clause structure, and argues for a possible analysis. It is also a contribution to the syntax of Greek, as it discusses various phenomena relating to both clausal and nominal structure.

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Pilar Barbosa, Danny Fox, Paul Hagstrom, Martha McGinnis & David Pesetsky (eds.), *Is the best good enough? Optimality and competition in syntax*. Cambridge, MA: MIT Press, 1997. Pp. vi + 450.

Reviewed by Kevin Russell, University of Manitoba

In the 1990s syntactic theories began making more and more use of the idea that the grammaticality of a sentence or derivation can crucially depend on how it compares to other similar sentences or derivations. In the various incarnations of Minimalism, there have been many economy conditions, such as the Minimal Link Condition, Procrastinate or Greed. Another research program is trying to apply to syntax the ideas and mechanisms that Optimality Theory (OT) developed to deal with phonology.

The 1995 workshop that this volume arises out of was intended to bring researchers from a variety of traditions together around the topic of the role of competition in syntax. One assumes there was a lively and healthy exchange of ideas between different approaches at the workshop. Unfortunately for the reader, little evidence of that has made it into the volume. For the most part, the papers stay firmly in their own tradition, with little or no reference to each other.

Instead of following the volume's order (alphabetical by author), I will discuss the papers in loose topical groups: papers specifically on syntax in OT, those in the minimalist tradition, then those that do not fall neatly into either category.

Beginning with the papers devoted to OT syntax, two contributions take rather similar approaches to very similar sets of data. In 'WHOT?', Peter Ackema & Ad Neeleman compare *wh*-movement in Bulgarian, Czech and English, with a smattering of Chinese and Japanese. In 'When is less more?: faithfulness and minimal links in *wh*-chains', Géraldine Legendre, Paul

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Smolensky & Colin Wilson compare *wh*-movement in English and Chinese, with a smattering of Bulgarian and other languages. Legendre et al. show how the idea of 'Shortest Link' can be formalized in OT constraints and explore the question of exactly what candidates can compete against each other.

Eric Baković's 'Optimality and inversion in Spanish' examines patterns of *wh*-movement and accompanying auxiliary inversion in several dialects of Spanish, showing that all (and almost only) the attested patterns can be obtained by reranking between various constraint families, each of which has a rigid ranking among its own members. For example, there is a constraint family requiring operators to occur in specific positions – the requirement universally being felt most strongly by argument operators, then by locations, manners and reasons. The ranking of members of this family with respect to other constraints (e.g. 'don't move') will determine whether there is movement of the *wh*-phrase and whether it is accompanied by inversion of the auxiliary.

In her paper 'Morphology competes with syntax', Joan Bresnan deals with patterns of weak cross-over effects in languages ranging from Malayalam to Navajo. She criticizes accounts that rely solely on hierarchical notions like c-command and argues that linear precedence plays an unavoidable role in the binding systems of many languages. After presenting an OT recasting of her Lexical-Functional Grammar theory, Bresnan proposes three different binding constraints requiring: (i) linear precedence in the raw phrase structure representation, (ii) outranking in the representation that represents grammatical relations such as subject and object, and (iii) outranking in the representation that represents thematic roles. She argues that the various patterns of weak cross-over effects can be obtained by differential ranking of these three constraints.

In 'Optimal subjects and subject universals', Jane Grimshaw & Vieri Samek-Lodovici try to formalize the syntactic and pragmatic constraints that control when a language can and cannot pro-drop a subject, when it uses expletive subjects, and where in the sentence the subject will end up.

In 'Some Optimality principles of sentence pronunciation', David Pesetsky examines the constraints that determine whether an item in a sentence is pronounced or not. He pays special attention to complementizers and their neighbourhoods in English and French, offering OT analyses for, among other things, differences in pied-piping and classic doubly-filled Comp effects.

Mark Newson argues in 'On the nature of inputs and outputs: a case study of negation' that with all the options open to an OT approach to syntax, 'our speculations are best confined to the simplest of the possibilities until we have reason to reject these' (315). He shows that his own preference for the 'simplest' possibility (input consisting of only lexical items, the candidate generator (GEN) applying only once, producing candidates that are essent-

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ially Brody's (1995) Lexico-Logical Form) is capable of accounting for the behaviour of negation in Hungarian and English.

I turn now to three papers that are written explicitly from within the Minimalist tradition. The first, 'Some observations on economy in generative grammar', contains Noam Chomsky's musings on the history of generative grammar and the role that economy principles have played in various incarnations of the theory.

In 'Reference set, Minimal Link Condition, and parametrization', Masanori Nakamura uses an analysis of agent-topic and theme-topic sentences in Tagalog to argue for changes to some basics of Minimalist Theory. He proposes that the reference set should be expanded from all derivations arising from the same numeration to all derivations arising from NON-DISTINCT numerations. He also argues for keeping the Minimal Link Condition as an independent economy constraint rather than trying to fold it into the definition of the movement operation (as in Chomsky 1995).

Geoffrey Poole's contribution, 'Constraints on local economy', looks at the contradictions that arise in trying to apply Chomsky's (1995) assumptions to former violations of the Empty Category Principle (now violations of the Minimal Link Condition). He rejects Chomsky's assumption that a Procrastinate constraint decides which choices are most economical, proposing instead his own Total Checking Principle, where the most economical movement is the one that allows all formal features to be checked.

Finally, there are a number of papers that do not fit easily into either the Minimalist or the pure OT-syntax categories.

Luigi Burzio's paper, 'Anaphora and soft constraints', shows that the patterns of anaphora in many European languages can be insightfully accounted for by different interleavings of three families of constraints. This paper is perhaps the one that comes closest to the ideal that the volume seems to have been aiming for. There is not a tableau in sight, yet Burzio very seriously applies the central ideas of OT to the problem, combining them with an understanding of the data that could only have come from a rich tradition of generative work on the question.

A couple of papers focus more on the role of competition in the semantic interpretation of syntactic structures. Danny Fox's paper, 'Locality in variable binding', tries to show how variations in strict and sloppy identity under ellipsis can be accounted for by 'Rule H' (deriving from Heim 1993), which prefers a local binding of a pronoun over more distant potential binding relationships. In 'Semantic and pragmatic context-dependence: the case of reciprocals', Yookyung Kim & Stanley Peters consider reciprocal expressions in English. The interpretation of sentences with *each other* may range from having reciprocal relations between each possible pair (*House of Commons legislators refer to each other indirectly*) to a much weaker degree of mutuality (e.g. *They stacked tables on top of each other*). Kim & Peters

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examine the role that optimality plays in selecting the strongest possible interpretation and the principles that determine the hierarchy of interpretation strength.

Edward Gibson & Kevin Broihier's contribution, 'Optimality Theory and human sentence processing', discusses whether the OT constraint evaluation mechanism can be applied more or less directly to on-line sentence processing. After reformulating various parsing principles (the Minimal Attachment and Late Closure principles of Lyn Frazier and her colleagues, and some of Gibson's own proposed alternatives) as OT constraints, they argue that the interaction of these constraints in the canonical OT fashion cannot explain the difficulties that live human readers get into when processing garden-path sentences. They argue that processing needs WEIGHTED CONSTRAINTS, i.e., that two weaker constraints can gang up and outrank a stronger constraint in a manner anathema to standard OT.

The last two papers focus on the acquisition of OT grammars, each proposing an algorithm that would allow the learner to arrive at the correct ranking of constraints, without having to consider each one of the astronomically large number of possibilities. Bruce B. Tesar, in 'Error-driven learning in Optimality Theory via the efficient computation of optimal forms', proposes an error-driven algorithm for demoting constraints until the correct ranking is achieved. In 'The logical problem of language acquisition in Optimality Theory', Douglas Pulleyblank & William J. Turkel discuss the dimensions of the learning problem (for phonology) and propose a genetic algorithm approach, with a continual process of selecting from a group of grammars the one that best matches the input data, introducing a number of slight changes to that grammar and selecting the best of the results.

In their introduction to the volume, the editors make a valiant, if ultimately doomed, effort to tie together the disparate strands from all these papers into a unified whole. The result is an interesting discussion of the state of syntax, especially OT syntax, in the 1990s. It is as worth reading as the papers themselves, something rare for the introduction of a collection. But the issues identified by the editors, while interesting and important, are not always ones that play large roles in the contributed papers. For example, the editors highlight the question of the reference set: if deciding the grammaticality of a sentence involves comparing it against competing alternatives, exactly what is the set of alternatives? But with few exceptions (such as Nakamura), the papers deal with reference sets only in passing, if at all.

The rest of this review will concentrate on the OT syntax papers, since it is in this field where the volume has made and will continue to make its greatest impact.

The unanswered or half-answered questions raised by the editors' introduction point out one of the most serious problems with OT syntax as

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a research program: vastly different underlying assumptions make it impossible to compare analyses. In Minimalism, most of the fundamental assumptions are relatively fixed and shared by all workers in the paradigm. Nakamura and Poole were able to examine the consequences of changing one theoretical assumption, while holding all others constant. For OT syntax, on the other hand, nothing is fixed. Several theoretical choices have to be made before one can even begin an analysis: What is the nature of the candidates? (Are they essentially LF? S-structure or the spell-out level? An LF-PF pair? Something else entirely?) Which constraints are part of the constraint hierarchy and subject to cross-linguistic re-ranking, and which are built into GEN? Does the OT constraint hierarchy explain all of syntax, or is some additional pre- or post-processing mechanism needed? No two papers in this volume share the same assumptions. With rare exceptions such as Newson, the authors seem to have chosen their particular set of assumptions mostly for convenience rather than for well-understood and well-argued commitments to a particular architecture. This makes it difficult to compare even those papers that tackle the same phenomenon, such as the two papers on *wh*-movement – where their results differ, it is not clear which assumption(s) the difference is due to.

It is tempting to try to compare this volume to an imaginary book which it doesn't try to be. There is a great need for an introduction to and overview of various OT approaches to syntax. Unfortunately, this volume does not fill the need. As an introduction, it lacks accessibility; as an overview, it lacks breadth.

In general, the volume assumes considerable prior knowledge on the part of readers. The OT papers assume familiarity with OT, sometimes with quite arcane aspects of it. The minimalist papers assume familiarity with Minimalism. Most papers assume detailed knowledge of the literature on the syntactic phenomena being discussed. Some authors even assume familiarity with their own earlier work on their topic. This makes the book as a whole difficult to read for either syntacticians with little knowledge of OT or OT specialists with little knowledge of syntax.

The volume is not entirely representative of the various strands of work in OT syntax (though it was somewhat more representative in 1997 and even more so at the time of the workshop in 1995). Most of the papers apply the ideas of OT to formal representations already developed by an established formal theory of syntax (Lexical-Functional Grammar or Principles and Parameters), without a great deal of consideration given to whether all the details of those representations are still needed or whether the problems that the details were meant to solve become irrelevant in an OT framework. For example, most authors would rather get a topic to the left edge of a sentence using a complex web of constraints manipulating scope, empty operators, chains and traces than by a simple constraint that says 'Put the topic on the left'. While Grimshaw & Samek-Lodovici make a few steps in this direction,

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there is no representative of a more concerted attempt to do things more directly, such as may be found in more recent work by João Costa or Ellen Woolford.

Also absent from the papers are any connections to the decades of research in the typological tradition of linguistics. For example, one of Baković's main results is essentially an elaboration of the lower end of Keenan & Comrie's (1977) Noun Phrase Accessibility Hierarchy. Baković's work might have benefited from taking the typological literature into account, and future typological studies would certainly benefit by taking Baković's work into account. One would hope that, as OT syntax continues to apply the factorial typology model to widespread comparison of different languages, there will come to be more contact between them and the researchers who have been doing it seriously all along.

Physically, the book's size is an awkward 10 inches by 7 inches. Surprisingly little of the page area is devoted to the text, which will delight those who love huge margins to write notes in, but others will wish the designers had chosen to use either larger type or smaller paper.

Is the best good enough? is not the imaginary book that introduces and surveys OT syntax. One can't even get from it a clear picture of what OT syntax will look like when it matures. 'But', as the editors conclude their introduction, 'it's a start'.

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Roger Blench & Matthew Spriggs (eds.), *Archaeology and language*, vol. III: *Artifacts, languages and texts* & vol. IV: *Language change and cultural transformation* (One World Archaeology **34** & **35**). London & New York: Routledge, 1999. Pp. xviii + 301 (vol. III); pp. xii + 253 (vol. IV).

Reviewed by Paul Sidwell, Australian National University

The two latest volumes in this welcome series continue the presentation of disparate papers by linguists, archaeologists and fellow travelers, grouped under loose themes. Growing out of, although not limited to, the Third World Archaeological Congress held in Delhi in December 1994, the series presents contemporary work that seeks to correlate the results of historical linguistics and archaeology. In all there are 20 papers over two volumes. I will mention each in turn, discussing in greater detail some which relate directly to my own expertise.

