

## REVIEWS

**Diana Archangeli & Douglas Pulleyblank**, *Grounded Phonology* (Current Studies in Linguistics 25) Cambridge, MA: MIT. Press, 1994. Pp. xiii + 502.

Reviewed by WILLIAM IDSARDI, University of Delaware

*Grounded Phonology* is a remarkable book. Archangeli and Pulleyblank (A&P) pull together facts and analyses from a wide variety of languages and synthesize a general and novel theory of phonology. Every phonologist should read this book carefully. Unfortunately, *Grounded Phonology* has not received the full attention it deserves, presumably because it was published shortly after Optimality Theory (OT; Prince & Smolensky 1993, McCarthy & Prince 1993) burst onto the scene. A&P seem to feel obligated to include a section on OT in the final chapter, but the OT material seems to be grafted onto the rest of the book. Indeed, many of the analytic positions taken earlier in the book are incompatible with standard assumptions in OT; some of these are pointed out below.

*Grounded Phonology* makes major points about practically every area of phonology, from the interface with phonetics to memorizing underlying representations. It is impossible to do justice to all of the subtle and creative arguments employed by A&P, so many of my comments are just minor quibbles about the places where I see things differently. In general this is a wonderful book, one which merits and rewards close study.

The book is divided into five chapters: an introduction ('A modular phonological theory'), a theory of features ('Combinatorial specification'), a theory of the phonetics-phonology interface ('Grounding Conditions'), a theory of rules ('Parametric rules') and a conclusion. The one disadvantage of this arrangement is that language analyses are scattered throughout the book. The breadth of theoretical topics covered is most impressive; each of the three major chapters offers bold new theories and analyses.

The introductory chapter sets the stage for the book, introducing the vowel harmony data that serves as the main empirical backbone of the book. A&P argue for a modular theory of linguistics, in which the syntax and morphology interact with the phonology, but the phonetics module talks only to phonology. The phonology itself is modular as well, with sub-theories of features and representations, phonological processes and the interface with phonetics. The distinctness of the sub-modules of phonology is highlighted by stating laws in different modules in different ways. This modular distinctness is lost in the translation to OT, where all empirical laws are stated as violable constraints. A&P certainly do show that 'significant

phonological insights are captured by focusing on the modular interaction of a small number of phonological subtheories' (14).

A&P evidently feel that the Grounding Conditions (chapter 3) are the centerpiece of the theory, for the book takes its name from these (rather than say, 'Combinatorial Phonology' or 'Parametric Phonology'). The most basic grounding condition is that F-elements are physically interpretable.

F-elements combine so as to enhance production or perception of F-elements (or both). For example, [+nasal] segments tend to be voiced because an open velum is conducive to voicing in production, and voiced nasals are more readily perceptible. But grounding conditions are not just perceptual and productive strategies, they are the grammaticalization of such phonetic tendencies inside the phonological module.

A&P are right to see the grounding conditions as the centerpiece of the book. The showpiece case, the relation between tongue root position ([ATR]) and tongue body height is robustly argued for, and the grounding conditions capture enhancing and antagonistic feature combinations. Grounding conditions also work well whether one assumes parameterized rules or OT-type constraints. However, the phonetic relationship between tongue root position and tongue body height is particularly simple phonetically. It remains to be seen whether much more of phonology can be driven by new grounding conditions.

One problem with A&P's account is that [−ATR] is ambiguous, serving both as merely the absence of the [+ATR] gesture, and also as an active gesture of its own. The tongue root is capable of two active antagonistic gestures – advancement, [+ATR] and retraction, [+RTR]. The general framework would seem better suited to an analysis with privative antagonistic [ATR] and [RTR]. Enhancement theory in general will pull privative oppositions into equipollent ones when an antagonistic gesture exists, that is an underlying o/[ATR] contrast will enhance into a [RTR]/[ATR] contrast. With privative [high] and [low] the grounding conditions are then simply \*[high RTR], \*[low ATR] and √ [high ATR], √ [low RTR]. Or, even more simply, [high] ≈ [ATR] and [low] ≈ [RTR] (also an accurate statement of the F1 perturbation effects). The privative analysis is a better formal encoding of the grounding conditions, as it eliminates [−high] and [−low], all of whose possible implication relationships A&P specifically proscribe (177). Furthermore, A&P argue that ternary power of binary features is required in Kalenjin (III, 273–274). If antagonistic gestures are involved the three-way distinction seems warranted, even necessary, but does raise the question of representations such as [ATR RTR]. Interestingly, A&P specifically allow mid vowels to be either [−high −low] or [+high +low] (279), so this question will have to be addressed whether binary or privative features are employed.

A&P do an excellent job of summarizing and formalizing standard conventions in autosegmental phonology and feature geometry. For

example, A&P's Paths formalize and justify the prevailing intuition that intervening structure is irrelevant and can be left out of rules or constraints. The core of A&P's feature theory is that F-elements may freely combine, subject only to general conditions on simplicity and recoverability.

A&P define Representational Simplicity similarly to Chomsky & Halle (1968). The value of a representation is the inverse of the number of terminal F-elements and associations to terminal F-elements (102). Smaller is better. By itself, Representational Simplicity favors Radical Underspecification, but A&P argue that Representational Simplicity is tempered by Recoverability. Recoverability requires that phonological representations and phonetic content be related (103) (where 'related' is construed broadly). A&P's Recoverability also allows the use of distinct behaviors to motivate distinct phonological representations. If two roots with (apparently) the same vowel induce different harmony patterns, then it must be due to a difference in F-elements. Different behaviors mean different representations. The net effect is that Representational Simplicity combined with Recoverability results in a variant of Contrastive Underspecification (110).

The predictions made by Representational Simplicity are diametrically opposed to those made by Lexicon Optimization (Prince & Smolensky 1993). Lexicon Optimization favors underlying representations that are as close to the surface representations as possible, whereas Representational Simplicity favors underlying representations that are as small as possible. Although this issue is not addressed directly, many of A&P's analyses can be turned into arguments for Representational Simplicity over Lexicon Optimization. A typical case is Japanese *rendaku* and Lyman's Law (325 ff.). Although Japanese sonorants are voiced phonetically, they do not block *rendaku*. Through underspecification, A&P can state the generalizations on the feature [+voiced]. OT accounts, with fuller specifications, must instead formulate constraints involving both [–son] and [+voiced]. Furthermore, as Ito & Mester (1997) point out, in Tokyo Japanese /g/ is often realized as [ŋ], always so in morpheme-internal position. But [ŋ] acts as /g/, and must be underlyingly /g/, contrary to the demands of Lexicon Optimization, but consistent with Representational Simplicity.

However, Representational Simplicity can be overdone. A&P argue that Representational Simplicity favors floating features wherever possible (112 ff.), resulting in disconnected morphemes. An alternative would be to again temper Representational Simplicity with Recoverability and limit the elimination of associations only up to the threshold of disconnection. Rather than creating morphemes with many floating features, each feature would generally have a segmental sponsor. The prevalence for left-to-right harmony systems might then be a result of greediness and laziness in constructing lexical representations. The listener would assign a phonetically spread feature to a unique segment as far to the left as possible. This will put more information toward the beginning of the morpheme, a benefit in lexical

access. The net effect of this alternative would be an alphabetic principle of lexical storage, coinciding with standard practice and with a variety of psycholinguistic results. It would also have the theoretical benefit of reducing the need for initial association rules, while not affecting the bulk of A&P's analyses.

A&P's most important analytic result in feature theory is the prohibition against gapping. Precedence considerations (the Locality Condition) absolutely ban gapped configuration (38). (This is reduced in OT to a violable constraint.) The significance of this is that it leads to new rules and analyses for harmony systems, especially the treatment of neutral elements, examined below.

A&P make an enormous contribution to the theory of phonological rules with their proposal for a general parameterized rule system (286 ff., especially 297). The parameters for rules are: Function (Insert or Delete), Type (Path or F-element), Direction (Rightward or Leftward), and Iteration (Iterative or Noniterative). Other conditions are handled by Argument and Target requirements (essentially partial structural descriptions and blocking conditions). The operation of rules is generally constrained by the Well-formedness Principle so that 'rules cannot create representations of types that are not independently motivated' (25). A&P are to be applauded for the explicitness and clarity of their proposals for parameterized rule systems.

One disadvantage of a parameterized system is that it obscures certain relations between rules. Parameterized rules do not contain a unified structural description, so it is hard to determine how to compare rules on that basis, such as with the Elsewhere Condition (Kiparsky 1973). In fact, in contrast to Pulleyblank 1986a, the Elsewhere Condition does almost no work in *Grounded Phonology*, not even meriting an index entry.

A&P restrict the operation of rules so that 'a single rule may affect no more than a single feature or node' (253). However, McCarthy (1993) argues persuasively that Eastern Massachusetts English must include a rule of /r/-insertion, and that /r/ is not the default consonant. This means that rules can at least insert a coherent chunk of phonological structure, and are not restricted to just individual F-elements. A&P use the restriction to force Okpe to be analyzed as phonologically non-neutralizing (contra Pulleyblank 1986b). Rather than phonological rules changing [+high –ATR] to [–high +ATR], A&P argue that in cases like Okpe the vowels are distinct phonologically, but nearly merged phonetically (248 ff.) (see also Labov 1994). But A&P argue both ways, against neutralization in the text (253) but for it in the footnotes, deriving the lack of a [+low +ATR] root class from neutralization and Representational Simplicity (457, n. 76). This is an issue where more phonological and phonetic research is badly needed.

The most interesting application of the parameterized rule theory is to provide new non-gapping accounts of neutral vowels in harmony systems. A&P argue that three different situations can conspire to produce similar

surface patterns of harmony with neutral elements (354 ff.). One way to have neutral elements is to skip prosodically less-prominent elements with spreading via prosodic anchors. The typical example is consonant transparency. Elements can also be rendered apparently neutral via subsequent phonetic (near-)neutralization. In this case the putatively neutral elements actually bear the spreading feature. Finally, elements can be neutral by analyzing harmony as iterative F-element insertion, interpreted to insert just paths when this is sufficient. In this case the harmonic effects are due to a 'contextual condition [which] restricts insertion to sites to the right or left of an existing token of the relevant F-element' (366), i.e. a feature doubling rule. While the various cases are carefully distinguished, the overall superfluity of devices is suspicious. Given the Well-formedness Principle, the neutralization analysis should only be available if the phonetic neutralization is incomplete or restricted to certain environments. But apparently very subtle phonetic evidence decides for the neutralization analysis in Kinande (364); in contrast Hungarian (366) must use the doubling analysis.

A&P have produced a wonderful book. The largest remaining problem is the analytic indeterminacy afforded by the theoretical devices. I have noted the multiple representational possibilities for mid vowels and the analyses of neutral element harmony. In addition A&P now contend that inserting default values is a language-particular matter (60), and there are still many ways to underspecify any given inventory. It is not clear how a learner would make the right choices. Of course, A&P are no worse than normal here, and on the plus side, A&P provide imaginative analyses which suggest many possible avenues of research into these questions. I am confident that *Grounded Phonology* will inform and enlighten phonological research for many years to come.

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**Mark C. Baker**, *The Polysynthesis Parameter* (Oxford Studies in Comparative Syntax). New York: Oxford University Press, 1996. Pp. xix + 556.

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The thesis of this book is that there is a class of languages all of which obey a single constraint that does not apply to other languages, and that this constraint is different from parameters that have been proposed previously within the Principles & Parameters framework in that its effects pervade the grammar and it 'influences the form of virtually every sentence in the language' (4). This constraint is called the Morphological Visibility Condition (henceforth MVC). Its first, informal description is 'Every argument of a head element must be related to a morpheme in the word containing that head'. Baker demonstrates the effects of the MVC primarily for Mohawk, and in so doing he presents the major facts of Mohawk grammar. But to support the claim that he is dealing with a macroparameter, he of course must consider the same structures and kinds of facts for other languages. Three of the broader effects of the MVC are polysynthesis, nonconfigurationality, and noun incorporation. So other languages which he considers and labels polysynthetic languages (henceforth PSLs) have these three characteristics.

The introduction (chapter 1) presents the major hypothesis, and the remainder of the book (Parts I–III) fleshes it out, providing support for it by discussing its implications and showing that they are valid in Mohawk and other PSLs.

A careful reading of the introduction provides much more than one expects. It not only introduces the major hypothesis; it puts it in theoretical and historical context, and even summarizes the most significant findings of the later sections. In fact, I found that rereading the introduction after having read the entire volume served the same purpose that a concluding summary chapter would have.

Part I deals with nonconfigurationality, arguing that the facts seen in languages described as nonconfigurational follow from the MVC in conjunction with the Case Filter, plus a stipulation that an agreement morpheme receives the Case that would otherwise be assigned to the argument triggering the agreement. Since agreement morphemes receive case, the NPs that would trigger agreement do not, and hence cannot be in argument position; instead they are adjuncts and show many characteristics of dislocated nominals in other languages.

Chapter 2 deals with the syntactic position of NPs. In 2.1 Baker finds evidence that such NPs are in an adjunct position, as proposed by Jelinek (1984). The evidence includes the following: NPs as object do not appear to

be c-commanded by subjects, for the subject-object asymmetry with regard to Condition C of the Binding Theory is not present; extraction from object NPs is impossible, indicating that they are not properly governed (as they would be if actually in object position rather than licensed by a *pro* in that position); absence of morphologically simple anaphors in argument positions; absence of nonreferring quantified NPs (such as *everyone*); and lack of subject-object asymmetry with regard to Weak Crossover effects. Then section 2.2 discusses the Case-theoretic reasons why the NPs must be adjuncts.

Chapter 3 goes into the details of licensing of adjunct NPs by null pronouns in argument positions, and the adjunction sites of the NPs.

Chapter 4 takes up discontinuous constituents, and shows that they are limited to two different types: quantifiers licensed as adverbs (and hence in adverbial position) but still modifying a non-adjacent nominal head, and NP modifiers that undergo *wh*-movement; included in the latter class of constructions are ‘internally-headed’ relative clauses.

Part II deals with the relationship between the empty category in argument position and the morpheme on the head that makes the argument ‘visible’, for theta-role assignment.

Chapter 5 covers verb agreement with arguments in Mohawk, which Baker feels provides a window into clause structure of PSLs. In Mohawk there are two lexically determined forms of agreement (+O and –O) with the arguments of simple single argument verbs. (This does not correlate with unaccusativity vs. unergativity of the verbs, despite the fact that several tests exist in Mohawk for unaccusativity vs. unergativity (214).) But this same difference in morpheme choice is configurationally determined otherwise, so that there is an interesting interplay between these two factors. In his treatment of verb agreement in Mohawk, Baker utilizes (and thus finds evidence for): binary branching; a higher VP ‘shell’ with null head and agent argument as specifier; and a strong version of his Uniformity of Theta Assignment Hypothesis (UTAH), in which theme, goal and agent roles are mapped universally as specifier of minimal VP, complement of minimal VP and specifier of the null-headed VP shell, respectively. He also says that ‘there is some evidence that agreement is generated only on case-bearing functional categories in the other [polysynthetic] languages, since none of them allow full three-way agreement in clauses with a ditransitive verb’ (234). (I argue against this below for Southern Tiwa.)

Baker’s findings regarding Mohawk reflexive verbs are interesting. He concludes that the reflexive morpheme in the verb ‘takes away’ the agent argument and absorbs the Case features of the verb. The remaining argument (theme or goal) is treated as a subject with regard to agreement. Mohawk verbs can index only two arguments, which Baker assumes is due to the fact

that Infl and the verb each assign one Case. If a goal argument is present, the verb agrees with it rather than a theme.

A theme that is not reflected in verb agreement must be incorporated to satisfy the MVC. If there is no nominal head of the theme to incorporate, Baker saves the MVC by positing a null ‘cognate’ object. (I can’t resist calling this ‘invisible visibility’.) He finds this less than ad hoc because some verbs have a long form, the extra portion of which can be analyzed as an overt cognate object.

