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REVIEWS

Thomas Berg, *Linguistic structure and change: an explanation from language processing*. Oxford: Clarendon Press, 1998. Pp. xiii + 366.

Reviewed by STEFAN FRISCH, University of Michigan

This book is an exploration of the hypothesis that synchronic linguistic structure and diachronic language change can be explained by factors that are external, rather than internal, to the grammar. In particular, Berg argues that 'the structure of language is shaped by the properties of the [processing] mechanism which puts it to use' (278). This study is thus an example of a functional account of language patterns, and as such it is contrasted with formal, autonomous linguistic theory. I am very sympathetic to Berg's approach, and the book can be praised for the breadth of linguistic and psycholinguistic patterns that are discussed, and for the way the results are integrated into a complete model of linguistic patterning. However, the book can be criticized stylistically for the tone of the attack on autonomous linguistics, and scientifically for analysis that often appears shallow, ad hoc, and consequently unconvincing. Perhaps it is too much to ask for both breadth and depth of empirical coverage, but I feel both are necessary if Berg's goal is to convince the skeptic that 'speaking and listening, as psychological activities, are so fundamental to language that productive and perceptual processes are likely to exert an influence upon the information to be processed' (54).

Conceptually, the book is divided into three sections: preliminary structure of the argument (1–67), empirical studies (68–277) and broad theoretical conclusions (278–313). The final section, which ties together the analyses and places the work in the broader context of linguistic research, is outstanding. I think the argument would have been better organized and more convincing if much of the material here had been in the introductory section. Fundamentally, Berg proposes that languages are subject to evolutionary pressures to avoid forms that are difficult to process. As a result, synchronic language structure and diachronic language change reflect psycholinguistic constraints on language processing. Berg adopts a parallel spreading activation model of processing to predict processing influences. The model predicts interaction between segmental and lexical levels due to parallel processing, influences of frequency and lexical competition that are encoded in activation-based representation, and constraints resulting from the serialization of segments in the parallel network.

Consider the following typical example. The processing model predicts that low frequency irregular morphemes are more vulnerable to regular-

ization than high frequency irregular morphemes, as demonstrated in the history of English strong verbs by Hooper (1976). In the model, low frequency words are less strongly activated than high frequency words, so low frequency word patterns are in greater danger of being overwhelmed by the prevailing regular pattern. This explanation crucially relies on interaction between lexical and phonemic/featural levels, as competition between different word outcomes is proposed to influence the encoding of phonological elements involved in allomorphy. In general, Berg bases his arguments on language processing constraints observed in patterns of speech error production. Ideally, constraints from speech perception would receive an equal treatment. While they are scarcely discussed here, perceptual constraints have played an increasingly important role in recent phonological theorizing (e.g. Silverman 1997, inter alia) that is congruent with Berg's line of reasoning.

The introductory section sets the stage for the analysis to follow. Berg discusses the explanatory value of internal and external evidence in linguistic theory and surveys potential external forces that could be used to explain linguistic patterns. Unfortunately, the first chapter sets a combative tone by flatly stating that formal linguistic theory cannot explain anything about linguistic patterns. Berg's discussion includes a virtual psychoanalysis of linguists' fears that explanation cannot be found solely within linguistics, and their subsequent grasp for autonomy of the linguistic system as a means of academic survival. I dearly hope formal linguists can overlook this problem and judge the rest of the book fairly.

The subsequent survey of potential external factors that could influence language is much more constructive. The survey includes a wide range of factors (neurology, phonetics, psychology, semiotics, communication, pragmatics, sociocultural factors, and historical factors) and also discusses the extent to which these factors overlap and can be reduced in number. Berg concludes that many of these factors either do not provide true explanation as they merely shift the locus of explanation to a different domain (e.g. historical), or are important factors but idiosyncratic with respect to linguistic form (e.g. sociocultural), or are factors that are reducible to psychology (e.g. semiotics). Thus, he argues that psycholinguistic factors are likely to have a strong influence on linguistic patterns. Finally, he rightly notes that the failure of any particular external factor to predict some linguistic pattern is not justification for dismissal of the external factor. External factors may contradict and interact, or be overruled by language particular idiosyncrasies. Clearly, he is correct, as no one external influence can predict all of the typological variation found in natural language. Similar insight is found in the current generative paradigm of Optimality theory, where formal constraints need not be surface true as they can be overridden by other constraints on a language particular basis. In addition, Berg points out that external factors are inherently gradient (or probabilistic, or 'fuzzy'),

so their absence in some particular cases is not entirely unexpected. However, processing based constraints are expected to be robust across multiple phenomena in any particular language and across all languages. In many cases, Berg employs corpus analysis to demonstrate that psycholinguistic constraints are present in a language, even if they are not categorically obeyed.

The empirical section is by far the largest, covering three chapters divided into 43 subsections, each corresponding to one 'major' analysis. Berg only considers psycholinguistic patterns in adult oral/aural language processing and corresponding linguistic patterns in the better known Indo-European languages. He does not examine child acquisition patterns, aphasic language patterns, written language processing, or typologically common cross-linguistic patterns. While the empirical coverage within the limited domain he selected is fairly broad, each analysis is only a few pages. In general, each analysis is admittedly suggestive.

Relatively more effort is spent on some topics, like serial processing constraints as an explanation for dissimilation. Berg claims that dissimilation occurs because repetition is difficult to process. This is because parallel spreading activation networks encode each linguistic unit once in the network, and so repetition can only occur by repeatedly activating a unit. But repeated activation is in conflict with serialization, as a unit is encoded in a particular serial location by having its activation peak at the appropriate time unit, and otherwise having relatively low activation. I have worked extensively on this particular issue (Frisch 1996, 1999), and I'm sure Berg would agree that a dozen pages cannot do justice to the nuances that this psycholinguistic account of dissimilation predicts. In fact, the basic pattern of dissimilation is perturbed by frequency effects, serial ordering asymmetries, and lexical activation/competition, as predicted by the language processing explanation (Frisch 1997). On the positive side, Berg's relatively light treatment of so many topics means this book is a cornucopia of research projects, as each analysis could be extended to a descriptive typological survey, or a quantitative study of any language for which a corpus exists.

The analyses are grouped into chapters by broad type (synchronic, diachronic, and poetic language) and organized by linguistic level (phonological to morphological to syntactic) rather than by the influencing psycholinguistic factor. I feel this organization detracts from the coherence of the argument. For example, serial ordering asymmetries are discussed in the structure of onsets and codas, the location of morphophonemic alternations, the attachment preferences of affixes, and the ordering of words in phrases. Berg claims that temporally early material is more important to lexical/conceptual access and thus temporally early material is primary and more robust while later material is secondary and more variable. Together, these analyses at different levels provide strong evidence for processing based serial asymmetries, but because they are scattered throughout the section

there is no feeling that converging evidence has been mounted. Berg's summary only gives a grand tally of data compatible or incompatible with some processing account, and does not illuminate which (if any) processing factors were generally more successful in explaining linguistic patterns. For the motivated reader, this is not a problem, but for the mildly interested skeptic it is too easy to dismiss the overall argument as not persuasive.

The analyses are a great reference for the potential of functionally based approaches. In general, Berg starts with a psycholinguistic pattern and then considers whether possible linguistic implications are borne out. This method of inquiry can stand as a model for others interested in novel explanations for linguistic patterns. While the structure of the argument reflects Berg's theory, his approach leads to potential circularity. Rather than psycholinguistic constraints leading to specific structures that reflect processing, it may be that a psycholinguistic pattern is the direct result of general processing mechanisms applied to a particular linguistic structure. Alternatively, it may be that the linguistic structure and the corresponding processing mechanism are inextricably intertwined. In Berg's words, 'psycholinguistic process and the linguistic product would be at the same time explanans and explanandum' (289). This is a difficult issue that will require a good deal of careful research to sort out. To its credit, Berg's approach leads to predictions that would not be considered solely on language internal grounds. For example, Berg's model predicts the existence of sound symbolism due to the interactive nature of the phonemic and lexical levels in the spreading activation model. Consistent sound/meaning relationships are rewarded by greater activation of analogous patterns throughout the lexicon. Paradigm leveling is given a similar analysis.

Overall, this book is a much needed contribution to a relatively understudied area of linguistic research that is gradually entering the theoretical realm. This work has broad implications for many of the foundational issues of linguistic theory. I believe Berg's approach is a necessary complement to the linguist's search for properties of cognition that are specific to language, but are linguistically universal. For the traditional formalist, it is actually desirable for some linguistic patterns, especially those that are gradient, to be explained by functional principles. The remainder, once language processing influences are factored out, might be a simpler, cleaner, and more accurate picture of the nature of the innate language faculty and its role in delimiting the set of possible human languages.

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Peter W. Culicover & Louise McNally (eds.), *The limits of syntax* (Syntax and Semantics **29**). San Diego: Academic Press, 1998. Pp. xi+401.

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In this volume the balance between syntactic and nonsyntactic principles in accounting for certain word order phenomena is investigated. Invoking nonsyntactic principles has the potential advantage that syntax itself can be made 'cleaner', with less stipulative principles, but it is mainly an empirical question which type of principle offers the most promising account for some phenomenon. The phenomena for which this question is most relevant broadly fall into two categories, concerning conditions on extraction and triggers for movement.

With respect to conditions on extraction, the question arises whether islands should be defined in syntactic or semantic terms. Similarly, is the defining property of elements that can be extracted from weak islands something semantic, like being D(iscourse)-linked, or syntactic? Concerning triggers for movement, it is not clear that there is a formal syntactic trigger for every movement, as required in the minimalist program. This holds especially for those movements that appear to be related to the discourse function of the moved element, like focus movements and topicalization. Such movements may be accounted for in purely syntactic terms by introducing formal features like [+Focus] in syntax, but as Culicover & McNally remark in their Introduction, 'it appears fairly clear that the unconstrained use of triggering features and movements to license these features will allow the description of any phenomena in syntactic terms' (8). The question then is what the relation between syntax and discourse function is and how it accounts for word order variation. Most papers relate to these problem fields. I will briefly discuss each one in turn.

Ray Jackendoff discusses the architecture of the language faculty. He argues against 'syntactocentrism' (22), the view of grammar in which only

the syntactic module is held responsible for the generative capacity of grammar, whereas the phonological and semantic modules merely interpret the structures provided by syntax. Instead, Jackendoff proposes a 'tripartite parallel architecture' (32): phonology, syntax and semantics are all autonomous generative systems, connected by correspondence rules. The paper is entirely convincing, but there is little discussion on the topic of what belongs to the syntactic module and what falls outside it.

Peter Culicover provides an alternative to Chomsky's (1995) 'abstract' minimalism in the form of 'concrete minimalism' (50). The idea is that a language learner only adopts syntactic patterns for which there is direct evidence in the input. The more frequent such a pattern, the more grammatical the learner considers it. Strictly speaking there is no place for autonomous syntactic principles in this inductive model at all, although Culicover in various places speaks of a residue of autonomous syntax (59). The paper is thought-provoking and enjoyable to read, but it still seems problematic to account for all language learning in terms of general, nonsyntactic, inductive mechanisms. One problem concerns parametric variation. For example, Culicover argues that extractions in Italian which seem to violate Subjacency can be accounted for: the little experience with such cases that the Italian-learning child has makes them a bit better than if they were not in the input at all, as in English. This explanation seems circular. Subjacency violations are considered ungrammatical by the English learning child because they are absent from the input. But of course Subjacency violations are absent from the input because they are ungrammatical in English. Another problem is that patterns for which there is no evidence in the input can be grammatical. According to Culicover it is unclear whether there are such cases (fn. 20). However, although hotly debated, the phenomenon seems to occur in creolization; a rather spectacular recent example is given by Kegl et al. (1999).

Enric Vallduví and Maria Vilkuna argue that the notion 'focus' comprises two distinct concepts. The first is rheme, which belongs to the domain of information packaging: rheme is the element that corresponds to the 'update potential' (82) of the utterance, the new information. The second is kontrast, an operator-like element, which generates a set of alternatives with which it is contrasted. Vallduví & Vilkuna show that these notions should be strictly separated. A kontrast can be rhematic, correspond to part of the rheme, or even be thematic (87). Also, kontrast and rheme/theme impose their own demands on their syntactic realization. These demands can conflict, in which case languages differ as to which demand (for the kontrast or for the rheme) takes precedence, much as in optimality theory. The point that in discussing the syntactic position of 'focused' elements these two notions must be distinguished is convincing, supported by Finnish, Hungarian and Catalan data.

Craige Roberts contributes a lengthy article on focus from which the

following claims emerge as central. A Focus Universal says that foci are always prosodically prominent (147). To achieve this, languages can adopt various strategies: use a marked prosodic structure, move the focus to the prosodically prominent position in the clause or move all non-foci out of this position. Although there is a subdivision into 'exhaustive' (contrastive) and 'non-exhaustive' foci, these are functionally so close that the universal applies to both types alike. Interesting though the universal is, this latter claim appears to be incompatible with Vallduví & Vilkuna's data that show the two notions of focus do *not* necessarily overlap and that there are (in their terms) nonrhematic kontrasts. Consequently, this theory will have a hard time accounting for the sometimes diametrically opposed syntactic conditions that both types of foci can impose. Roberts' claim that in Vallduví & Vilkuna's paper 'there is too little data' to determine whether contrastive focus is really different from information focus (126) seems too easy a way out of this problem.

Louise McNally advocates an approach to information focus that integrates semantics and pragmatics. The rheme semantically is the new information. Pragmatically, the basic function of an utterance is to update the 'information state' of the hearer. It follows that (a) every language must know the information packaging instruction 'Add Information', whereas there may be language variation in the other instructions, and (b) this rheme being default, it is never linguistically encoded as such. The illustration of this interesting second prediction unfortunately is mainly left for future research (177). McNally observes that rhemes often stay in situ in languages, but, apart from exceptions to this, it is not clear why being syntactically in situ should be considered as absence of encoding of informational status.

Rita Manzini proposes a purely syntactic account of weak islands. She assumes a version of relativized minimality that excludes intervening attractors rather than intervening attractees. This not only accounts for *wh*-islands (a Q-operator failing to attract a *wh*-element across another Q-operator), but generalizes to negative and factive islands under the assumption that these all involve operators that attract a feature that Q-operators also attract. For instance, Manzini assumes that in factive islands the verb governing the factive complement 'bears some F specification' which 'needs to be checked against some F head' (202). This F-head is immediately superordinate to the factive verb (203), hence if a *wh*-element is in the scope of this F-head, then a higher Q-operator cannot attract it (F intervenes) IF the *wh* also contains a feature attracted by F. The question is why F is an attractor for the 'attractee feature' (200) of *wh*: what does a factive verb have in common with *wh*-elements such that they are both attracted by the same head F? This version of minimality will also have to be qualified to allow a factive verb and *wh* in a single clause (*What do you regret*), where it also holds that Q c-commands F and F c-commands the base position of *wh*. Manzini further proposes that the difference between

elements that can be extracted from weak islands and those that cannot concerns pied-piping. In case a *wh* pied-pipes its restrictor (e.g. *which man*) and there is no reconstruction, there is no trace in its base position but an empty resumptive pronoun, a null D. If so, the *wh* can be assumed to be inserted directly in its surface position. Given that this option involves no movement, such *wh*s cannot be sensitive to weak islands. They ARE sensitive to strong islands, which Manzini explains by arguing that there must be covert movement of the null pronoun to the checking domain of *wh*. This is triggered by something different than the Q-feature, so that there still will not be intervention/minimality (weak island) effects. This again raises the question why Q-features do form a single 'attraction class' with Neg and F features, whereas the feature that attracts null D falls outside this class. Despite this somewhat stipulative nature of some parts of the proposal, it shows that a purely syntactic account of all aspects of weak islands is feasible. Interestingly, the next two papers show that alternatives are not to be ruled out either.