The first 20 pages of each volume are taken up by Blench & Spriggs' (B&S) 'General introduction', such that this particular text has now appeared four times without change. While I can see the value of some continuity, this strikes me as a combination of lost opportunity and redundant text with potential to confuse readers. For example, B&S take three paragraphs to give us an overview of lexicostatistics and glottochronology, yet neither of these closely related methodologies figures in the texts of volumes III and IV. One could also question the validity of their statements on the topic, e.g. 'However, very few historical linguists now accept the premises of such approaches' (9). I doubt that there is a linguist alive who would argue with the premise that languages have less inherited (versus borrowed) vocabulary in common with the greater passage of time. Rather the debate is over rate of change, which is only relevant to absolute dating. A persistent difficulty of interdisciplinary cooperation is thus highlighted – members of one field run into difficulty when they try to comment on another. I would much rather see this 'General introduction' dropped from any future volumes in order to simply let the various papers speak for themselves.

Volume III is divided into two parts. The first of these, 'Linguistic models in reconstructing material culture' is dominated by Austronesian linguistics (five out of six papers, the sixth relating specifically to Indo-Aryan).

The first paper, 'Early Oceanic architectural forms and settlement patterns: linguistic, archaeological and ethnological perspectives' by Roger Green & Andrew Pawley, is a product of the important ongoing Proto-Oceanic Lexicon project at the Research School of Asian and Pacific Studies at the Australian National University. The authors seeks to demonstrate correlations between the Lapita cultural complex reconstructed by archaeol-

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ogists and the Proto-Oceanic culture as reconstructed by linguists, combined with evidence of comparative ethnology.

The logical problem is how to justify linking historical linguistic data with archaeology. Their linguistic reconstructions are ultimately reflections of present day (or at least no more than 250 year old) language descriptions, and a similar caveat applies to ethnography – only archaeology gives us an unequivocal direct witness on the past. Therefore the challenge is to explain why particular material and linguistic forms found widely spread over the Oceanic area should be viewed as historical remnants. The technical capacity for long distance travel, and thus cultural transmission, has been maintained or increased over time. Therefore we must seek non-trivial structural correspondences rather than simple resemblances.

Green & Pawley present 35 Oceanic etymologies from the semantic domain of architecture and settlement to substantiate a material cultural reconstruction of Proto-Oceanic society. The results include the reconstruction of words for raised timber pole type houses with various characteristics. This is significant because, as they report, houses built on the ground are more common as one goes further east, and certain significant terms, such as reflexes of **gabwari* 'area underneath a house', are confined to Western Oceanic (76). Discussion of Lapita archaeology, including a detailed description of one particular site, confirms pole style housing, and the presence of lesser buildings that may also have designations in the Proto-Oceanic lexicon. Overall the paper offers some very satisfying preliminary results, and I can heartily recommend it as a clearly set out, well argued and well substantiated contribution to interdisciplinary scholarship.

The second paper, 'From pots to people: fine-tuning the prehistory of Mailu Island and neighbouring coast, south-east Papua New Guinea' by Tom Dutton, reconstructs a prehistoric although relatively recent language shift on Mailu Island, home of an important local ceramics industry along the southeast coast of New Guinea.

Significantly, the archaeological record of Mailu ceramics goes back 2000 years and clearly indicates an Austronesian heritage, even though today the population is non-Austronesian speaking. Archaeology suggests that the area was colonised by Austronesians around two millennia ago, and offers some clues as to when the Austronesians lost local dominance. There are indications of a discontinuity in the ceramics industry of the island around the year 1700 and there are significant basic words of Austronesian origin in the non-Austronesian speech of the islanders. The explanation appears to be that non-Austronesians learned the crafts of the Mailuans, and ultimately assimilated much of their culture. Once learning Mailuan Austronesian was no longer needed for working in the ceramics industry it stopped happening, and ultimately a complete language shift occurred. Dutton concludes reasonably that

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> [w]ithout corroborating written evidence, linguists cannot date absolutely events that may be reflected in their data, and archaeologists cannot put pots to speakers of particular languages. Taken together, however, linguistic and archaeological evidence may complement each other, to fine-tune a prehistory otherwise weakly outlined (106).

The third paper is 'Language, culture and archaeology in Vanuatu' by Darrell Tryon. This is a rather small paper that consists mainly of a very informative survey of the presently remarkable extent of linguistic diversity in Vanuatu, which can boast of being home to around 10% of all Austronesian languages. Radiocarbon dating shows habitation of the islands as far back as 1200 BC, and a complicated history punctuated by volcanic eruptions and tidal waves, and various periods/locations of settlement via back migrations into Melanesia from Polynesian islands that have contributed to this diversity.

The arrival of some of Polynesians Outliers can be correlated with geological and cultural events revealed in the archaeological record. For example, Tryon reports that the making and use of incised and applied relief ceased in central Vanuatu around 1250 AD, which correlates with the arrival of Outliers. Oral traditions also record these arrivals. Cultural discontinuities associated with geological disturbances can be dated absolutely by scientific analysis of the effects of ashfalls and tidal waves, thus helping to sequence events revealed by oral history and historical linguistic reconstruction. Tryon's paper contributes another useful case study in the interdisciplinary reconstruction of Austronesian prehistory.

Robert Blust, 'Linguistics versus archaeology: early Austronesian terms for metals', attempts to draw a broad brush picture of Proto-Austronesian metallurgical knowledge, opening with the indulgent boast:

> It has been a source of gratification over the ensuing two decades to observe that the great majority of culture-historical inferences which I proposed in 1976, based on the distribution of cognate linguistic forms, have been corroborated by advances in archaeological knowledge (127).

Blust reminds us that Dempwolff (1938) reconstructed Proto-Austronesian **emas* 'gold' on the basis of a distribution of forms that Blust considers is better explained as a borrowing from Mon-Khmer subsequently spread by Malay. As a comparative Mon-Khmer specialist I find this thesis is sound – the antecedent of Old Khmer *mas* 'gold' and cognates in neighbouring languages was probably transmitted variously via the culturally Indianised kingdoms such as Funan and/or Champa to the Austronesian world. However, rather than abandoning the notion that the Proto-Austronesians knew of gold, Blust rather boldly ventures that Dempwolff's **bulaw* 'reddish gleam' can be reconstructed to mean 'gold' on the basis of numerous widely spread reflexes that include this meaning and names for

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similarly coloured metals copper and brass. In his revised reconstruction it is a suffixed form **bulaw-an* bearing the locative *-an* that has the nominal meaning.

Significantly, Blust concedes that

> [w]hat is striking about this conclusion is that [sic] the lack of an archaeological support for gold in island Southeast Asia until the beginning of Indianisation, about two millennia ago (Bellwood 1985, passim) (131).

Striking indeed! This appears to me to be *prima facie* evidence that the nominalised form **bulaw-an* should not be reconstructed for Proto-Austronesian; instead we could suggest quite reasonably that the nominalisation was formed independently on the basis of the colour term according to the widespread pattern. Blust does not take this view, and instead offers a bolder explanation, that the lack of direct archaeological witness is explained by an absolute low level of use of the metal by Austronesians prior to the time when Indian cultural expansion gave it special significance.

On the basis of new Formosian comparisons Blust revises **besi* 'iron' to **bariS*, sure that it 'cannot be assigned any referent other than iron' (134). Again archaeology presents problems:

> Needless to say, this inference is widely at variance with the archaeological evidence for iron-working in island Southeast Asia, which does not predate the middle of the first millennium BC (Bellwood 1985) (134).

Blust dismisses any necessary connection between knowledge of iron and knowledge of iron-working, citing the known use of meteoritic iron in the ancient world.

Blust also posits a Proto-Austronesian **timeRuq* 'lead', with reflexes that typically mean 'lead' or 'tin', although again there are empirical difficulties:

> The reported distributions of native ores in inland Southeast Asia raise serious questions about the meaning and antiquity of **timeRuq*. Although the linguistic form itself appears to be old, it is difficult to see how it could have meant 'tin', since speakers of proto-Austronesian in Taiwain and of Proto-Malayo-Polynesian in the northern Philippines would not have had access to this material. On the other hand, if **timeRuq* meant 'lead' it is difficult to see why there is no convincing early term for 'silver' (see below), since lead, zinc and silver commonly occur together (138).

Dempwolff's **pirak* 'silver' is certainly a loan from Mon-Khmer. This leaves us with the unsatisfactory situation that there is little if any archaeological evidence for Blust's arguments based on comparative linguistics. Even if we accept his reconstructions, I would caution that the communication of a linguistic form over great time does not automatically suppose continuity of

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reference. Without strong archaeological support the present suggestions are quite inconclusive.

The fifth paper, Waruno Mahdi's 'The dispersal of Austronesian boat forms in the Indian Ocean', is pleasingly well illustrated and copiously supported with historical documentation. Reflexes of Proto-Austronesian boat terminology are found borrowed into languages around the Indian Ocean, e.g. Tamil *paṭavu/paṭaku* 'small boat', cf. Proto-Austronesian *p∂DaHu/paDaHu* '(sailing boat for) long distance navigation'. The weight of evidence suggests Austronesian arrival in India around 600 BC, prior to which there are no indications in any texts of double canoes, outrigger boats or ocean going rafts. Wahdi advises that this paper is intended to be read in conjunction with his contribution to volume IV (see below).

The last paper in this section is 'The formation of the Aryan branch of Indo-European' by Asko Parpola. It is an attempt to identify the 'prehistoric Aryans' archaeologically, with reference to textual, comparative and archaeological evidence. The climax of this piece is a two page table of suggested archaeological and linguistic correlations. I don't know that there's a lot of new material here. Parpola is an unashamed fan of the 'Kurgan' hypothesis of PIE origins, specifically identifying the Srednij Stog and Khvalynsk cultures (c. 4500–3500 BC) with PIE, and the later (c. 2800–2000 BC) Catacomb Grave and Poltavka cultures with Proto-Aryan. If the Indo-Europeans did originate on the Pontic steppes (on this question views differ considerably) then Parpola has sketched a possible historical scenario, suggesting numerous correlations that flow logically from this fundamental assumption. To be assessed properly, perhaps this paper should be carefully considered by Aryanists, particularly in relation to the textual interpretations.

Part 2, 'Interpreting text', collects six rather short papers, each less than 10 pages.

Harold Mythun, in 'The language of death in a bilingual community: nineteenth-century memorials in Newport, Pembrokeshire', surveys headstones in a Welsh graveyard. Some stones bear inscriptions in English, but many are bilingual in English and Welsh, typically biblical passages. Mythun finds that these stones are important texts for the community, playing 'an active role as triggers of memory and items with which blocks of genealogical and anecdotal information could be associated' (229).

The eighth paper, 'The pre-Classical circum-Mediterranean world: who spoke which languages?', was rewritten by Roger Blench from a text that appeared in *Mother Tongue* 21 (Daniel McCall & Harold Fleming 1994). Readers familiar with *MT* will know it as a repository of unsubstantiated speculation and linguistic comparabilia offered as bold 'hypothesis forming materials' (to use a phrase of John Bengtson, sometime editor). Of this revised draft Blench tells us 'Bridging passages have been written where the original text was incomprehensible' (247). I have myself heard the editors

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complaining about the efforts they have gone to preparing the various papers for publication, so I am struck dumb that they took the special effort to dredge up and rewrite this already published material for inclusion.

Michael Cosmopoulos contributes five pages of interesting discussion entitled 'From artifacts to peoples: Pelasgoi, Indo-Europeans, and the arrivals of the Greeks'. He considers whether the Pelasgoi, the pre-Greek inhabitants of the Aegean, were Indo-European, and whether they were conquered violently or absorbed gradually into incoming populations. Apparently we know quite a bit about the society of these people, although an account of their language(s) is lacking, and we are left to speculate that it may be connected with various non-IE place names. 'The ancient sources clearly state that the first Greeks were few and gradually infiltrated the Aegean; nowhere is there mention of an invasion' (253). Cosmopoulos endorses Renfrew's version of Greek colonisation as a 'process not an event', remarking that the archaeological record indicates gradual takeover rather than destruction.

I found absolutely fascinating the very brief paper by Rajesh Kochhar, 'On the identity and chronology of the Rgvedic river Sarasvati'. Kochhar explains that scholars have traditionally identified the mighty Sarasvati River of the Rgveda with the Old Ghaggar, today a minor stream that never reaches the sea, draining pitifully into the Thar desert, just one among many such channels. Kochhar's thesis is that the Rgvedic Sarasvati cannot be the Ghaggar; for this we must look to the west, the direction from which the Indo-Aryans came, bringing with them their language and, apparently, giving the newly encountered rivers Rgvedic names. Citing the physical similarity of the Helmand River in Afghanistan with the description of the Sarasvati, Kochhar proposes that this identification 'can consistently account for all its attributes' (263). Other toponyms in the vicinity of the Ghaggar are correlated with place names in Afghanistan as 'the Sarasvati traces the stepwise history of the Indo-Aryan migrations from Afghanistan to the Ganga plain' (264).