I do not agree with Baker that a null noun root is no more problematic than a phonologically null agreement morpheme. It is commonly accepted that the least offensive use of null morphemes is in those situations where they are ‘the significant absence’ of any other morpheme in the same paradigm. Null third person agreement morphemes fit this description, but the null cognate object does not.

Baker says (195) that Southern Tiwa verbs show ‘impoverished’ agreement with the theme in a ditransitive construction because that theme can never be first or second person. However, this restriction is due to a more general constraint against the presence of an indirect object if the absolutive (underlying subject of an intransitive or direct object of a transitive) is first or second person (Frantz 1995). It is not grounds for ignoring the fact that ditransitive verbs agree with the full range of third person categories as direct object and forcing them into the mold of Mohawk, which happens to be a language which allows only two surface morphosyntactic arguments. The Principles & Parameters theory needs a way to deal with languages which map thematic roles onto three morphosyntactic arguments, perhaps by allowing verbs to assign Case to two arguments in such languages (though then a different reason would have to be found why goals never incorporate, since their incorporation would not be blocked by the Head Movement Constraint).

Chapter 6 looks at agreement within the NP, where Baker considers the implications of the MVC for the arguments of nouns. He proposes that the number/gender prefixes on Mohawk nouns are agreeing with the referent (R) argument of those nouns, as required by the MVC. The R role of the noun head is assigned to the specifier of NP, and the N incorporates to Det, where Agr of the NP is adjoined. This structure is said in sect. 6.1.3 to explain why PSLs do not have semantically meaningful determiners like the articles of English. It also predicts (6.2.2 and 6.2.5) that nouns cannot have other arguments because only Det can assign Case in an NP. Thus nouns should not agree with possessors, despite the fact that nouns in some PSLs are inflected for features of a possessor. Other PSLs use a relative clause strategy to indicate possessors of an NP; Baker’s solution for languages such as Mohawk is to say that heads apparently marked for features of a possessor are ‘disguised relative clauses’ with a null copular possessive verb.

I find this chapter the least convincing. The problem I see with regard to



gender inflection being agreement with the referent is the fact that gender (when not totally determined semantically) is not a feature of the referent of a noun but of the lexical item chosen to describe it. Gender marking on the noun, then, is determined by the noun itself.

Chapter 7 covers the second way that PSLs can satisfy the MVC: incorporation of an argument head into the verb. As in his previous book (Baker 1988), noun incorporation is argued to be syntactic, i.e. the result of head movement, but in this work its existence is seen to be to satisfy the MVC, thereby explaining why it is not present in languages without the MVC. This is consistent with Chomsky's more recent view of Move alpha as applying to save a sentence from violating constraints, rather than applying optionally any time so long as the output did not violate constraints. Baker demonstrates that incorporated nouns in PSLs 'can have the full range of interpretations of any other nominal' (291), including discourse referentiality. Movement constraints, in conjunction with the phrase structure configurations assumed (including some very recent innovations), limit the host of incorporation to direct objects (technically, specifiers of VP), which, by the UTAH, are always themes. Convincing arguments are given against the viability of a lexical treatment of noun incorporation in PSLs, based on agreement patterns,<sup>1</sup> and using tests based on binding theory. (Frantz (1991: 36) gives additional evidence from Southern Tiwa that the empty category in argument position in the incorporation cases is different from *pro*, in that only the latter can be replaced by an emphatic pronoun.)

The MVC cannot be the reason for most noun incorporation in Southern Tiwa, however, even in the ditransitive cases, since the MVC is satisfied by verb agreement no matter what the person, number, or gender of the argument. (There are a few verbs, including causatives of transitive verbs, which take a theme argument which does not end up as a morphosyntactic argument and so does not trigger verb agreement (called 'Adjuncts' in Frantz 1995). These themes are necessarily incorporated and their incorporation CAN be seen to be required to satisfy the MVC.)

Baker says (208) that there must be an incorporated null cognate noun in Southern Tiwa verbs such as (1) and (2), which have a *pro* as theme along with a goal:

- (1) Ka-wan-ban.  
 2S\A-come-PAST  
 'She came to you.'

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[1] Though Baker does find that for a few Mohawk verbs with incorporated nouns speakers apparently differ as to whether the N+V is viewed as a compound (formed by lexical process) or syntactic incorporation.

- (2) Ben-wia-ban.  
 2S:IS\A-give-PAST  
 'You gave her to me.'

From this he concludes that examples such as (3) and (4) are bad, not because the theme is not incorporated, but because Tiwa, unlike Mohawk, does not allow an incorporated theme (in these cases a null cognate object) to be 'doubled' by an unincorporated noun with the same function.

- (3) \*Seuanide ka-wan-ban.  
 man 2S\A-come-PAST  
 'The man came to you.'
- (4) \*'U'ude ben-'u'u-wia-ban.  
 baby 2S:IS\A-give-PAST  
 'You gave me the baby.'

But this reasoning incorrectly predicts that (5)<sup>2</sup> will be bad because it would have both an incorporated null object and the external *Donna* (unincorporated because it is a proper noun):

- (5) Donna ka-wan-ban.  
 Donna 2S\A-come-PAST  
 'Donna came to you.'

Chapter 8 deals with complex predicates, which are inflectional domains made up of more than one morpheme, each of which takes at least one nominal argument. First to be discussed are constructions which have been called 'possessor raising'. These are very limited in Mohawk, but less restricted in some other PSLs. Baker concludes that the more freely formed possessor raising cases are actually applicative constructions, albeit with disguised or null applicative derivational morphemes which license a goal argument.<sup>3</sup> He suggests that in Southern Tiwa the applicative morpheme is part of the portmanteau ditransitive prefixes, but if that is so, it is present with all Southern Tiwa verbs which take indirect objects.

Causative complex verb stems are treated next. In Mohawk, the causal predicate takes only agent and VP (as theme) arguments; consequently it combines with only unaccusative verbs because if the embedded verb had other than a theme argument, the MVC could not be satisfied. Baker concludes that the Mohawk causative morpheme is a light verb, and can combine with verbs that have an argument structure that is non-distinct from the argument structure of the causative morpheme. After verb incorporation,

[2] I do not think that any examples such as (5) have appeared in print.

[3] See Frantz (1995), where possessors on the Semantic Role dimension can be linked to indirect objects on the Grammatical Relation dimension.

the theme of the embedded unaccusative verb 'counts' as a theme of the complex predicate, by the principle that sanctions light verb constructions (355). Tanoan languages like Southern Tiwa, however, have a causative verb structure in which the matrix causative predicate takes three arguments: agent, theme and event. The subject of the embedded verb is controlled by the object (theme) of the matrix verb, so when the verbs combine agreement satisfies the MVC with regard to all arguments except an object of the embedded verb; so if there is an embedded object, its head must incorporate. This correctly predicts a fact about Tiwa of which Baker could not have been aware: if the object of the embedded verb of the causative construction is first or second person, and hence unincorporable, the construction is ungrammatical; see (6), for example:

- (6) \*Yede euwan bi-miki-'am-hi           'î.  
       those men    IS:3P-feed-CAUSE-FUT 2  
       'I will make those young men feed you.'

The last class of complex predicates discussed are those Baker calls purposive. The examples he presents from Mohawk all translate as 'go to do ...'. Because of the class of verbs this morpheme combines with (basically all but unaccusative), he concludes that it is a light verb.

Part III deals with the implications of the MVC for the non-nominal categories: adpositional phrases and embedded clauses.

Chapter 9 discusses the argument structure and syntactic behaviour of adpositions, concluding that they take one argument, generated as a complement to P. In PSLs, the argument of a P must either agree with the P or be incorporated by it; a PP can be the argument of a verb only if the P is null or incorporates into the verb (or both). Also dealt with in this chapter are applicatives. Baker finds that his 1988 analysis, in which applicatives are derived by incorporation of an adposition, is not compatible with the MVC. His new analysis posits a higher three-place predicate as the source of applicative constructions. He does find some PSLs that have applicatives with an apparent adpositional source; for these he provides an analysis in which a PP as a whole moves into the specifier of AspP.

In chapter 10 Baker concludes that embedded clauses cannot be arguments unless they are appositional to an N (e.g. Mohawk *rihw* 'matter'), else they cannot satisfy the MVC. In Mohawk this N incorporates into the verb of the complement clause, though here again Baker posits a null N for cases where there is no incorporated morpheme. Other PSLs, Nahuatl, for example, might instead have the matrix verb agree with the adpositional N, and the complement clause will have the status of an adjunct. The MVC also correctly predicts that PSLs will not have infinitive clauses.

Chapter 11 is a sophisticated discussion of the existence, nature, and origin of the PSL macroparameter.

A very helpful feature of this book is that Baker provides an introduction and conclusion for most chapters. My only complaint is to the publisher: it is that endnotes are used rather than footnotes, and they are many. It seems to me that there is no reason to put this extra burden on the reader in this day and age. I did find very few typos, and only one that caused confusion: on p. 345, in the second line of the final paragraph, the word *inalienable* is used where *alienable* was apparently intended.

To conclude, this is an important book. Every linguist should at least read chapter 1. Almost the entire book is very readable. Baker has the ability to communicate the backbone of a linguistic argument to linguists (such as I) who work in other frameworks, so that one is able to gauge how significant a hypothesis is outside of the Principles & Parameters theory, i.e. whether the details of the argumentation are artifacts of the theory or not. And while one who has worked in GB and Principles & Parameters will have a much easier time following the discussion, Baker generally explains concepts and terminology. In fact, I learned more about GB and Principles & Parameters from this book than I have from putatively introductory works. Of course, many aspects of the analysis are theory specific; in particular, Baker relies on tree configuration for all syntax and linear order. But a hypothesis must be tested within a single theory, and this is particularly true of one the effects of which are said to pervade the grammar.

The one criticism that is easy to make is the apparent ease with which Baker posits null morphemes. It is difficult to come up with counterexamples for hypotheses which can be satisfied by the presence of null morphemes. But Baker does pull together and account for a large number of characteristics of a class of languages. I would like to see him consider next languages (such as Blackfoot) which exhibit virtually all the apparent effects of the MVC except noun incorporation.

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**Edwin L. Battistella**, *The logic of markedness*. New York & Oxford: Oxford University Press, 1996. Pp. 179.

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This informative, interesting and generally reader-friendly book sets out to address two questions (18): (i) How do various accounts of markedness differ from each other – in particular, those of Roman Jakobson and Noam Chomsky? and (ii) Has a comprehensive theory of markedness ever been proposed? The answer offered to the first question is that the Jakobsonian and the Chomskian approaches share a common core in that both view markedness as both ‘an evaluation of linguistic structure and as a factor in acquisition’ (135); but the two differ on the level of detail. The answer to the second question is simple: ‘...there is no single comprehensive theory of markedness’ (135; see also 92, 124, 133).

The 115 pages that fall between asking these questions and answering them provide a detailed narrative and analysis of various concepts of markedness focussing on the two main traditions. The volume is a sequel to the author’s 1990 book (Battistella 1990). While both books survey a range of approaches to markedness, the earlier one discusses markedness in terms of its components and its relevance to diachrony and typology, whereas the present book develops the main theme as a project in intellectual history, with focus on the evolution of the markedness concept both across scholars and within the thinking of its individual developers. The earlier book draws heavily on some of the Jakobsonian ideas, but Jakobson’s views of markedness as they relate to aphasia are a new theme in the second book and so is the markedness concept as adopted in generative grammar and in particular in the Principles and Parameters approach. Familiarity with the earlier book is not presupposed for reading the second.

Apart from the introduction and conclusions, the book traces the history of markedness in four chapters: two about Roman Jakobson and his heirs and two about Noam Chomsky and his followers, with each pair of chapters about 50 pages long. The former follows a more than 50 year long evolution of the concept of markedness in Jakobson’s oeuvre, while the part on Chomsky encompasses a similar conceptual history of over 25 years. Pertinent work by several other linguists is also discussed, such as work by Edna Andrews, Derek Bickerton, Catherine Chvany, Bernard Comrie, William Croft, Wolfgang Dressler, Talmy Givón, Joseph Greenberg, John Haiman, Nina Hyams, Steven Pinker, Alan Prince, Henk van Riemsdijk, Rodney Sangster, Michael Shapiro, Cornelius van Schooneveld, Paul Smolensky, Peter Tiersma, Linda Waugh, George Zipf, and many others.

The major points Battistella makes about how the notion has developed are the following. The evolution of Jakobson’s views took him from markedness as a classificatory asymmetry in semantics to applying the notion

to grammar and phonology and finally, to reconceptualizing markedness as a 'global structural principle' (124). Chomsky's earliest attempts explored markedness as an evaluator of grammars. Later, he extended the notion as a syntactic cost metric and as a principle to define defaults and preferences in first language acquisition (124, 89). While Jakobson ended up taking a broad view according to which markedness is a property of all sign systems, for Chomsky, markedness has remained a language-theoretical concept used alternatively as part of the simplicity metric of grammars, a means to categorize structures and rules, and a construct to account for first language acquisition. The main issues, however, were the same for both: 'distinguishing the marked from the unmarked and integrating markedness into a larger theoretical context' (89).

Battistella's own concept of markedness emerges most explicitly on the last few pages of the first part of the book (56–72). Closely related to the Jakobsonian view, for Battistella, the definitional feature of the marked-unmarked opposition is that the unmarked, non-A member has 'the double meaning of both opposition to A and indefiniteness (nonsignalization)' (57). In the light of this, he analyses markedness reversal as 'the situation in which a category is sometimes characterized as A and sometimes as non-B' (58) and views neutralization as non-specification. As an example of markedness assimilation, Battistella proposes an account of the use of the two genitive markers in English ('s versus apostrophe only), according to which the bare possessive – the marked alternative – is used in marked contexts, such as with proper names, polysyllabic words and in other special instances (63–64). Two other novel and insightful applications of markedness in English by Battistella are the use of name formats in various contexts (first name only, first and last name, first, last, and middle name) (65–66); and the use of punctuation marks (66–70).

How well-supported are the answers provided for the two central questions raised in the book?

As far as the first question is concerned – the comparison of the two main markedness concepts – I found two ways in which comparison could have been more conclusive. First, it is not always clear how the competing views are different: whether they are contradictory or complementary; whether they differ in substance at all rather than just in terminology; or whether it is a matter of one linguist taking a stand on an issue while the other does not consider the issue at all. In a few cases, the most interesting of these alternatives does apply: some views do seem entirely contradictory. This is so, for example, for Chomsky's view of the independent descriptibility of competence divorced from performance and Jakobson's holistic approach to language description (129–130). But are the two views regarding the nature of linguistics – Chomsky's, according to which it is a branch of psychology, and Jakobson's, according to which it is a social science (130) – also incompatible in the same way? A consideration of the ways in which two

claims can in principle differ and a specification of the nature of the difference in the actual instances cited would have been welcome.

Second, comparisons are not carried out systematically, by reference to a set of parameters defined in advance in a theory-neutral way, along which markedness theories could differ in principle. The lists of relevant issues that are provided are in large part theory-specific. For example, here is the set of problems that are said to arise in connection with markedness in Chomsky's Universal Grammar: '(1) the nature of core grammar, (2) the existence of marked and unmarked parameters within core grammar, (3) the triggering evidence required to set parameters, and (4) the determination of markedness relations' (93). Since these issues are defined on constructs specific to generative theory, they are not directly applicable to comparing different approaches. Similarly, the five main themes noted in connection with Jakobson's work (34) do not serve as *tertium comparationis* for theory comparison, either. The first chapter does suggest a set of crucial issues in a theory-independent fashion. These are:

- What are properties of marked–unmarked pairs?
- Which properties of marked–unmarked pairs are definitional and which are correlative?
- What are pairs of terms between which markedness relations may hold?
- Are markedness relations context-dependent – i.e., reversible within any one language and across languages?
- What is the predictive power of markedness relations in language change – individual and historical?

While this is a very useful list, there is no assurance provided for its comprehensiveness and it is not made consistent use of in the comparison of the different approaches.