Nomi Erteschik-Shir gives an overview of her theory of f(ocus)-structure. She argues that two different types of f-structure constraint account for two different types of dependency. R-dependencies, which include scopal dependencies, are regulated by the Topic constraint, which says that an f-structure in which the topic precedes the focus is unmarked. So in a simple $NP_{subject} - VP$ sentence the subject preferably is topic. I-dependencies, including anaphora and *wh*-movement, are constrained by the Subject constraint, which 'restricts I-dependencies to f-structures in which either the subject or a stage is the topic and the dependent is contained in the focus' (224). This accounts for strong islands. For example, subjects or relative clauses do not normally serve as focus. The Subject condition then forbids establishing an I-dependency between a moved *wh*-element and a variable inside a subject or relative clause. Weak islands are accounted for by the assumption that the same focus constituent cannot be a dependent in more than one I-dependency (compare Manzini's idea that an operator can only attract an element as far as the next operator that attracts the same element). Erteschik-Shir further argues that *wh*-questions can be interpreted in two different ways. Either the fronted *wh* functions as the topic of the main f-structure (which means the restrictor of the *wh* must range over a restrictive set given by the discourse/context – as with 'D-linked' *wh*s), or it is interpreted in trace position by reconstruction. Only in the latter case is an I-dependency between *wh* and trace required, so that the constraint against double I-dependencies applies. Hence, only *wh*s that are not 'D-linked' are sensitive to weak islands. Again, this is not unlike Manzini's account. So we see that similar accounts for the same range of data can be given which nevertheless differ in whether or not they appeal to nonsyntactic notions (although it must be noted that Erteschik-Shir's level of f-structure belongs to syntax in the broad sense in her model of grammar (212)).

Robert Kluender accounts for islands in terms of processing constraints. He characterizes the computational and storage functions of working memory in terms of activation. The capacity of the system as a whole is defined in terms of the total amount of activation available to subserve either or both of these functions (249). If the amount of activation is approaching this maximum because of some heavy computational load, the system will delete elements from its storage component to prevent overload, resulting in unparsability. Islands are then accounted for by the following assumptions: (a) filler-gap dependencies strain working-memory capacity because the filler has to be kept in storage until the position of the gap is found (b) this gets worse when the filler has to be taken across a clause boundary (c) this gets worse still when the extracted element has little referential content, like pronominals, as this requires 'holding "an uninterpreted place marker" in working memory' (251) (this accounts for the fact that 'D-linked', more referentially contentful, elements are more easily extractable) (d) this gets worse still when intervening elements are NOT without much referential content. Although it is easier to hold a D-linked element in memory (point (c)), it costs more to initially integrate it in the structure, as it refers to a less accessible discourse referent than, for example, a pronoun. This high cost can be fatal when at the same time the system is already strained by keeping track of the filler-gap dependency it is in the process of establishing. This explains that the felicity of extractions out of for instance relative clauses depends on how descriptively contentful the antecedent of the relative clause is, a fully referential antecedent like *the reviewer* giving worse results than an indefinite pronoun like *someone*. Kluender succeeds in making his proposal quite plausible, and substantiates it carefully with empirical evidence. Only the relation between the processing theory and the speakers' perception of the syntactic (un)acceptability of the relevant sentences is left a bit unclear. Ideally, it should be possible to calculate exactly how costly a given sentence is in terms of processing load, and from this be able to predict the degree of acceptability of the sentence. Kluender does offer some remarks on the relation between processing and grammaticality judgements, but I found these somewhat hard to interpret, cf. 'even in a language which allows extraction from island contexts […] speakers will reject them for reasons of processing difficulty' (268) – how can it be established that extraction from islands is possible if the speakers of the language do not accept such sentences? Nevertheless, this is an excellent paper which shows that syntax proper might indeed benefit from transferring some of its explanatory burden to processing mechanisms.

Ellen Prince argues that syntax 'neither follows from […] nor encodes discourse function' (281). Syntax makes available a number of representations which can be used for certain discourse functions, but the relation between syntactic representation and function is arbitrary (not iconic). Prince illustrates this with respect to the left-dislocation structure NP_i $[_{CP}$ …

pronoun$_i$ …]. It is often argued that left-dislocated NPs always have the discourse function 'topic', but Prince convincingly shows that this cannot adequately account for all the facts, unless the notion 'topic' is left unacceptably vague. She argues that in English left-dislocation has three different discourse functions. (a) It simplifies discourse processing: left-dislocation occurs when an NP that is discourse-new would be in a position typical for discourse-old NPs if it stays in situ. (b) It triggers a 'poset' inference. A 'poset' is a set whose members stand in a mutual relation like 'is a part of' or 'is a subtype of'. Left-dislocation can function to indicate that the dislocated NP is a part or subtype of some entity(s) mentioned earlier in the discourse. (c) It has the same discourse function as topicalization. In island configurations left-dislocation is hidden topicalization, the co-indexed pronoun only occurring to avoid an offending trace inside an island. There is no obvious connection between the functions (a)-(c) which makes left-dislocation particularly suitable for just them. This conclusion is reinforced by the observation that in another language, Yiddish, left-dislocation never has function (a). To remove discourse-new NPs from positions canonically associated with discourse-old material Yiddish uses a different syntactic construction, involving postposing. This shows the arbitrary nature of the syntax-discourse function connection. Though perhaps not directly pertaining to the limits of syntax, rather than to its autonomy, Prince makes a valid point.

Pauline Jacobson discusses the question 'where (if anywhere) is transderivationality located?' Her answer is that it cannot be located in the grammar, but that, if transderivational constraints exist, these must follow from processing/production mechanisms. She argues that it is too computationally complex to compute all alternative derivations in order to let grammar select the best. Moreover, it is a coincidence that, if all alternatives are computed anyway, the simplest derivation is chosen, rather than any other. Instead, the system must be driven to the simplest alternative, so that the more complex ones need not be computed. The grammar is not a suitable device for this, but the parser is. The problem with this reasoning is that, if correct, it must be that in all relevant cases the grammatically more complex derivation is also computationally more complex (as noted by Jacobson). Given that there is no necessary relation between grammatical devices proposed to account for a speaker's knowledge and actual production/processing, this is a complete coincidence. The point that some grammatical principles are spurious and are really parsing constraints may be valid as such (compare Kluender's paper), but the computational complexity of syntactic devices cannot make this view a priori necessary, as such devices are not intended to make claims about what is actually computed and what not.

Michael Rochemont's paper consists of two parts. The first discusses the relation between pitch accent and phonological focus, or 'focus projection'. The second discusses structural focus, in particular Heavy NP Shift.

Rochemont proposes an analysis which involves leftward movement of the heavy NP to the specifier of a functional head H, followed by leftward movement of the remnant VP to a higher specifier of H, thus deriving the VP-heavy NP order. Rochemont correctly notes that in the traditional analysis this order was derived by the stipulation that a heavy NP must move to the right edge of the clause. However, it seems he replaces this stipulation by at least four new stipulations: first, there is some functional head that has something to do with focus, second, this head has both a strong D-feature and a strong V feature, third, the strong V-feature must be satisfied by VP-movement rather than V-movement, fourth, the VP must move higher than the NP. His suggestion that some of these properties follow from Holmberg's generalization (overt object shift only if overt V-movement) is unconvincing, as the usual account of this generalization crucially relies on there being head-movement of V (not VP-movement) (cf. Chomsky 1995). Therefore, it is not clear what the advantages of this approach are, compared to the older one(s).

Jan-Wouter Zwart discusses the syntax of topicalization in Dutch. He argues that topicalized elements are really left-dislocated, with a coindexed 'd-word' (identical in form to a demonstrative pronoun) in spec-CP position. This d-word can be overt (*dat boek dat ken ik niet* 'that book that know I not'), but need not be. This analysis has been proposed before, but Zwart supplies some extra arguments for it. The fronted d-word occurs in these constructions to satisfy a condition on discourse-linking. Discourse may require a CP headed by a [+D] C^0; the [+D] feature then triggers movement of a d-word (388). A problem for this assumption may be that it predicts that sentences without a fronted d-word cannot have the same discourse-linking function as sentences with such a word (as they must lack the relevant feature). Zwart cites one case in which this is true (384, example (56)), and I agree with his judgement here, but I'm not sure whether this is true in general.

All in all, this is an excellent collection of papers which, while not deciding the issue of where the limits of syntax lie once and for all, at least shows that a fruitful and interesting discussion on this issue is possible.

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Mark Durie & Malcolm Ross (eds.), *The comparative method reviewed: regularity and irregularity in language change*. New York & Oxford: Oxford University Press, 1996. Pp. vii + 321.

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This wonderfully stimulating book, a compilation of ten essays on the comparative method, takes an honorable place in modern scholarship on reconstruction and language relatedness, exemplified by such works as Baldi (1990) and Dixon (1997). Drawing on 'difficult' data from largely non-Indo-European languages (notably Austronesian, Papuan and Australian), it highlights what the standard linguistics textbooks do not – the 'rough edges', the problems with the received wisdom.

I will only touch on Ross and Durie's 'Introduction' (chapter 1). It interweaves summaries of the essays into general discussion of the comparative method, conceived both as a procedure for reconstruction (6–7; cf. also 48 ff., 165–166, 221–222) and as the theory (family tree model, regularity of sound correspondences) that informs and justifies these procedures. The authors depict a 'paradigm shift' in recent ways of looking at language change: away from language as an organism or system which exists and changes in a separate ontological realm of its own, and toward a 'speaker-oriented' view of change (13).

The establishment of lexical sound correspondences is commonly considered the heart of the comparative method, but it is not the initial step. Johanna Nichols' essay, 'The comparative method as heuristic' (chapter 2), focuses on that first step: the assumption that the languages under comparison are related in the first place. In classical historical-linguistic work – Nichols illustrates with Slavic philology – relatedness was not argued for but simply emerged, self-evidently, from the languages themselves and from the pre-linguistic traditions of studying them. And when evidence WAS demanded for relatedness, it was not lexicon but grammar which the classical comparativists turned to, notably morphological paradigms – resemblances embodying both particular forms and 'multidimensional paradigmaticity' (46), e.g. the adjectival case endings of Indo-European, which match up formally and paradigmatically along the dimensions of case, number, gender and declension class.

Nichols seeks to get at the essence of this kind of evidence, which she terms 'individual-identifying evidence' (hereafter IIE) – 'the kind of evidence that identifies a unique individual protolanguage rather than … a set of languages or a type of language' (48). Her approach is probabilistic. With several thousand languages on earth, an individual language's occurrence probability is of the order of 0.001. To securely establish an 'individual-identifying' level, we combine this probability with 'at least a conventional

level of statistical significance' (49), yielding the product 0.001 × .01 = 0.00001. If a given linguistic phenomenon (form, paradigm) has a probability less than this, it can be taken as IIE (49; see also Nichols 1998: 132). Nichols provides four illustrations:

1) a LONG lexeme: Indo-European **widhewa* 'widow',
2) the Germanic mini-paradigm *good*, *better*,
3) the Indo-European system of gender endings,
4) the Indo-European numerals from one to five.

These all count as IIE – unlike short lexemes, or forms taken in isolation and not embedded in paradigms, or comparanda which are only approximately identical. Thus the presence of *n* or *m* in 1st- or 2nd-person pronouns is not IIE (54–55).

As a heuristic, however, this approach has a critical flaw, noted by Nichols herself in passing. When languages share an item of IIE, it means they 'have acquired it, ultimately, from a single source. The languages need not all be descended from that source; the word could be a LOAN in some of the languages' (emphasis mine, 50; cf. 'Introduction', p. 8). Precisely this is the difficulty. There are languages where loan-words, including hundreds of quite long loan-words, comprise a huge percentage of the vocabulary – e.g. Persian vis-à-vis Arabic (e.g. *istixdām* 'employment' in both). These long borrowings are just as much 'individual-identifying' as **widhewa*, and the ensemble of all of them together is crushingly so. Note that such resemblances are indeed legitimately 'individual-identifying': they do identify an individual historical source, here the shared history of Persian and Arabic. But as a heuristic they point, massively, in exactly the wrong direction if the focus is on GENETIC relatedness.

The difficulty seems inherent in the method, and apparently in any purely quantitative method. What the genetic comparativist is after is not 'shared history' per se, but a particular kind of shared history. The heuristic evidence for this is qualitatively special, i.e. special in kind: morphological paradigms. (Even this can be suspect, cf. Callaghan 1980.) Nichols' method of IIE can only compute probabilities; as such it reliably identifies probable shared history. But it has no 'qualitative' dimension to distinguish between morphological IIE and lexical IIE, and hence (so it seems) no way of getting at specifically GENETIC shared history. Ironically, there are cases where linguists have indeed assigned languages, on the basis of rich shared lexicon, to what ultimately proved to be the wrong genetic family. Nichols' heuristic, then, may perhaps model what linguists have actually done – but not necessarily what they should do.

Lyle Campbell's essay, 'On sound change and challenges to regularity' (chapter 3), is the one piece one might call 'traditional'. It lays out the conventional wisdom regarding irregularity in sound change, cataloguing it

under six rubrics: size-shape sound symbolism, onomatopoeia and affective/expressive symbolism, avoidance of homophony, morphologically conditioned sound changes, areal linguistic borrowing and language death. Particularly interesting here is the discussion of how expressive symbolism triggers irregularity in Mayan languages. A final section rehearses (rather apologetically) the familiar observations that a language's 'exoticness' or 'primitiveness' has nothing to do with whether sound change operates regularly, nor does its status as a (non-)written language – Algonquian and Athabaskan are mentioned as canonical success stories of the comparative method.

Campbell follows the conventional wisdom in downplaying the problem represented by irregularity. The above six categories 'do not, in fact, present true exceptions to the regularity of sound change' (72; cf. also 86). Rather, irregularity 'is rendered explicable only against the backdrop of the assumption that sound change is fundamentally regular' (78).

This approach, unfortunately, seems to turn a blind eye to the substance of almost all the essays in the book, which are precisely cases where the conventional wisdom does not suffice (see also 'Introduction', pp. 28–31). Most fundamentally there are cases, involving plainly related languages, where it is not clear what the 'regular' vs. the 'irregular' development IS, or indeed if anything can count as 'regular'. This is quite explicit in the essays by Durie, Blust and Grace. Here it is not the case that irregularity makes sense only against a backdrop of assumed regularity. Nor must such 'pandemic irregularity' be a theoretical monster. The theory of lexical diffusion (Wang 1969) presents sound change as proceeding gradually via word-by-word spread through the lexicon, not always going to completion (see 'Introduction', pp. 23 ff.). Durie, Blust, and Grace (below) provide further possible theoretical perspectives.

Even if postulated to be 100% valid, regularity of sound change is USEFUL in historical linguistics only if we can identify cognates in quantities large enough to apply the comparative method. Where this is not the case, regularity becomes increasingly a Platonic ideal with little observable effect or relevance in linguistic praxis. The establishment of cognate sets can, for example, be stymied by millennia of massive back-and-forth lexical replacement due to vocabulary tabooing, as in Australia, with routine adoption of words from neighboring languages (see 'Introduction', p. 30). This can 'swamp' the inherited lexicon completely; Campbell's dismissive comment that 'lexical borrowings are a fact of linguistic life that the comparative method has to contend with everywhere, not just in Australia' (83) is a serious misrepresentation and oversimplification. For semantic problems with cognacy, see Ross and Wilkins (below).

Nor is it clear, finally, that the family-tree model itself – the theoretical construct informing the notion 'regular sound change' – is always the best way of looking at language change. A language's 'exoticness' or 'primitive-

ness' is not the point; but other factors may be. Thus the family tree is arguably an appropriate model for spread zones but not residual zones (Nichols 1992: 23–24), or for periods of punctuation but not equilibrium (Dixon 1997). Moreover, even if 'correct' in the abstract, the family tree model may be useless in practice in cases where the main-line genetic filiation has been overlaid and re-overlaid to the point of invisibility by millennia of contact phenomena, as commonly in residual zones.

John Newman's essay, 'Footnotes to a history of Cantonese: accounting for the phonological irregularities' (chapter 4), is the first of a series of case studies comprising the core of the book. It addresses a classic problem in comparative reconstruction, visible only when documents from earlier stages of the language group are available: discrepancy between a theoretically reconstructed proto-ancestor and the actually attested historical ancestor. Such cases arise when all the daughter languages have lost a proto-feature F or altered it in the same or similar ways, so that the original F is no longer inferable and indeed would remain unknown except for the existence of concrete historical documentation. The parade example here is reconstructed proto-Romance vis-à-vis attested Latin; Newman gives us the case of Chinese. Middle Chinese (MC) had a long native tradition of lexicography reaching back at least to the *Qièyùn* in 601 A.D.: words are listed grouped according to various phonetic categories, notably tone and rhyme class, and the *Qièyùn* conveys information on alliteration (word onset) as well. Thus, despite the nonalphabetic nature of the writing system and the absence of phonetic discussion, a great deal is known about the pronunciation of Middle Chinese. Moreover, such lexica 'were not based on any one dialect, but represented instead a kind of norm of pronunciation which incorporated all the possible distinctions of a number of dialects, that is, a diasystem' (93).