Volume III ends with John Lynch & Philip Tepahae's 'Digging up the linguistic past: the lost language(s) of Aneityum, Vanuatu'. Today there are about 500 speakers of Anejom on the island of Aneityum, with minimal dialect variation. 'Yet there is a persistent oral tradition among older Aneityumese people that there were two, or maybe more, indigenous languages spoken on the island before the arrival of the first Christian missionaries in the 1840s' (12). Lynch & Tepahae offer some rather weak linguistic evidence to support the assertions of oral tradition, but for me it is unconvincing. I maintain that one must always treat the claims of locals skeptically, otherwise we would waste time chasing Yetis and lake monsters among other absurdities.

Volume IV is subtitled *Language change and cultural transformation*, and is further divided into two parts, the first of which is 'Rethinking language

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classification', a title that immediately evokes thoughts of numerous recent longranger proposals.

The first paper is 'The languages of Africa: macrophyla proposals and implications for archaeological interpretation' by Roger Blench. Blench is clearly under the spell of the macro-phylaphilia, an infantile disorder from which I myself have only recently made a full recovery. Consider the following by Blench;

> In contrast to the New World and Papua, the composition of the major language phyla of Africa is generally agreed within the scholarly community (Blench 1993, 1997). Their internal classification remains disputed, as does the position of various isolates. However, given that Africa has the highest absolute number of languages of any continent, their classification remains a considerable achievement (31).

It is great when you can cite yourself as the authority, and even better when you can do so without misrepresenting entire sub-disciplines. The classification of New World languages is well known and received among Americanists (an authoritative recent source is Campbell 1997) and currently excellent work on the classification of Papuan languages is proceeding at the Research School of Asian and Pacific Studies of the Australian National University (improving established models of Papuan classification). The division of opinion within the 'scholarly community' alluded to by Blench is effectively that between excessive lumpers like Joseph Greenberg and those who actually specialise in particular language families/areas. The 'achievement' of classifying African languages is a rough consensus of probabilities – what is 'agreed to' is that which is hardest to evaluate by any means, while views remain divided regarding lower level classifications. The paper concludes that it is problematic for archaeology to respond to the variety of views being offered by linguists.

Next is Vaclav Blažek's 'Elam: a bridge between Ancient Near East and Dravidian India'. Blažek is not sure of McAlpin's (1981) thesis that Elamite and Dravidian share a special relationship, and explores other possibilities. The difficulty is that Blažek accepts a version of the Nostratic hypothesis that includes Dravidian, but upon investigation finds many lexical comparisons between Elamite and 'other' Nostratic languages, particularly Afroasiatic and Sumerian. These comparisons are no more or less convincing than those originally proposed for Elamo-Dravidian by McAlpin, which leaves a problem of interpretation, which he does not solve. A more conservative scholar might take the view that these comparisons actually argue against the reality of a genetic explanation, reflecting only accidental resemblances.

The third paper is 'Language diversification in the Akoko area of Western Nigeria' by Chinyere Ohiri-Aniche. This is a very tidy summary of the state of the art vis-à-vis the classification of non-Yoruba languages that exist in enclaves across the Ondo State of Western Nigeria. Five language groups

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(Yoruba, Edoid, Ukaan, Akpes and Akokoid), all generally considered to be Benue-Congo at some level, are investigated. Methods applied include historical phonology, lexical isoglosses and lexicostatistics. In relation to the latter it is especially pleasing that basic word lists for languages representing four of these groups are included as an appendix. Significantly cognate scores level off at around 20%, which is comparable to the inter-branch densities found in Indo-European.

Somewhat lower order relationships among languages are investigated in Jeff Marck's 'Revising linguistic subgrouping and its culture history implications'. Marck reviews the structure of the standard sub-grouping of Polynesian languages (as established by Pawley 1966 and Green 1966) proposing a revision of the classification. The standard model divides Polynesian into two coordinate branches, Proto-Tongic and Proto-Nuclear-Polynesian (PNP). The latter further bifurcates into Proto-Samoic-Outlier (PSO, including Samoan and Ellicean Outliers) and Proto-Eastern-Polynesian (the latter spreads over much of the Pacific, from New Zealand to Hawaii and Easter Island).

Marck reviews the interpretation of phonological and lexical evidence and finds that 'Samoic-Outlier is not defined by any uniquely shared sporadic sound changes' (112). Furthermore,

> [b]y comparing uniquely shared sporadic sound changes from Eastern Polynesia, Ellicean Outliers and Eastern Polynesian are placed in a group separate from Samoa. The classification of Samoan as the first to diverge within the group is very tenuous [...] (113).

The revised classification eliminates the PNP-PSO binary split, and instead places all non-Tongic languages under the PNP node. A major culture history implication of this revision is that '[t]he disintegration of Proto Nuclear Polynesian seems to be associated with two events that cannot be ordered in relation to each other' (115). Marck discusses various historical implications and attempts to correlate these with recent advances in archaeological dating of the Polynesian spread, concluding with various concrete suggestions for further research.

The second part of this volume, 'Interpreting change', consists of three very short papers and Mahdi's 80 page follow-up to his volume III contribution.

In paper number five John Waddell & Jane Conroy raise the 'process versus event' perspective of language change/shift in 'Celts and others: maritime contacts and linguistic change'. They challenge the notion of a Celtic invasion of Britain, evidence for which is apparently lacking in the archaeological record. Rejected is the simplistic idea that the English Channel and Irish Sea constituted natural barriers to trade and communication, and instead they suppose the gradual Celticisation of Britain from at least the latter part of the second millennium BC.

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The sixth contribution is 'Archaeological-linguistic correlations in the formation of retroflex typologies and correlating areal features in South Asia' by Bertil Tikkanen. Interestingly, Tikkanen finds that the systems of retroflexion in Dravidian and other languages known to exist in India at the time are structurally different to the system acquired by Aryan. He suggests an unknown substratum, although he acknowledges that we do not know enough about the early Aryan dialects of the Indus Valley.

Next is John Lynch's 'Language change in Southern Melanesia: linguistic aberrancy and genetic distance'. The paper makes the important methodological point that calculations of absolute and relative genetic distances in linguistics have to be based on comparison of structural rather than superficial features. The paper is really a direct rebuttal to the idea of 'aberrant' languages raised by George Grace in a number of publications. Grace argued that various Austronesian languages are 'less exemplary' than others, such that it is more difficult to apply the comparative method to them (e.g. they may show unexpectedly low cognate percentages and/or very complicated sound correspondences). Lynch makes the point that there is nothing really strange about these situations, explaining that these are really the normal consequences of well understood processes.

The final contribution is Waruno Mahdi's 'Linguistic and philological data towards a chronology of Austronesian activity in India and Sri Lanka'. This is a very long paper that has the feel of a full length monograph that has been chopped down to fit. The dense text presents a complex pastiche of related topics and source materials as Mahdi traces textual references to Austronesians and presumed Austronesians in Arabic, Greek, Persian, Indian, Chinese and other sources up to about AD 700, after which written materials become much more common. Over all, the style is very interpretative, littered with many assumptions, confident readings and asserted connections that I find difficult to evaluate, although consistently fascinating.

It is clear that Blench & Spriggs are endeavoring to take an inclusive approach in compiling this series, one consequence of this being the continuing uneven standard of papers. Also, while it is pleasing that Indo-European topics do not dominate the text, there is still a noticeable imbalance in the language areas and families being covered. Areas under-represented in these volumes include the Americas and Australia, although this can't be due to any lack of contemporary scholarship. If the present series is to continue I would strongly encourage that contributions on a wider range of geographical/linguistic areas be included.

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William H. Calvin & Derek Bickerton, *Lingua ex machina: reconciling Darwin and Chomsky with the human brain*. Cambridge, MA: MIT Press, 2000. Pp. viii + 298.

Reviewed by Andrew Carstairs-McCarthy, University of Canterbury, Christchurch New Zealand

The evolution of language, as a research topic, was for many decades the preserve of anthropology and psychology. Specialists in these fields, however, are not experts on grammatical theory, and when their discussion turns to details of syntax it is likely to appear to the linguist either naive or frustratingly vague. Derek Bickerton sets out to remedy this deficiency. In earlier books (1990, 1995), he has introduced much-needed grammatical sophistication into research on human linguistic prehistory. In this new book he joins forces with the neurophysiologist William H. Calvin, who has published semi-popular works on the brain (e.g. Calvin 1996, Calvin & Ojemann 1994) as well as scholarly articles on language evolution (e.g. Calvin 1993). *Lingua ex machina* is semi-popular in style too, and much of it is written in the form of dialogues between the two authors. They suggest how factors as diverse as reciprocal altruism, the neural mechanisms for accurate throwing, and the effect of ice ages on the human gene pool may all have contributed to the emergence of language as we know it. It is an ambitious explanatory project, to which most linguists are likely to react with

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scepticism; for it is still widely believed that the Paris Linguistic Society was wise when in 1866 it forbade papers on language evolution. However, I think that even the most sceptical, if they ponder this book and use it as a guide to the growing literature, will concede that at least some aspects of the topic are now researchable.

Bickerton is squarely Chomskyan in his view of the language faculty. This makes the book all the more interesting, because, although Chomsky regards Universal Grammar as part of our biological endowment rather than our cultural inheritance, he is notoriously reluctant to say anything about UG's evolutionary prehistory. Chomsky is entitled to his reluctance, says Bickerton: 'That's his business. No one has to do everything' (4). But 'obviously', Bickerton immediately adds, 'once it's established that language is biologically determined, the next step is for someone to try and find out exactly how it evolved'.

To me, that 'obviously' is refreshing and – well – obvious. We cannot claim to understand how UG is unless we can distinguish those characteristics of it that are virtually inevitable, given what language is used for, from those characteristics that could well have been otherwise and are due to accidents of human prehistory. This distinction is like that between bilateral symmetry and quadrupedalism as characteristics of mammals. Bilateral symmetry is a virtually universal design feature of creatures that move in a preferred 'forward' direction (e.g. mammals, insects and snakes, by contrast with starfish and jellyfish), while quadrupedalism is not (most mammals have four legs, but insects have six and snakes have none). Or is it the case that virtually everything in UG is the way it is inevitably? This seems close to what Chomsky (2000) means when, within the framework of the Minimalist Program, he speculates that aspects of language are 'ideal', 'optimal' or 'perfect'.

Bickerton too takes the Minimalist Program seriously. For him, however, UG is a product not solely of quasi-mathematical economy but also of contingent aspects of the human situation, such as our 'social calculus' (the way in which we keep track of how members of our social group are related, and who has done what to whom). But which aspects of the early human situation are relevant to the evolution of language? How precise are the expectations that these aspects yield about grammar-as-it-is? And how many of these expectations are fulfilled? Bickerton deals with these questions in an ambitious appendix (215–46). To most readers of this review, this appendix will be the most interesting part of the book, and it is the part that I will focus on.

Let us consider first Bickerton's explanatory goals. He claims that 'the core phenomena of a Chomskyan universal grammar can be derived directly from the exaptation of a social calculus, plus a theta-role hierarchy, the Baldwin effects of the exaptation, and a procedure for joining meaningful units' (215). ('Exaptation' refers to the acquisition of a new function on the

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part of a trait which originally evolved through selection for some other function. 'Baldwin effects' refers to the genetically influenced spread within a population of characteristics that are acquired, not inherited, but whose acquisition can be favoured by genetic factors.) Bickerton adds: 'The stakes are quite high. If I fail in this attempt, then a substantial part of this book must be dead wrong. If I succeed, then the account of language evolution given in this book is strongly confirmed' (215). I think Bickerton fails; but his failure is instructive, because it reveals how (along with nearly everyone else) he underestimates the diversity of the alternative conceivable outcomes of grammatical evolution.

Bickerton invokes four 'mechanisms' as the basis from which language-as-it-is evolved out of the syntaxless protolanguage which he has discussed extensively in his earlier books. These are:

A. Argument structure (the obligatory representation, dependent on verb-class, of one, two, or three arguments).
B. Obligatory attachment of all arguments to non-arguments.
C. A process of binary attachment of constituents …
D. A hierarchy of thematic roles that determines their order of attachment to the right and left of the verb. (218–219)

For Bickerton's enterprise to succeed, it is crucial that these four mechanisms should be automatic outgrowths of the cognitive and communicative milieu of protolanguage-using hominids on the threshold of syntax. It is crucial, that is, that none of them should be influenced in its formulation by anything that we know about how language happens to have evolved since that time; for any such influence would undermine the explanation by introducing circularity. However, only mechanism A escapes this sort of influence, and it does so only in part. Of the other mechanisms, D in particular goes well beyond anything that can be ascribed to social awareness in users of protolanguage, as I will try to show.