When it comes to addressing the other question raised in the book – is there a comprehensive theory of markedness? – such a general checklist of markedness issues would have been even more welcome. After all, in order to establish whether there exists a comprehensive theory, one needs to know what such a theory would in principle look like. In the absence of a metatheory of markedness, one cannot really be sure whether any one approach does or does not fulfil the comprehensiveness requirement.

The overall structure of the book is clear but the structure of the individual chapters is somewhat loose. Concluding sections are very useful when they are provided but they are absent in the first two chapters. Sections within chapters are not numbered and thus the reader is deprived of a useful type of 'road-marking device'. Minor errors are the designation 'German naturalists' applied to the Austrian linguists Mayerthaler, Panagl and Dressler (55); marking *The students shared John's notes with each other* as ungrammatical (110); saying S'-deletion in complement clauses of *believe* is obligatory while for *want* it is optional (106), rather than the reverse; and

leaving Henk van Riemsdijk out of the list of references and the index. Notes are in the back of book and, since they are numbered separately for each chapter and chapter numbers are not included in the running page heads, the average reader is strongly deterred from bothering to look them up.

The strongest aspects of this thought-provoking and very valuable book are the extreme richness of the sample of markedness literature that is surveyed, including both descriptive, theoretical, historical and applied work; discussing both original sources and some reviews; the concise and clear summaries of individual bits of research; the insightful syntheses of relevant work from divergent fields (for example, research on the subset principle from first and second language acquisition, the acquisition of deaf sign language, and aphasia (113–115)); and the novel application of markedness to the use of the English genitive markers, name formats, and punctuation marks.

In light of the expressed goals of the book: theory comparison and assessment, more discussion on what a comprehensive markedness theory would in principle look like would have been welcome. While this job is left to the reader, the book does provide abundant material and useful hints for readers to develop their own ideas in this regard.

The same holds for the further issue of what a larger framework might be which a theory of linguistic markedness would be a part of. Based on the extensive body of stimulating ideas that the book offers, it seems that there are three candidates for such a global theory, two of which are actually hinted at in the book. One is markedness as it holds not only within but also outside language, such as in scientific and cultural belief systems and in art (3, 11, 18, 46, 62, 64–65, 130–133; cf. also Moravcsik & Wirth 1986). The other is a general theory of categorization within which the relationship between ‘unmarked’ and ‘prototypical’ would be clarified (10, 52, 71; cf. Newmeyer (to appear), chapter 4, section 5, where it is proposed that the notion ‘unmarked’ renders ‘prototypical’ unnecessary). The third higher domain is asymmetric relations in general, not only on the paradigmatic but also on the syntagmatic level. While markedness is an asymmetric relation between two opposite, paradigmatically related categories and thus a kind of categorial asymmetry, there is a parallel asymmetry between syntagmatically related categories: between head and dependent, which once again holds both within language and outside it (Hudson 1984: 38). The study of the relationship between taxonomic and partonomic asymmetry seems to be the most global research agenda for markedness to find its place in.

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**Guglielmo Cinque**, *Italian syntax and Universal Grammar*. (Cambridge Studies in Linguistics 77). Cambridge: Cambridge University Press, 1995. Pp. 332.

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*Italian syntax and Universal Grammar* is a collection of ten essays by Guglielmo Cinque, written over a period of approximately fifteen years, from the 1980's through the first half of the 1990's. Some of the articles have not been published before, whereas others appeared in journals, for example *Linguistic Inquiry* and *Natural Language and Linguistic Theory*, and in collections. The book provides a view of Cinque's reflections on syntax, as well as of the development through the last decade of the theoretical investigation into some of the most important questions in the study of Romance languages.

The essays have been elaborated in one or another version of the Principles and Parameters theory and concern several topics, for instance the constraints on *wh*-movement and extraction, the distribution of quantifiers, the internal structure of Noun Phrases and Adjective Phrases, the conditions on the distribution and interpretation of the clitic *si*, and others. The author uses the comparative method for his analysis. The differences among languages, most often Italian, French and English, but also other Romance languages, such as Portuguese and Rumanian, as well as many Italian dialects, and Germanic ones, such as German and Scandinavian languages, are analysed in great detail and traced back to minimal parametric choices. Beside the theoretical import of his essays, which will be further considered in a moment, the empirical accuracy of his work is certainly a most valuable feature and his observations are always reliable and rich in detail. Cinque's essays constitute one of the best examples of optimal balance between the discussion of theoretical issues and empirical investigation.

Chapter 1, chapter 7 and chapter 10 can be seen as interrelated works. The first and the last chapters of the book concern the structure of NPs: the first chapter considers extraction phenomena, whereas the last one analyses the position of the head N with respect to various classes of adjectives. The

analysis of the properties of adjectives is also pursued in chapter 7, where their internal structure is investigated. Though written over a period of several years, these three articles aim at clarifying the internal structure of constituents by developing the same basic theoretical intuition. Such an intuition could be summarized as follows. The structure of NPs, as well as that of APs, is internally complex. The constituents appearing within NPs and APs realize various functions, for instance subject and object of a Noun Phrase (ch. 1), or internal or non-internal argument of an Adjective Phrase (ch. 7). The different word orders which can be observed inside the constituents are due to the intervention of syntactic rules – e.g. move- $\alpha$  – and are constrained by the properties of both Universal Grammar and the grammar of the particular language. The generalization discussed in chapter 1, ‘On extraction from NP in Italian’, is the following: it is possible to extract from within an NP all and only those phrases which can be pronominalized in a pronominal possessive form as subjects of the noun phrase. This generalization opens the question of what counts as the SUBJECT of an NP; to answer it, Cinque individuates in Italian various classes of NP. His classification is a very useful instrument even after so many years – chapter 1 was in fact originally published in the *Journal of Italian Linguistics* in 1980. Let us briefly comment on it. Nouns such as *descrizione* ‘description’ are treated as transitive nouns – i.e., they can take both a subject and an object and can undergo passivization. The subject is defined as the argument expressing the external theta-role in the corresponding clausal structure, and the object is identified on a similar basis. Therefore corresponding clausal structure, and the object is identified on a similar basis. Therefore corresponding to the clausal structure in (1);

- (1) Giorgio ha descritto i particolari dell’incidente.  
 ‘Giorgio has described the details of the accident.’ (ch. 1, ex. (22))

we have the following nominal structure:

- (2) la descrizione di Giorgio dei particolari dell’incidente (ch. 1, ex (21))  
 ‘The description of Giorgio of the details of the accident’

The subject, *Giorgio*, can be pronominalized by means of a third person possessive pronoun, appearing in pronominal position (and co-existing with the article in Romance):

- (3) la sua descrizione degli avvenimenti  
 ‘his description of the events’ (ch. 1, ex. (26b))

Moreover, the structure can undergo passivization, in the sense that the internal argument can be pronominalized and the external one can either be eliminated or expressed by means of a *by*-phrase:

- (4) la loro descrizione (da parte di Giorgio)  
 ‘their description (by Giorgio)’

Cinque identifies also other classes of nouns. Two of them are especially interesting: a group of nouns which does not admit passivization, like for instance *desiderio* 'desire', and a class where it is obligatory, exemplified by *cattura* 'capture'. The corresponding clausal structures do not exhibit any special constraint. As Cinque observes, 'In the classes of NPs we will discuss we will find in general a systematic correspondence in properties of Ns with the related Vs. Furthermore it seems that in those cases where such correspondence fails, clear subregularities are found' (24). In addition to the goal of accounting for extraction out of NPs, therefore, there is another important question which Cinque addresses here and in other chapters: How can apparent idiosyncrasies be traced back to the general principles of the theory? The answer he provides in this and in other cases is based on a fine-grained analysis of the phenomena, aimed at individuating the correct level of theoretical abstraction.

Chapter 7 addresses a very similar question starting from the internal structure of adjectives. Cinque provides evidence showing that some APs have ergative properties. The contrasts he discusses are of the following type:

- (5) Ne        sono note        solo alcune (delle sue poesie)  
          of-them are well-known only some (of his poems) (ch. 7, ex. (13a))
- (6) \*Ne        sono buoni pochi (dei suoi articoli)  
          of-them are good few (of his articles) (ch. 7, ex. (16a))

*Noto* 'well-known' admits *ne*-cliticization, a traditional test for ergativity, whereas *buono* 'good' does not. Cinque also provides other tests. However, he notes that adjectives derived from ergative verbs are NOT themselves ergative – for instance, *morto* 'dead', from *morire* 'to die', is not – and suggests a very interesting explanation. He observes that these adjectival forms are derived by the verbal ones by means of a category-changing operation, contrary to what happens, according to the lexicalist hypothesis (Chomsky 1970), with the pair verb/noun. He then proposes, following Levin & Rappaport (1986), that (only) morphological derivations which produce a change of category affect the theta-grid of the input form. Cinque argues that the correct predictions follow if the change of theta-grid is considered as a consequence of the MORPHOLOGICAL process deriving the word. This contribution is especially relevant because it shows that only an analysis simultaneously taking into account both the syntactic AND the morphological properties of linguistic structures can lead us to the correct results. In a sense, this view of the theory of grammar anticipates some aspects which would be later emphasized in more recent developments of linguistic theory, such as the Minimalist approach (Chomsky 1995).

In the same vein, in chapter 10 Cinque argues that the surface ordering of nouns and adjectives is due to the application of move- $\alpha$ , where  $\alpha$  is the head N. The main conclusion of this chapter is that the surface order of a noun phrase is due to head-to-head movement of N to c-commanding positions,

Four of the ten chapters of this book investigate the topic to which Cinque has devoted most of his attention in the past years – including in his book *Types of A'-dependencies*, published in 1990 – namely, the operator-variable relation, and how the general principles of UG establishing such relations interact with particular grammars.

In chapter 3 Cinque argues that quantified NPs, such as *which movie* and *no film*, do not enter an operator-variable configuration at LF, contrary to what happens with bare quantifiers, such as *what* or *nothing*. The contexts investigated are Clitic Left Dislocation constructions in Italian and Left-*tous* in French. The conclusion is that only bare quantifiers but not quantified NPs can bind an empty NP as a variable from an A'-position at S-structure.

(7) This job isn't remunerative enough for us to accept *e* straightaway.  
(ch. 6, ex. (1d))

(8)(a) \*Questo lavoro non è abbastanza remunerativo per accettare *e* subito.  
'This job isn't remunerative enough for us to accept *e* straightaway.'

(b) Questo lavoro non è abbastanza remunerativo per accettar **lo** subito.  
'This job isn't remunerative enough for us to accept **it** *e* straightaway.'  
(ch. 6, ex. (2d))

In contrast, no difference between Italian and English arises in other cases, as, for instance, in the following examples:

- (9) The problem is not easy to solve *e* immediately. (ch. 6, ex. (1a))  
 (10) Il problema non è facile da risolvere *e* subito. (ch. 6, ex. (2a))

The solution proposed by Cinque attributes different properties to the prepositions introducing the embedded clause: COD constructions obligatorily require reanalysis of the preposition, and such a reanalysis is impossible in Italian. Where a gap is required, on the contrary, the item introducing the clause is a real complementizer.

Chapter \* concerns pseudo-relatives – i.e., the finite complements of perception verbs in Italian, which correspond to ACC-*ing* complement clauses of such verbs in English.

- (11) Ho visto Mario che correva a tutta velocità. (ch. 8, ex. (1))  
 I saw Mario running at full speed

Cinque compares perception verbs with verbs such as *incontrare* to ‘meet’ which, though being similar to the *vedere* ‘to see’ class, also exhibit a number of differences. He notices that *vedere*, but not *incontrare*, can take a small clause complement. The solution he argues for is that the construction in (11) can be analyzed as a special kind of small clause, even if it is realized as a finite one. Especially interesting is the final section on direct vs. indirect perception, where a correlation is proposed between the availability of syntactic operations, such as passivization or cliticization, and the obligatoriness of a direct perception interpretation – i.e. a semantic property.

Finally, chapter 4 is devoted to the study of *si*-cliticization and chapter 5 is a sort of appendix to it. The phenomena analyzed concern impersonal *si* in Romance. Cinque argues in favour of the existence of two impersonal *sis* in Italian, an argumental and a non-argumental one. He observes an asymmetry in non-finite clauses – but not in finite ones – between transitive and unergative verbs on the one hand, and all other verb classes on the other. Consider, for instance, (12) and (13).

- (12) Sembra non essersi ancora scoperto il vero colpevole.  
 it seems one not to have discovered the true culprit (ch. 4, ex. (5a))  
 (13) \*Sembra essersi arrivati troppo tardi.  
 it seems one to have arrived too late (ch. 4, ex. (5c))

The proposal can be summarized as follows: [+arg] *si* requires association with an external theta-role and Nominative Case. An external theta-role is available only in transitive and unergative contexts, therefore [+arg] *si* is excluded in all other cases. [–arg] *si*, on the other hand, has the peculiar property of identifying the content of *pro* as an unspecified person pronominal, and, to this end, it must ‘amalgamate’ with personal AGR. As a consequence, [–arg] *si* is uniformly excluded in non-finite contexts.

Therefore in (12) *si* can be [+arg], but not [–arg], whereas in (13) both options are excluded. Consider also the example in (14).

- (14) Spesso **si** arriva in ritardo. (ch. 4, ex. (1c))  
 ‘Often one arrives late.’

In this case, only the [–arg] specification is available, given the lack of an external theta-role.

Cinque further observes some constraints on the interpretation of *si* in sentences with a specific time reference, with respect to generic sentences. Impersonal *si* in generic sentences retains the generic reading – paraphrasable more or less, with ‘people, one’ – only when it cooccurs with transitive and unergative verbs. In the other cases, it has an INCLUSIVE reading, meaning ‘unspecified set of people including the speaker’. To account for these phenomena he develops a theory of the ARBITRARY interpretation, based on the distinction between a ‘quasi-existential’ reading and a ‘quasi-universal’ one. This contribution constitutes an example of how semantic observations can corroborate a syntactic theory, and vice versa. This chapter is also very rich in cross-linguistic observations, because the Italian data are carefully compared with those of other Romance languages, such as Portuguese, French, Rumanian, Catalan, Spanish and northern Italian dialects. Chapter 5 is in some sense an appendix to this analysis. A *si si* sequence is disallowed in Italian. In its place, *ci si* must appear. Traditionally it has been argued, for instance by Rohlfs (1968), that *ci* is a morphological variant of impersonal *si* in a reflexive/reciprocal context. This hypothesis, however, cannot be maintained once more carefully investigated. Cinque convincingly shows that the phenomenon of *si* apparently changing to *ci* is due to the presence of a clitic ‘template’ attached to the verb, so that there cannot be more than one clitic of a kind.

To conclude these brief remarks, it could be said that this book is a very important tool both for generative linguists interested in the theoretical ideas on the topics analysed here as well as for romanists in general, given the richness and accuracy of Cinque’s investigation of Romance languages. Many generalizations and basic intuitions are in fact valid, independently of the theoretical framework in which they might have been developed.

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**Aaron L. Halpern & Arnold M. Zwicky (eds.),** *Approaching second: second position clitics and related phenomena*. Stanford: CSLI Publications, 1996. Pp. xxiii + 629.

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The eighteen papers of which this excellent volume is composed (the outgrowth of a workshop held at the Ohio State University in 1993) are united by their authors' shared interest in explaining the special properties of second-position (2P) clitics; readers will be struck, however, by the heterogeneousness of the explanations proposed here. This diversity stems both from the disparateness of the authors' theoretical interpretations of the evidence and from the strong likelihood that 2P clitics do not, in any event, constitute a unified phenomenon – that their properties aren't susceptible to a single, cross-linguistically valid explanation.