The data found in these early lexica can be compared with the results of theoretically reconstructing a 'Proto-Chinese' by the comparative method on the basis of modern Chinese dialects. The two show excellent agreement overall (93), but not always. Newman explains these noncanonical cases in three lexeme-specific ways: first, by appeal to the influence of a graphically similar MC character having a similar but not identical pronunciation; second, as a deflection of phonetic outcomes that would sound like taboo words (sexual organs, death, etc.); third, by appeal to the ordering of phonological rules, including lexeme-specific reversal of the usual rule order in a few cases.

Mark Durie's essay, 'Early Germanic umlaut and variable rules' (chapter 5), deals quantitatively with a class of partly sporadic vowel raising and lowering rules that are operative in Old Icelandic (OI), Old High German (OHG), and Old English (OE). These rules convert Germanic **u* to *o*, **i* to *e*, and **e* to *i* in appropriate umlauting environments (raising before a high vowel in the next syllable, lowering before a non-high vowel). However, only the change **e* > *i* is 'regular' in the usual sense. The rule **u* > *o* applies in

roughly 3/4 of the expected cases, while *$i > e$ applies in about half the expected cases in OHG, more rarely in OI and OE. Moreover, the exceptions involve different words in the three languages, arguing that the relevant changes did not take place at the Proto-Germanic level but in the individual daughter languages. On the other hand, the choice of WHICH words are affected, though not deterministic, is not random either, but correlates significantly with a secondary conditioning factor: the surrounding consonantal environment. In general (excepting the change *$i > e$ in OHG) there is a clear preference whereby adjacency to a labial or velar favors nonlowering of the high vowel. Significantly, this is a statistical conditioning factor, not a yes/no environment. Durie urges the usefulness of such a probabilistic approach as an insightful reorientation toward the whole phenomenon of 'exceptional data' in sound change.

One of the classic success stories of the comparative method among unwritten languages is Austronesian – not surprisingly, given the special archipelagic nature of much of the family's spread through the Pacific. There is nothing gradual about a deep-sea migration, and boundaries are intrinsically sharp when speech communities are separated by large stretches of ocean. These are ideal conditions for the comparative method. But Austronesian diachrony is sometimes anything but clearcut, as three of the most thought-provoking studies in the book demonstrate.

Robert Blust's essay, 'The Neogrammarian hypothesis and pandemic irregularity' (chapter 6), discusses two cases of 'pandemic irregularity' in Austronesian, that is, irregularity which occurs in many lexemes and in many languages and resists all attempts to reduce it to rules. First, although many Austronesian etyma reconstruct unproblematically as *CVCVC and others as *CVNCVC, many etyma show conflicting reflexes, some languages reflecting *CVCVC, some *CVNCVC. Here Dempwolff reconstructed a 'facultative nasal' *CV(N)CVC, a mere notational makeshift that masks a serious problem. No convincing explanation has ever been offered for this. Second, most etyma beginning with a velar stop reconstruct unproblematically either as *g or as *k; but for numerous other etyma different languages point in contradictory directions, some toward *g, others toward *k. Interestingly, nothing analogous occurs with nonvelars. Here Blust offers a perceptual explanation. Velar voiced stops have a shorter voice onset time than with points of articulation further forward in the mouth. This leads to greater likelihood of perceptual confusion between voiced and voiceless stops – a constant potential for random mishearing and subsequent reanalysis of voiceless as voiced and/or vice versa, on a language-specific basis. The moral is that certain phonetic environments, apparently, would seem intrinsically prone to irregularity.

George Grace's essay, 'Regularity of change in what?' (chapter 7), narrates the story of a radically intractable case, namely his own 25-year lack of success in applying the comparative method to the Austronesian (Oceanic)

languages of southern New Caledonia (SNC). These languages constitute an 'aberrant' subgroup (159–160): they are structurally atypical for Oceanic, have relatively few clear cognates or clear sound correspondences with other Austronesian groups, and their phoneme inventories are inexplicably much larger than for Proto-Oceanic (164). The SNC languages evidently are quite closely related. Yet cognate words within SNC show a hodgepodge of different phoneme correspondence patterns, with no obvious conditioning of any kind. With such a bewildering multiplicity of intra-SNC correspondences, and so little in the way of clear cognates to Proto-Oceanic, one cannot readily set up 'regular sound correspondences', as there is no way to tell what counts as 'regular' and what as 'irregular'. (Such cases are hardly unique: cf. the Wandala-Lamang subgroup of Chadic (Wolff 1983: 214–215), or Tlingit vis-à-vis Athabaskan-Eyak ('Introduction', p. 30).)

The key to the irregularity is the fact that SNC languages are entangled in a Sprachbund of genetically related languages (169). The clearcut classic notion of speech community breaks down here, as Grace emphasizes; rather, many languages are used within a community of communicating speakers, and conversely a given language may be used in several communities. In such a situation of massive lexical diffusion, speakers of L1 may be implicitly aware of 'sound correspondences' between their own L1 words and the corresponding words in L2, and words undergoing interlanguage diffusion can thus readily undergo automatic adjustment in pronunciation (174; also 'Introduction', p. 29). When the interdiffusing languages are themselves related, the corollary is that regular sound CORRESPONDENCES need not always be the reflex of regular (i.e. genetic) sound CHANGE (174); they can equally well arise as an accompaniment to borrowing, and there is no good way of telling the cases apart. This, and in general non-congruence between 'linguistically relevant communities' and language boundaries, can fatally undercut the comparative method (175).

Malcolm Ross's essay, 'Contact-induced change and the comparative method: cases from Papua New Guinea' (chapter 8), probes the non-genetic, contact influences on language development. There are several hundred Austronesian languages spoken on Papua New Guinea, and these have typically undergone major structural revision away from their inherited Austronesian type and toward a New Guinea type (clause chaining, OV macro-typology, etc.). The sociolinguistic ground for these changes is (or was, in pre-contact times) the massive bilingualism of New Guinea: people normally speak – natively – both an 'emblematic' language, bound up with a village's ethnic self-definition and an 'intergroup' language, typically that of a neighboring village and serving as lingua franca. The intergroup language tends to be dominant; even within the village it often is used more than the emblematic language. The latter is thus under pressure and changes of two kinds can happen: either massive structural-typological shift of the emblematic language toward the intergroup language – Ross terms this

METATYPY – or else actual language shift to the intergroup language. Ross's concern is with metatypy, which can have drastic linguistic effects: a metatypized language will display 'correspondences in form and partial resemblances in meaning to its genetic relatives', but will show 'more precise correspondences in meaning and resemblances in morphosyntax to its metatypic model' (182).

These points are illustrated by case studies involving two Papuan Austronesian languages, Takia and Maisin. Ross demonstrates metatypy at work in Takia not only in the grammar but also in the organization of the lexicon (190–191), as reflected in highly idiosyncratic collocations calqued by Takia on the model of its non-Austronesian Papuan neighbor Waskia (e.g. 'animal' is expressed as *pig-dog*). The morphemes here, lexical and grammatical, are inherited from Austronesian, but they are deployed in a Papuan manner (190). In Maisin, the metatypic changes are more drastic still, with grammatical elements sometimes undergoing revolutionary functional and semantic recasting (195–196). Thus the Maisin postposition *-efe* ('Source') is etymologically *-e* ('Location') plus *-fe* ('Irrealis coordinate dependent'), i.e. 'at + and', reflecting a morphosyntactic metanalysis of the sort

I X-**at and** Y-to I-came 'I was at X and I came to Y.'
⟶ I X-**Source** Y-to I-came 'I came from X to Y.'

– an etymology with exact parallels in nearby (non-Austronesian) Papuan languages (196). Maisin has grown more complex still through 'esoterogeny' (198, 202), that is, a development which renders a language more esoteric, more of 'an "in-group" code from whose use outsiders are excluded' (183).

Metatypy has important implications for morphological reconstruction (cf. Koch (below) and 'Introduction', pp. 10–11). Metatypy allows an inherited grammatical morpheme to change its meaning and morphosyntax drastically but not arbitrarily: the change represents convergence with the language's neighbors. Hence if two morphemes, good cognates phonologically, show a troubling semantic/functional/morphosyntactic mismatch, the difference may be explainable in terms of a metatypic adaptation by one of them to its areal neighbors. This can provide a persuasive, nonarbitrary MOTIVE for even quite drastic regrammaticalization (a point Koch does not consider).

Harold Koch's essay, 'Reconstruction in morphology' (chapter 9), is presented as a how-to manual for morphological reconstruction, woven into a typology of morphological change: morph replacement, changes in allomorphy, morpheme boundary shift, meaning and functional change, change in morphosyntactic status (free word, clitic, affix, etc.), morpheme reordering, and morpheme doubling. Several case studies are given from

Australian languages, involving verb conjugations, case suffixes, pronoun paradigms, and possessive marking. An important option is that, in a given instance, the data may fail to support ANY particular reconstruction; this is Koch's conclusion for the 1-sg accusative pronoun in Arandic (253–256).

One point requires comment. Koch's initial step in morphological reconstruction (220–221) reads: 'Match tentative morphs, that is, formal bits that are potentially cognate according to established phoneme correspondences … this assumes the prior establishment of phonological changes on the basis of lexical cognates' (220). Here the methodological priority assigned to sound correspondences seems ill-advised. As Nichols' essay makes clear, the registering of morphological correspondences is typically the FIRST step in applying the comparative method, preceding the project of setting up lexical sound correspondences. The very fact of having patently comparable forms in hand can often make the morphological reconstruction (even if preliminary and tentative) immediately obvious – notably when the related forms, and the paradigmatic systems they are embedded in, are IDENTICAL or very nearly so across the languages (cf. Goddard 1975: 261, n. 13). Nowhere is this truer than in cases of relatively deep genetic relatedness, such as Algonquian/Algic (cf. Goddard 1975, also Nichols 1998: 130–134) or Afroasiatic. Here the number of secure lexical cognates is very low, the sound correspondences anything but settled – yet the person/number/gender paradigms are often so similar as to immediately suggest a reconstruction, despite the absence of established sound correspondences.

The final essay, David Wilkins' 'Natural tendencies of semantic change and the search for cognates' (chapter 10), is devoted to the semantic side of reconstruction. The comparative method has never been explicit as to how much semantic leeway, and what kind, is legitimate in assembling cognate sets. Wilkins' concern is to develop, in the particular domain of body parts, universals of semantic change which can be useful both in identifying cognate sets and in establishing a particular proto-meaning for the set. Recent decades have seen considerable work in articulating paths and universals of semantic change, but primarily as regards shift from lexical to grammatical meanings (grammaticalization). Wilkins' study of body-part diachrony WITHIN the lexicon falls into a tradition represented inter alia by Matisoff (1978) and Heine (1997, esp. ch. 7).

Wilkins proceeds typologically. Drawing principally on four large families for which good comparative and etymological tools are available – Dravidian, Bantu, Indo-European and Tibeto-Burman – he establishes empirically several widespread diachronic-semantic 'natural tendencies' in the realm of body-part semantics:

(1) A visible body-part term shifts to refer to the visible whole of which it is a part, but not vice versa – allegedly unidirectional.
(2) A body-part term shifts to a spatially contiguous body-part.
(3) Terms for upper-body parts shift to lower-body parts and vice versa (Heine (1997: 134–136) presents this shift as overwhelmingly unidirectional, i.e. upper-to-lower).
(4) Animal body-part terms shift to humans.
(5) Terms for a verbal action involving a body-part can shift to refer to that body-part.

Wilkins then proceeds to test these tendencies by 'apply[ing] them to … a language family which did not contribute to their original discovery' (282; cf. 297–298) – namely, the languages of Australia. Drawing on a 75-item comparative word list (282–284), several case studies in body-part semantic change are examined: a shift 'fingernail > finger/hand'; chains 'skin > body > person <–> man, woman'; bidirectional shifts 'liver <–> heart <–> chest'; and finally several instances of part-to-whole shifts 'hair > head' and 'forehead > head', not always clearly unidirectional (290–292).

To bring rigor and method into diachronic semantics is excellent, and Wilkins' typological approach is rooted solidly in crosslinguistic reality. At the same time it is worth highlighting how fundamentally this methodology differs (inevitably) from the classical comparative method as applied to sound change. For one thing, principles of semantic change established specifically for a single semantic domain – even a privileged domain like body parts – seem 'local' in their applicability in a way that principles of sound change are not. But a deeper contrast concerns how the two methods, in their respective realms of semantics and sound, go about justifying non-identity of cognates. For semantics, Wilkins appeals to typology, i.e. to universally preferred, 'natural' paths of change. For sounds, this is exactly what linguists do not do. Phonic non-identity between cognates is not justified by appeal to diachronic phonological universals, but through conformity to a language-specific SOUND LAW, grounded in the contingent comparative facts of the particular language family. Typological naturalness is almost incidental here, for very unnatural-looking sound changes do occur – e.g. PIE **dw-* > Armenian *erk-* (cf. Meillet 1967: 18, 46–47), or Proto-Celtic **ē* > Welsh *ui*. Natural or not, a well-grounded sound law is simply a FACT, and (canonically) all instances of the relevant sounds can be expected to follow it.

Proof via sound laws has always been a pillar of the comparative method, and is considered, rightly, a uniquely reliable mode of argument. In semantics, however, it is hard to imagine any good analogue to language-specific sound laws, or to the principle 'exceptionlessness of sound change'. For such a 'law' to hold, the affected meaning-component would have to shift simultaneously in all words whose definition included that component

– as if the part-to-whole change 'fingernail > finger' automatically carried with it a shift of all part-terms to their respective wholes.

Wilkins' methodological appeal to typology is thus unavoidable. But typology is intrinsically weaker than the classical comparative method in what it can deliver. A typological universal, if truly universal and if established with absolute confidence, would provide a powerful guide to reconstruction. But this never happens. Even 'exceptionless' universals have only statistical reliability, for they are always sample-based; and exceptions to the most natural-looking universals keep turning up. Rather, typology is overwhelmingly a matter of trends and preferences. Language history, however, like any kind of history, is always a matter of particulars, and quirky and unusual changes do occur in response to unique constellations of particulars. Reconstruction methodology cannot exclude such changes merely because typology shows the change to be odd or unexpected. Typology can contribute to our assessment of a reconstruction's plausibility; but it does not have the (near-)absolute veto power on a reconstruction that a sound law does.

I close this review with brief discussion of the one significant omission in the book: syntactic reconstruction. Here the comparative method is faced with a special problem. In phonological comparison, the warranty for the RELIABILITY of a particular reconstruction inheres in its conformity to sound correspondences across cognates. In syntactic reconstruction, the compared structures may typically involve no cognate morphs at all but only pure pattern; if so, sound correspondences, and the crucial reliability control they provide, will be absent by definition. In particular, the problem of coincidental resemblance haunts comparative syntax far more than comparative phonology. Syntactic reconstruction thus demands its own reliability control, different from sound correspondences. Here typology can do multiple service. As with diachronic semantics (Wilkins), one must reckon with typological naturalness regarding paths of diachronic change (grammaticalization, etc.). But typology also offers a way of dealing with coincidence. When an empirically RARE feature F – rare globally or (more weakly) areally – turns out nevertheless to recur commonly all over a given language family L, then the recurrence is especially likely to reflect shared history and unlikely to be just an accident (cf. Gensler 1994). With such a 'disfavored' phenomenon F, single genesis in L is ipso facto easier to motivate than multiple independent genesis.

The attractiveness of the traditional comparative method lies in its incomparable usefulness as a TOOL. It presents clearcut procedures that yield concrete reconstructions with a built-in warranty of high reliability, and it is uniquely well suited to distinguishing genetic inheritance from contact phenomena. But to canonize the comparative method would be to show it scientific disrespect. As the book reveals in fascinating detail, the orthodox method sometimes does not work, sometimes cannot work, sometimes works

badly, and sometimes tells only a tiny bit of the story. Here linguists must develop and use supplementary methods – typology (Wilkins), statistics (Nichols, Durie), metatypic analysis (Ross). It is no contradiction to expect such methods to be both less powerful and utterly indispensable.

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Lenore A. Grenoble & Lindsay J. Whaley (eds.), *Endangered languages: current issues and future prospects*. Cambridge: Cambridge University Press, 1998. Pp. xvii + 361.