Let us examine first Bickerton's use of the term 'verb' in his formulation of mechanism A. This looks innocuous at first sight. Even if 'verb' is taken here as a shorthand for 'argument-taker', it may be thought that no harm is done, because prototypical argument-takers are verbs. But harm is indeed done. A predicate-argument structure can be encoded syntactically by a sentence consisting of a verb and associated NPs:

(1) Columbus discovered America.

But the same predicate-argument structure can also be encoded nonsententially:

(2) Columbus's discovery of America

It is true that, in actual English, this nominal encoding is grammatical only with the help of the nonverbal 'non-arguments' *-'s* and *of*, as Bickerton says

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by way of illustrating mechanism B (221). However, the point remains that the same predicate-argument structure can be encoded in a fashion that does not include a verb as a sentential head. It is therefore by no means obvious that syntax must inevitably have evolved so as to distinguish two sorts of encoding: a verb-headed 'sentential' sort and a non-verb-headed 'nominal' sort (Carstairs-McCarthy 1999). Yet the fact that the actual syntax of all (or nearly all) languages distinguishes 'sentences' from 'noun phrases' is ignored in Bickerton's explanatory story.

I suspect that this omission is due in large part to the fact that the term 'verb', as Bickerton uses it, masks a crucial distinction between a syntactic sense (something like 'prototypical head of a minimal free non-elliptical syntactic unit') and a semantic sense ('argument-taker'). It may well be true, as Bickerton argues, that early humans' mental representations included 'verbs' in the semantic sense, because of social intelligence enhanced by evolutionary pressures towards efficient detection of cheaters and free-loaders; but it does not follow that these semantic 'verbs' should be encoded with all the syntactic trappings of verbs in contemporary language. In previous work, Bickerton has admitted that he has no idea why, in creoles, tense marking in the verbal complex should be so pervasive, yet relative clause marking should often be so deficient, given that '[a]nyone purpose-building a language could make a much better case for a converse state of affairs: relative clause markers would be obligatory but tense markers could be freely omitted … [F]or some mysterious reason that doubtless lies hidden in the history of brain evolution, language simply reverses the communicative priorities' (1995: 37–38). In the book under review, however, this reversal of priorities is not even mentioned, let alone explained.

On the distinction between 'nouns' and 'verbs', Calvin offers a contribution from neurophysiology: 'Try to put together the simplest noun and verb for the first time, and you're probably invoking a long-distance circuit in the brain, a link-up between the temporal and frontal lobes' (26). But the nouny and verby roles of the temporal and frontal lobes respectively may just as well relate to the semantic distinction between an argument-taker and its arguments as to the syntactic distinction between verbs and nouns. So both authors use syntactic terminology in a fashion that undermines their account by projecting back into the transition from protolanguage to language a distinction that, although extremely familiar, is by no means an essential characteristic of any imaginable syntax.

Secondly, let us examine what Bickerton says about the attachment and ordering of arguments. Bickerton claims that early humans with a sophisticated cheater-detection mechanism would not merely have represented their experience in terms of argument-takers and arguments; they would also have ascribed thematic roles to the arguments, such as Agent, Theme and Goal. Furthermore, these thematic roles were ranked in a hierarchy: Agent > Goal > Theme/Experiencer. Let us grant both these

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claims, at least for the time being. The question now is: How much of clausal syntax do they yield as a natural consequence? For Bickerton's explanatory project to succeed, the answer must be 'Quite a lot'. However, in his section on argument structure and syntax (224–8), in order to derive the word order of *Bill gave Mary a book* and *Mary was given a book by Bill*, Bickerton is forced to introduce the following supplementary assumptions:

1. In any clause, there is a distinction between a final argument and the non-final arguments, the final argument being the one which, among those present, is highest in the hierarchy.
2. Non-final arguments are attached (in English) to the right of the verb, in accordance with the hierarchy.
3. The final argument is attached to the left of the verb.
4. Deviations from the order of attachment just described are permitted, provided that higher arguments are demoted to prepositional phrases (at least in English).

I say 'is forced to introduce', because it seems clear that supplementary assumptions as detailed as these cannot be straightforwardly derived from what I called earlier 'the cognitive and communicative milieu of protolanguage-using hominids on the threshold of syntax'. For example, the four conditions listed on page 215 that are meant to be jointly sufficient for the evolution of syntax ('the exaptation of a social calculus', etc.) include nothing that would predict a requirement that one argument (the 'final' argument) should be treated differently from all the other arguments, and that this difference in treatment should involve its acquiring the status of a syntactic subject, in traditional terminology. It is puzzling, then, that Bickerton should conclude by claiming success so confidently (246).

Readers are entitled to ask whether, having criticized Bickerton's efforts to account for details of clause structure, I can offer any alternative. One assumption that Bickerton & Calvin never question is that the evolution of language is linked more tightly to innovations in cognition than to innovations in vocalization. But this assumption may be false. At least one language evolution scenario currently in the marketplace attributes a much more central role to the vocal tract than Bickerton & Calvin do, and this scenario can account (I think) for two aspects of syntax that they handle less than satisfactorily: the distinction between nominal and sentential encoding, and the special status of 'subject' arguments within sentences (Carstairs-McCarthy 1999).

I do not expect the debate between these and other rival scenarios to be resolved quickly. What is most important for now is that Bickerton and Calvin have made a readable, thought-provoking and, above all, linguistically well-informed contribution. I hope that it will encourage readers of this journal to take language evolution as a research topic seriously, and to explore the wide range of current opinion on it.

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Peter Collins and David Lee (eds.), *The clause in English: in honour of Rodney Huddleston* (Studies in Language Companion Series). Amsterdam: John Benjamins, 1999. Pp. xii + 321.

Reviewed by Bas Aarts, University College London

This book is a collection of papers written in honour of one of the most wide-ranging and perceptive grammarians of the English Language in modern times, Rodney Huddleston. The book contains an introduction, a curriculum vitae and a list of Huddleston's publications, as well as sixteen papers. In what follows I will discuss each of them, though not necessarily in the order in which they appear in the book.

John Lyons' paper ('Sentences, clauses, statements and propositions') is a synthesis of ideas dispersed throughout his earlier publications on how to draw distinctions between such pairs of terms as SENTENCE/STATEMENT, SENTENCE/CLAUSE, SENTENCE/PROPOSITION, DECLARATIVE/INDICATIVE, etc. Although all these terms are familiar to linguists, Lyons carefully prises them apart, and in the process debunks widely held views, for example, that statements are logically and ontologically more primary than other speech acts, or that declaratives are more basic than interrogatives or imperatives.

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Central to his deliberations is an earlier distinction he made between terms belonging to language as a SYSTEM (such that we must recognise SYSTEM-SENTENCES, SYSTEM-CLAUSES, SYSTEM-PHRASES, etc.), and terms belonging to language conceived as a PRODUCT (which recognises TEXT-SENTENCES, TEXT-CLAUSES, etc., also referred to as UTTERANCE INSCRIPTIONS).

Noël Burton-Roberts' 'Language, linear precedence and parentheticals' uses the notion of 'representation', introduced in earlier work of his, to explain problems in the treatment of non-restrictive relative clauses (NRR). The terminology of this paper bears affinities with Lyons' system-unit/text-unit distinction, as will become clear presently. NRR have been treated either as being outside the domain of grammar (Haegeman 1988, Fabb 1990) or within grammar (Emonds 1979, McCawley 1982). Burton-Roberts agrees with the first view. However, he signals a number of problems for the 'extra-grammatical' approaches, principally that they lead to the conceptually anomalous situation that NRR, which can be shown to be utterance phenomena (i.e. physical entities), are hierarchically embedded within linguistic expressions (i.e. grammatical entities). In Lyons' terms, text-units are interpolated in system-units. Burton-Roberts argues that an account of NRR should make reference, not to a type-token view of the relation between linguistic expressions generated by the grammar and their physical manifestations, in which NRR are utterance phenomena, but to a relation that appeals to the notion of 'representation' where the NRR is a representation of a linguistic expression that is interpolated within an independent representation of another linguistic expression. In this way representations are linearly embedded within representations, and constraints on the positions of NRR can be formulated as constraints on the positions of representations. While this approach successfully eliminates the anomaly of hierarchically embedding utterance phenomena within strictly grammatical structures, the question remains, if the locution that parentheticals 'intervene within a clause' (48) is indeed invalid, as Burton-Roberts plausibly claims, WHY representations of NRR can intervene within independent representations. In other words, the question remains what could be the motivation for interpolating NRR in the middle of independent linguistic representations. To be fair, though, this issue is arguably outside the scope of the paper.

Peter Peterson also discusses NRR ('On the boundaries of syntax: nonsyntagmatic relations'), and in addition deals with a number of instances of similar relations, among them vocatives, interjections and parentheticals. These are structures that are inserted in sentences, but are argued not to be part of the syntactic make-up of the host clause. Peterson proposes a number of general constraints on the positioning of parentheticals which must be regarded as pragmatic in nature, if such interpolated structures are indeed extra-grammatical. However, the constraints are not really explained, nor are they embedded in a principled account of non-syntagmatic relations.

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Bernard Comrie's paper ('Relative clauses: structure and typology on the periphery of standard English') is also on relative clauses. It starts out re-examining the old question whether *that* in relative clauses is a pronoun or a subordinator. Drawing on Haiman (1990), Comrie examines the possibility that *that* in some contexts is a subordinator, but a pronoun in others, and finds evidence for both analyses in peripheral constructions of English. Thus, the possibility of the sentence *Remember the man that's house got burnt down* in Irish English (Harris 1993) argues in favour of *that* as a pronoun, given its genitival inflection. But in a sentence like *You come to a group that you have to eat certain foods* (Matsumoto 1997) *that* is best analysed as a subordinator, given the fact that it has no grammatical function in the clause that follows it. Given their peripherality, it's not clear how heavily the examples cited by Comrie should weigh. Especially the second example looks like a performance error.

Geoffrey Pullum & Arnold Zwicky ('Gerund participles and head-complement inflection conditions') attempt to improve on previous accounts of the 'double *-ing*' constraint, exemplified by such strings as *it is continuing raining*. They reject earlier proposals, by Milsark and Williams, and conclude that a traditional statement like the following is adequate: 'It is not acceptable in most varieties of modern English for a complement (as opposed to an object) marked with gerund participle inflection to be adjacent to its matrix-clause verb when that verb is likewise in the gerund participle form.' (269) As Pullum & Zwicky admit, a number of questions remain, not only issues to do with learnability, but also the question exactly WHY such a constraint should obtain in English.

Three papers are corpus-based investigations. The late Sidney Greenbaum & Gerald Nelson ('Elliptical clauses in spoken and written English') discuss ellipsis in finite clauses, using data taken from the British component of the International Corpus of English (ICE-GB). They distinguish between two types of ellipsis: independent ellipsis (*That little plant grows,* [it] *doesn't matter what the soil conditions are*) and coordination ellipsis (*And so we unpacked our stuff and* [we] *trooped in*), and chart their distribution across written and spoken English. One of their findings is that coordination ellipsis is twice more frequent in writing than in speech, and that this may be due to the fact that in speech repetition aids comprehension. Greenbaum & Nelson also investigate the locations of ellipsis, and find that the most frequently elipted elements are subjects or a subject combined with an auxiliary verb.

Peter Collins ('The deictic-presentation construction in English') uses corpus examples to describe constructions instantiated by such utterances as *Here's John, There goes our train*, etc. He argues that a number of syntactic properties warrant positing a special construction, distinct from inversion structures. Nevertheless, according to Collins the construction is not (yet) fully grammaticalised.

Lesley Stirling ('Isolated *if*-clauses in Australian English') discusses the

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phenomenon of isolated *if*-clauses of the type *If you'd all leave now, the museum is closed*, where there is no apodosis. Such expressions can have a directive force, and then bear a similarity to indirect speech acts. Stirling found them to be common in a corpus of patient-doctor interactions (*If you'd like to take your socks off, please, Mr Jones*). Another type of isolated *if*-clause is optative in nature: *She's so brusque. If only she'd have more grace*. Stirling argues that both types of *if*-clauses (directive and optative) behave syntactically like main clauses, and are well on the way to being conventionalised as such.