There is, to begin with, a fundamental disagreement about the theoretical status of 'second position'. Some contributors portray it as a purely syntactic notion, while others characterize it in prosodic terms. Thus, Ljiljana Progovac ('Clitics in Serbian/Croatian: Comp as the second position') argues that in Serbo-Croatian, 2P clitics are fronted syntactically and end up right-adjoined to Comp; second position is here associated with a particular node in syntactic structure. Vesna Radanović-Kocić ('The placement of Serbo-Croatian clitics: a prosodic approach'), by contrast, argues that the Serbo-Croatian clitics are positioned by a rule of prosodic structure, which places them after the first phonological phrase in the intonational phrase to which they belong; on this view, second position needn't (and in fact doesn't) correspond to any uniquely identifiable syntactic node. The papers by Mark Hale ('Deriving Wackernagel's Law: prosodic and syntactic factors determining clitic placement in the language of the Rigveda') and Hans Henrich Hock ('Who's on first? Toward a prosodic account of 2P clitics') develop similarly contrasting accounts of 2P clitics in Vedic. Though it is at odds with much recent work on clitic syntax, the radically prosodic conception of second position advocated by Radanović-Kocić and Hock accounts for a range of facts (e.g. the interaction of 2P clitics with heavy

initial constituents, appositives, parentheticals and nonrestrictive relative clauses; parallelisms between clisis and sandhi; the incidence of 2P clitics after line-initial, post-caesura, and cadence-initial hosts in poetry) for which fundamentally syntactic theories of clitic placement afford no obviously credible explanation.

Whether second position is regarded as a syntactic or a prosodic notion, the central problem posed by 2P clitics remains the same: why must they occupy this position? This is, in fact, two problems: first, why are 2P clitics positioned near the left periphery of their domain, and second, why mustn't they appear AT this periphery?

A good many explanations are proffered for the proximity of 2P clitics to domain-initial position. Patrick McConvell ('The functions of split-Wackernagel clitic systems: pronominal clitics in the Ngumpin languages (Pama-Nyungan Family, Northern Australia)') argues that in one class of languages, the placement of clitics in second position serves the discourse-pragmatic function of marking the presence of a focussed or new topic constituent in sentence-initial position; Eloise Jelinek ('Definiteness and second position clitics in Straits Salish') suggests that the raising of pronominal clitics by head movement is motivated by semantic considerations – specifically, by an LF constraint requiring definite arguments to be external to VP; Liliane Haegeman ('Object clitics in West Flemish') argues that clitic raising takes place in steps motivated by the need for feature checking, and that these steps include both A-movement of the DP headed by the clitic and head movement of the clitic itself; Chiyo Nishida ('Second position clitic pronouns in Old Spanish and Categorical Grammar') proposes a movement-free analysis in which the devices of categorial grammar – specifically, those of functional composition and type raising – allow 2P clitics simply to be generated in situ; Hock attributes the leftward movement of clitics to their accentlessness, in virtue of which they gravitate to the most prominent member of their prosodic domain and anchor themselves to it; and so on.

Just as most contributors regard the proximity of 2P clitics to the left periphery of their domain as a syntactic effect, some likewise assume that syntactic principles are what prevent such clitics from appearing domain-initially: for instance, Olga Mišeska Tomić ('The Balkan Slavic nominal clitics') argues that the Macedonian definite article clitics head their domain DP, and that movement of the following word to [Spec DP] guarantees that they will be non-initial within that domain; similarly, Josep M. Fontana ('Phonology and syntax in the interpretation of the Tobler-Mussafia Law') argues that in Old Spanish,  $I^0$ -to- $C^0$  movement places a verb into preclitic position, satisfying the clitic's prosodic need for a preceding host (though without, he maintains, being in any sense triggered by that need). But even if one appeals to syntactic principles to explain a 2P clitic's proximity to the left periphery of its domain, one might perfectly well regard its failure to



appear domain-initially as a wholly prosodic effect. Thus, several contributors appeal to Halpern's (1992) principle of prosodic inversion, by which a domain-initial clitic acquires a preceding host by being flipped past the word which follows it: Ann Taylor ('A prosodic account of clitic position in Ancient Greek'), for example, shows that in Ancient Greek NPs containing a possessive-pronoun or indefinite-determiner clitic, the clitic ordinarily occupies second position, even if the NP itself isn't immediately post-pausal; she attributes this fact to the incidence of prosodic inversion after the phonological phrase boundary which ordinarily coincides with a NP's left edge. Nevertheless, the principle of prosodic inversion is a kind of compromise between the purely syntactic and the radically prosodic conceptions of 2P clitic placement, and the need for this principle is called into question by proponents of both of these more extreme perspectives (e.g. by Progovac and Hock).

Indeed, the notion of prosodic inversion is potentially problematic. A clitic following a domain-initial constituent and a clitic following the first prosodic word of a domain-initial constituent are alike in that each follows something initial in its domain; yet, a proponent of prosodic inversion is seemingly committed to the view that such clitics are positioned by different means – by syntactic movement (or by generation *in situ*) in the former case, but by (syntactic movement plus) prosodic inversion in the latter case. Moreover, a clitic whose distribution is regulated by prosodic inversion must be assumed to be inherently enclitic. It is not clear, however, that enclisis can be seen as a general property of 2P clitics; for instance, Jindřich Toman ('A note on clitics and prosody') demonstrates that in Czech, the same clitic may be enclitic or proclitic according to the requirements of its prosodic context. Such facts suggest that the properties determining a clitic's linear positioning are in principle independent of those regulating its phonological attachment, as Klavans (1980, 1985) has argued; Susan Pintzuk ('Cliticization in Old English') shows that this perspective affords a natural account of the differences between pronominal and adverbial clitics in Old English.

Another point of disagreement relates to the problem of accounting for the sequence in which multiple clitics appear. For example, Hale argues that in Vedic, the sequence of 2P clitics reflects the nesting of functional categories in syntax; Hock, by contrast, attributes this sequence to a prosodic template. *A priori*, the postulation of a language-specific template might seem to be the least explanatory account of clitic ordering, but Steven Schäufele ('Now that we're all here, where do we sit? Phonological ordering in the Vedic clause-initial string'), pursuing Hock's assumption, argues that the form of the Vedic template reflects the grammaticization of various independent properties which clitics tend to exhibit cross-linguistically; and an explanation based on functional categories nested in a particular way is only as strong as the independent motivation for postulating those categories in the required nesting.

Although the disparateness of the analyses proposed in this volume can be partly ascribed to differences of theoretical interpretation, the vast array of evidence catalogued here leaves no doubt that, however one might choose to explain their properties, 2P clitics are, as a class, remarkably miscellaneous. For instance, besides differing with respect to their relative ordering, a language's clitics frequently differ with respect to the kinds of hosts they allow: Taylor observes that in Ancient Greek, some 2P clitics require an accented host, while others do not; pursuing a distinction proposed by Halpern & Fontana (1994), Pintzuk suggests that among Old English adverbial clitics, some require a head as their host, while others require a phrase; Schäufele raises the possibility that in Vedic, discourse-particle clitics differ from pronominal clitics in requiring their host to have a certain degree of semantic weight; and so on. (On the other hand, instances in which clitics seem to impose different requirements on their hosts can sometimes be attributed to independent syntactic factors. Dutch object clitics, for example, are superficially very different from their French counterparts – unlike the latter, they fail to invert with the verb in questions, and they license parasitic gaps; but C. Jan-Wouter Zwart ('Clitics, scrambling, and head movement in Dutch') argues that object clitics are actually alike in the two languages – that their apparent differences follow from independent facts about the syntax of Dutch and French.)

A final, important highlight of this volume is the group of papers concerning diachronic developments by which once-robust systems of 2P clisis have become restricted in usage. Andrew Garrett ('Wackernagel's Law and unaccusativity in Hittite') demonstrates that the innovative system of 2P subject clitics in Hittite has a restricted distribution, co-occurring with unaccusative verbs but not with unergative or transitive verbs; he argues that this state of affairs – unusual in Indo-European – is the outcome of a historical reinterpretation of the subject clitics as phrasal affixes. McConvell argues that the restricted discourse-pragmatic function of 2P clisis in the Ngumpin languages is the outcome of a historical competition with a different pattern of cliticization (namely cliticization to an auxiliary). Dieter Wanner ('Second position clitics in Medieval Romance') documents a gradual historical shift in the Romance languages whereby an inherited system of 2P clisis was, after a period of co-existence, virtually replaced by an innovative pattern of verb cliticization; Pilar Barbosa ('Clitic placement in European Portuguese and the position of subjects') proposes a formal explanation for the exceptional conservatism of European Portuguese with respect to this shift.

Halpern's introduction to the volume helpfully lays out the web of theoretical issues posed by the phenomenon of 2P clisis and anticipates the controversies which follow. Cumulatively, the articles in this book open up these controversies in a detailed and exhaustive way (particularly with respect to the incidence of 2P clisis in Germanic, Old Indic, Romance and

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Slavic); they cannot be said to resolve any of these controversies, but together, they set an explicit agenda for future research on clitics and make it abundantly clear why this area of inquiry is so central to the goal of understanding how morphological, syntactic and prosodic principles are articulated in natural language.

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**Alice C. Harris & Lyle Campbell**, *Historical syntax in cross-linguistic perspective* (Cambridge Studies in Linguistics 74). Cambridge: Cambridge University Press, 1995. Pp. xvii + 488.

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In this book Harris & Campbell (henceforth H&C) chart a framework for a theory of syntactic change. They set out their aims in the introduction (chapter 1), which contains an initial discussion of a range of important topics that are to be dealt with in more detail later, such as predictability and explanation. Chapter 2 reviews the major themes that over time have played a role in explanations of syntactic change. H&C show that the issues that are much to the forefront today (e.g. reanalysis, the role of language acquisition) in many cases have been the concern of scholars in the past. A special section is devoted to the more recent history of the subject. Lightfoot as an exponent of an approach using more formal syntactic theories comes in for substantial criticism. According to H&C he relies too much on Universal Grammar and fails to address functionalist matters. On the other hand, an exclusive functionalist approach often suffers from lack of rigour and excessive speculation (47).

In chapter 3 H&C sketch their theory of syntactic change, in which only three mechanisms are recognized. A separate chapter is devoted to each of

these: REANALYSIS (chapter 4), EXTENSION (chapter 5) and BORROWING (chapter 6). The next four chapters discuss at length four processes that can be observed cross-linguistically. Chapter 7 treats the simplification of biclausal structures and formulates the principle that when two clauses are fused the main verb governs the syntax of the reflex clause (193). Word order changes are found in chapter 8, where there is a long discussion of typological harmony and what this entails. Chapter 9 deals with changes in alignment (the distribution of morphological markers or of syntactic or morphological characteristics) and shows in detail how languages can change the case marking of syntactic functions. The development of complex structures in chapter 10 is, among other matters, concerned with how they arise in the first place.

The last two chapters deal with some important general issues. Chapter 11 contains an extensive discussion of explanation, causation and prediction. There are many kinds of explanation, just as there are many causes, while absolute prediction is not an appropriate aim for linguists. Syntactic reconstruction may be controversial, but chapter 12 discusses the extent to which it is possible. Phonological reconstruction is possible because phonological change is regular. Syntactic change, H&C argue, is also regular, so that we can reconstruct syntax by using the comparative method in the way we reconstruct phonology. H&C point out that the presence of morphology makes syntactic reconstruction a great deal easier and that implicational universals increase our knowledge of what possible syntactic changes are and they are therefore of great assistance in reconstruction. Although H&C do their best to put forward a convincing case for syntactic reconstruction, I do not expect that this will remove the scepticism still felt by many.

H&C sketch a theory of syntactic change with only three mechanisms (reanalysis, extension and borrowing). In addition there are general diachronic operations implemented by means of one of these three mechanisms, a set of general principles that interact with these operations, as well as a set of syntactic constructions which are always available and can appear at any time (which they call 'exploratory expressions') (51). Reanalysis is defined as changing 'the underlying structure of a syntactic pattern', without however involving 'any immediate or intrinsic modification of its surface manifestation' (61). Underlying structure is understood to include at least constituency, hierarchical structure, category labels, and grammatical relations (50). The way reanalysis is defined means that it cannot be directly observed. It may therefore be hard to detect (as H&C admit) but this definition is chosen because reanalysis and its manifestation are two distinct processes. The manifestation of a reanalysis (called 'actualization') is one of the general diachronic operations in their theory. It may involve further reanalysis, extension, or even phonological reduction. No attempt is made, however, to delimit in detail what this process exactly

involves. Reanalysis is an important source for variation as it typically introduces new options into the grammar. The old analysis will exist side by side with the new analysis, at least for a while, and sometimes it will continue to do so (e.g. perfect *have* alongside possessive *have* from which it developed) (185).

Exploratory expressions have a role to play in H&C's theory. They are defined as 'expressions which are introduced through the ordinary operation of the grammar and which "catch on" and become fixed expressions and eventually are grammaticalized' (73). Examples include the reinforcement of negation ('emphasis', e.g. French *ne...pas*), and the use of modal verbs instead of the subjunctive. Exploratory expressions should be seen as another important source of variation, on which reanalysis can work when grammaticalization takes place. Some exploratory expressions are the result of errors (73) and can hardly be seen as the result of the ordinary operation of the grammar. It could be argued that others are themselves the result of reanalysis (as in the case of a modal + infinitive replacing a subjunctive), so that it is not quite clear what the status of exploratory expressions is.

In various chapters H&C formulate general principles in the form of generalisations or constraints. An example is found in the chapter on extension (defined as a change in the surface manifestation of a syntactic pattern (97), while surface manifestation includes morphological marking and word order). Various cases of extension are discussed and H&C note that it is systematic. They (informally) formulate the following constraint: extension is the removal of a condition from a rule. At first sight this seems to cover only rules with conditions, but H&C make it clear that, for example, lexical diffusion is also subject to the same constraint. In lexical diffusion each new word that can undergo a particular rule is one more exception removed, and this can be formulated in such a way that a condition is removed from the rule. I am not convinced that this constraint and its formulation provides a great deal of insight, as the term 'extension' implies that a rule applies in more cases than before.

Borrowing is the third mechanism of syntactic change. H&C assign an important role to it and show that there are really no absolute constraints on borrowing (given sufficient time and contact anything can in the end be borrowed), though there are general tendencies. Borrowing syntactic structure is easier when it fits into the structure of the language, but it can also take place when this is not the case, which can result in the introduction of structures that are not harmonious with the existing structures in a language. The new construction(s) can then be the source of extension and ultimately this can result in a language changing from one typology to another.

Throughout the book practically all the issues that play a role in explanations of syntactic change are reviewed and discussed. Case studies are taken from a wide range of languages, not only from those that are widely

known and well studied (e.g. English, French, German), but also from languages that are unfamiliar to many (e.g. Georgian, Mingrelian, Laz). H&C can therefore show that there are often similar developments in many (often non-related) languages and formulate general principles. Inevitably the changes illustrated are only briefly presented with just sufficient detail to see what is going on. This gives the impression that syntactic changes are not really complex and fairly clear cut. But are they? The experience of many scholars is that the more details become available the more complex things turn out to be. H&C are aware of this and dismiss criticism by pointing out that indeed the history of some languages such as English and Spanish appear to be messy, but that this is due at least partly to contact and dialect borrowing. Neither of these is discussed further, and may in any case be true only for part of their histories. It is obviously impossible for any reader to judge whether the details given are sufficient and comprehensive, but it seems unlikely that all of H&C's interpretations will be accepted. I will discuss one change in depth where the consideration of additional detail will lead to a different interpretation.