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Endangered languages assembles mature scholarship on issues surrounding threatened and moribund languages. The chapters fall into four groups: language ideology and a typology of language statuses; community responses to the threatened statuses of their languages; case-studies documenting what

is lost every time a language with its unique expressive structures disappears; and mechanisms of language loss and shift. This truly excellent volume would be appropriate as a text for courses on language shift and maintenance, or as a centerpiece in courses on language planning apropos to the twenty-first century. Selected chapters would function well in introductory typology or 'Languages of the world' courses.

In 'Western language ideologies and small-language prospects', Nancy Dorian argues that language loss is often precipitated by an 'ideology of contempt'. This Western (i.e. European-derived) ideology is characterized by ignorance of the complexity and expressive power of indigenous languages, belief in linguistic social Darwinism, and in the onerousness of bi- or multilingualism. Dorian demonstrates this ideology in the history of Western nationalism, in Western colonial expansionism, and in the European industrial revolution. France stands as a case of unusual intolerance both in prior centuries and in recent decades in its attempts to foster pure-French usage and, for example, in denying birth certificates and identity cards to children with Breton given names as recently as the 1970s (5). Dorian does not particularly address linguistic ideology in the United States of America, but it is evident in twentieth-century 'English-only' movements which routinely appeal to the supposed disruption and difficulty caused by bilingualism. Similarly, many South American national (also European-derived) cultures have historically been elitist in attitude and notorious for views of the 'deficiency' of indigenous 'dialectos'. Dorian suggests that this ideology is found around the world, and that few minority language groups have withstood its pressure.

Grenoble & Whaley (henceforth G&W) elaborate Edwards' (1992) proposal that endangerment of any particular language may be evaluated in terms of a matrix formed by the 11 parameters of demography, sociology, linguistics, psychology, history, political factors, geography, education, religion, economics, and technology/media, applied over the three domains of groups of speakers, language, and larger setting. G&W propose revisions to Edwards' framework to make it more predictive as to what conditions may retard, versus accelerate, language loss. They suggest that economic factors outweigh all others, and that a parameter sensitive to type of literacy should be added to the model, in that some kinds of literacy may accelerate language loss, while others may retard it. G&W also argue that not all of Edwards' variables are equally independent. Several case studies show how the parameters variously impact communities from sub-Saharan Africa, the former Soviet Union and New Zealand.

Nora Marks Dauenhauer and Richard Dauenhauer (henceforth D&D) write a compelling and articulate account of the 'Technical, emotional, and ideological issues in reversing language shift' experienced in Southeast Alaska. This chapter is essential preparation for anyone wishing to participate in language maintenance or revitalization efforts. D&D advocate

the importance of embracing the truth about what a community values, and the importance of clear-minded and objective assessment of the number of speakers, their levels of fluency, and honest prognoses for given languages. Of some twenty Native languages spoken in Alaska, only two are currently being learned by children. D&D focus specifically on Tlingit, Haida and Tsimshian. The latter two will probably die out within the next twenty years, and Tlingit will probably follow suit within the next fifty years. D&D say reversal of language shift is unlikely owing to the commonly-found situation where 'a broad gap and disparity have developed between verbally expressed goals on the one hand (generally advocating language and cultural preservation) and unstated but deeply felt emotions and anxieties on the other (generally advocating or contributing to abandonment)' (62). Emotions and anxieties contributing to language abandonment come from disparagement of the language by outsiders, by certain (but not all) religious players, by fears of 'how difficult' or 'alien' the language may be, and by fear that bilingual or Native-language ability may retard children's academic progress or economic achievement. At the same time, facing the loss of language or culture involves the same stages of grief experienced in death and dying, including denial about what is happening to one's own language.

The Mayan languages of Central America are among the most vigorous Native American languages. Nevertheless, they are under pressure and it is important to document what is occurring in the early stages of potential shift. Nora England discusses indigenous Mayan efforts toward language preservation including orthography decisions, standardization, expansion of domains of use including the schools, and current discussions about establishing official regional languages.

In contrast to the obsolescence facing Southeast Alaskan languages, and the robustness of Mayan languages, Mohawk stands in an intermediate position. In Kahnawà:ke, Quebec, efforts to increase the value and use of Mohawk are being realized. Kaia'titahke Annette Jacobs documents the chronology of Mohawk language instruction in the Kahnawà:ke schools and pithily notes that prior to 1970, 'the only thing native in our schools was the children' (117). Now this is no longer the case. One particularly important step in the evolution of classroom-based language instruction was a 1976 survey which showed that, although the community ASSUMED that the Mohawk language was being learned and widely used, in fact it was endangered. This survey motivated intensified efforts to collect and develop language materials and then to develop a Mohawk immersion school which incorporates aspects of Mohawk culture along with language instruction.

Given the vastness of South America where some 400 indigenous languages are spoken, Colette Grinevald provides a masterful survey of the diversity of situations encountered. She notes major surveys and significant descriptive sources ranging from early Catholic work from as early as 1595 up to the

present; and then surveys South American situations which range from that of Uru with two speakers in now-Quechuan territory, to Quechuan (8 to 12 million speakers, distributed among as many as 17 languages or dialects), Aymara (2 million), Guarani (3 million speakers and 5 languages), and Mapudungu (200,000 to 1 million speakers). Intermediate between these extremes are families like Cariban which contains Makushi with about 15,000 speakers and some 20 other languages with fewer than 1000 speakers. Grinevald also typologizes countries in terms of the proportion of indigenous population, the statuses of the indigenous languages, and the way that such patterns affect the 'identity' of a country. Of the 400-plus languages found in South America, Grinevald's assessment is that only Guarani is not endangered. This is partly based on the sociopolitical status that Guarani enjoys within Paraguay. In contrast, the extremely decimated indigenous languages in Argentina and Chile (the most Europeanized countries) illustrate Dorian's claims about European 'language ideology'. Grinevald concludes that recent language loss in South America is mostly due to language shift (rather than the population decimation seen in earlier times), as 87% of the ethnic groups have increased in population since 1970 and only 6% are suffering demographic decline. However, the overall dimensions of language loss since the colonial era are tremendous; Arawakan has declined from 65 distinct languages to about 34 today, Cariban from 43 to about 34, Chibchan from 24 to 18, and so on.

Grinevald concludes by outlining an agenda for professional linguists working in such a milieu, and her views of the philosophical, cultural, and attitudinal values she believes they should adopt. She asserts that the major challenge to the profession today is to bridge the gap between academia and the felt needs of indigenous communities, and argues for the application of linguistics to bilingual education, literacy, and language development.

The next set of papers focus on the artistic and intellectual resources that are lost with the disappearance of a language. Marianne Mithun's chapter is beautifully written in this regard. Avoiding technical linguistic jargon, she introduces fascinating differences among languages by contrasting Central Pomo and Mohawk word formation patterns with the reader's presumably English expectations: Central Pomo versus English lexicalization of number and shape of actor or theme into verb roots, and translation difficulties which might ensue; polysynthetic and agglutinative versus more isolating languages; how meaning can be differently distributed between roots versus affixes; Central Pomo instrumental prefixes; psycholinguistic issues of possible versus actual words; the rise of morphology out of free roots; Mohawk incorporation, word order, and particles which have evidential value – all without assuming that the reader controls the technical vocabulary that would be found in technical works.

Ken Hale takes the view that linguistic diversity is essential to the scientific study of the mind and of grammar, to developing a realistic theory of human

linguistic competence, and that language loss affects human intellectual life. To underscore these claims, Hale briefly discusses McCloskey's (1979) demonstration of how Modern Irish overtly shows that extraction must obey subjacency; the way in which Ulwa (Misumalpan) switch-reference and causative constructions challenge current theories of how grammar should work; and the implications of Lardil 'tense agreement' (which spreads across most, but importantly, not all, elements of the verb phrase) for principles that constrain the structuring of syntax. Finally, Hale reviews the highly abstract lexical structure of Damin, an 'auxiliary' men's language used by the Lardil in initiation ceremonies. For example, the single verb *tiiti* 'act' stands for activities 'other than those resulting in harmful effects'; while the verb *titi* stands for any verb with harmful effects. Hale illustrates how Damin achieves a balance between efficiency (and ease of learning) and expressive power as seen in the kinship system. As the Lardil people abandoned initiation practices under the influence of outside religious groups, Damin was largely abandoned and the last fluent speaker died a few years ago. In eloquent concluding words, Hale addresses the personal costs and grief for individuals who wish to, but cannot, acquire their parents' language or who cannot pass on the language and full culture to their children.

The title of Christopher Jocks' article on Mohawk, 'Living words and cartoon translations: Longhouse "texts" and the limitations of English', succinctly summarizes Jocks' primary point. He is careful to point out that most Native people do not embrace the radical view that loss of their heritage language implies they are no longer Native Peoples; but he also shows that without the rich cultural knowledge that a language carries, an ethnic group may become a 'caricature' of what it wishes to remain. Perhaps this can be best expressed by quoting Jocks directly: 'Not only does ceremonial work too easily become rote and formalistic, but when we must depend on English-language contextualization, it becomes easy for us to adopt and incorporate alien images and understandings of ourselves, without knowing it' (219). To explain what is at issue, Jocks discusses the translation of *-onhwentsi-* 'nation'; *karihwíio* 'righteousness' or 'good message'; and *Kaianeren'kó:wa* 'great law' or 'great goodness', which differs from Western notions of law, order, and what is conveyed by the standard translation of this word as 'The Constitution of the Five Nations'. Further, in the English context, one may 'possess' knowledge; but the Longhouse conception is that knowledge is an activity – something one does and which must be maintained. What can be expressed in translation may be a mere cartoon of the former indigenous concept.

In somewhat the same vein, Anthony Woodbury first supports the view that languages of wider communication can be adapted ideologically, if not always structurally, to communicative needs that were previously fulfilled by an ancestral language, particularly in situations of abrupt and radical language shift; nor does loss of one's heritage language imply that one's

culture or heritage ceases to exist. Nevertheless, Woodbury's particular concern is to document cases where expression is crucially dependent on form – and where the loss of a form (with the demise of its associated language) necessarily entails loss of ability to express something. Cup'ik Eskimo has affective suffixes translated as 'poor dear N; poor dear (subject) does V', 'darned N; darned (subject) does V'; 'funky N; funky (subject) does V'; and 'shabby old N; shabby old (subject) does V'. In an 'experiment', a speaker told a story in Cup'ik, retold it in English, and then provided a sentence-by-sentence translation in response to a tape recording played back one sentence at a time. The sentence-by-sentence translation contained NO English words translating the affective suffixes, and the free retelling contained only a few. The affective forms in Cup'ik Eskimo are suffixes and they may resonate and focus effects already evoked in the story; a few translations of these suffixes with lexical items like English *poor* resonate with nothing else in the story, and the translation and retelling loose much of the affective nuances in the original – English simply has no means to achieve the same narrative effect.

The final section of the volume concerns mechanisms of language loss. André Kapanga raises sociolinguistic issues involved in language maintenance and shift which motivate linguistic accommodation to other dialects or languages. This chapter is a somewhat classic variationist study of phonological and lexical accommodation among speakers of the 'creolized' or 'mixed' and 'less prestigious' Auxiliary Shaba Swahili in eastern Zaire, Educated Shaba Swahili (which contains some French lexical items), and the 'more prestigious' Standard Shaba Swahili used in the church and in radio broadcasting. However, Kapanga also notes that the non-standard Shaba Swahili dialect denotes local pride and an allegiance to local culture, and appears to be thriving at the expense of Standard Shaba Swahili.

Carol Myers-Scotton's chapter presents a condensed version of her (1993) theory concerning the Matrix Language (henceforth ML) turnover hypothesis, which aims to be a falsifiable theory that accounts for most cases of structural borrowing. When socio-political factors motivate the process of language shift, speakers must change their ML, i.e., the language supplying the grammatical frame or lexical predicate-argument structure and morphological realization patterns. In intra-clausal code switching and language turnover, there must be a stage in which a ML structure contains an 'embedding language' island, which is a maximal projection such as an NP or PP. In some situations, the ML is a composite of lexical structure from two or more source languages.

Myers-Scotton suggests that some 'mixed languages' are cases of incomplete language shift. For example, Ma'a/Mbugu of northeastern Tanzania has two varieties or registers (Mous 1994). Inner Mbugu has Bantu grammatical structure but Cushitic lexical items, while Normal Mbugu has Bantu lexicon and grammar; in fact, the grammar of the two varieties is

identical. Myers-Scotton argues that Inner Mbugu 'is the result of a late arrested ML turnover from Cushitic to the Bantu variety of the neighboring groups' (307). Though Myers-Scotton tries hard to make a case for why this instance of apparently intact Bantu GRAMMAR and Cushitic LEXICON illustrates a case of shift TO a Bantu language, rather than an arrested shift FROM a Bantu language towards a Cushitic language, barring the existence of independent evidence not presented in this chapter, I see no reason not to side with what is apparently Mous' opposite view of the language's history. The theory is, nevertheless, a rich and sophisticated treatment of exactly what happens in the process of language shift, which deserves serious testing with additional 'mixed' languages – particularly against languages where there is independent historical evidence of what happened.

In 'Copper Island Aleut: a case of language "resurrection"', Nikolai Vakhtin argues that a 'mixed' language with Russian grammar and Aleut lexicon arose where earlier Russian speakers shifted to incorporate Aleut lexical roots (apparently contra Myers-Scotton's scenario for Inner Mbugu). Vakhtin speculates that 'resurrection' of an Aleut language (though in a new mixed-language form) may have been precipitated by younger speakers who were ethnic Aleuts and who wished to adopt an Aleut 'language' as a badge of identity. Part of Vakhtin's argument is based on observations of younger Russian speakers attempting to speak the language of an older generation which they really do not know – and so end up incorporating (in this case) Yup'ik roots into otherwise Russian grammatical word and sentence forms.

The geographical coverage of the book is broad, though it does not provide any foci on south or east Asia, the middle east, or much of the former Soviet Union. Most importantly, it marks a maturing in the sophistication and depth of what can and cannot be learned cognitively, linguistically, and socio-culturally about processes of language shift and loss.

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Larry M. Hyman & Charles W. Kisseberth (eds.), *Theoretical aspects of Bantu tone* (CSLI Lecture Notes **82**). Stanford, CA: CSLI Publications, 1998. Pp. x + 366.

Reviewed by Laura J. Downing, University of British Columbia

This collection of eight papers is the first book devoted entirely to work on Bantu tone since Clements & Goldsmith (1984a). The editors of this volume intend for it to represent the same sort of state of the art collection as the earlier work, and to that end have brought together papers by established researchers on Bantu tone (like the editors themselves) as well as by researchers who are less well known. They have taken care to include work from a variety of approaches, making the unfortunately all too rare choice of bringing together papers from both the European and North American traditions, theoretical papers and those with more descriptive aims. (However, almost all of the papers are theoretical enough that a strong background in current phonological theory and in Bantu tone is required of the reader.) They have also included papers discussing a variety of languages. While Southern Bantu languages (Ikalanga, Tswana, Shona, Xhosa) are especially well represented, Eastern Bantu languages (Olusamia, Shingazidja, Tanzanian Yao) and Western Bantu (Kongo) also receive attention. This eclectic choice of languages and approaches promises to provide a stimulating overview of current issues in Bantu tone. How well does it succeed?

In many ways, very well. One important contribution of the volume is to present new tone data for several languages: Tanzanian Yao, Olusamia, Ikalanga and Tswana. A continuing challenge for Bantu tone researchers is that detailed tone data from a variety of morphological and phrasal contexts is lacking for most Bantu languages. Sources of new data by reliable researchers is essential for developing a typology of tone systems and testing whether current theories of tone are able to account for the range of attested variation. The generally very generous documentation the papers provide for the patterns they discuss is thus most welcome.