Lynn Wales' contribution ('Functional *and* structural: the practicalities of clause knowledge in language education') is an applied linguistics paper. Overall a somewhat diffuse study, it argues that a sound knowledge of clause structure is needed by teachers of English, who in their turn may impart this knowledge to students, enabling them to improve their practical linguistic skills. Some of the consequences of this conclusion are discussed with reference to textbook writing and syllabus design.

John Payne ('The English accusative-and-infinitive construction: a categorial analysis') and Hisashi Higushi ('On the nature of *?I believe Jack to arrive tomorrow*') both write on V + NP + *to*-infinitive constructions. Payne gives an account of the A-and-I construction in English using the Categorial Grammar framework. His proposal is to modify the 'wrap' analysis in which a string like *believes to be a genius* is wrapped around an object like *her*, deriving *believes her to be a genius*. The proposed new account regards objects like *her* as infixes. This analysis has its attractions, as in some sense *believes to be a genius* in *he believes her to be a genius* is a complex predicate acting as a constituent, as the following sentence shows: *Joan believes everyone that Fred does to be a genius* where the proform *does* substitutes for *believes to be a genius*.

Higushi tries to account for the impossibility of sentences like **I believe Bill to win tomorrow*. He expresses doubts about the grammatical and formal explanations for this judgement found in the literature. In the former, for example, it has been noted that the infinitival must express a state, but Higushi notes that e.g. *I believe her to beat the children* is fine. A number of other factors, such as, for example, modality and aspectuality, are pinpointed as being relevant to the judgements.

Five papers in this volume are not directly related to its general theme, or only very vaguely.

The aim of Keith Allen's paper ('The semantics of English quantifiers') is to 'use just one metalanguage to provide a comprehensive account of number, countability, quantification, and (in)definiteness in English and to show semantic interrelations between them' (1). It deals with clauses to the extent that the semantics of quantification depends to some degree on clause semantics.

Ray Cattell ('The English modifier *well*') explores the syntax and semantics

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of the string *well* + passive participle. He distinguishes two types of *well*: 'quality'-*well* (as in e.g. *The ball was well fielded by the bowler*), which has two subtypes where *well* means either 'effectively' or 'favourably', and 'quantity'-*well* (as in e.g. *The sisters were well separated by the other runners*). The different meanings display distinct syntactic patternings, sometimes constrained by pragmatic factors. With regard to *well*-expressions, the author concludes that 'it seems clear that there is a continuum of "passive participle" interpretations, running from more to less "adjectival". The awkward fact seems to be that at this point of the grammar, two categories flow together' (64). As to why this should be awkward is not made clear.

James McCawley's posthumous, squib-length paper ('Some interactions between tense and negation in English') discusses, amongst other issues, the grammaticality contrast in the following pair of sentences: *Have they still not answered our letter?*/**Haven't they still answered our letter?* As the second of these examples shows, the negative element cannot be pied-piped to the front of the sentence along with the auxiliary verb. Where there is no adverb like *still* the judgements are exactly the other way round: *??Can you not help me?*/*Can't you help me?* McCawley suggests that the explanation has to do with the fact that because in the first pair of sentences *not* is within the scope of the adverb ('it is *still* the case that they have*n't* answered our letter'), it will resist fronting to a position where it is linearly outside its scope. (The auxiliary *have* is also within the scope of the adverb, but must of necessity be fronted to form an interrogative.) Where there is no adverb, as in the second pair of sentences, the scope relations are not upset if the negative is fronted.

Peter Fries ('Post nominal modifiers in the English noun phrase') re-examines the distinction between complements and adjuncts in noun phrases by critically discussing seven criteria put forward in Radford (1988). He argues that the difference between these grammatical functions is not always obvious, and considers explanations of the differences in ordering, substitution, coordination, etc. in functional terms, especially information ordering. Fries is right to question the distinction between complements and adjuncts, as the two are really only clearly distinguishable within verb phrases in the X-bar theoretic framework. However, as is often the case with functional explanations, they are stated here in rather vague, impressionistic terms. Ultimately, Fries does not 'take sides', but offers a collection of NPs which are a challenge from a structural point of view for linguists of both formal and functional persuasions.

David Lee ('Intransitive prepositions: are they viable?') re-assesses the grammatical status of intransitive prepositions. He argues that, although commonly adopted in much of the current linguistic literature, they are by no means always clearly distinguishable from adverbs. Adopting a Socratic argument-counterargument-response format he carefully considers a variety of analytical options for elements such *aboard*, *away*, *here*, *home*, *now*, *then* and *where*. The conclusion is that linguistic frameworks which assume that

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lexical categories (conceived as clusters of morphosyntactic properties) project into higher-order categories cannot handle the data. Instead, Lee proposes that an emergence theory of category structure, in the sense of Langacker (1990), might be helpful here. In this theory different grammatical categories can share properties to different degrees: some categories are more highly schematised (e.g. nouns) than others (e.g. adverbs and prepositions). It's not entirely clear that emergence theory actually explains the problematic data under discussion, or merely offers a different descriptive account of them.

The book is well-produced, though it does contain a number of minor infelicities, such as the fact that the endnotes in Lyons' paper and the example numbering in Lee's paper are out of sync with the text. And then there is Lyons' odd complaint (171) that certain typographical conventions which he used in the manuscript version of his paper '[r]egrettably … have not been maintained in the published version'. If Lyons was able to insert this comment, why did he not simply reinstate the said conventions? Did the publishers not allow him to do so, perhaps because of financial considerations? ('Inserting a comma costs £3.50, you know', a copy editor once snapped at me.) Whatever the reason, it appears that someone tampered with this author's work. Another minor criticism, already hinted at above, is that not all the papers address the theme of the book. Overall, though, this volume represents a fine collection of stimulating studies.

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Steven Franks & Tracy Holloway King, *A handbook of Slavic clitics*. Oxford: Oxford University Press, 2000. Pp. xv + 403.

Reviewed by ANDREW CAINK, University of Wolverhampton

The book's stated aims are modest: to be a descriptive reference book of Slavic clitics, and to review some recent generative treatments. The imagined readers are both general linguists interested in clitic phenomena in Slavic and those Slavic linguists who may be less familiar with recent generative theory. Strictly speaking, therefore, this handbook does not purport to argue for a particular new theory, though in fact, prominence is given to the authors' analysis in the final section.

Following a general introductory chapter, there are three sections, the first two descriptive and the final section focusing on theoretical analysis. Section I deals with clitics on a language-by-language basis, the chapters addressing in turn South Slavic (Serbian/Croatian, Slovenian, Bulgarian, Macedonian), West Slavic (Czech, Slovak, Polish, Sorbian) and East Slavic (Russian, Ukrainian and Belorussian).

Section II is also largely descriptive, but marshals both additional data and data from section I around specific cross-linguistic issues. Chapter 5 compares the cross-linguistic data showing the order of pronominal and auxiliary clitics in the clitic cluster. Chapter 6 addresses the position of the clausal clitic cluster, distinguishing between those languages that adopt a 'second position' and those whose clitics appear adjacent to the verb. A final, somewhat hurried section discusses clitic climbing phenomena in Serbian/Croatian including new data from Slovenian. Chapter 7 returns to data from section I that exhibits clitic doubling in Bulgarian and Macedonian, and rehearses Rudin's (1997) analysis of pronominal clitics as functional heads. Chapter 8 focuses on the question particle *li*, conditional modal verbs and the negative particle cross-linguistically. In chapter 9, the authors review pronominal clitics inside the NP in Bulgarian, Macedonian and Polish, with a final section arguing that the determiner in the first two languages is an inflectional morpheme.

Turning to section III, chapter 10 provides a survey of some recent analyses. It includes an overview of purely prosodic and purely syntactic accounts of clitic phenomena in, largely, South Slavic and outlines the problems they encounter. There is a very brief glance at 'non-derivational' accounts before Franks & King (henceforth F&K) review what they deem to be the most promising approaches that take a middle way between prosody and syntax.

Chapter 11 presents a fundamentally syntactic account of South Slavic

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clitic cluster location and formation, with additional machinery in the syntax-to-morphology mapping and the phonological component. Chapter 12 deals with a treatment of the question particle *li* cross-linguistically, the possibility in Serbian/Croatian for clitics to split constituents and so-called Long Head Movement (Lema & Rivero 1989). Chapter 13 provides a brief summary for both chapters 11 and 12.

Evaluating first the descriptive, 'handbook' nature of this work, section I is a clear and useful contribution to the field, with generous use of tables to exemplify paradigms. The authors have gone to considerable lengths to add to the stock of data in the anglophone linguistics literature. There is considerable disagreement amongst native speakers about some data (a fact that rather undermines those analyses that are founded on such marginal constructions), so it is worthy that data has been extensively checked with various native speakers, and conflicts are, in places, carefully documented.

Section II also serves a useful purpose in summarizing the data around specific issues, at times pursuing theoretical analysis and at times concentrating solely on re-formulating data from section I in preparation for later analysis. In fact, this ground-preparation in sections I and II undermines the descriptive claims of the book. One example will suffice: the descriptive generalization that Bulgarian always places the clitic cluster adjacent to the verb is interestingly undermined by data discussed by several authors, but this data does not appear in section I and is referred to only in footnotes later (237, fn. 9 and 290, fn. 4). In terms of the analysis in section III, these are anomalous data. This is entirely reasonable in any formal analysis; one generally finds problematic data consigned to the footnotes, if included at all. However, it undermines a little the implicit claim both in the introduction and in the term 'handbook' that this is a descriptive reference book. It is partly that, but partly a sustained argument for a theoretical position set out in section III.

Section II also exhibits an increased casual use of undefined formal terms ('extended projection', 'AgrS^0', 'T^0'), which suggests the intended reader is one relatively well-versed in generative theory (though not one so pedantic as to require precise definition of 'extended projection' when the analysis later posits a nominal K^0 (= 'Kase') head that projects an AgrP in the extended projection of the verb (317)).

The other aim, to 'assemble and compare the extensive range of approaches to Slavic clitics' (4), is an enormous task given the way in which the field has developed in the last decade. Doubtless every researcher has a different list of contenders that might have been included. In general, F&K present an impressive summary of the more high-profile analyses. One substantial loss worthy of mention is that, given the lack of an effective account of Macedonian clitic placement in section III (or anywhere else in the literature), it is a shame that Legendre (1998) receives only a token paragraph with no critique (292). Her Optimality Theoretic account is

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revealing because she captures the tensed/non-tensed clause distinction in Macedonian clitic placement by arguing that [tense] competes for second position with the clitics. Anderson's (1993) influential article also receives scant attention (291). Its significance lies not in the proposal of the parameters of 'scope', 'anchor' and 'orientation' (he adopts these from Klavans 1982) but in giving generative teeth to Wackernagel's link between verb second and clitic second (see citation in Anderson 1993).

Chapters 11 and 12 mainly argue for a fundamentally syntactic approach to South Slavic clitic clusters, with additional extensive post-syntactic apparatus. Broadly, F&K's account is as follows. Serbian/Croatian pronominal clitics are arguments that move to check features with Agr heads. Bulgarian/Macedonian clitics differ in being generated as heads of Agr phrases (372) or adjoined to Agr heads (317) which the finite verb carries up to $AgrS^0$. The motivation for the typology is the distinction between the verb-adjacency of Bulgarian/Macedonian clitics (stemming from this step-by-step clustering in the syntax) and second position clitics in Serbian/Croatian. Supporting evidence is found in the presence of clitic doubling in Bulgarian/Macedonian and its absence in Serbian/Croatian: in the former languages, arguments may co-occur with pronominal clitics (or be *pro*), in the latter, the pronominal clitics are the arguments. Additional evidence is taken from diachronic linguistics (318): older Bulgarian was clitic second (i.e. like Serbian/Croatian) but changed to being a 'verb-adjacent' language at the same time as determiners appeared and case morphology was lost. The appearance of the determiner 'triggered' the reanalysis of the pronominal clitics into being Agr heads (319). Unfortunately, no historical data is included, and no formal explanation is forthcoming as to how the appearance of determiners leads to reanalysis of pronominal clitics. Further support for the account is found in Macedonian dialects where the masculine singular clitic can double non-masculine objects; this is taken to indicate that gender is no longer a part of argument checking, hence the checking relation has more in common with subject-verb agreement (no Macedonian data is provided here). The account is intriguing, linking as it does the rise of determiners, the loss of case morphology and the shift from being a 'clitic second' language in Bulgarian. However, in the absence of data and any formal detail, it remains essentially a thumbnail sketch here.