It is generally accepted that English underwent a change from SOV to SVO. Exactly how this came about remains controversial, however. I want to take issue with the way H&C characterize the change. In chapter 8 (on word order), two operations affecting word order are discussed (220), one of which they call 'reordering head and dependent to adjacency' (constituents that are not adjacent become adjacent). H&C see constructions in English with auxiliaries (e.g. the progressive) and modals as the result of clause fusion. As a result the verb of the main clause was reanalysed as an auxiliary and was now in second position in main clauses (Wackernagel's Law) with the content verb in final position. By extension this order also appeared in subordinate clauses. Part of the actualisation of clause fusion, according to H&C, was the reordering to adjacency in both types of clauses, so that the verbal complex was placed in the position occupied by the auxiliary (the head), that is, in second position. It is not immediately obvious why this reordering should take place and why in the case of English this leads to a verbal cluster with the finite verb preceding the non-finite verb and not the other way round. From the discussion in H&C it is clear that they regard reordering of constituents when they become adjacent as a distinct possibility. Hock & Joseph (1996: 207 ff.) discuss this word order change in similar terms. In this interpretation it is a coincidence that there are no longer any Vv verbal clusters in English with the finite verb (= v) following the non-finite verb (= V). The syntactic patterns in Old English do not support H&C's interpretation. Main clauses frequently show the results of Wackernagel's Law and have the finite verb in second position (personal pronouns do not 'count') and there are thus frequent VO patterns. In subordinate clauses this is somewhat less frequent but nevertheless found from the earliest texts onwards. In both types of clauses we can find vV

clusters followed by clause elements such as objects, similar to those in Modern English, but there is an important difference. All clause elements can precede the non-finite content verb, but not all of them can actually follow (as they can in Modern English). Only objects, complements, and PPs can regularly do this. Pintzuk (1991) has shown that particles cannot normally follow a non-finite verb in any type of clause. The same goes for personal pronoun objects, stranded prepositions and certain adverbs. Only in late Old English texts are there a few examples of the modern word order with particles and the like. This distribution is unexpected if reordering to adjacency was a factor in the word order change. If it were we would have to say that reordering to adjacency was prevented if it resulted in structures with particles after the non-finite verb. There is not much explanatory power in reordering to adjacency if we have to add complex exceptions to it. It should also be noted that objects, complements and PPs can also follow a Vv cluster (see Koopman 1992 for details), so that their position cannot be due to reordering to adjacency. Vv syntax is typically associated with OV languages. It is only after Old English was reanalysed as VO that we find particles, personal pronouns, stranded prepositions freely after the verbal cluster. Reanalysis also explains why there are no longer Vv clusters in English.

Modern Dutch does not show reordering to adjacency. Middle Dutch, however, had syntactic patterns similar to those of Old English, with vOV patterns in main and subordinate clauses. These were subsequently eliminated from the language (Burridge 1993), so that Modern Dutch only has subordinate clauses with verbs in final position. In H&C's terms, this could be interpreted as reordering to adjacency to the position occupied by the dependent (the content verb in final position) rather than to the position occupied by the head (the auxiliary in second position). If so, this is a counterexample to the proposed universal that 'discontinuous constituents that are reordered to be adjacent occupy the position held by the grammatical head' (224).

Occasionally a printing error has eluded the proof-reading, but the book is well produced. H&C are not always accurate in their references, particularly where they rely on the work of others: the references to the quotations on p. 221 are incorrect: (32) should be *Orosius* 134.14 and (33) comes from a different text (*Boethius* 18.7).

In conclusion, H&C have written a book that deals with practically every aspect of syntactic change, and brings together a mass of detail from widely different languages. The supporting studies from many languages show clearly that certain syntactic changes are common and can be generalized as such. In the process H&C isolate constraints and principles, of which many are defined only informally and are sometimes no more than (strong) tendencies. It is a pity that these are not collected in an appendix. A particularly interesting feature of the book is the attempt to find answers to

frequently asked questions about the directionality of syntactic change. H&C show for example how a language can change its typology and how complex constructions can arise. The many thought-provoking discussions make the book worthwhile in itself. Adherents to more formal linguistic theories may find the absence of a specific theory disturbing, especially in the chapter on word order where H&C assume that constituents are positioned by positional rules, often on the basis of superficial syntax, but it would be unwise to ignore what this book has to offer. It must be welcomed as a major contribution to the field and should be compulsory reading for everyone interested in syntactic change.

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**Roy Harris**, *The language connection: philosophy and linguistics*. Bristol: Thoemmes Press, 1996. Pp. xix + 193. *Signs, language and communication: integrational and segregational approaches*. London: Routledge, 1996. Pp. xii + 279.

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In *The language connection*, Harris suggests that language connects philosophers and linguists not only because of their common focus on words, but also because of their mutual dependence on being able to use language itself to discuss language. Harris sees this reflexivity as problematic, and for him the failure of the majority of linguists and philosophers to identify it as such is evidence that 'the language connection is not just a point of contact but a shared vested interest' (xiv). He is uneasy about obviously metalinguistic terminology such as the 'type/token' and 'use/mention' distinctions, but also about any discussion of 'words', 'sentences' and



‘grammaticality’. He labels such attempts ‘segregational’. They all presuppose that a language exists independently of any use; for the segregationalist, language as system and language as behaviour by speaker are two different domains of enquiry.

In order to illustrate what seems to amount to a conspiracy theory covering more than two thousand years, Harris embarks in *The language connection* on an historical survey of philosophical and linguistic thought. Developed over eight short, lucid chapters, this survey takes the reader from the ancient Greek distinctions of ‘parts of speech’, through traditional grammar, structuralist linguistics, logical positivism and ordinary language philosophy to transformational grammar. He claims that throughout the history of both philosophy and linguistics, speech has been ‘treated as yielding a product that could be analysed without reference to the producer or the occasion of production’ (33); linguists and philosophers are accused of abstracting away from usage to discover units such as ‘subject’ and ‘noun’, discarding all that cannot be fitted into such categories. Harris contends that all such accounts depend on some notion of language as an autonomous, identifiable, code.

To challenge assumptions which, as Harris observes, underlie both modern linguistics and modern philosophy, is a courageous and engaging undertaking. But it is rather disappointing that, even in the final chapter, Harris does not offer a viable alternative. He appeals for a more detailed understanding of the ‘pattern of relationships that include our own role – as speakers and hearers, writers and readers – in the communicational process’ (164), but does not explain how this would enable the linguist to dispense with any segregationalist account. Harris suggests that, when he repeats what he has heard someone say, by uttering ‘Peter said “John has pneumonia”’, he is not producing different tokens of the same type as were used, but is ‘simply recounting an episode of my OWN linguistic experience, i.e. telling you what I understood Peter as saying when I heard him speak’ (159). But this begs the question of how he is to explain his recognition of what was heard AS an episode of linguistic experience rather than, say, as Peter sneezing or humming.

In the preface to *The language connection*, Ray Monk describes it as being ‘at one and the same time, an original contribution to current debates, AND a stimulating introduction to its subject, readily intelligible to non-experts’ (vii). Harris’s readiness to question even the most fundamental of premises is certainly an interesting and provocative contribution. For instance, he refers to Chinese and African philosophical traditions to illustrate ‘the extent to which assumptions concerning words and reasoning are culture-bound’ (52). However, Monk’s claim that this book would be accessible as an introduction to either linguistics or philosophy is less convincing. Harris’s coverage of the ideas of individual thinkers is irregular. He often refers to ideas as having been advocated by unnamed ‘philosophers’ or ‘theorists’,

but devotes twenty pages of this short book to an exposition and critique of one paper by A. J. Ayer. And is difficult to imagine what a non-expert would make of the attack which Harris launches on Chomsky, whom he mentions only twice by name, on each occasion castigating him for ignoring the communicative function of language. In his postscript he suggests that, becoming aware during the 1960's that they lacked an adequate account of linguistic knowledge, 'philosophers turned with relief to a new light shining from M.I.T. as towards a star of Bethlehem... It must have been a disappointment when they got there to discover that the Messiah's message was a retelling in metalinguistic terminology of the old, old story about the parts of speech and the sentence' (179). This overblown metaphor seems to serve little purpose other than to allow Harris to rehearse an old enmity; it certainly adds nothing to the book's credentials as an introductory work.

As suggested, Harris's 'non-segregationalist' account is disappointingly sketchy. In *Signs, language and communication*, however, he does describe, or at least suggest a name for, such an alternative. He advocates an 'integrational approach', in which signs 'are not waiting to be 'used': they are created in and by the act of communication' (7). His account of signs is an ambitious one, encompassing forms of communication as diverse as language, music and the visual arts. His targets are again the academic disciplines of linguistics and philosophy, which have ensured that 'all the arts of communication become alien objects of enquiry' (ix). Harris instead appeals to a 'common sense' approach; unlike such theorists, we should not lose sight of the interactive, situated nature of all communicative events.

Harris's pursuit of this ideal is here organised by theme, rather than by history. In the second chapter, for instance, he considers language acquisition, and the origins of language. Both, he argues, can be seen as natural results of the 'communicational infrastructure' (24) which human beings have developed and on which human society depends. Harris is particularly suspicious of any account based on the pre-existence of innate signs, such as Fodor's Mentalese, which he sees as motivated purely by the need to maintain a segregationalist account of language. In the following chapter he reveals similar uneasiness with theories of communication based on choices between alternatives, such as Hallidayan systemic linguistics. Such accounts depend on the concept of language as a pre-existing system which defines these alternatives before any utterance is produced.

Much of the book is taken up by Harris's rejection of linguistic signs as a specific, identifiable class. He claims that a sign is significant if it is interpreted as such, whether it be a plume of smoke rising on a hill, a speaker's utterance, or a dog's bark. This refusal to distinguish between natural and conventional signs leads him to reject any definition of communication as being dependent on the intention, and the recognition of that intention, to communicate. For the integrationalist, signs are significant to the extent that they are meaningful to an individual in the context in which

he or she encounters them. Harris offers an account in which the audience is 'responding to a communicative initiative' (58), an initiative which can be taken without any intention to do so on the part of the communicator.

In his chapter on 'Communication and signs', Harris returns to the theme of *The language connection*: the problem of using language to discuss language and the vested interests of the academics who perpetuate this practice. This time he implicates all of Western education, and the grammarians and lexicographers who feed it. Grammars and dictionaries are 'the prime example of attempts to codify linguistic behaviour' (103), attempts which Harris links to the maintenance of the notion that the individual values of words are relatively stable over 'generations'. For Harris, this is embodied in the physical form of a printed dictionary, with what he sees as its claims to present the available stock of words with their correct forms and meanings. This criticism, which may have held some weight twenty or even ten years ago, is surely less compelling now that dictionaries are increasingly developed from the collection and analysis of large corpora of language use. Such recent developments also call into question his claim (103) that 'it is in the professional interests of the lexicographer' to present a picture of vocabulary as relatively stable. Many lexicographers depend for their livelihood on the demand for dictionaries constantly updated in line with changing usage.

A rather more successful commentary on change, and on the concept of words as fixed signs, appears in the following chapter, 'Sign and signification'. Harris argues that metaphors relating words to currency, which he suggests have been widely used, cannot unproblematically be taken as arguing that words are arbitrary signs as coins are, nowadays, arbitrary tokens. Harris shows that the precise significance of this metaphor depends on the time in which it is produced and that, for instance, in Francis Bacon's time when coins had the same worth in bullion as their face value, coins were far from arbitrary in their significance.

After this engaging analysis, Harris returns to his sustained criticism of previous linguistic accounts. In Chapter 10, in which he addresses more specifically the relevance of context to the value of a sign, he confronts the notion of the ideal speaker/hearer. For Harris it is not appropriate, or indeed possible, to identify certain parts of a communicative initiative as performance errors, and to dismiss them from consideration. As in his treatment of lexicographers, Harris seems overly eager to identify prescriptivists, suggesting that modern linguistic theories, such as transformational grammar, define a 'fixed code as an ideal towards which our imperfect system of human communication should aspire' (146). But to suggest that there is a formal system underlying language use which can be identified and described is not to suggest that language use is in any way 'inferior' to this system. Nor is it to claim, as Harris appears to believe, that such a code is, or ought to be, sufficient to explain communication. To

propose a model of communication in which exclusively linguistic factors can be isolated and scrutinized is not to suggest that other factors of context and interaction are irrelevant. Harris describes a class of 'weak segregationalists' who 'have come to the conclusion that code alone is not sufficient to explain communication' (147). This is not the challenge to theoretical linguistics which Harris seems to see it as: formal grammar of the type Harris is criticising has never claimed to be able fully to describe actual linguistic interaction. Harris suggests that for the integrationalist no such problems arise because communication is seen as a process of the constant creation of signs; their signification is dependent entirely on context.

In the chapter on 'Communication and change' Harris addresses what he sees as a further threat to the code model of language. If language can change there is no guarantee that at any point a form of words will mean the same for speaker and hearer; an account based on a shared code will founder. He considers Saussure's account of language as a series of discrete but internally unchanging 'states', explaining that for the integrationalist these are unnecessary abstractions. The problem of linguistic change raises, for Harris, the problem of defining time. Here Harris's 'common sense' approach is particularly apparent. Answering a hypothetical challenge to come up with an integrationalist's definition of time, Harris offers: 'Time is what has elapsed since you started reading this page' (209). This adds little to our understanding, especially since the word 'elapse' is itself dependent on a concept of time for its definition. The example of language change which Harris considers, and which he discusses in great detail, is the change of brand name for a make of toothpaste from 'Darkie' to 'Darlie'. Such an occurrence is atypical of language change in being both easily dated and clearly motivated.

In the final chapter, Harris advances in support of an integrationalist account the fact that 'There is no way any human being in this world can secure semantic assurance that *two plus two equals four* – or any other form of words – formulates a truth that must always hold' (253). This is surely a fact about the world which has little to tell us about language. It would only jeopardize our ability to state what the form of words in question means if segregationalist accounts of meaning were necessarily concerned with mapping words directly onto truth values. Harris's assault on segregationalist theories is not well served by such implied but unexplained criticisms.

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**John Honey.** *Language is power: the story of standard English and its enemies.* London & Boston: Faber & Faber, 1997. Pp. x + 298.

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If we take John Honey at his word, he means well. He proclaims throughout this book that he is primarily interested in opening windows of opportunity to all by insisting on a clear standard for the English language. Through a language academy those standards will be open and public and all who meet them can be sure of being understood and respected for their language use. His position is weakened, however, by his inability to define 'standard English' in a consistent manner, by his unwillingness to recognize that his standard has a very definite class origin, by his belief in the inherent qualities of the English language and by his misrepresentation of work both for and against his position.

The book is divided into ten chapters. The introduction takes an initial stab at defining a standard language. The second, 'The language myth', attacks the 'linguistic equality' theory – Honey would call it a myth – which states that all languages are equal. The third, 'The dialect trap', is devoted to showing why knowledge of the standard language is necessary. The fourth, 'Some enemies of standard English', is divided between an analysis of Pinker's *The language instinct* in the light of the 'linguistic equality theory', and dismissal of a number of scholars' contentions that standard English is class-based. The fifth, 'Rewriting history', continues this line of attack, aiming specifically at the work of Tony Crowley and Raymond Williams. Honey objects primarily to the 19th-century date of origin of standard English (which he mistakenly imputes to these authors), and pushes the date back to the 15th century. Chapter 6, 'Authority in language: anagogy and Prescription', is primarily an attack on the work of the Milroys, followed by praise for the social effects of following established norms, and a call for the creation of a linguistic governing body for the English language. Chapter 7, 'Safeguarding English', provides a list of the types of errors that Honey would like to see eliminated through such a mechanism. Chapter 8, 'Language in school: the lost generation', traces the purported effects of linguistic liberalism in the classroom, and the evolution of the National Curriculum in English. Chapter 9, 'The language trap debate', claims that linguistic dogmatism shaped the critical reception of Honey's previous book on this topic. Chapter 10, 'A national and international language', proclaims the many benefits of English, and specifically British English, as an international language, and attacks recent books which criticize the role of the English language in cultural imperialism.