The important theoretical contribution of the volume is to highlight a couple of central tone problems which continue to challenge our understanding of possible tone systems. One problem that provides the focus of several papers is how to define the domain of tone realization in Bantu languages. Bantu tones are notoriously 'mobile', generally spreading or shifting from the input anchor or source syllable. As both Philippson's and Cassimjee & Kisseberth's papers note, these tone spread/shift domains are typically isometric with prosodic constituents, either binary or unbounded in size. This observation is the foundation of a new theory of tone realization, Optimal Domains Theory (ODT), adopted in Cassimjee & Kisseberth's paper. In this theory, feature realization is not mediated by association lines, but rather by featural domains. The domains approach accounts for the

properties that tonal domains have in common with prosodic constituents: they are subject to the same size parameters (binary vs. unbounded); they are subject to minimality and non-finality; and they may be limited to contain one and only one most prominent syllable. In this paper, ODT is first illustrated with examples from a variety of languages, chosen to show how a few constraints defining possible domains can account for some of the important parameters of variation in Bantu tone association. Then two detailed case studies, of Xhosa and Shingazidja, are presented to show how ODT can analyse a fairly complete portion of two complex tone systems. While these analyses succeed in conveying how ODT works, they are unlikely to persuade a reader unfamiliar with this new theory (as most readers will be) to adopt it in their work. The problem is that by the end of the two analyses, so many new constraints have been presented that one gets the impression that ODT analyses are exceedingly complex. This impression is compounded by the fact that much of the article is devoted to very theory internal discussion of choices of domain configurations or constraint formalizations, topics which are only of interest once one is persuaded that the overall approach is better than the alternatives. Unfortunately, alternatives are not discussed at all – or even referred to – even though the Xhosa tone pattern has been the subject of a number of recent autosegmental analyses (see Downing 1990 for one such analysis and reference to others). Shingazidja tone likewise has been previously analyzed in different frameworks, by Cassimjee & Kisseberth (1989, 1993). One would have appreciated the ODT analyses better if this paper had demonstrated clearly how they solved problems raised in these earlier analyses.

Philippson's contribution also focuses on tone domain size, and proposes that the fact that Bantu tone domains are typically isometric with prosodic constituents (either binary or unbounded in size) is a key factor in explaining the shift from tonal to accentual systems in many Eastern Bantu languages. In emphasizing the role of tone spread and tonal domain size, Philippson challenges earlier proposals (see, e.g., Clements & Goldsmith 1984b) suggesting that the crucial step in the change from a tonal to an accentual system which is typical of Bantu is Meeussen's Rule (MR). This process eliminates tone contrasts within morphemes by disallowing more than one high tone per morpheme (the one tone per morpheme restriction is considered a diagnostic of an accentual vs. tonal system). Philippson argues convincingly that MR is not a sufficient explanation for the shift to an accentual system because several languages which have lost Proto-Bantu tone contrasts provide no evidence that MR is active in all the crucial contexts. Instead, tone spread or shift can be shown to have eliminated contrasts (*HH, *HL > HH after tone spread). The fact that the domains for tone spread or shift are isometric with metrical feet would have compounded the tendency to reinterpret tone as accent in these languages.

Both Philippson's and Cassimjee & Kisseberth's papers crucially rely on

the observation that tone domains are constrained to be either binary or unbounded in size. However, there are Bantu languages, like Tswana and Ikalanga, where high tones commonly spread two or three times, in violation of the locality condition (see Downing 1996 for discussion) defining possible prosodic constituents as either binary or unbounded. Creissel's paper in this volume presents Tswana data illustrating this problem. While it does not provide a formal analysis, the paper makes an important contribution in laying out clearly and thoughtfully a range of data illustrating this problematic pattern in a variety of morpho-syntactic contexts. Hyman & Mathangwane's paper discusses multiple spreading in Ikalanga. They show that the apparent examples of non-local spreading can be accounted for in that language by proposing that there are three distinct tone spread rules (H1, H2, H3) applying cumulatively in different prosodic domains: stem, phonological phrase and utterance. Evidence that the three tone rules are indeed distinct comes, first, from their interaction with depressor consonants: H3 is blocked by depressors but H1 and H2 are not. Further, H1 is iterative, while H2 and H3 are not. Evidence is also presented motivating the distinct prosodic domains for the three rules. While Hyman & Mathangwane's analysis of Ikalanga is quite convincing, a weakness of their theory, as they point out, is that it cannot straightforwardly account for other cases of multiple spread, like the Tswana data discussed in Creissel's paper.

The interaction of morpho-syntax and tone is the second main theme of the volume. Another important contribution of Creissel's paper is to show that several tone processes apply differently within (prosodic) words than across word boundaries, providing a reliable diagnostic distinguishing prosodic words from morphological words and phrases. A similar prosody-syntax mismatch is illustrated in his discussion of final H retraction. This process generally occurs pre-pausally and is also shown to be triggered by a number of post-nominal modifiers even though they are not preceded by pause.

Blanchon's paper discusses morpho-syntactically motivated diachronic tone change, focussing on the development of tone case in Western Bantu languages like Kongo. He shows that in several Western Bantu languages each noun has two distinct tone patterns, each with a different grammatical function. While there is some synchronic variation in the realization of the two tone patterns in the languages surveyed as well as in the grammatical functions associated with the patterns, Blanchon argues plausibly that both the tone patterns and the grammatical functions are diachronically related. Tone case developed as the definite high tone and predicate low tone were reinterpreted, first, as distinguishing referential vs. non-referential nominals (rather than marking a definite/indefinite distinction), then reinterpreted again as distinguishing subject, phrase-initial from non-subject, non-phrase-initial nominals.

Myers' important contribution further develops arguments from his thesis

(Myers 1987) showing the morphemes composing Bantu verbs are not grouped into a flat structure or a simple affix-adjoining structure as has been assumed by most other linguists. Rather, they are grouped hierarchically into two major constituents, the inflectional stem and the verb stem. The inflectional morphemes are further argued to group into two classes. Both morphological and phonological arguments are presented to motivate these groupings. Morphological coocurrence restrictions among inflectional morphemes can be stated as subcategorization requirements on sisters in this analysis, but not in competing analyses. The distribution of pronominal clitics in Swahili and the affixation of focus markers in Kirundi also support the constituency argued for here. Tonal processes from a number of languages (Tonga, Digo and Kirundi) and Swahili stress are shown to provide phonological motivation for the proposed constituency and affix classes.

Odden's and Poletto's papers discuss the morphologically conditioned tone paradigms of Tanzanian Yao and Olusamia, respectively. In both languages, it is shown that Proto-Bantu lexical tone contrasts have been lost for verbs. Both authors propose that verbs are underlyingly toneless and that the surface high tones that are found in most tenses are associated to the stem by morphologically conditioned constraints. In both languages, it is shown that these grammatical high tones associate to the V1, V2 and/or Final stem vowel positions, as is typical in Bantu stem tone association. (Other constraints account for the variation in realization of high tones in different morphological and phrasal contexts.) The main contribution of both papers is to provide detailed data and develop an Optimality theoretic analysis of a complex tone system, clearly demonstrating the role of morphology in conditioning the association and realization of high tones both in words in isolation and in phrases. Odden's paper also discusses nominal tone and provides an interesting comparison of parameters of variation in nominal tone realization in three Tanzanian Yao dialects.

The range of problems discussed by the papers in this volume and the general high quality of the contributions are certainly impressive. The collection does, however, contain some weaknesses. First, there are numerous typos. These are often found in the data and representations where they make analyses hard to follow (see, especially, p. 250, figs (31) and (33) and p. 253 where mistakes marking tones and determinants in the data and mistakes in the derivations mar what is otherwise an extremely clear exposition of the Tonga tone system). Also, most papers lack the references which would allow readers to place the work presented in any relevant theoretical or Bantuist context. For example, neither Odden nor Poletto mention that the V1, V2, Final V stem tone association pattern is widespread in Bantu and has been the subject of several previous analyses (see Downing 1996 and references therein). Most of the authors mention that tone spread is common in Bantu without citing any sources for that claim (like, for

example, van Spaandonck's seminal work (1971) on common Bantu tone processes). As a result, readers must bring sufficient knowledge of both Bantu and tonal phonology to the papers to evaluate how typical the tone patterns described are, how easy they are to analyze in any theory, how the analysis presented compares to previous work on the same problem or similar problems in other languages; even whether the observations are original. Hyman & Mathangwane's and Myers' papers stand out in this collection as especially scholarly contributions which combine significant theoretical results with solid descriptive work and with sufficient references provided for the interested reader to pursue the topics they discuss.

The strengths of this volume outweigh the weaknesses, however. It is an indispensable addition to the library of anyone with a serious interest in Bantu tone systems or, more generally, in prosodic structure and the interaction of phonology with morphology and syntax. Even the weaknesses of the volume should further the editors' aim of stimulating further research into the complexities of Bantu tone.

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Giulio Lepschy (ed.), *History of linguistics*. Vol. IV. *Nineteenth-century linguistics*, by Anna Morpurgo Davies. London: Longman, 1998. Pp. xxvi + 434.

Reviewed by MICHAEL MACKERT

The two volumes under discussion are the latest installments in a projected five-volume work translated from previous Italian versions. In spite of the title under which these volumes are published, *History of linguistics*, the goal of the contributions to this series is not to outline the development of the science of linguistics. In his introduction to volume III, which is included in all the published volumes, the editor makes clear that the volumes in this series are to provide a 'history of linguistic thought' (x). The latter label refers to a type of history that delineates the 'interests and attitudes towards language' existing in past societies and epochs (xiv). The scope of this inquiry includes the study of the 'social, cultural, religious and liturgical functions' of language and of 'the prestige attached to different varieties, the cultivation of a standard, the place of language in education, the elaboration of lexical and grammatical descriptions, the knowledge of foreign idioms, the status of interpreters and translators and so on' (x).

The essays contained in volume III live up to the editor's expectations to various degrees in terms of the topics explored. Given the large amount of material included in each contribution, I will only highlight topics representative of each author's approach. Chapter 1 is organized into three different sections exploring the linguistic thought in Western Europe, Roman Slavdom and Orthodox Slavdom during the fifteenth and sixteenth centuries respectively. Each of the three sections explores themes whose presence and development are traced in different national traditions within the above divisions.

Mirko Tavoni's section on Western Europe starts with an overview of the study of Latin grammar and of the emancipation, orthography and grammar of vernacular languages. This is followed by a discussion of the emergence of diachronic and comparative linguistics in the Romance and Germanic language areas, which documents the role of the biblical tales of Noah and Babel and the nationalistic motivations behind various theories concerning the origin and relation of vernaculars. For instance, during the 1540s Florentine scholars, influenced by the work of Annius, maintained that Tuscan was an offspring of Etruscan or Aramaic, the language brought to Ertruria by Noah after the Deluge. This short-lived 'Aramaic theory' was to legitimize Cosimo de' Medici's political domain and to oppose the view that

Tuscan was a corrupted form of Latin. Tavoni concludes with bibliographical information on lexicography, translation theory and missionary linguistics, topics which should have received fuller consideration.

The two subsequent essays on Slavdom make the distinction between the affiliation of different peoples to either the Roman or Orthodox Church. Linguistic activities within Roman Slavdom (in the Croat, Slovak, Slovene, Czech, Polish, and Upper and Lower Sorbian speaking areas) were characterized by their dependence on the Western tradition and the function of Latin as a supra-national language. Orthodox Slavdom (Bulgaria, Serbia, Ruthenia and Russia) is characterized by the central role of Church Slavonic and the influence of the Greek culture and grammatical tradition. These sections are not restricted to the Early modern period, but include information on nineteenth-century work 'to outline the long transition from the Middle Ages to the modern age' (2).

The contribution on Romance Slavdom by Maria Delfina Gandolfo focuses on the development of orthographies, grammars and dictionaries by highlighting the connection between religious reform and the need for Bible translations into the vernacular. These translations provided the initial stimulus for linguistic work establishing norms for the nascent languages of different nations. An example from the Czech language area is the work on orthography attributed to the religious reformer Johann Hus (1369–1405), which was driven by the desire to establish a standard for presenting Christian teachings accurately. Early grammars produced along with Bible translations were modelled after available western grammatical treatises. Gandolfo suggests that the first grammar of Czech, the *Grammatika česká* (1533) by V. Optát, P. Gzell & V. Filomates, was based on an already existing Czech version of Donatus' grammar whereas the Latin grammar of Petrus Ramus was the model for Vavřinec Benedkt Nedožerský's *Grammaticae bohemicae* (1603).

Silvia Toscani's synopsis of developments within Orthodox Slavdom concentrates on early reflections on accurate translation, orthography and the production of grammars of vernacular languages and Church Slavonic. Early work on grammars followed Greek models and was driven by religious motives. For example, the first work exclusively dedicated to grammatical matters, the *Osъmь čestii slova*, identified as early as the fourteenth century in Serbian manuscripts, covered the eight parts of speech and drew on the work of Byzantine grammarians. In the Balkans the increasing interest in philology was connected with the ideas underlying the hesychastic movement, whose members considered sacred texts as icons 'of revealed Truth' (128). The focus on the revision of corrupted religious texts, as in the work of Euthymius, the late fourteenth century Patriarch of Trnovo, was aimed at protecting orthodoxy from heresy while giving Church Slavonic the status of a liturgical language.

Raffaele Simone's account of the early modern period is most com-

prehensive in its coverage of topics. At the outset, Simone identifies the following themes that informed linguistic thought during the seventeenth and eighteenth century: language and theology; language and education; human language, animals and machines; the misuse of language and its reform; the origin of language; the unity of language and the diversity of languages; and language change, usage and society. Throughout the essay Simone alternates between sections focusing on the presence of these themes in the work of individual thinkers or movements (Bacon, Port Royal, Hobbes, Locke, Leibniz, Vico, Condillac and the French encyclopédists) and sections exploring the above themes across the board. Simultaneously, Simone traces the roles of a 'higher' and a 'lower' line of linguistic inquiry that shaped linguistic activities during this period. The first line was directed toward more 'global, philosophical and speculative considerations', while the second one produced 'specific and factual analyses, aimed principally at teaching', compilations of data and etymologies, and 'complicated hypothesis on the origin and relationship of languages' (156). Both of these lines of inquiry are present in the work of Leibniz: in his plans for a *characteristica universalis* and in his comparative and etymological work.

The bibliographic information on the majority of primary texts is hidden away in footnotes, which makes locating it somewhat laborious. Its inclusion in the existing excellent bibliographies or in separate ones would have been more reader-friendly. As a whole volume III does accomplish what it was conceived to be: a history of linguistic thought. As such it is an excellent text that can serve as a useful supplement to existing histories of linguistics.

Volume IV by Anna Morpurgo Davies does not follow the editor's conception of a history of linguistic thought, but turns out to be a history of Indo-European comparative and historical linguistics in Europe and the USA. Morpurgo Davies narrowly focuses on the 'interests, theories and achievements of the professional linguists of the time' (xxii). The author justifies this move by stating that the establishment of the linguistic profession and discipline and its achievements were 'unique events', which 'must be given priority in any account of ninenteenth-century linguistics' (xxiii). The themes and topics highlighted by the editor and successfully covered in the previous volume are usually only mentioned in passing and relegated to the background in this volume. The only exception to this is chapter 4, which provides a detailed discussion of historicism and organicism and of the influence of biological models of classification on linguistics.

Chapter 1 addresses the function of university expansion and the formation of an academic class in the establishment of linguistics as a university discipline within Germany and other Western countries and discusses the role of early linguistic histories in establishing the basic pattern of precursors (Jones, F. Schlegel), founders (Bopp, J. Grimm, Humboldt), consolidators (Curtius, Schleicher, Fick) and developers (Leskien, Verner, Brugmann, Delbrück) that pervaded nineteenth-century linguistic hagiography.

Chapter 2 counteracts nineteenth-century histories that created the impression that Indo-European comparative-historical linguistics originated out of nothing. It surveys work on general grammar and data-oriented approaches that preceded the new linguistics emerging in the 1830s and examines the role of etymological studies and non-Indo-European languages in the formation of historically-oriented frameworks. The collections of Hérvas y Panduro, Adelung and Balbi, in which language is seen as a key to explaining the history and relations of different peoples, are given detailed treatment. The remainder of the chapter is dedicated to work on language kinship and already existing methods of lexical and grammatical comparison. The subsequent chapter briefly sketches the history of the study of Sanskrit in the West and evaluates W. Jones' contribution to Indo-European linguistics. Most of this chapter concentrates on F. Schlegel's role in identifying the importance of Sanskrit for linguistic comparison and in the development of linguistic typology.

Chapter 5 foregrounds the philosophical and data-oriented lines of research present in the work of W. von Humboldt. However, more emphasis is given to the familiar theoretical aspects of Humboldt's work: energeia/ergon, innere Sprachform, linguistic diversity, linguistic typology and linguistic relativism. The discussion of Humboldt's descriptive work is restricted to a brief section on his treatment of the Javanese verb.

Chapter 6 covers the work of Rask, Bopp and J. Grimm. Rask's data-oriented approach and emphasis on the historical explanation of existing grammatical forms are highlighted. The structure of Rask's descriptive grammars and his early version of Grimm's Law are examined. The movement towards the institutionalization and specialization of linguists received its major impulse from Bopp. Special attention is given to his method of comparative morphological analysis and his ideas on agglutination. The final section examines Grimm's data-oriented historical approach, Grimm's Law, his explanations for Umlaut and Ablaut, his views on linguistic deterioration and the relation between nationalities and languages.