With respect to the formation of the clitic cluster, a prime aim of the authors is to defend the notion that the clitic cluster is a result of syntactic processes, clitic order being a reflection of a functional hierarchy. Yet to avoid the array of stipulations that are necessary to ensure the right clitic order both within the clitic cluster and in relation to the rest of the clause, the internal order of the cluster is determined in a post-syntactic Optimality Theory-influenced component. A constraint LEFT EQUALS HIGHEST (LEH) says 'pronounce the syntactically higher head first' and another constraint PROSODIC SUPPORT (PS) requires a clitic to have a host. The PS constraint is

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higher than LEH in Bulgarian, hence enclitics cannot appear in first position (1b) and the alternative spell-out (1a) wins out.

(1)	(a)	Dade	mi	go	včera.	[Bulgarian]
		gave.3SG	me.DAT	it.ACC	yesterday	
		'He gave me it yesterday.'				
	(b)	*Mi	go	dade	včera.	
		me.DAT	it.ACC	gave.3SG	yesterday	

(I am recreating the data intended. Unfortunately, examples (34a) and (35a) on page 431 do not show the verb-initial examples the authors intended.) In (1a), the LEH constraint is violated, in order to satisfy the requirement of PS.

An additional constraint PRONOUNCE HIGHEST COPY (PHC) leads to other 'second position' effects. Thus in so-called Long Head Movement (2), the participle does not move to C^0 across the clitic cluster (see Lema & Rivero 1988):

(2)	(a)	Predstavio	sam	mu	se.	[Serbian/Croatian]
		introduced	auxiliary.1SG	him.DAT	REFL	
		'I introduced myself to him.'				
	(b)	~~Sam mu se~~ [predstavio [sam mu se [~~predstavio~~]]]				

Rather, a lower copy of the auxiliary and clitics is spelled out, indicated in (2b). The mechanics of this approach are not given in any detail. In particular, it is unclear what the lower position of the auxiliary is or the higher position of the participle in (2b). Clarity on these points is surely essential for the account to viably compete with even the movement to C^0 account, let alone others in the literature.

For any Optimality Theoretic approach, the onus is on the researcher to provide evidence that languages exhibit the various possible constraint rankings. What we are not shown here, for example, is a language where the PHC is higher than, say, PS; that is, a language with clitics that ordinarily require a host to the left, and which in some contexts exhibit a clitic without a host in first position. In the absence of such fundamental Optimality Theoretic argumentation, this account does little more than describe the facts. (Note that F&K appear to independently retain a Prosodic Inversion mechanism (Halpern 1995) for particularly recalcitrant data such as the infamous name-splitting clitics in Serbian/Croatian (348). This mechanism equally predicts (1) and (2).)

The effect of the Optimality-style machinery is to ensure that the clitic order mirrors the hierarchical order of functional projections, with languages differing between whether or not there is a first position constraint. Despite this, a number of syntactic stipulations are still necessary to arrive at the attested word orders: clitics must jump over T^0 on their way to $AgrS^0$ if T^0 contains a 3rd person singular auxiliary clitic, but they carry the clitic along if it is a 1st or 2nd person clitic (329); the Bulgarian auxiliary *šte* 'will'

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moves up in an ad hoc way to $AgrS^0$ in order to appear in front of the clitic cluster (330).

The complete picture thus involves the stipulation of some novel syntactic movements, an OT-style machinery between syntax and morphology, and the apparent retention of a Prosodic Inversion mechanism in the phonology. The intuitive approach, combining syntax and prosodic factors, is surely right, but this is ultimately a rather stipulative way of tackling it. Sadly, the formal precision of the account in chapters 11 and 12 is not always apparent, in contrast to the laudably clear descriptive sections. There are, additionally, some curious uses of terminology: 'percolation of V^0 to the top of its extended projection' appears to mean 'verb movement' (326); for clitic doubling to become 'grammaticalized' in Macedonian (72, 251, 257) means to move from being optional (in Bulgarian) to being obligatory.

To conclude, despite the caveats, the analysis of South Slavic has some useful and promising insights whilst the descriptive sections bring together and substantially extend a wealth of Slavic data. The book is a significant contribution to the field and will immediately become a much-cited starting point for any discussion of (particularly South) Slavic clitics.

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Peter L. Patrick, *Urban Jamaican Creole: variation in the mesolect*. Amsterdam: John Benjamins, 1999. Pp. xx + 329.

Reviewed by SALIKOKO S. MUFWENE, University of Chicago

Urban Jamaican Creole (UJC) is a stimulating and thought-provoking book, quite an informative and useful addition to the literature on speech continua, though there are respects in which, as I hope to show below, the author's interpretations of the data remain disputable. Patrick addresses several interrelated questions about the creole mesolect, which he summarizes in the conclusions of the book as follows: 1) what is 'the nature of mesolectal grammar?' and 2) what is 'the sociolinguistic structure of variation in the creole continuum?' (292). While the second question is answered to my satisfaction, the first is not, due in part to assumptions about the mesolect and its relation to both the basilect and the acrolect that I do not accept. I discuss the assumptions first, so that the reader can better understand my criticisms of a book that I otherwise think is competently put together.

Patrick presents the mesolect and its lectal continuum as a reality that can clearly be distinguished from both the basilect and the acrolect. This position is disputable, first because a basilect is just an analytical construct intended to depict the extreme systemic level of divergence from the acrolect that a linguist can infer of a creole. The standard, often misidentified with the acrolect (the speech of the educated and/or upper class), is the other extreme of the continuum within which most speakers in a creole community gravitate in one or the other direction. Natural speakers whose discourse evidences all basilectal features where they are expected are as rare as native English speakers who exhibit only characteristics of the standard variety in all contexts. Much of acrolectal speech is colloquial and often contains non-standard features. In the case of Jamaican society, even acrolectal speakers often display some of the features associated with Creole, such as the merger of 17th-century /æ, ɔ, ə/ and /a/ into /a/. Thus positing a mesolectal grammar that is distinct from basilectal grammar is problematic, as much as I agree with Patrick that mesolectal grammar is structurally heterogeneous. In fact, grammars are generally not monolithic (Mufwene 1992). They exhibit principles that sometimes are not consistent, overlap, and thus compete with each other, as is evidenced by UJC. Utterances that they generate can be accounted for with two or more coexistent systems (Labov 1998), though it is sometimes difficult to determine unequivocally which system is at work. This is precisely where Patrick often errs, when he excludes almost a priori the possible role of the basilectal system in the production of some of the variants he discusses.

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He is right in assuming that these features – be they 'creole' or otherwise – originate in English. However, one must also remember that 'creole continua' are partly a consequence of the fact that different dialects of their lexifiers came to coexist and presented conflicting models in the colonies. The fact that the vast majority of the populations speak mesolectal varieties that diverge significantly from the acrolect reflects a contact history in which slaves far outnumbered the European colonists and the lower and working classes still far outnumber the upper class today.

The book is otherwise efficiently structured into eight chapters. In the first (1–22), Patrick situates the subject matter and formulates the central questions he addresses. He correctly states that mesolectal Jamaican speech is structurally heterogeneous, dissociates the continuum from 'decreolization', and argues that a multidimensional characterization of this linguistic situation is preferable to a unidimensional one – a position he proves well in the chapters where he discusses variables other than 'phonolexical' KYA (see below).

In chapter 2 (23–64), Patrick presents Kingston as an urban setting, which developed differently from rural Jamaica, and he identifies the Veeton community in which he conducted his field research. He characterizes UJC as largely mesolectal, in contrast with rural, basilectal speech. Occasionally he identifies the latter varieties as 'conservative', suggesting that rural speech is older, perhaps where contemporary Jamaican speech started overall. He actually observes that '[t]he linguistic clocks in rural areas do not simply run more slowly – they operate in a distinct social context and cannot be expected to slavishly follow urban developments a generation behind' (49).

His position is disputable. Chances are that since Kingston and the rural sugar cane plantations developed concurrently, all the lects of the Jamaican continuum (with probably the exception of the standard introduced through the scholastic medium) evolved at the same time, with the varieties closer to the basilect concentrated in the rural areas, where the slaves and their descendants have always been the overwhelming majority. Although rural speech is stigmatized, it is not obvious that people who always live in a rural environment ever wish to speak like urbanites. Attitudes toward the latter are not always positive. Nevertheless, Patrick stratifies his consultants in a useful way that shows later in the book why education and social status, for example, cannot independently account for mesolectal variation.

Chapter 3 (65–82) explains the author's field methods: a combination of interviews, English-to-Patwa translations, and standard English reading tests, complemented by a language attitude questionnaire and several informal observations during his interactions with the population in various Jamaican vernaculars, which he speaks fluently.

Chapter 4 (83–119) introduces the first variable for analysis: KYA, a convenient representation for words that contain a velar stop that is followed by a vowel that in American and British English mainstream varieties

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corresponds to /æ, a:, ar/ or /ɔ/. The vowels /a:/ and /ar/ are conveniently represented as AR. In UJC they are both produced as /a:/ and thus distinguished from the other instances of /a/ by length. In lower mesolectal speech, only some of the words containing /a/ (those which should historically be non-back, assuming /a/ was a central vowel) show palatalization of the velar stop. Thus, *cart*, *garden* and *can* do but *cot*, *caught*, *got* do not. The explanation is that this pattern represents continuity from seventeenth-century English (118). In varieties close to the acrolect, a phonological constraint prevents palatalization in words that have AR, as 'in the modern Midlands dialects, Northern Ireland, and Charleston' (119).

Here we see the first evidence for systemic mixedness in UJC, with one group using a phonological principle in the production of the relevant words but another not. Patrick argues perhaps too fast against substrate influence, which must have favored the merger of /a:/ and /ar/ into /a:/ and the elimination of the phonological constraint still operating in the acrolect. On the other hand, he could have considered the following argument against substrate influence: in African languages, as in most others around the world, palatalization of velar stops usually occurs before high front vowels, not before lower ones. My point is simply that diverse influences are not necessarily mutually exclusive.

One practical problem readers not familiar with UJC may experience in reading this chapter and the following ones is that Patrick is slow in adducing the relevant examples in his arguments. They tend to be presented rather late. He also claims that the KYA variation represents change in progress, since it is mostly the older speakers who have no phonological constraint regulating the production of the relevant words. This position is disputable in the Jamaican context, since no evidence is provided of such an ongoing change in rural Patwa and the young may just reflect the fact that in Kingston the phonological constraint has been strong since UJC's inception. After all, most of the older speakers immigrated from the rural area.

Chapter 5 (121–166) is about the deletion of word-final /t, d/. It includes comparisons of constraints on this process with those that operate in American and British English varieties. For reasons of primarily analytical economy (159), Patrick concludes that this process operates in basically the same manner as in English dialects, except that ratios of '(TD)-absence' are higher in UJC (even before vowels) and regular verbs show the highest rate of all verbs, next to the weakening of negative *n't* (150).

Although there is independent evidence for hypothesizing consonant cluster reduction in UJC (e.g. *lost* [last] > [las] and *last* [la:st] > [la:s]), the high rate of absence in 'past verbs' suggests that this continuum may be underlain by a non-monolithic system. In this case, lower mesolectal speakers may be using a basilectal principle that allows using the verb stem with PAST meaning when the discourse context makes this obvious. The minority of cases where /t, d/ are attested correspond to insertions under pronunciation

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specifications that overlap with those specified for 'deletion' in other English varieties.

Patrick discards this alternative analysis because it is not consistent with the traditional assumption that morphemes are inserted first and phonological rules apply to their outputs. First of all, this convention is not *ipso facto* an argument for psychological reality. Second, morphological specifications can also consist of amorphous abstract specifications, e.g. PAST, and abstract morphophonological rules can be posited that give phonemic shape to the morphological abstractions. Surely, early generative phonology in the 1970s provided evidence for preferring deletion to insertion rules for the sake of generality. However, aside from the fact that generality does not entail psychological reality, insertion would be just as general as deletion in this particular case, especially since the absence rate is almost the same for regular verbs as for semi-weak ones (e.g. *send*), viz. 56% and 59%, respectively (157).

Chapter 6 (167–222) is about the 'pre-verbal past-markers' *did* and *neva*. Aside from noting accurately that their text frequency is very low – contrary to what might be expected from theoretical analyses – Patrick concludes again that the pattern of their distribution is English. What the reader should remember is that in UJC, as in the basilect, *neva* also has another meaning, viz. PAST&NOT, which is more or less suppletive for *no ben* ('did/had not') and different from the English meaning NOT EVER. Like basilectal *ben*, *did* is also part of a relative tense system and is not always a morphosyntactic alternant of *-ED*. The structure of UJC may be more non-monolithic than Patrick admits. It is perhaps not by accident that the durative *dida* is so similar to *bena*. *Did* is also used almost exclusively by lower-mesolectal speakers (204–205), subject to discourse constraints similar to those of *ben*. These observations are not arguments against Patrick's position that the markers originated in English. My point is simply that English origin is not a sufficient reason for suggesting that the grammatical functions of preverbal *did* and *neva* (as of *ben*) have remained the same as in the lexifier. The restructuring of English dialects into UJC involved some concurrent changes (though only minor in some cases) in the functions of the selected grammatical morphemes.