One fundamental problem for Honey is that he cannot define 'standard language'. His first attempt is to state that it is the written form of English

‘used in books and newspapers all over the world’. Thus it would be a learned form of the language, used for context-free communication, among people of a certain level of education. The standard written language ‘is a special form representing a superimposition upon natural language, one which develops its own structures, vocabulary, styles, qualities and functions, all of which need to be specially learnt’ (48–49). However, shortly afterwards Honey argues that the standard language does not require special training:

But you did not even need formal schooling to have access to standard English. Indeed you did not even have to possess any degree of literacy: a great deal of traditional lore, in the form of ballad and narrative in standard English, was handed down orally to the children and grandchildren of the labouring poor, so that it was possible for an almost illiterate nineteenth-century father, a merchant seaman, to tell ‘wonderful stories in choice English, never using a word of dialect.’ (85)

This view contradicts Honey’s earlier statements about the written and formally learned nature of the standard language. Still later he concludes that pronunciation, previously omitted by his definition of the standard as a written language, is important. He devotes a number of pages to Received Pronunciation, and, commenting on the ‘transcendental’ benefits of the standard language, he notes that in 1750 the miners of Kingswood in Gloucestershire were ‘notorious for their barbarous and savage behaviour’ and ‘their language was described as “the roughest and rudest in the nation”’. However, after schools and churches were established by Methodist pioneers, they were ‘much more civilised and improved in principles, morals, AND PRONUNCIATION’ [Honey’s emphasis] (136).

Reliance on this type of evidence might seem strange – Honey also cites the Earl of Gowrie, Thatcherite ministers and Prince Charles – but it reflects his belief that non-linguists have a better grasp of these issues than do professional linguists. This is not a coincidence, Honey would claim, for linguists have all been brainwashed to accept the ‘linguistic equality theory’, ‘the notion that that [sic] all languages, and all dialects of any language, are equally good’ (5). From a variety of sources Honey pieces together five propositions which he claims ‘represent the consensus of opinion ... about the nature of “linguistic equality”’ (8). These are:

- (a) there is no valid basis for the COMPARISON of languages or dialects;
- (b) no language or dialect is more DEVELOPED than another, nor more EFFICIENT or EXPRESSIVE;
- (c) no intellectual HANDICAPS can attach to speakers in virtue of speaking one language or dialect rather than another;
- (d) all languages and dialects are perfectly adapted to the PRESENT needs of all their speakers, and have the power to adapt virtually instantaneously in order to meet NEW needs.

- (e) Those who argue otherwise are reprehensible/harmful/ridiculous, even if their arguments are supported by EVIDENCE. (8–9) [emphasis Honey's throughout]

Considering these one at a time, this reviewer concludes that each is correct. The criteria for comparison suggested by Honey include the size of vocabulary, the capacity for abstraction, the use of passive voice, a wide range of verb tenses, and subordinating conjunctions. However, vocabulary is infinitely and easily expandable, as has been demonstrated repeatedly by many languages (including English as it replaced Latin and French as the language of learning and the law in the 15th through the 17th centuries). The capacity for abstraction is certainly not limited to English, or to any variant of English. Moreover, Honey seems to reject abstraction when he rails against a more abstract use of the verb 'decimate' (153–155).

The rejection of 'developed' vs. 'undeveloped' in the classification of languages counteracts the organic vision of linguistic development, popular in the first half of the 19th century, in which languages that used more monosyllables were somehow viewed as more primitive, and at an earlier stage of development, than languages with inflected forms. The lie to this system was (a) that Chinese had a cultural history at least as rich in literary production (the main criteria used by the linguists of this school) as the European inflected languages and (b) the most advanced languages in this rating system would be the agglutinating languages like Turkish, Hungarian and many native American languages. The unwillingness of Eurocentric linguists to accept a second-class rating with respect to those languages led to the abandonment of that theory.

'Efficiency' and 'expressivity' require an explanation of 'efficient for what', 'expressive of what'. For Honey, the measuring stick is always the ability to express Western European/North American culture. Other languages have to use circumlocutions to express these notions, and thus are deemed less 'efficient' (although Einstein has been translated into Wolof, precisely to counteract this type of generalization). However, if non-Western languages express ideas that Western European languages can express only with circumlocution, then they are guilty of 'specificational overload' (10–13).

In the realm of 'intellectual handicaps', one must ask what type of intellectual handicap Honey has in mind. If he means real intellectual ability, then he is clearly wrong; if he means ability to express the ideas important to the speakers of a particular linguistic tradition, then it is obvious (but has no place in science).

Concerning the adaptation of languages to present and future needs, I would quibble with the use of the term 'perfectly': no language is capable of expressing all that its speakers desire to express; this is one reason language is always changing. 'Instantaneously' is also an odd word to use with respect

to languages. Let's just say in a generation or two. There is no better example than the language he most wants to defend. English and French were widely judged incapable of being languages of educated discourse in the 15th century, but had become capable of this, through 'instantaneous' change, by the end of the 16th century.

The characterization of positions contrary to the linguistic equality theory as reprehensible/harmful/ridiculous might seem 'unscientific', but the fact is that the defence of standard English as inherently superior goes hand-in-hand with race, class, and ethnic prejudice. Consider the history of the organization 'U.S. English', which promotes the 'defence of the English language' in the United States. John Tanton, its first president, was forced to resign in 1986 when an internal memo of his attacked Hispanic Americans, wondering if Catholic immigrants with large families would take control in the numbers game of a democratic society:

Can *homo contraceptivus* compete with *homo progenitiva* [sic] if borders aren't controlled? ... Perhaps this is the first instance in which those with their pants up are going to get caught by those with their pants down ... As Whites see their power and control over their lives declining, will they simply go quietly into the night? Or will there be an explosion? (Cited in Crawford 1992: 172–173.)

If Honey's ideas are labelled 'reprehensible', it is because the theories lend themselves so easily to reprehensible political movements. The current president of US English, Mauro Mujica, is reported to have advised the Slovakian government as it prepared the harshest language laws in Europe, laws which have earned the universal condemnation of human rights groups (Kontra, 1998). Larry Pratt, the founder of English First, another 'defence of the English language' group in the US, was forced to resign from presidential candidate Pat Buchanan's campaign staff after it was revealed that he had been speaking to Neo-Nazi groups (Crawford, 1998). The equation of supposedly 'inherent' qualities of standard English with social behavior, as demonstrated by Honey in the case of the Kingswood miners, is used by demagogues to justify class/ethnic/racial discrimination. This is why the opponents of the 'linguistic equality theory' are not only wrong, but reprehensible. This is not necessarily a condemnation of Honey, for if he understood the linguistic equality theory, he might well drop his opposition.

Finally, Prof. Honey's greatest weakness is his distortion or out and out misrepresentation of the points of view of those he criticizes. For example, Honey claims that groups in the US were 'promoting the school use of the form of Black English labelled "Ebonics" ... in place of standard English' (26). In fact, as in the 1996 Ebonics controversy in Oakland, California, the emphasis was always on helping teachers understand the nature of African-American Vernacular English, so that they could help students make the transition from that variety of English to 'standard' English. Most of the



linguists he castigates as ‘enemies of standard English’ are nothing of the sort. They do not confuse, as Honey does, the potential of a language to express anything with the current expressive needs of its speakers. For the purposes of the scientific study of language, all languages, all dialects, all idiolects, are of equal interest.

The most blatant example of Honey’s proclivity for misrepresentation is the example drawn from Hollingworth 1977, in which he discusses a controversy in 1890 relating to the use of dialect in teaching in the schools of Rochdale. Honey’s version is as follows:

In Rochdale, an interesting preservationist inspector of the schools (HMI Mr Wylie) caused controversy in the local press in 1890 by his attempts to foster the use of local dialect in school. The response of some parents is illuminating: ‘Keep the old Lancashire dialect out of the schools, Mr Wylie, for I want my children to talk smart when they’re grown up.’ (100)

The source, however reads quite differently. The letter referred to is not from a parent but rather from John Trafford Clegg, who used these letters to launch his career as a dialect writer. His two letters on the subject were written in dialect:

Keep th’owd Lanky eawt o’th’schoo’s, Mesther Wylie, for aw want my childher to talk smart when they grown up. (Letter to the *Rochdale Observer*, March 15, 1890; cited in Hollingworth 1977; the full text of his letters is available in Clegg (1898: xviii–xxiv).)

Clegg’s real objection was that the life of the dialect would be lost if reduced to the stultifying form of grammatical analysis employed in the instruction of literary works from the canon. In a second letter he notes

...if yo’r begun’ to thrim it deawn to rules an’ teych it ‘systematically’ yo’ll just get a tuthrie roughseaundin’ words an’ sayins’, an’ yo’ll find ‘at o’ the flavour an’ beauty an’ power’s flown away (Letter to the *Rochdale Observer*, March 29, 1890; cited in Hollingworth *ibid.*)

Clegg’s objection to Wylie’s plan is based in part on a misunderstanding of what Wylie proposed, and in part on fear that scholarship would destroy or at least alter the regional language. The use of dialect in these letters lends at the very least an element of irony to the anti-dialect stance expressed by the author. By changing the language of the text and misidentifying its author Honey has seriously misrepresented its nature and its importance.

The point is important, for it supports Honey’s rejection of transitional use of dialect in the schools, a perspective that has robbed schools of a potentially valuable resource for aiding students to learn the standard language. All the research evidence has shown more rapid attainment of literacy, in both the non-standard and the standard language, through the use of the dialect readers (see Rickford 1997: 178–180). In the 1970s

Houghton-Mifflin experimented with dialect readers for native speakers of African-American Vernacular English, and found that the students gained, on average, 6.2 months in measured reading comprehension skills over a 4 month period (using the Iowa Test of Basic Skills in Reading Comprehension).

If Prof. Honey were not so anxious to heap scorn on his critics (and admittedly some of them have heaped scorn on him), if he were to read their works with an eye to determining what points of common ground he has with linguists, I believe he would be surprised by the extent of common agreement. Language is power, and no one would deny it. However, the status of standard English has everything to do with the power of the groups that come the closest to speaking it as a native language. My objections are to the notions that (a) standard language is arrived at by means other than power relationships; (b) further institutional support in the form of a language academy for the standard language is effective and fair; and (c) denying institutional use of non-standard language is the most effective and fair way of teaching the standard. *Language is power* uses bad logic, based on bad examples, to support ideas that feed linguistic demagoguery; in short, bad science in support of bad public policy.

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**Geoffrey J. Huck & John A. Goldsmith**, *Ideology and linguistic theory: Noam Chomsky and the Deep Structure debates*. London & New York: Routledge, 1995. Pp. x + 186.

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The Deep Structure debates took place when many of today's graduate students were not yet born. Huck & Goldsmith (H&G)'s goal is to set the empirical record straight, erasing various misconceptions which they believe are found in most accounts of the dispute. To this end, they examine both the central empirical arguments and the rhetoric and argumentation of the dispute. They also have an interesting discussion of what happened to the proponents of Generative Semantics after the debates wound down. An appendix contains interviews with four central figures in the debate (Jackendoff, Lakoff, Ross and Postal), which are fascinating, and well worth the price of the book.

H&G choose four sets of empirical arguments and examine them in order to make points about the nature of the argumentation which they feel have not been adequately made elsewhere. The first and best known is Chomsky's (1970) claim in 'Remarks on nominalization' that derived nominals should not be derived transformationally from verbs. Chomsky's central claim was that derivational morphology should not be done with transformations, and a corollary was that transformations should not be allowed to change category. H&G show that the arguments against using transformations for derivational regularities were not utterly conclusive, but had problems of their own and simply set aside the question of how derivational regularities were to be captured. They go on to explain that the issue of category-changing transformations actually had little bearing on the central tenets of generative semantics: perhaps lexical items are inserted at some time after the deepest underlying level. Furthermore, they point out that it is possible to use syntax-like derivations to account for the regularities found in word formation and still be an interpretivist, as work by Lieber, Hale and Keyser, and M. Baker shows.

The second set of arguments involved Chomsky's advocacy of the traditional philosophical view that logical formulae must be quite distinct from natural language sentences. Because of this view, he insisted that semantic representations were quite unsuited as underlying syntactic structures. Furthermore, he was skeptical that the meaning of a linguistic expression could be given an exact representation, isolated from belief and knowledge about the world. McCawley was interested in expanding the mechanisms of logic in constrained ways, so that they would be able to represent meanings of linguistic expressions and then be derived into Surface Structures, but Chomsky was never satisfied with McCawley's attempts to demonstrate that such an enterprise could be possible. H&G show the extent

to which Chomsky and McCawley were at cross-purposes in their debate about this issue. They also point out elsewhere that work on the nature of the level of Logical Form from the late 1970's to the present has in some ways vindicated many of McCawley's claims (although not the central Generative Semanticist claim that semantic representations can be transformed into surface structures).

The third argument was that the Interpretive approach was better because these questions about the nature of semantic representations need not be settled or speculated about in order to proceed with syntactic analysis. Of course, as H&G point out clearly, this view did nothing to solve questions of the relationship between syntax and semantics, it simply set them aside, and it was based upon a rather strong hypothesis about the nature of semantics, namely that semantics does not come into play at all in syntactic analysis.

Finally, Chomsky argued that Generative Semantics, with its global rules and transderivational constraints, effectively allowed an arbitrary relation between form and meaning. I wish that this section had been more detailed. It seems to be assumed that readers know what global rules and transderivational constraints are, but I think most linguists who came of age after the early 1970's have heard the mantra that these devices are too unrestrictive but do not know enough about them to judge the seriousness of the charge. H&G point out that the unrestrictiveness in the Interpretivist accounts of the relevant data got shoved into the poorly-elaborated realm of semantics, so the Interpretivist syntax was more restrictive, but only because the data calling for relaxing the restrictions was effectively left unaccounted for. It would have been helpful to have enough information for the contemporary linguist to be able to compare global rules and transderivational constraints with current proposals for comparing derivations with respect to economy conditions.

In the third chapter, H&G try gently to take linguists to task for their sometimes inflammatory rhetoric, and show how the hyperbolic nature of some of the claims made this debate more acrimonious. This chapter reminded me of the sessions that my female colleagues and I would have in graduate school where we would only half jokingly take each other's papers and globally replace what we thought of as 'girl language' with what we characterized as 'boy language'. 'Therefore, it seems to me that' would become 'Thus I have shown', 'I will give some evidence that' would become 'I will prove that', 'supporting data' would become 'conclusive empirical evidence' and 'my suggestion' would become 'my theory'. I always wondered whether those (of whatever gender) who wrote that way really believed their inflated claims, or were just adopting a certain style. H&G claim that this style was much more prevalent among Interpretivists than among Generativists, which doesn't seem like news to me.

The empirical and rhetorical cases that H&G examine are meant as examples, and they do not rehash all of the arguments detailed in other

works such as Newmeyer (1986) and Harris (1993). They establish, I think successfully, that the Generative Semanticists took reasonable premises and drew from them interesting conclusions which were perhaps questioned but not disproved. They also show clearly that much of the dialogue on these issues was at such cross-purposes that it may be a stretch to call it a debate. These two considerations provide the support for their central point, which is that the demise of Generative Semantics cannot be attributed to fatal empirical flaws.

For me, this central point was a straw man. I never thought that Generative Semantics had been disproved. Rather, I thought it was made to look less promising than the Interpretive approach as a research strategy, for a combination of empirical and sociological reasons. H&G do give a number of quotes from standard sources (Newmeyer 1990, Jackendoff 1983, van Riemsdijk & Williams 1986) to verify the existence of what they call the 'standard view', but even one of these 'standard view' authors says elsewhere, 'It is tempting to think that it was the weight of interpretivist counterattack that led to the demise of generative semantics. While it played an important role, it was not the deciding factor' (Newmeyer 1986: 132). So it is not clear to me that the position H&G argue against is very widely held.

Lurking throughout this book is a confusion that seems also to have been present during the Generative-Interpretive battle: a distinction fails to be made between an interesting empirical result and a result that has the force to cause someone to abandon a system of beliefs in favor of another. In case after case, H&G assert that the arguments against Generative Semantics were not conclusive. But a claim does not need to be conclusively refuted in order to fail to be convincing enough. I do think that H&G understand this, since they quote Thomas Kuhn in a footnote,

But paradigm debates are not really about relative problem-solving ability, though for good reasons they are usually couched in those terms. Instead the issue is which paradigm should in the future guide research on problems many of which neither competitor can yet claim to resolve completely. A decision between alternate ways of practicing science is called for, and in the circumstances that decision must be based less on past achievement than on future promise. (Kuhn 1970: 157)

Although H&G place this footnote on material near the end of the book, Kuhn's point is precisely what kept occurring to me throughout. Generative Semanticists came up with some interesting evidence in favor of lexical decomposition and the interdependence of semantics and syntax, and they tried to show how semantic representations could be transformationally derived into surface structures. But they failed to develop their program in a way that convinced others of its future promise.