The expansion and institutionalization of historical and comparative linguistics in Western Europe by the middle of the century is traced in chapter 7. This is followed by an account of the increasing connections between phonetics and linguistics. The consequent discussion of Indo-European linguistics focuses on the work of A. Schleicher: his reconstruction of Indo-European, his family tree model and his emphasis on the importance of sound laws.

Chapter 8 provides an overview of general works existing during the mid century and contrasts in detail Schleicher's organicism with Steinthal's psychologism and Whitney's institutionalism. The ramifications of Schleicher's and Steinthal's approaches for linguistic typology and classification are then explored.

Chapter 9 concentrates on the rise and achievements of the Neogrammarians. Their interest in language change and emphasis on well-defined procedural and methodological principles are foregrounded and the basic tenets of the Neogrammarian framework are illustrated with Paul's *Prinzipien der Sprachgeschichte*. Special sections are reserved for the controversial debates on sound laws and analogy.

The final chapter opens with a discussion of two challenges to Neogrammarian doctrine: Schmidt's wave theory and Hugo Schuchardt's views on dialects, sound laws and language mixture. This is followed by a brief survey of the role of linguistics in ethnography, anthropology, experimental psychology and language teaching and of Baudouin de Courtenay's and Kruszewski's ideas on phonetics, phonology and morphophonemics. The concluding sections give a synopsis of work in syntax and semantics at the end of the century. The volume is completed by an extensive 70-page bibliography of primary and secondary sources.

Morpurgo Davies has succeeded in giving a comprehensive and excellent account of the development of Indo-European comparative and historical linguistics that serves as a useful introduction to the history of the field. Because of the focus of the volume, however, simultaneous important developments in anthropological linguistics and ethnolinguistics are not given the treatment they deserve. For instance, work in North America by non-professional linguists like Duponceau, Brinton and the early Boas is referred to only briefly. Given the framework of philology developed by DuPonceau, the fieldwork and theoretical writings on phonetics/phonology by Boas and the presence of the ideas of Humboldt and Steinthal in the work of some of these writers, a detailed discussion of these writer's works would have added to a more well-rounded picture of the period.

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Jacob L. Mey (ed.), *Concise encyclopedia of pragmatics*. Oxford: Elsevier Science, 1998. Pp. xxviii + 1200.

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Pragmatics has long been acknowledged to encompass two schools of thought: on the one hand, there is a school that recognises a disciplined research agenda focusing on topics emerging from the technical concerns of analytical philosophy; on the other hand, there is a school that takes seriously the claim that 'it is a sufficiently accurate characterization of pragmatics to say that it deals with the biotic aspects of semiosis, that is, with

ALL the psychological, biological and sociological phenomena which occur in the functioning of signs' (Morris 1938: 30; emphasis added). The first school talk about indexicals, speech acts, contextual inferences, presupposition and the semantics-pragmatics interface; the second school talk about 'everything'.

Jacob Mey has compiled, with the volume under review, an attractive, if not exactly concise, encyclopedia that is firmly within the second school of thought. Thus, in addition to modest entries on indexicals (Levinson, Leezenberg), contextual inferences (Thomas, Kasper, Talbot), the semantics-pragmatics interface (Jucker) and not so modest entries on speech acts (Allan) and presupposition (Seuren), there is a battery of entries on computer-related topics – adaptability (Mey), artificial intelligence (Wilks), cognitive technology (Gorayska), electronic mail communication (Vasconcelos), human-computer interaction (Campbell), hypertext (Hardman) and telematics (Qvortrup) – and media-related topics – broadcasting (Forrester), comics (K-A. L. Mey), journalism (Maclean), media, language and communication (Schrøder), media, mass media and multimedia (Maclean), newspeak (Chilton) and rock music (Ortner) – as well as entries that represent the editor's personal inflection of the second school of thought (cf. Mey 1985, 1993, 1994) – class language (Gregersen), critical language awareness (Mey), emancipatory linguistics (Signorini), hegemony (Mininni), ideology (Luke), manipulation (Fairclough), Marxist theories of language (Mininni), power and language (Fairclough), social class (Macy) and Universal Pragmatics (Nørager). The diversity is extreme. It is as if, in response to the opening sentence of his own entry on Pragmatics, viz., 'Among pragmaticians, there seems to be no agreement as to how to do pragmatics, nor as to what pragmatics is, nor how to define it, nor even as to what pragmatics is *not*' (716), the editor has selected any topic bearing on 'the science of the relation of signs to their interpreters' (Morris 1938: 30) in optimistic anticipation that some of it (the signs) will bear some relationship to some of the readers (the interpreters).

The following question naturally arises: which, of the two schools, is the better? Those in the first school will say that the study of 'everything' is hardly a viable academic project. Mey in the Preface in fact acknowledges, apparently in all seriousness, that his preferred perspective 'opens a potentially infinite window on all human activities' (xxvi). Those in the second school will say that the narrow, technical concerns of the first school appear scholastic and have little or no bearing on what they discern as the 'real world'. Both of these perspectives have been fairly stable for a number of years. The most sensible answer to the question, and one that is most faithful to the historical and conceptual roots of the discipline as a whole, was given twenty-five years ago and supports the first school: 'The best way to delimit the field of pragmatics, at least from one side, is probably to try to fix the boundary between it and its neighbour, semantics' (Hansson 1974:

163). Essentially the same answer is given in Thomason (1990), Rescher (1995) and Stalnaker (1998).

With this answer, which I commend, a perspective can be taken on the broad spectrum of topics covered by this encyclopedia. First, semantics. There is very little in this encyclopedia that deals with its subject matter's most immediate and obvious neighbour. A short entry on each of entailment (Read), indexicals (Leezenberg), metalanguage versus object language (van Eijck) and sense (Crimmins) just about covers the notions of philosophical/logical origin and equally short entries on semantic primitives and stereotypes in semantics (both by Geeraerts) allude to topics of more linguistic providence. On the topic of stereotypes, it needs to be said, something of an opportunity has been missed. This notion was introduced in some early work by Hilary Putnam and it can be used to tie together two otherwise distinct research traditions. The argument would go like this: if, as Putnam (1975) claims, semantics has enjoyed little theoretical or analytical progress and if, as he continues, this is because semantics is not a formal or technical science, but a typical social science, and if, as he insists, as a typical social science, semantics has to be sensitive to numerous divisions of labour, including the sociolinguistic division of labour, then semantics cannot provide a 'general and precise theory ... until one has a general and precise model of a language-user' (Putnam 1975: 152). Different language-users will have different stereotypes, but this doesn't matter, for as Putnam further remarks, in conclusion to his discussion of the sociolinguistic division of labour: 'there are two sorts of tools in the world: there are tools like a hammer or a screwdriver which can be used by one person; and there are tools like a steamship which require the cooperative activity of a number of persons to use. Words have been thought of too much on the model of the first sort of tool' (Putnam 1975: 229). The resonance of this argument with items on the Mey agenda is striking and the kind of 'societal pragmatics' that the editor is keen to define would be brought more sharply into focus had some reference been made to Putnam's work from the 1970's.

Second, pragmatics. If it is assumed that pragmatics is a field of inquiry that is disciplined by the application and perhaps interaction of pragmatic principles then this assumption is well-served in this encyclopedia. The entries on pragmatic principles (Togeby), the cooperative principle, conversational maxims (both Thomas), politeness (Kaspar) and relevance (Talbot) are, by and large, exemplary models of the genre. The éminence grise here is, of course, Paul Grice and any project such as the present volume cannot fail to give his work, and the work that it has inspired, anything but the most intense attention. The original theses, as Thomas acknowledges, are flawed and unsatisfactory, but they have motivated an enormously successful research programme. The politeness principle, for example, is intended as an addition to the cooperative principle and it accounts for discourse that lies beyond the 'maximally effective exchange of information' (Grice 1989: 28)

that the cooperative principle was designed to explain. The principle of relevance, further, is intended as an alternative to the other principles but as the politeness principle is a sociological principle oriented towards the speaker, and as the principle of relevance is a psychological principle oriented towards the hearer, it is not at all obvious that these two principles are incompatible and cannot be brought together in a super-theory. The entry on metapragmatics (Caffi) precisely recommends detailed metatheoretical reflections but is silent on this specific issue.

The entry on the conversational maxims contains a useful section on common misrepresentations of Grice's theory, which the author of the entry on conversational implicatures (Koktova) would have done well to have read, as her principal criticism of Grice's programme relies upon one of these common misrepresentations. She also, in the biographical entry for Grice, makes a number of other simple mistakes: (1) Grice never worked WITH Austin: the two were fiercely independent (Grice 1986); (2) his major interest was not the analysis of discourse but (i) the conceiving of the possibility of a settlement to the dispute between logicians like Quine and ordinary language philosophers like Strawson over the nature(s) of logic and language, which leads immediately to (ii) the distinction between logical and contextual inferences, which, in turn, leads immediately to (iii) the semantics-pragmatics interface (Grice 1989: 171–180, 339–385); (3) additionally, *Studies in the way of words* was published in 1989, not 1979; (4) finally, Grice died in 1988, not 1985. These are irritating mistakes, but, if, as Mey observes in the Preface, the purpose of an encyclopedia is to promote truth and dispel error, then they should be noted and brought to book.

If, alternatively, it is assumed that pragmatics is a field of inquiry that is disciplined by the statement of felicity conditions for illocutionary forces then this assumption is also well-served in this encyclopedia. All the entries – on felicity conditions, indirect speech acts, performative clauses, speech act classification and definition, and more – are by Keith Allan, which makes him the most prolific contributor to this volume and speech act theory the most extensively represented theoretical perspective. But whereas speech act theory is undoubtedly important in the historical development of pragmatics – Mey goes as far as saying (incorrectly, in my view; a much more persuasive point of origin is Carnap's *Introduction to semantics*) that pragmatics began with the publication of Austin's *How to do things with words* – recent innovations are very hard to find. The problems seem to be (at least) twofold: on the one hand, this kind of inquiry is less theoretical and explanatory than it is classificatory and, in the end, classifications without accompanying explanations are only of modest interest; on the other hand, the notion of speech act has been absorbed into the much richer notion of face-threatening act. If there is a lingua franca these days in the pragmatics community, it is that outlined in Brown & Levinson (1987) and further refined in hundreds of

articles and books since. Kasper's entry on politeness is an excellent account of the issues arising out of this recent work.

Third, the semantics-pragmatics interface. There is nothing in this volume on an influential school of thought that is known as radical pragmatics. This school holds that recalcitrant problems in syntax and especially semantics should be given solutions found with the assistance of an independently motivated pragmatic theory. In this way, both the syntax and semantics can be simplified and their character made more perspicuous. The pragmatic theory is often, but not always, taken to be Gricean in spirit and, on a Gricean strategy, this entails that the semantic theory will be univocal and minimal (the entry on conjunction and pragmatic effects (Carston) discusses this strategy with reference to the semantics and pragmatics of the word *and*). The pragmatic theory will then add implicatures of various kinds to the uniquely postulated senses. Other divisions of labour are conceivable. For example, it is possible to design a semantics which licences ambiguity and which is accompanied by a pragmatics which selects, in the appropriate context, the relevant sense. Alternatively, it is possible to design a semantics which licences indeterminacy or vagueness and which is accompanied by a pragmatics which contributes, in the appropriate context, to the determination of the relevant sense. (There are no entries in this volume on ambiguity or vagueness.) There are no doubt other ways of distributing the 'energy' of a grammar over the semantics and pragmatics. This topic is currently coming under intense scrutiny (Carston 1999, Levinson 1999, Turner 1999) and is likely to remain high on the research agenda for at least the beginning of the new millenium. But, to repeat, there is nothing on such concerns in the book under review.

This, in conclusion, is a book that contains much of interest to students of linguistics and to the discipline's more seasoned professionals. It is unlikely that readers trained in the analytical tradition or one of its offshoots will find many of the entries relevant to their concerns. Such readers can be referred to one of this encyclopedia's sister volumes (Lamarque 1997). On the other hand, readers who share the editor's preference for a sociological perspective on their discipline will find much in this book for them to consult with frequency.

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Andrea Moro, *The raising of predicates: predicative noun phrases and the theory of clause structure* (Cambridge Studies in Linguistics **80**). Cambridge, New York & Melbourne: Cambridge University Press, 1997. Pp. x + 318.

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In this book, Andrea Moro proposes a unified treatment for the following four empirical problems:

(i) in specificational (or inverse) copular sentences, extraction from the constituent on the right is not possible:

(1) (a) The cause of the riot was [a picture of the wall].
(b) *Which wall$_i$ was the cause of the riot [a picture of t_i]?

(ii) extraction from the subject DP of *there*-sentences is forbidden in English:

(2) (a) There were many girls in the room.
(b) *Which girls do you think there were in the room?

(iii) the subject clause of *seem* cannot appear in subject position:

(3) (a) It seems that John left.
(b) *That John left seems.

(iv) the Italian *ci*-construction presents the same diagnostic properties as unaccusative constructions, e.g. allows the extraction of the clitic *ne* from the postverbal subject:

(4) (a) Ci sono [molti ragazzi].
there are many boys
(b) Ce ne_i sono [molti t_i]
there of-them are many

The author's proposal consists of two parts: (1) in all these constructions the verb governs a Small Clause, and (2) the subject position of the matrix clause (SpecIP) may be occupied not only by raising into this position the subject of the Small Clause, but also by raising of the predicative DP of the Small Clause, so that the Specifier of I″ is not reserved for a (referential) argument (the subject), but may be occupied by a (non-referential) predicative constituent, too, provided this constituent is a DP.

So, taking the examples in (1), from the abstract structure (5a) we can derive both the predicative sentence (5b), with raising of the subject of the Small Clause, and the specificational sentence (5c) (= (1a)), with raising of its predicative DP:

(5) (a) was [$_{SC}$ [a picture of the wall] [the cause of the riot]]
(b) [a picture of the wall] was [$_{SC}$ t [the cause of the riot]]
(c) [the cause of the riot] was [$_{SC}$ [a picture of the wall] t]

According to the discussion in chapter 1, the structure (5c) directly explains the ungrammaticality of (1b). The DP *a picture of the wall* is on a left branch in the structure, so extraction from it (and extraction of the constituent itself: **Which picture do you think the cause of the riot was?*) is forbidden as a violation of locality conditions. This is contrasted with the grammaticality of the extraction from the constituent on the right in (5b), which is on a right branch: *Which riot was a picture of the wall the cause of?*.

For the examples in (2), in chapter 2 Moro argues against the analysis of *there* as an expletive and proposes an analysis where *there* is the predicate of a Small Clause which is raised into the matrix clause (the locative PP would be right-adjoined to the clause):

(6) (a) are [$_{SC}$ [many girls] there] in the room
(b) *there* are [$_{SC}$ [many girls] t] in the room

This analysis explains the impossibility of extracting the DP in (2b), because it is on a left branch. An analogous analysis is proposed for *ci*-constructions in Italian.

Notice that English *there*-sentences observe the Definiteness Effect, whilst the parallel Italian *ci*-sentences do not:

(7) (a) *There is John in the room
(b) C'è Gianni nella stanza

This phenomenon receives the following explanation in chapter 3: an existential sentence such as (6b) has the meaning 'girls are many in the room' (not 'many girls are in the room' (144)), which is obtained by raising the NP *girls* from the DP *many girls* in Logical Form. After the extraction, *many* is interpreted as the main predicate of the sentence, as in the interpretation above. The Definiteness Effect follows from the fact that only those determiners/quantifiers that can be interpreted adjectivally (e.g. *many/few/three*) qualify as possible predicates, whilst those which have no such interpretation (*every/the/most*) cannot.

The sentences in (7) cannot thus be interpreted as existential constructions, because they don't contain an appropriate quantifier; they must be examples of locative constructions: so, (7b) is a variant of *Gianni c'è nella stanza* with the subject first raised into SpecIP and then moved to a postverbal adjunct position, a possibility that exists in pro-drop languages such as Italian, but not in English. The Definiteness Effect is thus not parametrized: existential sentences are alike in English and Italian in showing it. The difference between (7a) and (7b) is a consequence of the pro-drop parameter, which rules in/out the adjunction of the subject DP in postverbal position.