In chapter 7 (223–266), Patrick helps us put the discussions in a larger picture, as he focuses on 'past-marking by verb inflection'. Interestingly, even irregular verbs have a very low rate of past-marking, viz. 32%, as opposed to 44% for semi-weak verbs and 46% for regular verbs (231). Stative verbs are more often inflected than nonstatives (256), consistent with *ben*-marking in the basilect. The alternative analysis which Patrick seems to disfavor becomes more appealing, viz. that most speakers know principles associated with Patwa and those associated with standard English and they alternate between both (not in the sense of code-switching!) but they do not possess a uniform norm that first inserts a PAST morpheme and then allows

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them to delete it. Patrick's figures 4.7, 8.1 and 8.2 seem consistent with this interpretation of the linguistic performance of his consultants. The figures suggest clearly differentiated systems for some speakers but more or less blurred ones for others. The speakers' productions reflect the extent to which they command one or the other system better or their ability to gravitate between them. In this chapter, Patrick acknowledges that there are competing forms in the mesolect (251) and in fact competing grammars (264–265), although this correct observation is not quite like the single-norm position he defends in the previous chapters.

Chapter 8 (267–295) concludes the book, noting for instance that UJC is not a product of code-switching (292), that it is underlain by a mixed grammatical system (293), that the continuum model that emerges from his analysis is 'non-discrete' (292), that there is an 'asymmetry of dual sets of norms for (synchronically-)related varieties in the creole continuum' (284), that 'mesolectal speakers do not have (or at any rate, use) a full basilectal grammar, but have not fully acquired an English one' (293), that 'most varieties show a relatively sharp break in their distribution across the population, but these breaks tend not to coincide' (283), and that a unidimensional account of the continuum is not valid (286). He admits that similar continua exist elsewhere in non creole-communities, but unfortunately he does not suggest ways in which the present study can enrich our understanding of such continua. I suspect that the working assumptions I disputed at the outset of this review have something to do with this shortcoming.

Nonetheless, the book remains very informative on UJC and leaves us with the challenge of how to best articulate non-monolithic systems – which are not two separate languages or dialects, but coexistent forms and overlapping principles that compete for use within the same system.

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Nanda Poulisse, *Slips of the tongue: speech errors in first and second language production* (Studies in Bilingualism **20**). Amsterdam & Philadelphia: John Benjamins Publishing Company, 1999. Pp. xii + 266.

Reviewed by Almeida Jacqueline Toribio, The Pennsylvania State University

Slips of the tongue is inspired by the laudable aim of drawing together two lines of research: linguistic investigations into errors of performance in second language speech and psycholinguistic inquiry into models of bilingual speech processing. Of course, the judgement as to whether this broader study of second language production adequately redresses the oversights of the antecedent literature and affords original insights that will in turn advance second language acquisition and psycholinguistic enterprises ultimately remains with those scholars firmly entrenched in the respective disciplines. However, this well-executed study and the sound implications drawn therefrom are certain to leave even the non-specialist reader with the impression that the author is the precursor of a novel and fruitful treatment.

In the first three chapters of the book, Poulisse presents the uninitiated reader with the requisite review of the literature on slips of the tongue in child and adult native speech and adult second language production. Her approach is uniform across these chapters: she first gives an overall view of the subtopic, then examines and compares relevant theories and research, and finally proffers suggestions for further research. As will become evident, this careful and critical reading of the literature serves to both contextualize and motivate the study that is the kernel of the work.

Chapter 1 (5–33) surveys the literature on slips of the tongue in adult native speech, dating from the late nineteenth century, revealing two major methods in the collection of slip data: a corpus-based strategy, which yields a large variety of slips produced under normal circumstances, and an experimental approach, which is more appropriate for testing specific hypotheses. The sampled literature unveils some important regularities in slips, which Poulisse puts forward as fourteen claims:

1) individual segments are the most important units in speech production;
2) anticipations are more common than perseverations;
3) exchanges are very infrequent;
4) phonological units involved in movement errors usually keep their original position in the syllable;
5) word- or syllable-initial consonants are more likely to be involved in slips than final sounds;
6) errors occur more frequently in stressed than in unstressed syllables;

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7) errors occur more frequently in open-class words than in closed-class words;
8) when two segments exchange, they tend to be phonetically similar;
9) two segments are more likely to exchange if they are preceded or followed by an identical sound;
10) slips of the tongue will not result in sequences of phonemes that are not possible in the language;
11) phonological errors usually result in existing sounds;
12) lexical substitutions often involve phonologically and/or semantically related words;
13) lexical blends usually involve two (near) synonyms;
14) lexical slips normally involve words belonging to the same word class.

These points have been germane to the articulation of speech production models, among them, the influential models of Dell (1986) and Levelt (1989), which Poulisse invokes in framing subsequent discussion. The claims also prove of relevance to her ensuing review of developmental studies.

In chapter 2 (35–47) Poulisse surveys the literature on slips of the tongue produced by children acquiring their native language, devoting special attention to deviations from slips produced by adults. Although there are some differences observed (e.g. with respect to number of slips, word-initialness effect, number of slips in function words, proportion of malapropisms, and proportion of anticipations and perseverations), Poulisse argues that these may be ascribed to methodological inconsistencies across studies. More significantly, she concludes, the majority of the claims that hold true for adult slips also hold true for children's slips, indicating that children's speech production processes can be accommodated within the adult models discussed previously. Moreover, these findings have implications for the development of fluency that could be applicable to adult second language learning, her primary focus of interest.

Poulisse's review in chapter 3 (49–77) confirms that slips of the tongue in second language production have gone largely unexamined in the research literature. In particular, the author notes that unlike studies of slips produced in child native speech, studies of adult second language slips have not been analyzed for the frequency or kinds of slips, the constraints governing second language slips, or slips specific to second language production. She considers several bilingual models of speech production and models of second language acquisition to determine to what extent they can account for the extant second language slip data, however scant. The exercise exposes notable lapses: second language production models such as those promoted by Green (1986) and De Bot (1992) are largely based on existing monolingual speech production models (although some discussion is devoted to accounting for observations regarding bilingual aphasics, code-switching, native language use during spontaneous second language production, and

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the results of bilingual experiments involving lexical decision, picture naming and word translation); and although second language slips data have played a role in the development of Poulisse & Bongaerts' (1994) bilingual production model, that collaborative endeavor was admittedly limited in its scope. Finally, Poulisse considers four models of second language acquisition: three cognitive models – McLaughlin's (1987) model of restructuring and automatization, Anderson's (1983) ACT* theory, and MacWhinney's (1987) Competition Model – and the second language acquisition model advocated by Gass (1997), whose focus on output is particularly pertinent. The author determines that only a combination of these can account for the increase in knowledge and fluency and the concomitant decrease of first language influence, which are typically attendant to advancing second language proficiency.

The foregoing chapters converge in signaling the need for a more rigorous and detailed examination of adult second language learners' slips of the tongue as well as the potential import of such an investigation for the elaboration of models of second language acquisition and bilingual production – to that end, the second language slip study, set out in chapter 4 (79–89). Therein Poulisse presents the specifics of the project, whose expressed goal is in informing speech production processes of second language learners and bilinguals more generally. Four guiding research questions are submitted –

1) Do second language learner slips show the same regularities as slips produced by monolingual adult native speakers?
2) Are there any proficiency-related differences in the slips produced by second language learners?
3) How do second language slips demonstrate influence from the first language?
4) How are areas of second language learning manifested in second language slips?

– and corresponding hypotheses are generated for each. The discussion then turns to motivating the choice of subjects (three groups of Dutch learners of English at three proficiency levels) and tasks (four different speaking tasks, ranging from strictly controlled to fairly natural: concrete and abstract picture description, story retelling, and personal interview). Methodological issues are discussed in chapter 5 (91–114), which will be appreciated as a set of guidelines for the study of slips of the tongue. Explicit procedures are specified for the identification and classification of slips, and illustrative examples of the coding are rehearsed. This careful exposition of the methodologies employed is inspired by keen attention to the question of reliability, which has plagued (even undermined) previous research enterprises.

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Chapter 6 (115–159) presents the results of the extensive study, in view of the four research questions raised previously. In determining whether the regularities observed for first language slips also hold for second language slips, Poulisse analyzes the data by reference to the fourteen claims (and three sub-claims related to claim 1) above: eight were supported, five were contradicted, and four were unsupported. In taking account of these differential findings, Poulisse considers whether they might be due to differences in elicitation procedures or to developmental differences in proficiency. Subsequent analysis confirms methodological biases (inherent in pen-and-paper corpora versus tape-recorded speech) and proficiency-related effects (e.g. with respect to vulnerability of closed-class words, repeated phonemes and the lexical bias). Addressing the second research question, concerning proficiency-related differences in number and types of slips produced by second language learners and similarities with slips produced by child first language learners, the author substantiates earlier findings that second language speech is less automatized than first language speech. More specifically, the number of slips during lexical access, verb formation and phonological encoding is inversely related to learners' proficiency level. In addition, the slips produced by the most proficient second language learners are more similar to the slips of monolingual adult native speakers than are those of the least proficient learners; and like children, the latter learners produce a greater number of slips, more perseverations, and many phonological slips in function words. In answering the third research question, regarding the origin of learners' speech errors, Poulisse establishes the type and frequency of slips originating in the first language. The results demonstrate a profound influence of the first language on second language speech: nearly one third of the slips were imputed to the first language; frequently attested were lexical substitutions, many of which involved function words. Finally, regarding question four, whether certain types of slips are typical for Dutch learners of English, Poulisse identifies two unstable, error-prone areas: the production of the third person singular verb morpheme {s} and the production of the voiceless interdental fricative [θ]; interestingly, learners demonstrated both non-application and over-application of the relevant rules. While these findings may not be particularly surprising, their true merit resides in their validity and potential significance.

In chapter 7 (161–179) Poulisse presents the synthesis of the results and their import for models of speech production and theories of second language acquisition. The discussion is prolonged and the implications are abbreviated here:

1) first and second language lemmas and words can be simultaneously activated;

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2) first and second language word forms that are phonologically related may activate each other;
3) there is no need to assume parallel encoding of speech forms;
4) syntactic and morphological encoding is language-specific; word forms are stored as units and also in decomposed form;
5) phonemes are stored in one network and may also be distinguished by language tag;
6) the language of the most activated word form determines which language is used for phonological encoding.

These findings are modestly interpreted as consonant, to some measure, with the speech production models of Dell (1986), Green (1998), and Levelt et al. (1999), and the proficiency-based differences lead to suggestions for refinement of the cognitive theories considered and exhortations to greater attention to the role of output in second language acquisition research, as suggested by Gass.

In addition to the well-documented findings and sensible suggestions, the necessary bibliographical references, and the useful analytical index that correlates discussions of related topics, the book benefits immensely from Poulisse's inclusion of appendices, which comprise the collection of slips of the tongue compiled as part of the second language slip project: 2,000 second language slips of the tongue (Appendix 1, pages 199–253) and 137 first language slips (Appendix 2, pages 255–261). The slips are classified in terms of slip type (e.g. substitution, anticipation, perseveration, exchange, deletion, addition, etc.) and the unit involved (e.g. phoneme, morpheme, word, etc.), and include information about who produced the slips, the correction of the slips, and the relevant context.

In summary, there is much to recommend this book as boasting an ambitious, well-defended and important account of the production of slips of the tongue in second language speech. If the tone of the remaining sentences is in any way critical, it is not intended to detract substantially from the highly positive judgement of the work or discourage the reader from consulting the book, but as evidence of its broad scope and application. Remarkably absent in Poulisse's careful consideration of bilingual speech is any regard to slips of the tongue produced in the deliberate language alternations of proficient bilinguals. It would be very interesting and potentially very informative to evaluate the proposals set forth herein against slip data drawn from code-switching performance; such a study could suggest solutions to long-standing debates in psycholinguistics and linguistic theory. For example, proficient bilinguals could be shown to differ from second language learners in encoding two speech plans simultaneously or in the tagging of particular lexical items (e.g. function words), and the identification of particularly (in)opportune sites in the alternation between language encodings could support particular syntactic constructs, as

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suggested in Toribio (forthcoming). In addition, questions will no doubt arise concerning the types of linguistic universals represented in the slips and the nature of the types of errors that do not occur. Still, though the work does not itself present arguments pertinent to current theories of phonology or morphosyntax, the comprehensive documentation of slips can serve as a useful resource for researchers in these core disciplines. One area that could incite controversy regards the assertion that base forms and inflectional morphemes are accessed separately; this stands in contraposition to the current Strong Lexicalist position of generative theory, e.g. Chomsky (1995), according to which words appear in the lexicon in their final shape. Finally, Poulisse's discussion of Gass's model in the interpretation of the findings is well-motivated, but is somewhat superficial and merits further scrutiny; it is hoped that the findings presented will inspire the study that is essential in further developing this line of argumentation. More generally, however, this focus on errors in the production of output is a welcome contribution to second language studies, an admirable corrective to a body of research literature that is oriented almost exclusively towards errors of competence. To be sure, *Slips of the tongue* serves as an excellent point of departure for further investigation.