In fact, they failed to convince even themselves of its future promise. Newmeyer (1986: 133) maintains that '... the fact is that generative semantics

destroyed itself. Its internal dynamic led to a state of affairs in which it could no longer be taken seriously by anyone interested in the scientific study of human language'. H&G's account shows Newmeyer to be on the mark. Although they would probably argue that it ought to have been taken more seriously, they do show quite clearly that the Generative Semanticists themselves abandoned the enterprise, each for different reasons. The fourth chapter and the interviews reveal that by the mid 1970's, the main thing the Generative Semanticists had in common was a belief that things were wrong with the work of Chomsky and his followers.

The interviews give a vivid sense of how different the personalities of these four are, which makes it easy to see how conflicts could arise. They also show clearly how the Generative Semantics movement disassembled of its own accord in the mid 1970's. More than the body of the book, they give positive examples of how the participants' different disagreements with Chomsky's program resulted in the development of interesting research in alternative frameworks.

H&G try hard at the beginning to establish a common core of beliefs for Generative Semantics and also to extricate the core of beliefs that can be said to hold of Chomsky's program as it has developed from 1955 to the present, and to set this discussion up in a way that will highlight the commonalities. I was looking forward to a book which would illuminate those areas of Generative Semantics which could still hold true and be important, or which would show how congruences between Generative and Interpretive approaches might help to sort out questions that arise in current theories where D-Structure is eliminated, lexical decomposition is explored and semantic or pragmatic properties such as focus, definiteness and topichood seem increasingly to bear on syntactic analyses. I suppose I cannot fault H&G for failing to write the book that I hoped to read, but my hopes were for a new enlightening perspective on the issue, and I was disappointed. I think much of the problem may lie in the authors' great efforts to be evenhanded, which undermined their apparent desire to show that Generative Semanticists had been treated unfairly. The ostensive goal of this book is to show that Generative Semantics was not proven to be false, and as I've said above I find this to be a bit of a straw man. At this point twenty years after the fact, a useful book about the issues would need to address more closely which specific glib dismissals resulted in a loss for our current state of knowledge. This issue is addressed only in the interviews, where in particular Lakoff and Jackendoff discuss the direction their research wound up taking in terms that illuminate which aspects of the Generative Semantics challenge proved valuable to each of them. The subliminal goal of this book, as evidenced by the picture on the book jacket of Chomsky gazing over the red words 'Ideology and linguistic theory' is to decry the weapons used to win this war, presumably as a warning to those unaware that these weapons continue to be deployed. This goal is subliminal in the sense that the authors

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try not to be accusatory, but the evidence seems to be that the Interpretivists were much freer with the verbal nukes (although the Generative Semanticists were probably as free with the hyperbolic claims). H&G's assumption seems to be that rhetoric is really a factor that influences some people's paradigm choices, but I think this is an open question. Personally, I find arrogant rhetoric to be more distracting than persuasive, but I'd be interested to find out whether it has some unconscious effect on me. If rhetoric does not significantly influence paradigm choice, then it's not clear that an examination of it is useful. If it does have a significant influence, then I think H&G should have drawn stronger conclusions about how rhetoric might be used to enhance rather than discourage dialogue among linguists with diverse viewpoints.

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**Johan Rooryck & Laurie Zaring**, *Phrase structure and the lexicon*. Dordrecht: Kluwer, 1996. Pp. vii + 298.

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The task that Rooryck & Zaring undertake, namely to study the relation between the lexicon and phrase-structure, is both ambitious and well worth the trouble, particularly because it comes from a rich tradition (the 'linguistics wars' were fought over that turf) and the issues are still as alive as ever. In part to pursue this goal, the editors of the present volume co-organized a series of lectures, some of which made it into the book. This has the advantage of diversity, but also the disadvantage of an somewhat unfocused, uneven result.

The editors were fortunate to have famous, well-circulated pieces from Sportiche (on clitics) and Stowell (on Tense). Within the general view that non-local dependencies are always of the same type, Sportiche treats clitics as agreement heads, or 'voices', that may or may not have an interpretation, resulting in various intriguing associated properties, some better understood than others. Stowell builds on work by Zagana, and treats Tense as a dyadic predicate that organizes temporal arguments, for which purpose he proposes a new syntactic category; interestingly, he accounts for temporal dependencies in terms of standard syntactic dependencies, such as Control and others, and thus succeeds in dropping any appeals to Reichenbachian 'reference times'. Since these are excellent pieces that have received much attention as manuscripts for over half a decade, I won't comment on them any further.

The most creative piece is contributed by Lebeaux, whose leading question is how a language learner arrives at underlying representations that are massively masked by movement. He goes with a two stage model: one sets the government direction, ignoring passive sentences; a second stage posits the passive trace. Setting aside that second stage, the key is to be able to ignore movement structures. The proposal: throw out representations which bear the mark of functional structure, which by hypothesis always stand in correlation with movement processes. This general idea about movement-as-checking (and vice-versa), now central to Minimalist speculations, forces Lebeaux into some delightfully provocative gymnastics. Naturally, the author wants the child to be able to reach structural decisions on very small domains. To achieve this goal, he first goes into what he calls The PF Conjecture, whereby PF realigns kernel structures in familiar ways, basically cliticizing successive functional heads onto the domain of a head. This results in a small head with a basic code for much structure around it; the child just examines this head, which stores in a paradigm much useful functional information. Furthermore, the maximal alignment ('greatest common denominator') of the PF phrase-marker and the kernel phrase-marker turns out to be one that removes all functional elements from the structure, which Lebeaux suggests is nothing less than telegraphic speech.

Working mainly with Navajo, Speas is concerned with why rich object agreement doesn't always license null objects. She assumes Huang's Generalized Control Rule, which forces co-indexation of an empty pronominal with the closest overt nominal element. Object pro is forced into co-indexation with the overt subject, in violation of Binding Principle B; when the subject is covert, co-indexation is not forced. Another class of examples follow from the idea that agreement morphemes are the actual arguments (relevant associates being looser dependents). When this agreement is 'rich' enough, it induces an intervention effect, liberating the object pro from a forced co-indexation with the subject. The solutions generalize from objects to instances involving dislocations across subjects.



Speas also develops her previous analysis that *pro*-drop inversely correlates with whether the spec of AgrP (currently, *v*) is necessary. She inverts the usual idea that the real verb moves up to *v*; instead, the real verb is up and controls *v*. Since in those structures the *v* projection is necessary, null objects are not licensed. Speas leaves the process of verb raising (as opposed to verbal control) for those situations where null objects are licensed, the rationale being that, then, the *v* projection isn't doing any work.

Another interesting piece is signed by Baker, who within the context of studying the place of thematic hierarchies within the grammar, poses the question of why unaccusative verbs do not enter into Dative Shift alternations, as usual providing all sorts of very interesting data from various languages. The key idea is that C(omplete) F(unctional) C(omplexes) be defined in terms of the notion of 'thematic completeness', which is intended to characterize a VP with all of its lexical arguments. Baker assumes that an Asp(ect) projection between Infl and (the lowest) VP is the normal target of dative shift, and also that themes are hierarchically superior to goals. Then a hierarchically low theme, trying to target the AspP specifier, will have to abandon its CFC, leaving behind an NP-trace that violates Binding Principle A. In contrast, in normal transitive structures, a higher VP shell is generated, above AspP; the verb moves all the way up to this V shell, thereby extending its CFC and licensing dative shifted traces. This allows the trace left to satisfy Binding Theory.

The book also has two programmatic pieces, which re-open some of the wounds that resulted from the 'linguistics wars'. Di Sciullo suggests that (derivational) morphological selection is configurational, and stated in terms of X' positions – in the process predicting important argument structure asymmetries. Any such prediction is indeed welcome, but Di Sciullo insists on the sort of syntactic parallelism that was at the core of the arguments for generative semantics (thus finding an analogue of the condition against Vacuous Quantification, or the process of feature checking in Minimalist terms, within the word domain). The rhetoric emphasizes the parallelism between morphology and syntax, specifically treating words as (PF, LF) pairs analogous to their sentential counterparts. Nonetheless, the specific configurations where lexical selection seems possible is, as Di Sciullo shows, not the same that Minimalism allows in similar syntactic conditions (e.g., spec of complement of a complement to a head). This would have been fine (morphology isn't syntax), except for the fact that the author explicitly relies on the assumption that 'the grammar provides the X' configuration' (84). If Chomsky is right in his Bare Phrase Structure, what provides the X' configuration is Merge, and Merge is not defined for units smaller than words (and defining it relevantly would be generative semantics). Second, Minimalism relies on morphology being different from syntax (e.g. for Linearization); the system collapses without an independent Word Interpretation component. What is then one to make about aligning the two

systems? Minimalism could, of course, be wrong, but then Di Sciullo's theory should perhaps distance itself from it; otherwise, a contradiction looms in the background.

In a very-well written paper, Kratzer embarks on a worthy program: that verbs require both neo-Davidsonian and standard ordered association with their arguments, the division being manifestly syntactic and correlating with the traditional external/internal cut (which she carefully argues for). This is a fascinating prospect, but unfortunately this paper is just too brief to go into the juicy stuff, some of which Kratzer herself mentions ('to tell a good story about the addition of external arguments in natural languages' (111)). This might be just a matter of taste, but in spite of her insistence in fn. 8 that the proposal is different (in fact incompatible) with Hale & Keyser's, this reviewer has failed to see how a generous reading of that piece wouldn't converge with the present thesis. I say this not just as a point of philological accuracy, but most importantly because it would seem that, in particular, Kratzer's ideas face exactly the same difficulties that Fodor & Lepore have recently pointed out with Hale & Keyser's view; in particular, the deep philosophical issue of Atomism is right there to wrestle with, if as the author suggests 'the agent argument of a verb is not really one of its arguments anymore' (131). This, of course, is of vast consequence, and touches directly on what it means to be a word, and exactly how, if not lexically, argument dependency is to be distinguished from clausal dependency (particularly of the bi-clausal reduction sort).

It was hard to follow Sadock's short (less than 12 page) paper. As I understood it, it was an argument for a 'separate dimension of linguistic organization', based on English and West Greenlandic data. For Sadock, a grammar is a collection of phrase structure grammars (one for syntax, one for semantics, and one for morphology), thought of as an alternative to transformational grammar. Since I confess to know nothing about this model, I will only comment on a couple of reasons Sadock mentions in his paper for preferring it to traditional alternatives. Allegedly, a multiple PSG is more realistic than 'TG, at least as it is commonly practised nowadays, the levels DS, LF, and even SS [being] quite abstract and unavailable to intuition' (175). In contrast, in multiple PSG 'only genuine morphological information is to be found in the morphological representations and only genuine semantic information in the semantic structures' (175). The question is: Who decides, at least prior to investigation, on what is genuine this or genuine that? Similarly, What relevance does it have for a scientific theory that its constructs be abstract? Generative grammar uses linguistic intuition as a way to gather data for a phenomenon which, in its base, is obviously mental. A priori, being close or far away from intuitions has about as much importance as, for a theory of physical particles, being close or far away from describing the Moon; if the descriptive work's done, what matters is not distance to available data, but depth of explanation. Sadock also mentions

‘clarity, formality, and mathematical handiness’ (174) as a virtue of his alternative to current models; one wonders whether a physicist would argue for the Copernican system, as opposed to current (highly abstract) alternatives, on such grounds.

I didn’t understand Croft’s paper, or his initial question. ‘What’s a head?’. As I was (perhaps poorly) trained, a head is whatever we need a head to be, assuming the concept is even useful. But Croft is concerned with ‘how... we give substance to our intuitions about the identity of a basic grammatical concept, such as headhood... What grounds do we have for choosing some syntactic criterion for that concept and discarding others?’ (36). Again, one imagines a physicist or a biologist asking such questions about atoms or cells; here, obviously, answers are contingent to theories, and success is determined a posteriori in terms of the usual metrics of coverage, depth, elegance, and so forth. Yet, Croft is interested in establishing how ‘we know that we have captured the intuition that was intended by the original coiners of the term’ (36), which as far as I can see is like trying to know what Democritus really had in mind when he cooked up the idea of an atom, presumably to then go on to adapt our present conception of particle interactions to such important considerations. It is actually curious to see what could possibly be the knock-down criteria that this paper is looking for; one that I understood likened the (head, dependent) relation to a mathematical function. The problem is that most functions one can think of (and certainly this one) have a left-inverse; so you may define either head (complement) as your function, or complement (head) – there plainly is no insight to be gained from such mathematical gimmicks. If there is something good to be said about this paper (from my admittedly narrow perspective), it is this: it has loads of information about the traditional notion of head, and it questions the usual criteria for finding out what a head is.

All in all, I think this book is worth its price for the professional syntactician or lexical semanticist, or for whoever is interested in the fascinating topic that it approaches.

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Mark Steedman, *Surface structure and interpretation* (*Linguistic Inquiry Monographs* 30). Cambridge, MA: MIT Press, 1996. Pp. xiv + 126.

Reviewed by SUSAN STEELE, University of Connecticut

Before I was asked to review this volume, I received a complimentary copy from the author with a note advising me that I shouldn't expend too much effort reading it because the next book would be much more fun. I'm glad that I didn't take his advice.

This isn't to say that *Surface structure and interpretation* is a load of laughs. Although Steedman's analytical precision is a welcome respite from the hand-waving that too often passes for a formal account, the book is unflinchingly technical. The framework is Combinatory Categorical Grammar (CCG), a fact that yields categories like that in (1) (Steedman's (12) in chapter 2); abbreviatory conventions like that in (2) (Steedman's (42) in chapter 2); and a set of combinatory rules that includes not only functional application, but also three kinds of operations mapping functions into functions – functional composition, type-raising and functional substitution. Steedman's formalization of these last (his (81), (84) and (92) in chapter 2) are provided in (3), (4) and (5) below.

- (1)  $\text{eats} := (S \backslash NP_{3s}) / NP : \lambda x. \lambda y. \text{eat}'xy$
- (2) *The \$ Convention*  
For a category  $\alpha$ ,  $\{\alpha/\$ \}$  (respectively,  $\{\alpha \backslash \$ \}$ ) denotes the set containing  $\alpha$  and all rightward (leftward) functions into a category in  $\{\alpha/\$ \}$  ( $\{\alpha \backslash \$ \}$ ).
- (3) *Forward composition*  
 $X/Y : f \quad Y/Z : g \Rightarrow_B X/Z : \lambda z. f(gz)$
- (4) *Type-raising*
  - (a)  $X : a \Rightarrow_T T/(T \backslash X) : \lambda f. fa$
  - (b)  $X : a \Rightarrow_T T \backslash (T/X) : \lambda f. fa$

where  $X$  is an argument type
- (5) *Backward crossed substitution*  
 $Y/Z : g \quad (X \backslash Y)/Z : f \Rightarrow_s X/Z : \lambda x. fx(gx)$   
where  $X = S \backslash \$$

None of these are, of course, peculiar to this work. The categories are relatively standard categorial grammar fare. The first part of the category for *eats* in (1) specifies its syntax: it takes an NP on its right to make something that takes a third person singular NP on its left to make a sentence – in other words, *eats* is a transitive verb that requires a third person singular subject. The second part of (1) uses the lambda notation to specify the semantics of *eats* as a two-place predicate. *The \$ Convention* in (2) will be familiar from Steedman's previous work; this convention makes it possible to schematize over verbs with different numbers of arguments. And, finally, few categorial

grammars have a combinatory arsenal which is limited to application; most involve combinatory rules that can create functors. For example, the type-raising rule in (4) takes the NP *apples* and yields a category that can combine with the transitive verb *eats*; because of the category of *eats* and because the symbol T is a variable, the result is an intransitive verb.

$$(6) \quad \frac{\frac{\text{eats}}{(S \backslash NP) / NP} \quad \frac{\text{apples}}{T \backslash (T / NP)}}{S \backslash NP}$$

In fact, Steedman is at pains to show that, even with the addition of type raising and substitution, the combinatory options are, in fact, a small and tightly constrained set.