Seem governs a Small Clause in one of its uses:

(8) (a) seems [$_{SC}$ John sad]
(b) John seems [$_{SC}$ t sad]

Moro generalizes this to all uses, so the sentences in (3) would have the D-structure in (9), where *it* is a pro-predicate:

(9) seems [$_{SC}$ [that John left] it]

(3a) is obtained by raising of the pro-predicate into SpecIP; (3b) is ungrammatical because *it* cannot be deleted, as it is not an expletive, but a pro-predicate. **That John left seems it* is ungrammatical too, and this example is ruled out by the principle that grammatical elements must raise in the syntax in order to be visible for the rules of interpretation. This is discussed in chapter 4.

The similarities in syntactic behaviour between Italian *ci*-constructions and unaccusative constructions may be explained if we assume that unaccusatives, too, govern a Small Clause: the predicate position is occupied by an abstract locative element or, optionally, by *ci*:

(10) (a) arrivano [$_{SC}$ [molti studenti] *pro*/ci]
(b) *pro*/ci arrivano [$_{SC}$[molti studenti] t]
there arrive many students

This analysis, as Moro shows in chapter 5, can explain all the syntactic features of the unaccusative construction, as can the traditional analysis (according to which the postverbal subject is in object position), and, in addition, has some advantages, mainly of theory internal character.

Notice that this new hypothesis that the author arrives at in a purely formal way would provide us with an explanation of a semantic fact, first noted by Paola Benincà (Benincà & Salvi 1988): the interpretation of presentational sentences always implies a deictic or an anaphoric locative, even if it is not expressed; so, (10) is interpreted as 'many students are arriving *here*' or 'at the place just referred to', and not as 'many students are arriving somewhere'. The abstract locative Moro assumes would thus be the support for this interpretation. A difficulty for this extension of the author's original view would be represented by the fact that presentational sentences with unergative verbs have the same interpretation (*Ha telefonato Piero* means 'Peter phoned me/us/the person just referred to', and not 'Peter made a call'), but there doesn't seem to be a straightforward manner of extending Moro's proposal to these sentences, too.

Andrea Moro's book is a very beautiful work: the argumentation is well constructed, the exposition is always clear, the author's background is not limited to current generative literature, but is firmly based on the linguistic (and philosophical) tradition. The place of semantics in this study is rather modest, especially in comparison to the role it generally plays in the research dedicated to copular sentences, but the book's main aim is to examine in depth the consequences of a syntactic hypothesis, so here the role of semantics is rather that of general background.

The quantity of data examined, mainly from English and Italian, is impressive and well beyond what can be reported in a review. Besides the phenomena presented above, detailed attention is devoted to the differences in agreement rules between English and Italian (1.3.3), the different syntactic behaviour between the English *there*-construction and Italian *ci*-construction (2.4), the different uses of *seem*/*sembrare* (4.4) and the similarities between *esserci* and *averci* (5.2.2). As is natural in a work that encompasses so wide a range of data, the role and the applicability of many general principles is also discussed (ECP and Subjacency (1.3.2), Opacity (1.4.2), Theta-role assignment (1.4, 5.2.1) and Burzio's Generalization (5.2.3)). Due to its centrality in argumentation, the controversial problem of the structure of Small Clauses is also examined in depth (1.3.2 and 4.4.2). The author favours the analysis of Small Clauses as adjunction structures.

Finally, the book is supplied with an insightful Appendix 'A brief history of the copula', in which Moro outlines the different approaches taken by

philosophers and linguists on the question of the status of the copula. Three main opinions are distinguished: copula as the sign of tense (stemming from Aristotle), copula as the sign of affirmation (stemming from Abelard), and copula as the sign of identity (stemming from Russell) and discusses how the problem of the identification of the GRAMMATICAL SUBJECT of copular sentences is treated in some modern syntactic theories.

I conclude this review with some minor critical remarks.

It is not clear to me why (53b) on p. 113 is assigned the structure in (11):

(11) *a chi_j sembra che $[_{IP}$ $[_{IP}$ ci_i siano $[_{SC}$ molte persone $t_i]]$ $[_{AP}$ debitrici $t_j]]$
to whom seems that there are many persons indebted

We can't assume that the AP may be adjoined to IP or, more generally, externally to the DP *molte persone*, for we don't find examples where the two constituents are not in the canonical order: *molte persone debitrici a Piero* 'many persons indebted to Piero':

(12) (a) Ci sono molte persone debitrici a Piero.
(b) *Ci sono debitrici a Piero molte persone.

Compare *Ci sono molte persone in casa* vs. *Ci sono in casa molte persone* 'There are many persons at home', where *in casa* is a real case of adjunction. We propose the analysis in (13). The impossibility of the extraction in (11) follows directly, for the AP is a modifier, presumably on a left branch.

(13) *a chi_j sembra che ci_i siano $[_{SC}$ $[_{DP}$ molte persone $[_{AP}$ debitrici $t_j]]$ $t_i]$

Moro assumes that the simultaneous presence of *ci* and a locative PP in (55) of p. 155 may be explained assuming that *mai* 'never' (which could not appear inside a Small Clause) forces the adjunct reading of the PP, thus avoiding the redundancy:

(14) Gianni non c'è mai in questo giardino.
Gianni not there is never in this garden
'Gianni is never in this garden.'

But examples such as (15) show that this cannot work. In (15) the locative PP is the main predicate and cannot thus be an adjunct in spite of the presence of *mai*.

(15) Gianni non è mai in questo giardino.
Gianni not is never in this garden
'Gianni is never in this garden.'

All the same, Moro is right in considering the locative PP in (14) an adjunct, as the obligatory presence of an intonational break between *mai* and *in questo giardino* shows. This intonational fact could have been used as an argument for the adjunct (more precisely, right dislocated) character of the locative PP in (7b), too. But facts seem a bit more intricate: (7b) may be read with two

different intonations and two different interpretations: with a break between *Gianni* and *nella stanza* the meaning is 'GIANNI is in the room'/'In the room, you can find Gianni'; without a break the meaning is 'the situation/problem/…is that Gianni is in the room'. This second interpretation is clearer in examples like *C'è Piero in difficoltà* 'The problem is that Piero is in trouble'. This interpretation seems to require a Small Clause analysis, which is not possible in the framework developed by Moro.

The fact that *seem* allows the raising of the predicative DP in the SpecIP it governs (*the cause of the riot$_i$ seems* [$_{IP}$ t_i *to be a man*]) may not be used as an argument that it allows the raising of the predicative DPs in general (174, ex. (18)), because with the verbs that allow the raising of a predicative DP, the raising is not from SpecIP, but from the predicate position of a Small Clause. So it is not clear if the ungrammaticality of **Peter is that John left* (175, ex. (22)) may prove that a clause cannot act as a predicate, for it is not even clear what such a sentence might mean.

On the basis of agreement facts, Moro classifies identificational copular sentences of the type (*io*) *sono Gianni* 'I am Gianni' along with the predicative ones (172, ex. (14a), 270, n. 55). Relevant syntactic tests are difficult to find, but the semantics of these sentences points to a dual classification (Higgins 1979): if they are used to give a name to a referent ('I am John' = 'My name is John'), they are predicative; if they are used to identify a (not yet classified) referent with a(n already classified) referent ('I am John' = 'I am/the man you see is the John you already heard of'), they have specificational interpretation (Salvi 1991: 165) and ought to be inverse copular sentences as are all specificational sentences. But notice that this difference in interpretation has no consequences for the agreement facts. If the two different interpretations of copular sentences are tied to different structures, this questions the use of agreement facts as criteria for structure.

The fact that with certain uses of Italian *ne* we have no agreement of the past participle (272, n. 67) can be explained if we take account of the fact that we have agreement only when *ne* stands for an NP (16a), but not when it stands for a PP (16b, c):

(16) (a) **Ne** ho visti [$_{DP}$ molti **np**] (cioè: ragazzi)
ne I-have seen-PL. many (i.e. boys)
'I saw many (boys).'

(b) **Ne** ho incontrato [$_{DP}$ il padre **pp**] (cioè: dei ragazzi)
ne I-have met the father (i.e. of the boys))
'I met their father (of the boys).'

(c) **Ne** ho dedotto questa
ne I-have deduced this
conclusione **pp** (cioè: da queste premesse)
conclusion (i.e. from these premises))
'I deduced this conclusion from them (from these premises).'

The difference in agreement may be reduced to a difference of category if we have at least two *ne*s (a pro-NP and a pro-PP). This solution is not very elegant, but might be supported by the fact that in many dialects only the pro-NP *ne* exists.

The idea (273, n. 79) that the grammaticality of *Beatrice è il desiderio di Dante* 'Beatrice is the desire of Dante' (*vs.* **Beatrice è la foto di Dante* 'Beatrice is the photograph of Dante') is tied to the possibility of *Desidero un desiderio impossibile* 'I desire an impossible desire' (*vs.* **Fotografo una foto impossibile* 'I photograph an impossible photograph') is undermined by the impossibility of **Temo un timore mortale* 'I fear a mortal fear' (cf. *Beatrice è il timore di Dante* 'Beatrice is the fear of Dante') and by the possibility of *Racconto un racconto divertente* 'I narrate an amusing narration' (cf. **Beatrice è il racconto di Dante* 'Beatrice is the narration of Dante').

These remarks do not affect the general evaluation of Andrea Moro's book which is without any doubt one of the best studies on sentence structure of recent years: in it many apparently unrelated facts are traced back to a unitary and illuminating explanation – an excellent achievement in the best generative tradition.

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Anna Siewierska (ed.), *Constituent order in the languages of Europe* (Empirical approaches to language typology, Eurotyp **20–1**). Berlin: Mouton de Gruyter, 1998. Pp. xvi + 829.

Reviewed by BARRY BLAKE, La Trobe University

The volume under review is one of nine deriving from the research project, 'Typology of languages in Europe' (EUROTYP) funded by the European Science Foundation. The first part of the book consists of surveys: Celtic (Tallerman), Romance (Arnaiz), Germanic (Holmberg & Rijkhoff), Slavic

(Siewierska & Uhlířová), Modern Greek (Lascaratou), Uralic (Vilkuna), Kartvelian (Testelec) and Daghestanian (Testelec). These surveys are written in the traditional framework often found in typological studies in the Greenberg tradition, so, for instance, 'indirect object' is used in a notional sense embracing prepositional phrases and noun phrases, but the surveys are all very clear and well illustrated, often with examples from languages not so commonly encountered in the literature. The surveys of Uralic and the Caucasian languages are particularly welcome additions to the accessible literature.

The second part of the book, *Parameters of word order variation*, comprises analytical articles.

Europe is not a well defined area, neither in terms of geography nor culture. As Dryer points out in 'Aspects of word order in the languages of Europe' eastern Europe is the western edge of a vast Eurasian linguistic area that runs through central Asia to Japan and also takes in south Asia. In the Eurasian linguistic area languages are consistently modifier-head, i.e. they have SOV word order and the noun is the final constituent in the noun phrase. Central and western Europe is predominantly SVO with the Celtic languages on the western periphery being VSO. Dryer puts the characteristics of European languages in a global perspective on the basis of a data base of over 700 languages drawn from all round the world. He points out that the Eurasian type has been wrongly assumed to be characteristic of SOV languages in general, but the only noun modifiers to be consistently prenominal in SOV languages are genitives (302).

Dryer also demonstrates that the VO languages of Europe illustrate a hierarchy of modifiers in terms of their position with respect to the noun (289). All the VO languages of Europe place the numeral before the noun and they all place the relative clause after the noun. Demonstratives, adjectives and genitives are ranged in between, but demonstratives follow the noun only in the Celtic languages. Dryer warns against assuming this is related to the fact that these languages are verb-initial, an assumption one might make on the basis of the Greenbergian literature. He notes that VSO languages are not more likely to place modifiers after the head than SVO languages.

In 'Order in the noun phrase' Rijkhoff begins with some salutary warnings. Nouns may not be universal; adjectives are certainly not universal, and numerals may be expressed verbally. Even if a language has nouns, it may not have true noun phrases, but use appositional sequences. Rijkhoff shows that European languages do have nouns, adjectives and numerals and they do have noun phrases. Like the other contributors to the volume he takes the traditional noun phrase to have a noun head and does not discuss the possibility that the determiner might be the head. The 'DP hypothesis' is not just a fad of the Chomskian tradition. In many languages the traditional determiner is homophonous with a third person pronoun and looks as if it might well be the head.

Rijkhoff claims that a number of principles, such as one of scope, conspire to predict that of 24 possible orderings of determiner (D), numeral (Num), adjective (A) and noun (N), only 8 will occur.

(1)	D Num A N	D Num N A	D N A Num	Num N A D
	A N Num D	Num A N D	D A N Num	N A Num D

These are the patterns consistent with the adjective being the innermost modifier and the demonstrative the outermost. Rijkhoff confirms that in Europe and globally these orders are pretty much the only ones that occur, though those in the bottom row of (1) do not occur in Europe and are rare globally. The most common order in Europe is D Num A N followed by D Num N A, as in Romance, an order that Rijkhoff's investigations suggest is not found outside Europe (357 f.).

Primus discusses 'The relative order of recipient and patient' in terms of two hierachies, one semantic and the other formal. The thematic hierarchy is proto-Agent > proto-Recipient > proto-Patient and is built on thematic dependency: a Recipient is dependent on an Agent, a Patient on a Recipient (432). The formal hierarchy is based on the morphological complexity of the marking: nominative/absolutive > accusative/ergative > dative/other oblique > adposition (436). Where the Patient and Recipient are marked the same (as in *Fred gave Jim the book*) the order is determined by the thematic hierarchy, where the recipient is in the dative, or more obviously where it is marked by an adposition, there is conflict and this is responsible for the two orders that are found. In an accusative language the hierarchies conspire to ensure that the Agent precedes the Patient and Recipient. In an ergative language there would be conflict and the theory predicts that the Patient should precede the Agent in some such languages. This was not confirmed for the ergative languages of Europe (463).

Bakker's article 'Flexibility and consistency in word order patterns in the languages of Europe' is concerned with degrees of variation from a basic order. On the basis of a sample of 86 languages Bakker reports that the Altaic and Caucasian are the least flexible families while Latin, some Slavic and Uralic languages are the most flexible. Modifier-head languages are more consistent than head-modifier ones, which is related to the fact that pre-head modifiers are more stable than post-head ones, the latter frequently having a pre-head alternative (390, 409 f.). Consistent languages tend to be inflexible and inconsistent languages flexible (410). Certain modifiers are more flexible than others; the adjective is the most flexible modifier of the noun, the demonstrative the least flexible (388).

Siewierska discusses variation in 'Variation in major constituent order: a global and a European perspective'. She confines herself to independent, declarative, finite, transitive clauses with noun arguments. She finds that the languages of eastern Europe have the greatest flexibility: the Caucasian

families, the Uralic family and especially the Balto-Slavic group of Indo-European, which exhibits all possible orders. The Celtic languages on the other hand show no variation, at least within Siewierska's parameters. She notes that it might be thought that flexibility is related to the degree of case and/or agreement marking. She reports that on the basis of a global sample of 171 languages it appears that morphological marking does not entail flexible order and that rigid order does not entail absence of marking. However, it is true that flexible order tends to be associated with case or agreement and conversely lack of case and agreement tends to be associated with rigid word order (504 ff.). Interestingly nearly half of the languages in the European sample have highly flexible word order as opposed to only 14% of those in the global sample (326).

In striking contrast to the conservative, surfacist approach of the papers reviewed up to this point the papers by Holmberg, Tallerman, Testelec and Kiss employ a Chomskian framework with a Verb Phrase (VP) inside an Inflection Phrase (IP) inside a Complementiser Phrase (CP). These authors adopt the VP-internal hypothesis in which the verb and its arguments are in the VP in underlying structure. Various movements leftward/upward are allowed and it is assumed that adverbs are outside (adjoined to) the VP and that their position is a diagnostic for movement. A constituent that appears before an adverb is taken to have moved out of the VP.

In 'Word order variation in some European SVO languages: a parametric approach', Holmberg begins by pointing out that surface order is not an unambiguous guide to structure and that sentences with a particular order may differ in structure. A subject, for instance, could be in the VP or raised out of the VP, similarly with a verb or an object. Holmberg compares English, Icelandic, Finnish and Russian and seeks to account for the different ordering possibilities they allow in terms of four parameters:

a. whether verb movement is allowed,
b. whether only the nominative (highest) argument in the VP can move to Tense (positioned within IP),
c. whether there is a focus feature in the projection immediately above VP, and
d. whether there is a focus feature in C.