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Ken Turner (ed.), *The semantics/pragmatics interface from different points of view*. Oxford: Elsevier, 1999. Pp. x+491.

Reviewed by ALESSANDRO CAPONE, University of Messina

This is an intriguing book, in which a number of eminent authors contribute (each from his or her point of view) to the issue of the semantics/pragmatics debate, an important and controversial chapter of linguistics.

In her article 'The semantics/pragmatics distinction: a view from Relevance Theory', Carston favours the view that semantics deals with the context-independent aspects of meaning (semantic retreat; in fact, this view was proposed by Higginbotham in an earlier reader) and that pragmatics (mainly context as selected through some cognitive principle such as Relevance) is responsible for enriching and developing truth-evaluable propositional forms in which variables for tense and contextual coordinates for deictic elements are given specific interpretations. In this way, what is said (a Gricean category) comprises both semantics and pragmatics. Pragmatics might (but need not) intervene again to enrich further these truth-evaluable propositions.

Of course, while Grice's main focus was the distinction between defeasible and non-defeasible inference, now the focus is shifted to the difference between decoding and inferring in a way which is distinct from decoding (say by appealing to the Principle of Relevance).

Peregrin's paper on 'The pragmatization of semantics' seems to express a position similar to that expressed by Carston, and, despite its title, also assumes that semantics is context-independent. The insistence on meaning as being extracted from all (or the most typical) uses is not sufficient to justify the title, which, instead, might be appropriate to Levinson's recent pragmatic programme.

Bach's paper offers a more classical picture of the semantics/pragmatics debate. It starts with the usual distinction between sentence and utterance, and argues convincingly that, granting that the semantics of a sentence depends entirely on the meanings of its constituents and its syntactic structure, pragmatics is concerned with the acts, intentions and inferences of language users. Bach's view of the work done by pragmatics includes, among other things, the inferences which he calls 'expansions' and 'completions', and although he includes illocutionary acts among the phenomena pragmatics has to explain, he argues against Gazdar's ideas on the pragmatization of semantics, by claiming that the time variables in conjunctions of statements are assigned explicit content by a process of expansion. He also argues against those pragmatic theories of polysemy

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according to which the context in which a word is situated shapes its meaning pragmatically. In rebutting this view, he refers to Pustejovsky, but, although his argument is impeccable, reference to Stati's (1986) much earlier work on lexical semantics would have been in order.

I find that Bach's invective against Relevance Theory is not justified; since we do not (yet) know exactly the ways in which human minds calculate processing efforts and information inferred (contextual effects), we cannot deduce that the theory is wrong. Furthermore, since Relevance Theory ignores Gricean communicative intentions, we cannot deduce that it can be dispensed with. I find it useful to distinguish (within non-truth-conditional inferences) between those inferences which are automatic and instantaneous (the hearer need not take time to think of the speaker's intentions) and those which require the hearer to construct arguments that take into account the speaker's intentions, plans, the Gricean maxims, etc. Relevance Theory specializes in instantaneous, non-self-conscious inference.

Unlike Bach, Asher denies that 'the content of a discourse derives essentially from an author's intentions and that capturing the content of the message involves reconstructing the intentions of the speaker' (25). In order to defend this quite remarkable opinion, Asher cites the example *John fell. Mary ran into him* (25). He says that although the speaker may intend these sentences not to be causally related, the hearer will infer that Mary's running into him caused John's fall. This is not a good example and I am not sure what it proves. First, if the author's intentions are not considered, one could also interpret the sentence as saying that as a result of John's falling, Mary, who found her way obstructed, ran into him. Secondly, the fact that a speaker's intended meaning and a hearer's reconstructed intention sometimes do not coincide does not imply that the hearer must not make the effort to understand the speaker's intentions with all the means available to him or her. Thirdly, Asher arrives at the surprising conclusion that the sentence, but not the sentence's speaker, means something. Fourthly, he implies that there is no procedure in conversation to check whether the hearer's reconstructed meanings and the speaker's intended meanings coincide. In Capone (1997), following ideas of Weigand (1989), I assumed that conversational sequencing ensures that intended meanings are ratified by the hearer's reply, which evinces the understanding of the previous statement.

Apart from these considerations, the importance of Asher's paper lies in a reformulation of Grice's examples and considerations (Asher does not introduce any new examples) in terms of goal analysis. However, as he reaches the unwarranted conclusion that Grice's Cooperative Principle is wrong because a speaker's goal need not be taken up by a recipient, I am now persuaded that he misinterprets the Cooperative Principle just because he applies goal analysis to Grice's theory. Most importantly, he fails to understand that Grice's Cooperative Principle does not say anything about the speaker's extra-linguistic goals, but is a theory of the ways in which

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speakers maximise the efficiency of information transfer. It might be useful to distinguish, as does Gu in another important paper in the same collection, between the COMMUNICATIVE and the EXTRA-COMMUNICATIVE GOALS.

Jaszczolt's paper entitled 'Default semantics, pragmatics and intentions' is, in my view, a very important attempt to do away with ambiguities, in connection with presuppositions in negative utterances, referential/attributive interpretations of NPs and belief-utterances. The so-called pragmatic ambiguity related to presuppositional expressions that appear in negative utterances evaporates in her view, since default interpretations search for referential interpretations of NP. It is not clear how she disposes of the case of metalinguistic negation (*The king of France is not bald because there is no king of France*). She is persuaded that 'in interpreting an utterance we do not select among the available readings but rather construct one interpretation by means of pragmatic inference in the given context' (204). In the case of the presuppositions of NPs in negative utterances she proposes to deal with the NPs as proper names. It is clear that one way she could deal with the cases of presuppositional failure in negative utterances is to consider the NPs in question to be echoic NPs ('*The king of France' is not bald*), but this causes some problems because the echoic use of the NP prevents the speaker from negating the sentence *The king of France is bald*. My impression is that Jaszczolt dispenses with ambiguity and thinks that one does not choose an interpretation but constructs one. However, while the case of referential NPs in negative utterances where no metalinguistic negation occurs is straightforward to explain under this CONSTRUCTIONAL view (the author argues that referential interpretations are preferred since we normally talk about existing entities and not about non-existing entities), I am not persuaded that one can construct the metalinguistic interpretation where the NP fails to refer without resorting to (at least pragmatic) ambiguity. Where would the appropriate logical form come from? It could be argued that context yields an appropriate transformation that cancels the existential presupposition; however, if context were so powerful, we would expect this transformation to occur even in positive assertions (e.g. *The king of France is bald. I do not think there is a king of France*). We expect that the power of the context in cases of metalinguistic negation is to interact with logical forms that are given by Russellian truth-conditions. Of course, Jaszczolt might now reply that one directly constructs the non-referential interpretation in context, as a result of the failure of the referential interpretation by searching for other plausible interpretations. However, if these plausible interpretations were not within the range of possibilities offered by the logical forms of the sentence, one could still have an interpretation but a marked one, with a resulting sense of deviance. Jaszczolt would have to commit herself to this view.

Let us now consider the attributive/referential interpretation of NPs as in *The man drinking a Martini is happy*. At first I thought that Jaszczolt was

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interested in claiming that the semantics of this expression was underdetermined and that in context, one interpretation (referential/attributive) was preferred. But then I realized that she says that 'the level of an underdetermined semantic representation is excluded' (213). She is correct in this claim, I think, since there is such a gulf between the attributive and the referential interpretation that I do not see how it is possible to factor out a common denominator in order to ensure that a principled interaction with the context can yield the correct interpretation. But now Jaszczolt is eager to get rid of the pragmatic ambiguity posited by philosophers (semantic ambiguity has long since been rejected) and argues that there is a default interpretation which is overridden in certain contexts. I believe that this is incorrect. I have found a principled interaction between sentential context and interpretation. Thus despite Jaszczolt's predictions, *The man who marries a benevolent woman is happy* does not favour the referential interpretation but an attributive interpretation in a null context. *The man who is in that corner* favours a referential interpretation. *The man drinking a Martini is there* favours a referential interpretation. *The man drinking four Martinis at midnight must be drunk* (in a context where there is no such man) favours an attributive reading. *The man who builds skyscrapers must be happy* favours an attributive reading. It seems that both the semantic features of the VP in the NP and in the main sentence contribute to co-select either a specific or an attributive reading. Jaszczolt's argument that referential readings are preferred because we generally talk about specific things cannot be correct because if we took her seriously we would always have to talk about concrete objects, whereas we are quite capable of making generalizations and of talking about abstract entities (e.g. *The state protects the citizen*). Despite my reservations, I think that Jaszczolt's paper broaches a very important line of investigation.

Krifka's paper analyses the contrast between *at least three boys* and *three boys*, which, in the current literature, are said to have the same meaning. Krifka, however, finds that while the phrase *three boys* gives rise to some scalar implicatures (*no more than three boys*), *at least three boys* does not conversationally implicate *no more than three boys*. Krifka then goes on to build up a theoretical semantic explanation of the differences between these two expressions on the basis of the notion of alternatives. In his opinion, the phrase *three boys* generates some alternatives, which are the basis of the scalar contrast, while the phrase *at least three boys* does not generate alternatives (or generates null alternatives). Of course, he does not grant that if one recognizes the semantic ambiguities of cardinal determiners, as Higginbotham (1983) does, this problem does not arise. The ambiguity is blocked in the *at least* … context, but remains in other contexts, where the preferred interpretation is the *exactly n* … reading. If anything, the data Krifka wants to explain seem to favour the ambiguity view. Krifka – in order to prove that *at least n boys* does not give rise to semantic

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alternatives – proposes that the alternatives of [[at least three boys]] are [[λ x [n(x) $\wedge$ boys (x)], λ x [m (x) $\wedge$ boys (x)]] | n $\leqslant$ m]. Krifka then calculates the union of these alternatives which is:

[λ x [$\geqslant$ 3 (x) $\wedge$ (boys (x))], λ x [$\geqslant$ 3 (x) $\wedge$ boys (x)]].

In other words the sentence *At least three boys left* has no proper alternatives. He offers no detailed arguments about how *three boys* should be analysed on the basis of semantic alternatives. For example, he says that the alternatives of *Three boys ate seven apples* are the following: [$\exists$ x $\exists$ y [n(x) $\wedge$ boys (x) $\wedge$ m (y) $\wedge$ apples (y) $\wedge$ ATE (x, y)] | n, m $\in$ N] on the basis of a compositional rule for alternatives of expressions of the type $[\![[\alpha\ \beta]]\!] = f([\![\alpha]\!], [\![\beta]\!])$ (268). However, I should like to contend that if he were to spell out the semantic alternatives of *three boys* in detail, unless he wanted to say that these alternatives include values $\leqslant$ 3, which would be useless for the purpose of generating scalar implicatures (one never implicates by saying *Three boys left* that it is not the case that two boys left), he would have to admit that the union of these alternatives is (as in the *at least three* case) $\geqslant$ 3 (x). In this case, he would analogously have to say that the phrase *three boys* generates no proper alternatives. But, of course, $\geqslant$ 3 (x) are the values of x that represent the proper alternatives of both *three boys* and *at least three boys*. The differences in terms of conversational effects must be explained by following Higginbotham in saying that cardinal determiners are ambiguous. Furthermore, Krifka does not mention the fact that the phrase *at least three boys* is felicitous in contexts where a minimal threshold questionnaire is being answered or where this statement occurs in a dialogic context requiring unspecific information or it occurs in a narrative.

Summing up, I believe that this is a very interesting collection of papers and that it presents some important views about language and semantics and firmly establishes the point that semantics and pragmatics are entangled to a greater extent than linguists are ready to accept. Nevertheless, I believe that a serious deficiency of this book is to have neglected the issue of how to establish whether an inference is semantic or pragmatic and to have said nothing about the dispute on the validity of the Gricean tests for conversational implicature. Pace Levinson (1983), I believe that this issue has to be resuscitated in a period in which it is believed that pragmatics has a lot to promise (in the area of anaphora, for example), while I am myself persuaded that Sadock's important paper (1978) on the difficulties that arise from the application of Grice's tests for conversational implicature (which Grice was certainly aware of) has gone unheeded in the current literature. In Capone (1997) I showed that this issue has to be addressed again with greater intellectual honesty.

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