As non-exotic within categorial grammar as this technical apparatus might be, this brief taste will suggest that *Surface structure and interpretation* should probably not be a reader's first experience with Combinatory Categorial Grammar specifically and with categorial grammar more generally. (Steedman 1985 or a selection from Oehrle, Bach & Wheeler 1988 are reasonable places to start.)

Given experience with categorial grammar and comfort with Steedman's implementation of it, the beauty of this book derives from its compact, yet comprehensive and elegant treatment of the 'the dependencies that the metaphor [of movement] describes' (1). Consistent with the division of these dependencies into two types, the bulk of Steedman's argument and analysis is to be found in two chapters, one focusing on bounded dependencies (chapter 2) and the other on unbounded dependencies (chapter 3). In order to treat binding, control and *tough*-movement, chapter 2 introduces the fundamental properties of Combinatory Categorial Grammar, primarily its combinatory options and the two principles that limit the kinds of rules they allow. The treatment of unbounded dependencies in chapter 3 is the book's core and the fundamental idea is that the foundation laid to handle bounded dependencies accommodates as well such unbounded phenomena as extraction asymmetries, ECP effects, and interactions of extraction and the binding theory.

These two chapters are framed by an effective introduction and an all too brief conclusion. The introduction emphasizes the contrast between bounded and unbounded dependencies. 'The striking thing about unbounded movements ... is that they seem to preserve the canonical linear order of the language ... The striking thing about bounded dependencies is that they seem to depend more directly on the kind of relations that one would expect to find embodied in the meaning representation' (1). The conclusion speaks more to the subtext of the intervening two chapters, minimizing the differences between CCG and Chomsky's work on the one hand and more traditional categorial grammar work on the other. The following three sentences (90) are illustrative of the connections that Steedman has in mind.

To the extent that no other levels of representation intervene, CCG can therefore be viewed as a ‘minimalist’ theory in the sense of Chomsky (1993, 1995) although we have made no use here of ‘economy principles’ attending ‘movement’ according to that hypothesis. It might appear that CCG is not minimalist in the stronger, ‘monostratal,’ sense of Dowty (1991) and much other work in the tradition of Montague (1974), which eschews the intrinsic use of any logical form, including predicate-argument structure, mediating between the derivation and model-theoretic semantics. However, we have noted that surface structure in the present sense is not a representational level at all.

*Surface structure and interpretation* has the elegance one associates with Steedman’s work. His knowledge of the relevant literature – both within and without categorial grammar – is near-encyclopedic and his command of the linguistic phenomena at issue is difficult to match. And, in spite of his warning, I have to confess that I found this book lots of fun. A major source of fun is the liberating sense of syntactic possibilities it engenders, if one is not tied to standard notions of constituency and is willing to consider non-doctrinaire proposals. For example, Combinatory Categorial Grammar allows modals, like *might*, to be analyzed as subject-control verbs – (S\NP)/VP (i.e. something that combines with a VP on its right to yield something that combines with an NP on its left to yield a sentence). The consequence is that modals combine by functional composition with non-finite verbs (e.g., *eat* VP/NP, i.e. something that combines with an NP on its right to yield a VP) to yield finite verbs (e.g., *might eat* (S\NP)/NP). Whether any particular proposal turns out to be right or not matters much less than the idea that such options are possible. But one result is that the book is filled with constructions like *packages which I sent and which you carried to Philadelphia*, the analyses of which Steedman’s framework accommodates non-problematically and almost in passing.

I also enjoyed the absence of fanfare in the presentation. The understated style is a pleasure to read, although it is possible that the lack of major hoopla will obscure the results for some readers. Chapter 3 could be the major offender on this score. The organization of this chapter is driven by the necessity to respond to ‘a number of published and unpublished criticisms of CCG’ (xii). The result is essentially a list of construction types that have been identified as problematic for one or another reason, accompanied by their treatment under the grammatical system introduced in chapter 2. Unless one is intimately familiar with the literature behind this discussion, chapter 3 has too few signposts marking the significance of Steedman’s analyses and arguments.

Stemming from my experience with a language very different from those Steedman draws on, I have to confess, however, that I find some aspects of Combinatory Categorial Grammar less than entirely satisfactory. One

problem has to do with the primitives invoked in the categories. Steedman notes that ‘symbols like *S*, *NP*, and *S/NP* can, and in fact must, be regarded as complex objects’ (10) and he acknowledges this fact at many points by employing subscripts (as in the category of *eats* in (1) above). The problem is that, lacking a full-blown treatment of the structure that *NP* and the rest abbreviate, it is difficult to determine whether the subscripts are arbitrary or not. More critically, if one were to take the subscripts seriously, the fundamental character of the categories – and, thus, of the analyses – might be substantially different. It is a non-trivial task to decompose categorial symbols like *S* and *NP* to their atoms. However, my work with Luiseño, a Uto-Aztecan language spoken in Southern California, led me to conclude that an acceptance of the non-atomic nature of standard category symbols eliminates their necessity (see Steele 1990). The issue of categorial primitives permeates the literature and this book is clearly not the place to address it. Yet a low-level concern on this score takes the edge off the general pleasure that Steedman’s account induces.

Another nagging issue has to do with the place of order in a grammar. Consistent with the approach of most categorial grammarians, Steedman’s categories are directional, thus directly coding ordering relations in the lexical item and also instantiating the presumption that order is a linguistic primitive. There is no question, of course, that the words in any sentence token are fixed. There is also no question that some languages have far more word order freedom than does English. Hale’s work on Warlpiri (e.g. 1981) is perhaps the most salient example, but Luiseño exhibits many of the same properties – to wit, a fixed second position clitic complex and freedom of constituents around it. For example, all of the reorderings of (7) in (8) are possible and appear to count simply as repetitions, but (9) where the clitic complex occurs other than in second position is unacceptable.

- (7) hengeemal up                      hunwuti ’ariq  
      boy                      CLITIC.COMPLEX bear.OBJ is.kicking  
      ‘The boy is kicking the bear.’
- (8) hengeemal up ’ariq hunwuti  
      hunwuti up hengeemal ’ariq  
      hunwuti up ’ariq hengeemal  
      ’ariq up hengeemal hunwuti  
      ’ariq up hunwuti hengeemal
- (9) \* hengeemal hunwuti up ’ariq

Insofar as Combinatory Categorial Grammar turns on the necessity of specifying order in the lexical category, some skepticism is healthy.

In the final analysis, however, given the quality of *Surface structure and interpretation*, we should look forward with anticipation to Steedman’s next book – and it doesn’t even have to be more fun.

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**Ton van der Wouden**, *Negative contexts: collocation, polarity and multiple negation* (Routledge Studies in Germanic Linguistics). London & New York: Routledge, 1997. Pp. xi + 301.

Review by LJILJANA PROGOVAC, Wayne State University

## I. INTRODUCTION

The author's main claim in the book is that Ladusaw's (1980) concept of Downward Entailment/Monotonicity (DM) is at work not only with negative polarity licensing, but also with other phenomena previously described as involving negation: negative concord, litotes, denial, and emphatic negation, although the author does not actually engage in proving this.<sup>1</sup> The author describes his book as a 'balance between analysis and description' in which he 'didn't want to hide all the counterexamples' (ix).<sup>2</sup> I would say that the book is primarily a survey of the relevant literature and

[1] In his own words: 'what I would like to claim – but don't have time or space to prove – is that in all natural language phenomena where negation seems to be at work, description and explanation in terms of DM is both more fruitful and more correct' (92).

[2] My interpretation of the quote in the text (perhaps in error) is that approaches based on analysis tend to 'hide all counterexamples'. To me, this seems like an impossible task: (counter)examples are created and discovered, not offered on a tray or hidden away. They are created by analyses that make clear predictions; they are to be discovered by both creators and readers. I say we should all be grateful to the creators of clear and falsifiable analyses, even when they are not perfect. Without them, there would be no new (counter)examples to discover (and write papers or books on), and no new roads to lead us forward.



descriptive generalizations. It does not go deep enough into any of the theories discussed to be able to effectively choose between them. Instead, the book is rather extensive in its coverage of the data. It provides numerous examples pertaining to various topics on negation (from various languages, but most notably Dutch), as well as references to a number of authors who have dealt with the issues.

I will illustrate this general point about the book in section 3, by focusing on one of the described phenomena: paratactic negation (Part III, section 2). Section 4 deals with the role of semantics vs. syntax in the author's view. Due to the wide variety of topics covered, it is hard to see what the common thread is that unites all the sections of the book. This is especially true of the inclusion of a chapter on Collocation (Part I of the book), which I discuss briefly in section 2.

## 2. COLLOCATION AND POLARITY

The author's passionate claim is that polarity should be subsumed under collocations, which, in turn, should receive a 'respectable' place in language theory (54–55).

The usefulness of subsuming negative polarity under collocation is not made clear, however. In (1) below, the polarity dependency is between *nobody* and *any*, which can never form a constituent. The collocational dependency in (2) can at least be paraphrased as a constituent, as in (3). As I understand the term 'collocation', it provides instructions as to which elements can be successfully combined to form larger units/phrases. If this is correct, then the dependency between *nobody* and *any* in (1) cannot be that of collocation, especially given the fact that this dependency is not clause-bound. It is just as unlikely that polarity dependencies are collocational as it is unlikely that bound pronouns are collocationally related to their antecedents (4).

- (1) **Nobody** said that you should read **any** of these books.
- (2) His **hair** is **blond**.
- (3) He has [**blond hair**].
- (4) **John<sub>i</sub>** said that Peter should read **his<sub>i</sub>** books.

There is no doubt that one can come up with a definition of collocation that would subsume polarity, but then I must wonder about the usefulness of this move. Would it contribute to a better understanding of either polarity or collocations, and how? Moreover, as the author repeatedly and rightly points out, collocations are already a heterogeneous class (if a class at all), and adding polarity to the bunch would render a uniform explanation even more unlikely.

## 3. PARATACTIC NEGATION

To give a taste for the book and its general approach, I select the discussion of paratactic negation (Part III, section 2). This choice makes sense for two reasons: it is illustrative of both the strengths and the weaknesses of the whole book; the topic is manageable in terms of the space limits of this review.

The author offers cross-linguistic examples of ‘paratactic’ negation, i.e. occurrences of superfluous negative particles.

- (5) Nature forbedeth that **no** man make hymself riche. (from Chaucer)
- (6) Dubito **ne** veniat. (Latin)
- doubt-1SG NEG come-3SG
- ‘I doubt that he will come.’

The coverage of cross-linguistic examples in this section is rich and useful. It shows that this is not an isolated and marginal phenomenon, but rather a deep-rooted property of human languages. The phenomenon itself has been known and described at least since Jespersen (1917). Syntactic explanations have been offered, too. For example, there have been arguments for the existence of a null negative Operator in the Comp positions of clauses licensing negative polarity, but which are not overtly negative (see Progovac 1988, 1994; Laka 1990).

- (7) I doubt [<sub>CP</sub> **Op** that he will invite anybody].

This particular syntactic analysis makes the correct prediction that such licensing will only occur in the clausal complement, given that the negative feature is only realized in a clausal functional projection:

- (8) \*I doubted any of the speakers at the meeting yesterday.  
(cf. I did not trust any of the speakers at the meeting yesterday.)

The author compares this analysis with his own proposal: that paratactic negation is licensed in the same positions as polarity is: in DM contexts.<sup>3</sup> He claims that the syntactic analysis is circular (201): ‘an abstract element is postulated to explain a fact or a group of facts, but these facts are the only argument in favor of the postulated element’. It turns out that this is not true: the analysis postulates an abstract operator in order to account for something else: polarity licensing in non(overtly)-negative contexts. The appearance of paratactic negation in the environments in which polarity is otherwise licensed is therefore independent support, or overt support, for the operator; there is nothing circular about it. In fact, it explains why these phenomena go together: it follows from the former, but is only stipulated by

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[3] In fact, the author makes a slightly different claim, namely that the negation itself is a negative polarity item (204), which I cannot accept for many reasons. One is that negation in general is not a dependent element requiring licensing, which is the basic criterion for identifying negative polarity items.

the latter, that paratactic negation will occur basically in those contexts in which polarity is licensed.

As the author illustrates, not all polarity contexts and not in all languages allow paratactic negation. This is expected under a syntactic approach. Functional categories have an undeniable tendency to remain silent in different places, in different languages. Pesetsky (to appear) proposes a soft constraint to this effect (in the Optimality framework), which he calls Telegraph:

(9) Do not pronounce function words.

This constraint can be seen as following from Economy, basically saying that function words are not pronounced unless their presence is required by some principle of Grammar. For example, there are positions in which complementizers are obligatory, optional or impossible, and these positions vary across languages and times. The same goes for overt pronouns, articles, conjunctions, etc.

Given that paratactic negation is redundant, as is usually the case with complementizers, conjunctions or overt pronouns, the possibility vs. impossibility of its occurrence in various constructions across various languages must be a matter of other principles. Understanding these principles is just as important as understanding the distribution of complementizers or pronouns. A better understanding of paratactic negation may shed light on the broader issue of the overtness of functional categories.

The virtue of this chapter, to my mind, is in providing a glimpse into variation by offering a host of cross-linguistic data on paratactic negation, which may stimulate further research on this topic. The drawback is that it does not present in sufficient detail the competing analyses, making it impossible to effectively compare them, and ultimately reach a better understanding of the phenomenon in question.

#### 4. SYNTAX VS. SEMANTICS

The author's claim throughout the book is that polarity and related phenomena receive semantic explanations, but this claim remains unsupported (see footnote 1 of this review). The author explicitly claims that 'not all subtle details of the many proposals for syntactic solutions to the problems ... can be found' in his book (x). Another example where the author briefly mentions the significance of syntax is when it comes to c-command, in his section on scope (chapter 7). The contrast below cannot really be ascribed to anything else but to crude structural positioning among elements:

(10) \*Anyone has never been able to solve this problem.

(11) Never has anyone been able to solve this problem.

The author recognizes this in his discussion, but dismisses the whole issue by saying: ‘As I am not offering syntax here, I remain agnostic as regards to precise details’ (165). Most will agree that the question about polarity is no longer whether it is syntax or semantics, but rather how much of it is syntax and how much of it is semantics, and how they fit together. I don’t think that it is possible anymore to have an explanation/analysis of polarity which does not address this fundamental question.

## 5. RESIDUAL ISSUES

### 5.1 *Recommended sections*

Sections that I would recommend to the reader interested in negation are the following:

- the already discussed section on paratactic negation, and, more generally, Part III of the book, which contains a wealth of new data on multiple negation;
- chapters 4 and 5, Part II, which offer classifications of various polarity items, useful for bringing up variation among polarity items.

### 5.2 *Sections that require caution*

The reader should perhaps be alerted to some inaccuracies in descriptive generalizations or in the use of terminology. For example, the well-established descriptive generalization about PPIs (Positive Polarity Items) is that they are illicit only in the scope of clausemate negation (e.g. Ladusaw 1980), and can occur in the scope of superordinate negation or other DM operators, contrary to what the author says (47; 61). If the author disagrees with this established descriptive generalization, he should explicitly address the question.

Also, the author uses the terms ‘bridge’ verbs and ‘negative raising’ verbs interchangeably (223), which is contrary to the usual practice (see Erteschik (1973) for definition of the former and Horn (1978) for the latter). This makes the discussion of clausal vs. cross-clausal licensing of polarity items confusing (144, footnote).

The asterisk should be removed from the Serbo-Croatian example (187c) on p. 134: the example is grammatical.

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