Holmberg successfully captures the differences between the four languages under consideration. It remains to be seen how his theory will hold up when applied to further languages; nevertheless, he does expose the weakness of considering linear order without regard to what structure the sequence might represent.

In *Celtic word order: some theoretical issues* Tallerman points out that although the Celtic languages are generally VSO, Cornish and Breton require a preverbal constituent, which is usually the subject, and Middle

Welsh had an 'abnormal construction' in which a subject moved to the front of the clause. These P-Celtic languages are similar to Germanic V2 languages in that they have constructions requiring a constituent to be fronted, but Tallerman argues that whereas in Germanic it is mainly a matter of moving various constituents (XP) to the specifier position in CP and a finite verb to C, with verb movement blocked by the presence of an overt complementiser, in Celtic VSO occurs with complementisers. This and other evidence suggests the subject does not raise higher than IP (621).

Tallerman devotes a good deal of space to showing flaws in Ouhalla's treatment of verb-initial languages. Ouhalla (1991) considers that VSO languages have tense outside agreement and higher in the tree, whereas SVO languages have the opposite order.

(2) (a) VSO $[_{\text{TenseP}}\ [_{\text{AgrP}}\ [_{\text{VP}}\ ]]]$
(b) SVO $[_{\text{AgrP}}\ [_{\text{TenseP}}\ [_{\text{VP}}\ ]]]$

The verb moves to Tense and the subject to the specifier position in the Agreement Phrase. This yields VSO order in (2a) and SVO order in (2b). Ouhalla does not consider the Celtic languages to be true VSO languages like Arabic. He claims the VSO order arises in these languages from the subject receiving Case in its underlying position in the VP and not moving while the verb does. Tallerman shows that in Welsh the subject appears to the left of adverbs and would therefore appear to have moved (632).

Testelec begins his paper 'Word order variation in some SOV languages of Europe' with an analysis of NPs in Georgian. In that language NPs are head-final with the head taking a full range of case marking and preposed modifiers taking a restricted range. Where a modifier is postposed it takes the full range of cases. A preposed genitive, for instance, shows no agreement, but a postposed one appears to do so. Compare *davit-is mama-s* (David-GEN father-DAT) and *mama-s davit-isa-s* (father-DAT David-GEN-DAT) 'to David's father'. Testelec suggests that apparent postposed modifiers are better analysed as separate, sometimes headless, appositional NPs (652).

The main body of Testelec's paper deals with focus, mainly in Caucasian languages. He notes that where the focus falls in a relative clause or other 'island', the clause may be pied-piped into the main clause so the focus can be in the independent, finite clause.

In 'Discourse-configurationality in the languages of Europe' Kiss first makes a distinction between categorial and thetic judgements. A categorial judgement predicates about a notional subject, whereas a thetic judgement does not: *It's raining*. *A car stopped in front of the house* (683 f.). She recognises three types of language. A TOPIC-PROMINENT LANGUAGE encodes categorial judgements as primary predication structures and thetic judgements as predicate phrases. A SUBJECT-PROMINENT LANGUAGE encodes both categorial and thetic judgements as primary predication structures. In the third type neither type of judgement is expressed as a primary predication

structure (692 f.). In Europe the third type is represented by the VSO Celtic languages on the assumption that their order derives from the movement of the verb from the VP with the subject remaining in situ. Kiss claims that most if not all European languages are not subject prominent since thetic judgements do not show the non-specific subject raising from the VP. In Italian, for instance, we find *E salita sull'autobus una ragazza* 'A girl got on the bus' with the subject in the VP. Even English fails to be subject prominent by Kiss's criterion. A specific subject can precede the negative particle (*John was not born on time*), but the non-specific subject of a thetic sentence cannot (**A baby was not born*). This suggests the nonspecific subjects remain in the VP (698). Kiss points out that this throws doubt on the notion that subjects raise to receive Case. If that were so, there should not be a difference between specific and non-specific subjects. The data suggests subject raising is motivated by the requirement to create a predication structure (699).

Kiss also treats structural focus and finds that in the SOV languages of Europe and in Bulgarian and Rumanian the focus is preverbal, whereas in the SVO languages it is clause-initial (722).

Hawkins reviews his theory that ordering phenomena can be explained by processing in 'Some issues in a performance theory of word order'. This theory is concerned with the number of words that need to be processed before the constituent analysis becomes clear. The prediction is that orders will be chosen that minimize the number. Hawkins discusses, for instance, the alternative orders found in German: *Ich habe* [$_{VP}$ *der Frau* [$_{NP}$ *das Buch das sie bestellt hat*] *geliehen*] and *Ich habe* [$_{VP}$ *der Frau* [$_{NP}$ *das Buch*] *geliehen*] [$_{NP}$ *das sie bestellt hat*]. In the first version, literally, 'I have to the woman the book that she ordered lent' the structure of the VP is apparent only at the last word, *geliehen*. In the second version the structure is apparent earlier, namely at *geliehen*, but this assumed advantage is offset by the fact that there is a discontinuous NP with an extraposed relative clause: *das Buch … das sie bestellt hat*. Hawkins predicts that the greater the discontinuity the less likely extraposition is to occur. There is a threshold at which the advantages of early immediate constituent recognition for the VP are offset by the disadvantages of the discontinuous NP (738). Hawkins reviews quantified analyses of word order alternations in a variety of languages, which, in general, support his hypothesis.

The volume concludes with an appendix by Siewierska, Rijkhoff & Bakker listing values for 12 word order variables that have figured prominently in the typological literature. The list covers more than 100 languages.

This is a substantial volume and it makes a substantial contribution in terms of useful surveys, analyses based on large data bases, and analyses that go beyond considering language as linear sequence. It will prove a useful reference for years to come. The European Science Foundation money appears to have been well spent.

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Michael Tomasello (ed.), *The new psychology of language: cognitive and functional approaches to language structure*. Mahwah, NJ: Lawrence Erlbaum Associates, Publishers, 1998. Pp. xxiii + 292.

Reviewed by FREDERICK J. NEWMEYER, University of Washington

The purpose of *The new psychology of language* (*NPL*) is to inform psycholinguists and psychologists about the cluster of approaches that fall under the headings 'cognitive linguistics' and 'functional linguistics'. As the title of the volume suggests, Tomasello regards these approaches as taking 'a basically psychological approach to grammar – as opposed to the formal mathematical approach of generative grammar…' (xiii). Linguistic structures are said to derive from 'communicative function', since language evolved for purposes of communication phylogenetically and it is learned for purposes of communication ontogenetically' (xiv). In his view, cognitive-functional linguists, as opposed to generative grammarians, explain 'linguistic skills…in fundamentally the same terms as other complex cognitive skills', since in their approach 'the structures of language are taken directly from human cognition' (xx).

Tomasello's introduction, 'A Cognitive-functional perspective on language structure' (vii–xxiii), is followed by ten chapters, each written by a leading developer of some version of cognitive or functional linguistics: Ch. 1, 'Conceptualization, symbolization and grammar' by Ronald W. Langacker (1–39); Ch. 2, 'The functional approach to grammar' by T. Givón (41–66); Ch. 3, 'The structure of events and the structure of language' by William Croft (67–92); Ch. 4, 'Language and the flow of thought' by Wallace Chafe (93–111); Ch. 5, 'The semantics of English causative constructions in a universal-typological perspective' by Anna Wierzbicka (113–153); Ch. 6, 'Emergent grammar' by Paul J. Hopper (155–175); Ch. 7, 'Syntactic constructions as prototype categories' by John R. Taylor (177–202); Ch. 8, 'Patterns of experience in patterns of language' by Adele E. Goldberg (203–219); Ch. 9, 'The acquisition of WH-questions and the mechanisms of language acquisition' by Robert D. Van Valin, Jr. (221–249); and Ch. 10, 'Mental spaces, language modalities and conceptual integration' by Gilles Fauconnier (251–279).

For two reasons, I have decided against a chapter-by-chapter summary of *NPL*. First, there simply is not sufficient space in the journal to outline and evaluate each of the ten very different contributions. Second, there is very little in any of the chapters that has not been presented before by the same author. It seems best then to discuss the book from the point of view of its probable success in achieving its goal. So the question is how a psycholinguist eager to learn the ins and outs of 'the new psychology of language' would react to it.

The answer is that he or she would be confused. Tomasello promises that the contributors will develop two complementary themes: the shaping of grammatical form by communicative function and the inadequacy of models of grammar embodying an 'autonomous syntax'. But they do neither. The more puzzling absence is the former. Normally, when functionalists discuss the effects of function upon form, they attempt to demonstrate that efficient strategies of information flow in discourse have left their mark on morphosyntax. Such a demonstration typically involves arguing that the order of elements has been influenced by the natural tendency to place old information before new, that gaps and pronominal elements have an economy-based motivation and so on. But aside from Van Valin's Gricean account of island constraints and some programmatic and unexemplified remarks by Hopper, nobody does that at all.[1] The one paper that is devoted wholly to discourse, namely Chafe's, barely raises issues related to the motivation of grammatical form. I cannot imagine what our psycholinguist reader, who has at least a lay linguist's understanding of what functional linguistics is all about, would make of the absence of discussion of the shaping of grammatical form by the exigencies of discourse.

Our psycholinguist would be even more baffled by the glaring absence of parsing-based explanations of grammatical structure, which are simultaneously 'functional', 'communicative' and 'cognitive' and also likely to be familiar, as they are carried out by scholars in his or her own subfield. I am not sure if the word 'parsing' appears once in the 292-page volume. Throughout *NPL*, contributors ignore published processing accounts, appealing instead to vaguely adumbrated cognitive dispositions. For example, Taylor (188) suggests that the possibility of (1a-b) in English and the impossibility of corresponding (2a-b) in German has something to do with 'prototype theory':

[1] Croft proposes a cognitive account of subject and object positioning. Perhaps Tomasello would consider such an account 'communicative', since he uses the terms 'cognition' and 'communication' virtually interchangeably. By way of example, consider the following (to my mind, impenetrable) remark: '[I]n general it is accurate to say that the structures of language are taken directly from human cognition, and so linguistic communication, including its grammatical structure, should be studied in the same basic theoretical constructs as all other cognitive skills' (xx).

(1) (a) The hotel forbids dogs.
(b) The tent sleeps six.
(2) (a) *Das Hotel verbietet Hunde.
(b) *Das Zelt schläft sechs.

Yet there is a perfectly reasonable parsing explanation for the tendency of verb-final languages such as German to demand a stricter match-up of grammatical relations and thematic roles than one finds in verb-second languages (for discussion, see Hawkins 1995).[2]

As far as the autonomy of syntax is concerned, only Van Valin devotes more than a paragraph or two to contrasting an autonomy-assuming analysis with one that rejects autonomy. In an interesting and challenging exposition, he argues that extraction of *wh*-phrases in questions is subject to semantic and pragmatic conditions, rather than to the purely formal principle of Subjacency. The more data- and generalization-rich papers, those of Croft, Wierzbicka and Goldberg, are devoted to demonstrating subtle correlations of form and meaning. But as useful as such demonstrations are, they bear not at all on the question of whether syntax is autonomous, that is, whether grammar embodies a system of formal elements whose principles of combination make no reference to system-external factors. The autonomy question is independent of the question of how tight the 'linkage' is between form and meaning (on this point, see Moravcsik 1991 and Newmeyer 1998: ch. 3). Suppose that Langacker were right that 'lexicon, morphology and syntax form a continuum fully describable as assemblies of symbolic structures (form-meaning pairings)…' (2). Would that refute the autonomy of syntax? Not necessarily. There still might be evidence – and, indeed, I believe there to be such evidence – for the independent characterization of a formal system of purely grammatical elements.

The psycholinguist reader of *NPL* will not fail to note that in several papers one finds wording that seems highly congenial to a view of grammar embodying syntactic autonomy. Givón's paper, in particular, falls into that category. He devotes several pages (48–52) to stressing the linguistic and psychological independence of grammar from lexicon (thereby taking a position diametrically opposed to that of Langacker, Hopper and Goldberg) and points to properties of the grammatical signal that include hierarchical constituency organization and grammatical category labels. Givón does, to be sure, open his chapter with an attack on 'structuralism' (whose linguistic

[2] The lexical properties of any verb are the best predictors of the grammatical and thematic properties of that verb's complements. Therefore, if a verb follows its complements (as in verb-final languages), one grammatical role manifesting a multitude of thematic roles causes processing difficulty. German, of course, is V2 in root clauses. Hawkins notes that rigid SOV languages like Korean are even stricter than German in enforcing a one-to-one correspondence between grammatical and thematic roles.

manifestation is autonomous syntax), by claiming that it is 'a dead horse in biology … where common-sense functionalism is taken for granted like mother's milk' (42). He bolsters this claim by means of the following quote from a contemporary introductory textbook:

> … anatomy is the science that deals with the structure of the body … physiology is defined as the science of function. Anatomy and physiology have more meaning when studied together … (Crouch 1978: 9–10)

Note that we are invited to compare autonomous syntax to anatomy. But if anatomy is a science, as Crouch tells us it is, then autonomous syntax must be one too![3]

Turning to two other chapters, we find Croft observing that 'any verb can combine with any aspectual construction, although in practice, some construals are impossible to imagine' (74) and Goldberg recognizing that the semantic extension of particular structures (which she misleadingly calls 'constructions') are conventional (212–213). Such views are highly congenial to an autonomist view, in which structures are generated independently and interpreted by means of interacting semantic and pragmatic principles.[4]

Tomasello actually cites psychological evidence FOR autonomy, noting that:

> … a number of linguists and psychologists have attempted to amass empirical evidence in favor of the autonomy of syntax position … by invoking phenomena such as linguistic savants, brain-damaged individuals, children acquiring language in impoverished circumstances and the nature of pidgins and creoles. (x)

Astonishingly, he immediately lets the matter drop. None of this evidence is evaluated by him in the introduction, nor by the contributors in any of the chapters. Equally astonishing is Tomasello's willingness to accept the research results of theoretical linguists over those of psycholinguists. Along these lines, he cites Pullum (1996), which concludes that the stimulus presented to language learners is sufficiently rich that no innate component to grammar need be posited (xi). But Pullum's corpus was articles that appeared in the *Wall Street Journal*. Tomasello makes no mention of Gordon (1985), which, based on experiments with child language learners,

[3] Do generative linguists deny that syntax and semantics/pragmatics 'have more meaning when studied together'? Certainly some do. But the wealth of recent generative studies probing the interaction of these three areas (not one of which is cited in *NPL*) suggest that such is a minoritarian position. In the most recent full volume (16, from 1998) of the generative-oriented journal *Natural Language and Linguistic Theory*, two-thirds of the non-phonological contributions deal with interactions of form, meaning, and discourse.

[4] Croft regards such observations as REFUTING autonomous syntax, since he is assuming a distribution-based pre-modular version of generative grammar (90), long ago abandoned by practitioners of that approach.

concludes that level-ordering is an innate structural property of the lexicon. Of all the contributors, only Van Valin sets out to show that acquisition evidence might favor a non-innatist solution where generativists had posited an innatist one.

There are other, even more striking, instances where the rhetoric of the Introduction is not matched by the delivery of the contributors. For example, Tomasello deplores the data that are used in generative grammar, which 'are almost always disembodied sentences that analysts have made up ad hoc… rather than utterances produced by real people in real discourse situations' (xiii). Yet only two contributors (Chafe and Hopper) present segments of natural discourse, neither filling even a page of text. All of the other contributions employ the 'disembodied sentences' supposedly spurned by the cognitive-functional approach. Or consider Tomasello's assertion that 'cognitive-functional linguists [as opposed to generative linguists? – FJN] are committed to investigating empirically as many of the world's 4,000 to 8,000 languages as possible, with an eye toward establishing both universals and typological differences' (xiii). This may be true, but a reader of *NPL* would never know it. English examples predominate in every one of the chapters. Only four of the ten chapters cite any non-English data at all.

To conclude, I must say that my own personal reaction to this volume was not entirely negative. I found that each chapter presented readable sketches of the current thinking of some linguist whose ideas I mainly disagree with, but by no means disrespect. Croft, Goldberg and Van Valin, in particular, present thought-provoking semantically-based analyses of phenomena that in the past have been treated mainly syntactically. But the stated target of this book, the psycholinguist or psychologist who would like to learn more of the cognitive-functional approach, will turn the last page wondering what on earth the 'new psychology of language' might be.

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